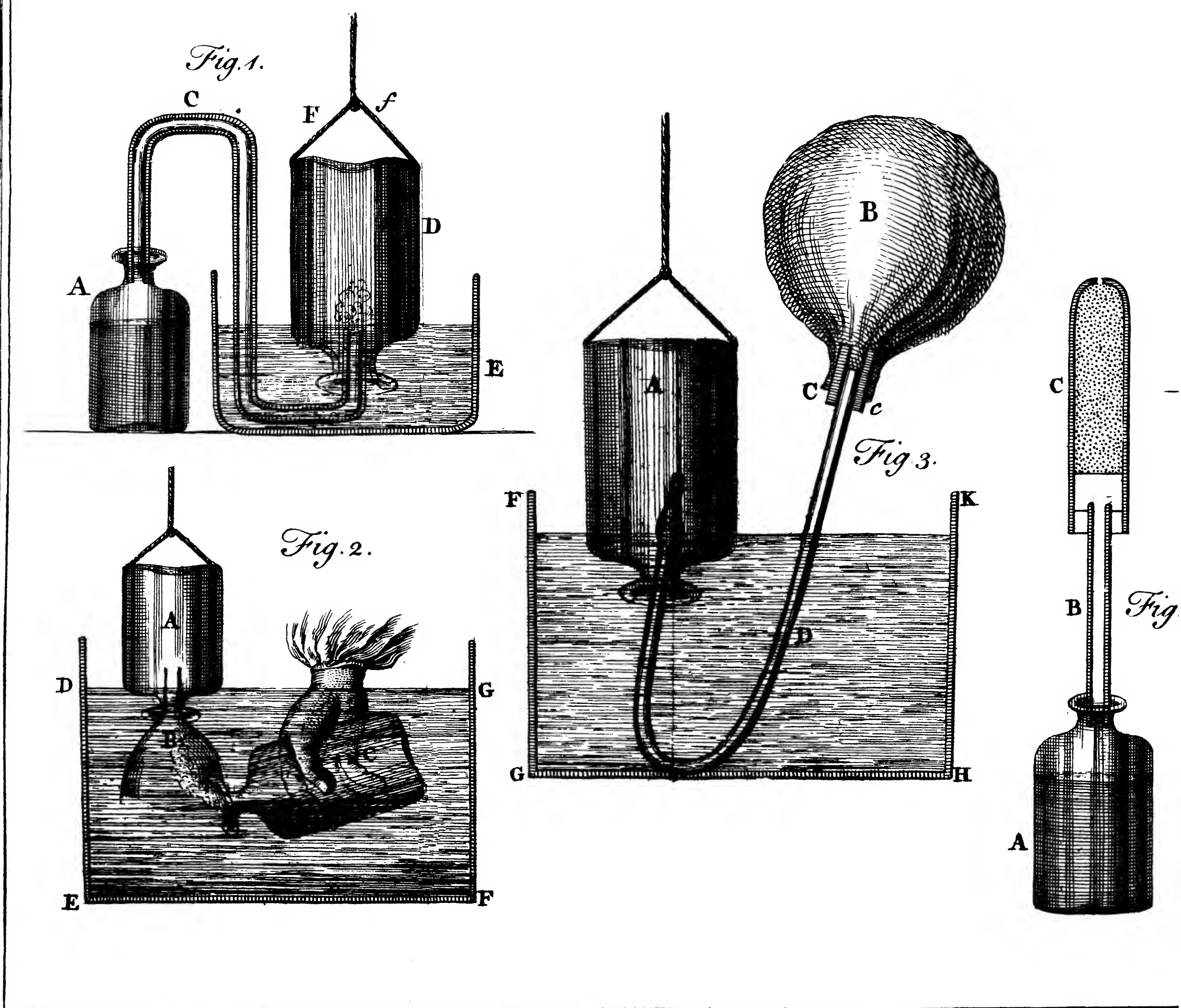
Philos. Trans. Vol. LVI. TAB. VII*. p. 141.



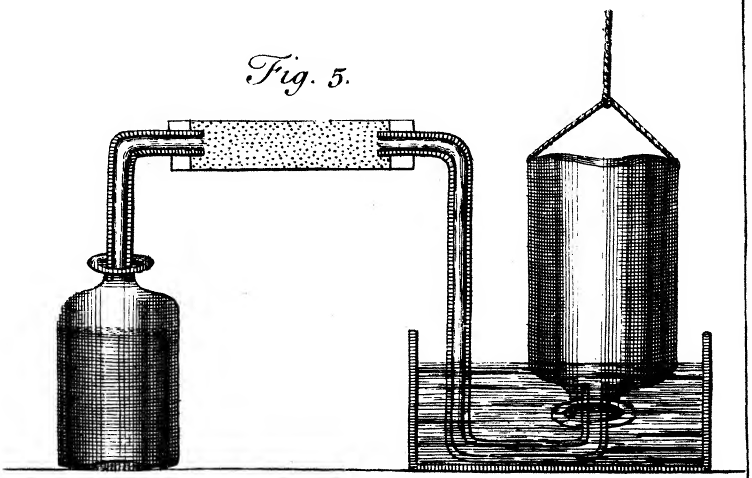


Fig. 5.

Fig. 4.

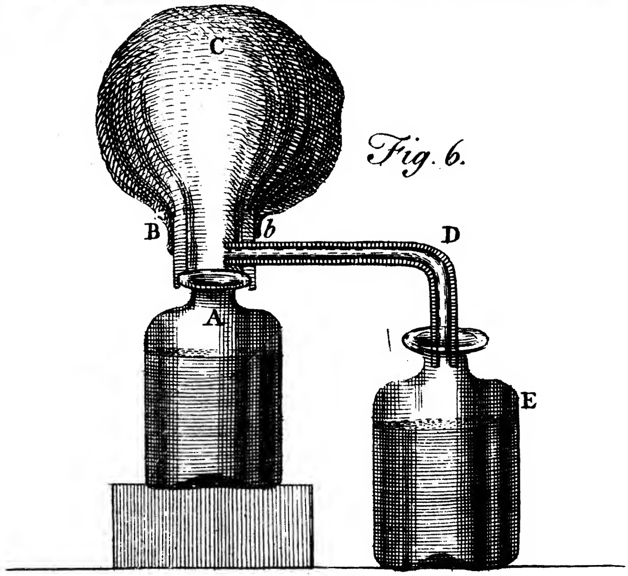


Fig. 6.

action of the diaphragm and abdominal muscles, and of the fotus of their generally neighbouring parts: requisites, as has been imagined, towards the carrying on their several functions, for the benefit of the animal oeconomy.

George Carlisle.

Received May 12, 1766.

XIX. *Three Papers, containing Experiments on factitious Air, by the Hon.* Henry Cavendish, *F. R. S.*

Read May 29, Nov. 6, and Nov. 13, 1766.

BY factitious air, I mean in general any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art.

By fixed air, I mean that particular species of factitious air, which is separated from alcaline substances by solution in acids or by calcination; and to which Dr. Black has given that name in his treatise on quicklime.

As fixed air makes a considerable part of the subject of the following papers; and as the name might incline one to think, that it signified any sort of air which is contained in other bodies in an unelastic form; I thought it best to give this explanation before I went any farther.

Before

Before I proceed to the experiments themſelves, it will be proper to mention the principal methods uſed in making them.

In order to fill a bottle with the air diſcharged from metals or alcaline ſubſtances by ſolution in acids, or from animal or vegetable ſubſtances by fermentation, I make uſe of the contrivance repreſented in TAB. VII. Fig. 1. where A repreſents the bottle, in which the materials for producing air are placed; having a bent glaſs tube C ground into it, in the manner of a ſtopper. E repreſents a veſſel of water. D the bottle to receive the air, which is firſt filled with water, and then inverted into the veſſel of water, over the end of the bent tube. F*f* repreſents the ſtring, by which the bottle is ſuſpended. When I would meaſure the quantity of air, which is produced by any of theſe ſubſtances, I commonly do it by receiving the air in a bottle, which has diviſions marked on its ſides with a diamond, ſhewing the weight of water, which it requires to fill the bottle up to thoſe diviſions: but ſometimes I do it by making a mark on the ſide of the bottle in which I have received the air, anſwering to the ſurface of the water therein; and then, ſetting the upright, find how much water it requires to fill it up to that mark.

In order to transfer the air out of one bottle into another, the ſimpleſt way, and that which I have ofteneſt made uſe of, is that repreſented Fig. 2. where A is the bottle, into which the air is to be transferred: it is ſuppoſed to be filled with water and inverted into the veſſel of water DEFG, and ſuſpended there by a ſtring: the line DG is the ſurface of the water: B repreſents a tin funnel held under the mouth of the bottle: C repreſents the inverted bottle, out of which the

the air is to be transferred; the mouth of which is lifted up till the air runs out of it into the funnel, and from thence into the bottle A.

In order to transfer air out of a bottle into a bladder, the contrivance Fig. 3. is made uſe of. A is the bottle out of which the air is to be transferred, inverted into the veſſel of water FGHK: B is a bladder whoſe neck is tied faſt over the hollow piece of wood C*c*, ſo as to be air-tight. Into the piece of wood is run a bent pewter pipe D, and ſecured with lute*. The air is then preſſed out of the bladder as well as poſſible, and a bit of wax E ſtuck upon the other end of the pipe, ſo as to ſtop up the orifice. The pipe, with the wax upon it, is then run up into the inverted bottle, and the wax torn off by rubbing it againſt the ſides. By this means, the end of the pipe is introduced within the bottle, without ſuffering any water to get within it. Then, by letting the bottle deſcend, ſo as to be totally immerſed in the water, the air is forced into the bladder.

The weights uſed in the following experiments, are troy weights, 1 ounce containing 480 grains. By an ounce or grain meaſure, I mean ſuch a meaſure as contains one ounce or grain Troy of water.

* The lute uſed for this purpoſe, as well as in all the following experiments, is compoſed of almond powder, made into a paſte with glue, and beat a good deal with a heavy hammer. This is the ſtrongeſt and moſt convenient lute I know of. A tube may be cemented with it to the mouth of a bottle, ſo as not to ſuffer any air to eſcape at the joint; though the air within is compreſſed by the weight of ſeveral inches of water.

EXPERIMENTS on FACTITIOUS AIR.

PART I.

Containing Experiments on Inflammable Air.

I Know of only three metallic ſubſtances, namely, zinc, iron and tin, that generate inflammable air by ſolution in acids; and thoſe only by ſolution in the diluted vitriolic acid, or ſpirit of ſalt.

Zinc diſſolves with great rapidity in both theſe acids; and, unleſs they are very much diluted, generates a conſiderable heat. One ounce of zinc produces about 356 ounce meaſures of air: the quantity ſeems juſt the ſame whichſoever of theſe acids it is diſſolved in. Iron diſſolves readily in the diluted vitriolic acid, but not near ſo readily as zinc. One ounce of iron wire produces about 412 ounce meaſures of air: the quantity was juſt the ſame, whether the oil of vitriol was diluted with $1\frac{1}{2}$, or 7 times its weight of water: ſo that the quantity of air produced ſeems not at all to depend on the ſtrength of the acid.

Iron diſſolves but ſlowly in ſpirit of ſalt while cold: with the aſſiſtance of heat it diſſolves moderately faſt. The air produced thereby is inflammable; but I have not tried how much it produces.

Tin was found to diſſolve ſcarce at all in oil of vitriol diluted with an equal weight of water, while cold: with the aſſiſtance of a moderate heat it diſſolved ſlowly, and generated air, which was inflammable: the quantity was not aſcertained.

Tin diſſolves ſlowly in ſtrong ſpirit of ſalt while cold: with the aſſiſtance of heat it diſſolves moderately faſt.

faſt. One ounce of tinfoil yields 202 ounce meaſures of inflammable air.

Theſe experiments were made, when the thermometer was at 50° and the barometer at 30 inches.

All theſe three metallic ſubſtances diſſolve readily in the nitrous acid, and generate air; but the air is not at all inflammable. They alſo unite readily, with the aſſiſtance of heat, to the undiluted acid of vitriol; but very little of the ſalt, formed by their union with the acid, diſſolves in the fluid. They all unite to the acid with a conſiderable effervefence, and diſcharge plenty of vapours, which ſmell ſtrongly of the volatile ſulphureous acid, and which are not at all inflammable. Iron is not ſenſibly acted on by this acid, without the aſſiſtance of heat; but zinc and tin are in ſome meaſure acted on by it, while cold.

It ſeems likely from hence, that, when either of the above-mentioned metallic ſubſtances are diſſolved in ſpirit of ſalt, or the diluted vitriolic acid, their phlogiſton flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are diſſolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiſton unites to part of the acid uſed for their ſolution, and flies off with it in fumes, the phlogiſton loſing its inflammable property by the union. The volatile ſulphureous fumes, produced by uniting theſe metallic ſubſtances by heat to the undiluted vitriolic acid, ſhew plainly, that in this caſe their phlogiſton unites to the acid; for it is well known, that the vitriolic ſulphureous acid conſiſts of the plain vitriolic acid

 united

united to phlogiſton *. It is highly probable too, that the ſame thing happens in diſſolving theſe metallic ſubſtances in the nitrous acid; as the fumes produced during the ſolution appear plainly to conſiſt in great meaſure of the nitrous acid, and yet it appears, from their more penetrating ſmell and other reaſons, that the acid muſt have undergone ſome change in its nature, which can hardly be attributed to any thing elſe than its union with the phlogiſton. As to the inflammable air, produced by diſſolving theſe ſubſtances in ſpirit of ſalt or the diluted vitriolic acid, there is great reaſon to think, that it does not contain any of the acid in its compoſition; not only becauſe it ſeems to be juſt the ſame whichſoever of theſe acids it is produced by; but alſo becauſe there is an inflammable air, ſeemingly much of the ſame kind as this, produced from animal ſubſtances in putrefaction, and from vegetable ſubſtances in diſtillation, as will be ſhewen hereafter; though there can be no reaſon to ſuppoſe, that this kind of inflammable air owes its production to any acid. I now proceed to the experiments made on inflammable air.

I cannot find that this air has any tendency to loſe its elaſticity by keeping, or that it is at all abſorbed, either by water, or by fixed or volatile alcalies; as I have kept ſome by me for ſeveral weeks in a bottle inverted into a veſſel of water, without any ſenſible

* Sulphur is allowed by chymiſts, to conſiſt of the plain vitriolic acid united to phlogiſton. The volatile ſulphureous acid appears to conſiſt of the ſame acid united to a leſs proportion of phlogiſton than what is required to form ſulphur. A circumſtance which I think ſhews the truth of this, is that if oil of vitriol be diſtilled, from ſulphur, the liquor, which comes over, will be the volatile ſulphureous acid.

decreaſe of bulk; and as I have alſo kept ſome for a few days, in bottles inverted into veſſels of ſope leys and ſpirit of ſal ammoniac, without perceiving their bulk to be at all diminiſhed.

It has been obſerved by others, that, when a piece of lighted paper is applied to the mouth of a bottle, containing a mixture of inflammable and common air, the air takes fire, and goes off with an exploſion. In order to obſerve in what manner the effect varies according to the different proportions in which they are mixed, the following experiment was made.

Some of the inflammable air, produced by diſſolving zinc in diluted oil of vitriol, was mixed with common air in ſeveral different proportions, and the inflammability of theſe mixtures tried one after the other in this manner. A quart bottle was filled with one of theſe mixtures, in the manner repreſented in Fig. 2. The bottle was then taken out of the water, ſet upright on a table, and the flame of a lamp or piece of lighted paper applied to its mouth. But, in order to prevent the included air from mixing with the outward air, before the flame could be applied, the mouth of the bottle was covered, while under water, with a cap made of a piece of wood covered with a few folds of linnen; which cap was not removed till the inſtant that the flame was applied. The mixtures were all tried in the ſame bottle; and, as they were all ready prepared, before the inflammability of any of them was tried, the time elapſed between each trial was but ſmall: by which means I was better able to compare the loudneſs of the ſound in each trial. The reſult of the experiment is as follows.

With one part of inflammable air to 9 of common air, the mixture would not take fire, on applying the lighted paper to the mouth of the bottle; but, on putting it down into the belly of the bottle, the air took fire, but made very little ſound.

With 2 parts of inflammable to 8 of common air, it took fire immediately, on applying the flame to the mouth of the bottle, and went off with a moderately loud noiſe.

With 3 parts of inflammable air to 7 of common air, there was a very loud noiſe.

With 4 parts of inflammable to 6 of common air, the ſound ſeemed very little louder.

With equal quantities of inflammable and common air, the ſound ſeemed much the ſame. In the firſt of theſe trials, namely, that with one part of inflammable to 9 of common air, the mixture did not take fire all at once, on putting the lighted paper into the bottle; but one might perceive the flame to ſpread gradually through the bottle. In the three next trials, though they made an exploſion, yet I could not perceive any light within the bottle. In all probability, the flame ſpread ſo inſtantly through the bottle, and was ſo ſoon over, that it had not time to make any impreſſion on my eye. In the laſt mentioned trial, namely, that with equal quantities of inflammable and common air, a light was ſeen in the bottle, but which quickly ceaſed.

With 6 parts of inflammable to 4 of common air, the ſound was not very loud: the mixture continued burning a ſhort time in the bottle, after the ſound was over.

With

With 7 parts of inflammable to 3 of common air, there was a very gentle bounce or rather puff: it contined burning for ſome ſeconds in the belly of the bottle.

A mixture of 8 parts of inflammable to 2 of common air caught fire on applying the flame, but without any noiſe: it continued burning for ſome time in the neck of the bottle, and then went out, without the flame ever extending into the belly of the bottle.

It appears from theſe experiments, that this air, like other inflammable ſubſtances, cannot burn without the aſſiſtance of common air. It ſeems too, that, unleſs the mixture contains more common than inflammable air, the common air therein is not ſufficient to conſume the whole of the inflammable air; whereby part of the inflammable air remains, and burns by means of the common air, which ruſhes into the bottle after the exploſion.

In order to find whether there was any difference in point of inflammability between the air produced from different metals by different acids, five different ſorts of air, namely, 1. Some produced from zinc by diluted oil of vitriol, and which had been kept about a fortnight; 2. Some of the ſame kind of air freſh made; 3. Air produced from zinc by ſpirit of ſalt; 4. Air from iron by the vitriolic acid; 5. Air from tin by ſpirit of ſalt; were each mixed ſeparately with common air in the proportion of 2 parts of inflammable air to $7\frac{7}{10}$ of common air, and their inflammability tried in the ſame bottle, that was uſed for the former experiment, and with the ſame precautions. They each went off with a pretty loud noiſe, and without any difference in the ſound that I could

could be fure of. Some more of each of the above parcels of air were then mixed with common air, in the proportion of 7 parts of inflammable air to $3\frac{1}{3}$ of common air, and tried in the fame way as before. They each of them went off with a gentle bounce, and burnt fome time in the bottle, without my being able to perceive any difference between them.

In order to avoid being hurt, in cafe the bottle fhould burft by the explofion, I have commonly, in making thefe fort of experiments, made ufe of an apparatus contrived in fuch manner, that, by pulling a ftring, I drew the flame of a lamp over the mouth of the bottle, and at the fame time pulled off the cap, while I ftood out of the reach of danger. I believe, however, that this precaution is not very neceffary; as I have never known a bottle to burft in any of the trials I have made.

The fpecific gravity of each of the above-mentioned forts of inflammable air, except the firft, was tried in the following manner. A bladder holding about 100 ounce meafures was filledwith inflammable air, in the manner reprefented in Fig. 3. and the air preffed out again as perfectly as poffible. By this means the fmall quantity of air remaining in the bladder was almoft intirely of the inflammable kind. 80 ounce meafures of the inflammable air, produced from zinc by the vitriolic acid, were then forced into the bladder in the fame manner: after which, the pewter pipe was taken out of the wooden cap of the bladder, the orifice of the cap ftopt up with a bit of lute, and the bladder weighed. A hole was then made in the lute, the air preffed out as perfectly as poffible, and the bladder weighed again. It was found to have increafed

creafed in weight $40\frac{3}{4}$ grains. Therefore the air preffed out of the bladder weighs $40\frac{3}{4}$ grains lefs than an equal quantity of common air: but the quantity of air preffed out of the bladder muft be nearly the fame as that which was forced into it, *i. e.* 80 ounce meafures: confequently 80 ounce meafures of this fort of inflammable air weigh $40\frac{3}{4}$ grains lefs than an equal bulk of common air. The three other forts of inflammable air were then tried in the fame way, in the fame bladder, immediately one after the other. In the trial with the air from zinc by fpirit of falt, the bladder increafed $40\frac{1}{2}$ grains on forcing out the air. In the trial with the air from iron, it increafed $41\frac{1}{2}$ grains, and in that with the air from tin, it increafed 41 grains. The heat of the air, when this experiment was made, was 50°; the barometer ftood at $29\frac{3}{4}$ inches.

There feems no reafon to imagine, from thefe experiments, that there is any difference in point of fpecific gravity between thefe four forts of inflammable air; as the fmall difference obferved in thefe trials is in all probability lefs than what may arife from the unavoidable errors of the experiment. Taking a medium therefore of the different trials, 80 ounce meafures of inflammable air weigh 41 grains lefs than an equal bulk of common air. Therefore, if the denfity of common air, at the time when this experiment was tried, was 800 times lefs than that of water, which, I imagine, muft be near the truth *, inflammable

* Mr. Hawkfbee, whofe determination is ufually followed as the moft exact, makes air to be more than 850 times lighter than water; vid. Hawkfbee's experiments, p. 94, or Cotes's Hydroftatics, p. 159. But his method of trying the experiment muft in all probability make it appear lighter than it really is. For

mable air must be 5490 times lighter than water, or near 7 times lighter than common air. But if the density of common air was 850 times less than that of water, then would inflammable air be 9200 times

having weighed his bottle under water, both when full of air and when exhausted, he supposes the difference of weight to be equal to the weight of the air exhausted; whereas in reality it is not so much: for the bottle, when exhausted, must necessarily be compressed, and on that account weigh heavier in water than it would otherwise do. Suppose, for example, that air is really 800 times lighter than water, and that the bottle is compressed $\frac{1}{12000}$ part of its bulk; which seems no improbable supposition: the weight of the bottle in water will thereby be increased by $\frac{1}{12000}$ of the weight of a quantity of water of the same bulk, or more than $\frac{1}{15}$ of the weight of the air exhausted: whence the difference of weight will be not so much as $\frac{14}{15}$ of the weight of the air exhausted: and therefore the air will appear lighter than it really is in the proportion of more than 15 to 14, *i. e.* more than 857 times lighter than water: whereas, if the ball had been weighed in air in both circumstances, the error arrising from the compression would have been very trifling.

It appears, from some experiments that have been made by weighing a ball in air, while exhausted, and also after the air was let in, that air, when the thermometer is at 50°, and the barometer at $29\frac{3}{4}$, is about 800 times lighter than water. Though the weight of the air exhausted was little more than 50 grains, no error could well arise near sufficient to make it agree with Hawksbee's experiment. Air seems to expand about $\frac{1}{500}$ part by 1° of heat, whence its density in any other state of the atmosphere is easily determined. The density here assumed agrees very well with the rule given by the gentlemen, who measured the length of a degree in Peru, for finding the height of mountains barometrically, and which is given in the Connoissance des mouvemens celestes, année 1762. To make that rule agree accurately with observation, the density of air, whose heat is the same as that of the places where these observations were made, and which I imagine we may estimate at about 45°, should be 798 times less than that of water, when the barometer stands at $29\frac{3}{4}$.

lighter than water, or $10\frac{8}{10}$ lighter than common air.

This method of finding the denſity of factitious air is very convenient and ſufficiently accurate, where the denſity of the air to be tried is not much leſs than that of common air, but cannot be much depended on in the preſent caſe, both on account of the uncertainty in the denſity of common air, and becauſe we cannot be certain but what ſome common air might be mixed with the inflammable air in the bladder, notwithſtanding the precautions uſed to prevent it; both which cauſes may produce a conſiderable error, where the denſity of the air to be tried is many times leſs than that of common air. For this reaſon, I made the following experiments.

I endeavoured to find the weight of the air diſcharged from a given quantity of zinc by ſolution in the vitriolic acid, in the manner repreſented in Fig. 4. A is a bottle filled near full with oil of vitriol diluted with about ſix times its weight of water: B is a glaſs tube fitted into its mouth, and ſecured with lute: C is a glaſs cylinder faſtened on the end of the tube, and ſecured alſo with lute. The cylinder has a ſmall hole at its upper end to let the inflammable air eſcape, and is filled with dry pearl-aſhes in coarſe powder. The whole apparatus, together with the zinc, which was intended to be put in, and the lute which was to be uſed in ſecuring the tube to the neck of the bottle, were firſt weighed carefully; its weight was 11930 grains. The zinc was then put in, and the tube put in its place. By this means, the inflammable air was made to paſs through the dry pearl-aſhes; whereby it muſt have been pretty effectually deprived of any acid

 or

or watery vapours that could have aſcended along with it. The uſe of the glaſs tube B was to collect the minute jets of liquor, that were thrown up by the effervеſcence, and to prevent their touching the pearl-aſhes; for which reaſon, a ſmall ſpace was left between the glaſs-tube and the pearl-aſhes in the cylinder. When the zinc was diſſolved, the whole apparatus was weighed again, and was found to have loſt $11\frac{3}{4}$ grains in weight*; which loſs is principally owing to the weight of the inflammable air diſcharged. But it muſt be obſerved, that, before the effervеſcence, that part of the bottle and cylinder, which was not occupied by other more ſolid matter, was filled with common air; whereas, after the effervеſcence, it was filled with inflammable air; ſo that, upon that account alone, ſuppoſing no more inflammable air to be diſcharged than what was ſufficient to fill that ſpace, the weight of the apparatus would have been diminiſhed by the difference of the weight of that quantity of common air and inflammable air. The whole empty ſpace in the bottle and cylinder was about 980 grain meaſures, there is no need of exactneſs; and the difference of the weight of that quantity of common and inflammable air is about one grain: therefore the true weight of the inflammable air diſcharged, is $10\frac{3}{4}$ grains. The quantity of zinc uſed was 254 grains, and conſequently the weight of the air diſcharged is $\frac{1}{23}$ or $\frac{1}{24}$ of the weight of the zinc.

* As the quantity of lute uſed was but ſmall, and as this kind of lute does not loſe a great deal of its weight by being kept in a moderately dry room, no ſenſible error could ariſe from the drying of the lute during the experiment.

It

It was before ſaid, that one grain of zinc yielded 356 grain meaſures of air: therefore 254 grains of zinc yield 90427 grain meaſures of air; which we have juſt found to weigh 10$\frac{3}{4}$ grains; therefore inflammable air is about 8410 times lighter than water, or 10$\frac{1}{2}$ times lighter than common air.

The quantity of moiſture condenſed in the pearl-aſhes was found to be about 1$\frac{1}{4}$ grains.

By another experiment, tried exactly in the ſame way, the denſity of inflammable air came out 8300 times leſs than than that of water.

The ſpecific gravity of the air, produced by diſſolving zinc in ſpirit of ſalt, was tried exactly in the ſame manner. 244 grains of zinc being diſſolved in ſpirit of ſalt diluted with about four times its weight of water, the loſs in efferveſcence was 10$\frac{3}{4}$ grains; the empty ſpace in the bottle and cylinder was 914 grain meaſures; whence the weight of the inflammable air was 9$\frac{3}{4}$ grains, and conſequently its denſity was 8910 times leſs than that of water.

By another experiment, its ſpecific gravity came out 9030 times lighter than water.

A like experiment was tried with iron. 250$\frac{1}{2}$ grains of iron being diſſolved in oil of vitriol diluted with four times its weight of water, the loſs in efferveſcence was 13 grains, the empty ſpace 1420 grain meaſures. Therefore the weight of the inflammable air was 11$\frac{3}{8}$ grains *i. e.* about $\frac{1}{22}$ of the weight of the iron, and its denſity was 8973 times leſs than that of water. The moiſture condenſed was 1$\frac{1}{4}$ grains.

A like experiment was tried with tin. 607 grains of tinfoil being diſſolved in ſtrong ſpirit of ſalt, the loſs in efferveſcence was 14$\frac{3}{4}$ grains, the empty ſpace 873 grain meaſures: therefore the weight of the inflammable air was $13\frac{3}{4}$ grains *i. e.* $\frac{1}{44}$ of the weight of the tin, and its denſity 8918 times leſs than that of water. The quantity of moiſture condenſed was about three grains.

It is evident, that the truth of theſe determinations depend on a ſuppoſition, that none of the inflammable air is abſorbed by the pearl-aſhes. In order to ſee whether this was the caſe or no, I diſſolved 86 grains of zinc in diluted acid of vitriol, and received the air in a meaſuring bottle in the common way. Immediately after, I diſſolved the ſame quantity of zinc in the ſame kind of acid, and made the air to paſs into the ſame meaſuring bottle, through a cylinder filled with dry pearl-aſhes, in the manner repreſented in Fig. 5. I could not perceive any difference in their bulks.

It appears from theſe experiments, that there is but little, if any, difference in point of denſity between the different ſorts of inflammable air. Whether the difference of denſity obſerved between the air procured from zinc, by the vitriolic and that by the marine acid is real, or whether it is only owing to the error of the experiment, I cannot pretend to ſay. By a medium of the experiments, inflammable air comes out 8760 times lighter than water, or eleven times lighter than common air.

In order to ſee whether inflammable air, in the ſtate in which it is, when contained in the inverted bottles, where it is in contact with water, contains any conſiderable quantity of moiſture diſſolved in it, I forced 192 ounce meaſures of inflammable air, through a cylinder filled with dry pearl-aſhes, by means of the ſame apparatus, which I uſed for filling the bladders with inflam-

inflammable air, and which is repreſented in Fig. 3. The cylinder was weighed carefully before and after the air was forced through; whereby it was found to have increaſed 1 grain in weight. The empty ſpace in the cylinder was 248 grains, the difference of weight of which quantity of common and inflammable air is $\frac{1}{4}$ of a grain. Therefore the real quantity of moiſture condenſed in the pearl-aſhes is $1\frac{1}{4}$ grain. The weight of 192 ounce meaſures of inflammable air deprived of its moiſture appears from the former experiments to be $10\frac{1}{2}$ grains; therefore its weight when ſaturated with moiſture would be $11\frac{3}{4}$ grains. Therefore inflammable air, in that ſtate in which it is in, when kept under the inverted bottles, contains near $\frac{1}{9}$ its weight of moiſture; and its ſpecific gravity in that ſtate is 7840 times leſs than that of water.

I made an experiment with deſign to ſee, whether copper produced any inflammable air by ſolution in ſpirit of ſalt. I could not procure any inflammable air thereby: but the phenomena attending it ſeem remarkable enough to deſerve mentioning. The apparatus uſed for this experiment was of the ſame kind as that repreſented in Fig. 1. The bottle A was filled almoſt full of ſtrong ſpirit of ſalt, with ſome fine copper wire in it. The wire ſeemed not at all acted on by the acid, while cold; but, with the aſſiſtance of a heat almoſt ſufficient to make the acid boil, it made a conſiderable efferveſcence, and the air paſſed through the bent tube, into the bottle D, pretty faſt, till the air forced into it by this means ſeemed almoſt equal to the empty ſpace in the bent tube and the bottle A: when, on a ſudden, without any ſenſible alteration of the heat, the water ruſhed violently through the bent tube into the bottle A, and filled it almoſt intirely full.

The experiment was repeated again in the ſame manner, except that I took away the bottle D, and let out ſome of the water of the ciſtern: ſo that the end of the bent tube was out of water. As ſoon as the effervefcence began, the vapours iſſued viſibly out of the bent tube; but they were not at all inflammable, as appeared by applying a piece of lighted paper to the end of the tube. A ſmall empty phial was then inverted over the end of the bent tube, ſo that the mouth of the phial was immerſed in the water, the end of the tube being within the body of the phial and out of water. The common air was by degrees expelled out of the phial, and its room occupied by the vapours; after which, having chanced to ſhake the inverted phial a little, the water ſuddenly ruſhed in, and filled it almoſt full; from thence it paſſed through the bent tube into the bottle A, and filled it quite full. It appears likely from hence that copper, by ſolution in the marine acid, produces an elaſtic fluid, which retains its elaſticity as long as there is a barrier of common air between it and the water, but which immediately loſes its elaſticity, as ſoon as it comes in contact with the water. In the firſt experiment, as long as any conſiderable quantity of common air was left in the bottle containing the copper and acid, the vapours, which paſſed through the bent tube, muſt have contained a good deal of common air. As ſoon therefore as any part of theſe vapours came to the farther end of the bent tube, where they were in contact with the water, that part of them, which conſiſted of the air from copper, would be immediately condenſed, leaving the common air unchanged; whereby the end of the tube would be filled with common air only; by which means the vapours, contained in the reſt of the tube and bottle A, ſeem to have been defended from the action of the water. But when almoſt all the common air was driven out of the bottle, then the proportion of common air contained in the vapours, which paſſed through the tube, ſeems to have been too ſmall to defend them from the action of the water. In the ſecond experiment, the narrow ſpace left between the neck of the inverted phial and the tube would anſwer much the ſame end, in defending the vapours within the inverted phial from the action of the water, as the bent tube in the firſt experiment did in defending the vapours within the bottle from the action of the water.

Experiments on Factitious Air.

PART II.

Containing Experiments on Fixed Air, or that Species of Factitious Air, which is produced from Alcaline Subſtances, by Solution in Acids or by Calcination.

Experiment I.

THE air produced, by diſſolving marble in ſpirit of ſalt, was caught in an inverted bottle of water, in the uſual manner. In leſs than a day's time, much the greateſt part of the air was found to be abſorbed. The water contained in the inverted bottle was found to precipitate the earth from lime-water; a ſure ſign that it had abſorbed fixed air *.

* Lime, as Dr. Black has ſhewn, is no more than a calcarious earth rendered ſoluble in water by being deprived of its fixed

Experiment II.

I filled a Florence flask in the same way with the same kind of fixed air. When full, I stopt up the mouth of the flask with my finger, while under water, and removed it into a vessel of quicksilver, so that the mouth of the flask was intirely immersed therein. It was kept in this situation upwards of a week. The quicksilver rose and fell in the neck of the flask, according to the alterations of heat and cold, and of the height of the barometer; as it would have done if it had been filled with common air. But it appeared, by comparing together the heights of the quicksilver at the same temper of the atmosphere, that no part of the fixed air had been absorbed or lost its elasticity. The flask was then removed, in the same manner as before, into a vessel of sope leys. The fixed air, by this means, coming in contact with the sope leys, was quickly absorbed.

I also filled another Florence flask with fixed air, and kept it with its mouth immersed in a vessel of quicksilver in the same manner as the other, for upwards of a year, without being able to perceive any air to be absorbed. On removing it into a vessel of sope leys, the air was quickly absorbed like the former.

It appears from this experiment, that fixed air has no disposition to lose its elasticity, unless it meets with

air. Lime water is a solution of lime in water: therefore, on mixing lime water with any liquor containing fixed air, the lime absorbs the air, becomes insoluble in water, and is precipitated. This property of water, of absorbing fixed air, and then making a precipitate with lime water, has been taken notice of by Mr. McBride.

water,

with water or ſome other ſubſtance proper to abſorb it, and that its nature is not altered by keeping.

Experiment III.

In order to find how much fixed air water would abſorb, the following experiment was made. A cylindrical glaſs, with diviſions marked on its ſides with a diamond, ſhewing the quantity of water which it required to fill it up to thoſe marks, was filled with quickſilver, and inverted into a glaſs filled with the ſame fluid. Some fixed air was then forced into this cylindrical glaſs, in the ſame manner that it was into the inverted bottles of water, in the former experiments; except that, to prevent any common air from being forced into the glaſs along with the fixed, I took care not to introduce the end of the bent tube within the cylindrical glaſs, till I was well aſſured that no common air to ſignify could remain within the bottle. This was done by firſt introducing the end of the bent tube within an inverted bottle of water, and letting it remain there, till the air driven into this bottle was at leaſt 10 times as much as would fill the empty ſpace in the bent tube, and the bottle containing the marble and acid. By this means one might be well aſſured, that the quantity of common air remaining within the bent tube and bottle muſt be very trifling. The end of the bent tube was then introduced within the cylindrical glaſs, and kept there till a ſufficient quantity of fixed air was let up. After letting it ſtand a few hours, the diviſion anſwering to the ſurface of the quickſilver in the cylinder was obſerved and wrote down, by which it was known how much fixed air had been let up. A little rain water was then introduced into the cylindrical glaſs, by pouring ſome rain water into the veſſel of quickſilver, and then lifting up the cylindrical glaſs ſo as to raiſe the bottom of it a little way out of the quickſilver. After having ſuffered it to ſtand a day or two, in which time the water ſeemed to have abſorbed as much fixed air as it was able to do, the diviſion anſwering to the upper ſurface of the water, and alſo that anſwering to the ſurface of the quickſilver, were obſerved: by which it was known how much air remained not abſorbed, and alſo how much water had been introduced: the diviſion anſwering to the ſurface of the water telling how much air remained not abſorbed, and the difference of the two diviſions telling how much water had been let up. More water was then let up in the ſame manner, at different times, till almoſt the whole of the fixed air was abſorbed. As all water contains a little air, the water uſed in this experiment was firſt well purged of it by boiling, and then introduced into the cylinder while hot. The reſult of the experiment is given in the following table; in which the firſt column ſhews the bulk of the water let up each time; the ſecond ſhews the bulk of air abſorbed each time; the third the whole bulk of water let up; the fourth the whole bulk of air abſorbed; and the fifth column ſhews the bulk of air remaining not abſorbed. In order to ſet the reſult in a clearer light, the whole bulk of air introduced into the cylinder is called 1, and the other quantities ſet down in decimals thereof.

Bulk of air let up = 1.

Bulk of water let up each time.	Bulk of air abſorbed each time.	Whole bulk of water let up.	Whole bulk of air abſorbed.	Whole bulk of air remaining.
.322	.374	.322	.374	.626
.481	.485	.803	.859	.141
.082	.048	.885	.907	.093
.145	.079	1.030	.986	.014

I imagine that the quantities of water let up and of the air abſorbed could be eſtimated to about three or four 1000th parts of the whole bulk of air introduced. The height of the thermometer, during the trial of this experiment, was at a medium 55°.

This experiment was tried once before. The reſult agreed pretty nearly with this; but, as it was not tried ſo carefully, the reſult is not ſet down.

It appears from hence, that the fixed air contained in marble conſiſts of ſubſtances of different natures, part of it being more ſoluble in water than the reſt: it appears too, that water, when the thermometer is about 55°, will abſorb rather more than an equal bulk of the more ſoluble part of this air.

It appears, from an experiment which will be mentioned hereafter, that water abſorbs more fixed air in cold weather than warm; and, from the following experiment, it appears, that water heated to the boiling point is ſo far from abſorbing air, that it parts with what it has already abſorbed.

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Experiment IV.

Some water, which had abſorbed a good deal of fixed air, and which made a conſiderable precipitate with lime water, was put into a phial, and kept about $\frac{1}{4}$ of an hour in boiling water. It was found when cold not to make any precipitate, or to become in the leaſt cloudy on mixing it with lime water.

Experiment V.

Water alſo parts with the fixed air, which it has abſorbed by being expoſed to the open air. Some of the ſame parcel of water, that was uſed for the laſt experiment, being expoſed to the air in a ſaucer for a few days, was found at the end of that time to make no clouds with lime water.

Experiment VI.

In like manner it was tried how much of the ſame ſort of fixed air was abſorbed by ſpirits of wine. The reſult is as follows.

Bulk of air introduced = 1.

Spirit let up each time.	Air abſorbed each time.	Whole bulk of ſpirit let up.	Whole bulk of air abſorbed.	Bulk of air remaining.
.207	.453	.207	.453	.547
.146	.274	.353	.727	.273
.074	.103	.427	.830	.170
.046	.030	.473	.860	.140

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The mean height of the thermometer, during the trial of the experiment, was 46°. Therefore ſpirit of wine, at the heat of 46°, abſorbs near $2\frac{1}{4}$ times its bulk of the more ſoluble part of this air.

Experiment VII.

After the ſame manner it was tried how much fixed air is abſorbed by oil. Some olive oil, equal in bulk to $\frac{1}{3}$ part of the fixed air in the cylindrical glaſs, was let up. It abſorbed rather more than an equal bulk of air; the thermometer being between 40 and 50. The experiment was not carried any farther. The oil was found to abſorb the air very ſlowly.

Experiment VIII.

The ſpecific gravity of fixed air was tried by means of a bladder, in the ſame manner which was made uſe of for finding the ſpecific gravity of inflammable air; except that the air, inſtead of being caught in an inverted bottle of water, and thence transferred into the bladder, was thrown into the bladder immediately from the bottle which contained the marble and ſpirit of ſalt, by faſtening a glaſs tube to the wooden cap of the bladder, and luting that to the mouth of the bottle containing the efferveſcing mixture, in ſuch manner as to be air-tight. The bladder was kept on till it was quite full of fixed air: being then taken off and weighed, it was found to loſe 34 grains, by forcing out the air. The bladder was previouſly found to hold 100 ounce meaſures. Whence if the outward air, at the time when this experiment was tried, is ſuppoſed to have been 800 times lighter than water, fixed air is 511 times lighter than water, or $1\frac{57}{100}$ times heavier

heavier than common air. The heat of the air during the trial of this experiment was 45°.

By another experiment of the same kind, made when the thermometer was at 65°, fixed air seemed to be about 563 times lighter than water.

Experiment IX.

Fixed air has no power of keeping fire alive, as common air has; but, on the contrary, that property of common air is very much diminished by the mixture of a small quantity of fixed air; as appears from hence.

A small wax candle burnt 80″ in a receiver, which held 190 ounce measures, when filled with common air only.

The same candle burnt 51″ in the same receiver, when filled with a mixture of one part of fixed air to 19 of common air, *i. e.* when the fixed air was $\frac{1}{20}$ of the whole mixture.

When the fixed air was $\frac{3}{40}$ of the whole mixture, the candle burnt 23″.

When the fixed air was $\frac{1}{10}$ of the whole, it burnt 11″.

When the fixed air was $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of the whole mixture, the candle went out immediately.

Hence it should seem, that, when the air contains near $\frac{1}{9}$ its bulk of fixed air, it is unfit for small candles to burn in. Perhaps indeed, if I had used a larger candle and a larger receiver, it might have burnt in a mixture containing a larger proportion of fixed air than this; as I believe that large flaming bodies will burn in a fouler air than small ones. But this is sufficient to shew, that the power, which common air has

has of keeping fire alive, is very much diminiſhed by a ſmall mixture of fixed air.

This experiment was tried, by ſetting the candle in a large ciſtern of water, in ſuch manner that the flame was raiſed but a little way above the ſurface; the receiver being inverted full of water into the ſame ciſtern. The proper quantity of fixed air was then let up, and the remaining ſpace filled with common air, by raiſing the receiver gradually out of water; after which, it was immediately whelmed gently over the burning candle.

Experiments on the Quantity of Fixed Air, contained in Alcaline Subſtances.

Experiment X.

The quantity of fixed air contained in marble was found by diſſolving ſome marble in ſpirit of ſalt, and finding the loſs of weight, which it ſuffered in effervеſcence, in the ſame manner as I found the weight of the inflammable air diſcharged from metals by ſolution in acids, except that the cylinder was filled with ſhreds of filtering paper inſtead of dry pearl aſhes; for pearl aſhes would have abſorbed the fixed air that paſſed through them. The weight of the marble diſſolved was $311\frac{1}{2}$ grains. The loſs of weight in effervеſcence was $125\frac{1}{2}$ grains. The whole empty ſpace in the bottle and cylinder was about 2700 grain meaſures: the exceſs of weight of that quantity of fixed, above an equal quantity of common, air is $1\frac{3}{4}$ grains. Therefore the weight of the fixed air diſcharged is $127\frac{1}{4}$ grains. The cylinder with the filtering paper was found to have increaſed $1\frac{3}{4}$ grains in weight during the effervеſcence. The empty ſpace

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in the cylinder was about 1160 grain meaſures: the exceſs of weight of which quantity of fixed air above an equal bulk of common air is $\frac{3}{4}$ grains. Therefore the quantity of moiſture condenſed in the filtering paper is one grain, or about $\frac{1}{125}$ part of the weight of the air diſcharged.

As water has been already ſhewn to abſorb fixed air, it ſeemed not improbable, but what there might be ſome fixed air contained in the ſolution of marble in ſpirit of ſalt; in which caſe the air diſcharged, during the effervescence, would not be the whole of the fixed air in the marble. In order to ſee whether this was the caſe, I poured ſome of the ſolution into lime water. It made ſcarce any precipitate; which, as the acid was intirely ſaturated with marble, it would certainly have done if the ſolution had contained any fixed air. It appears therefore from this experiment, firſt, that marble contains $\frac{127\frac{1}{4}}{311\frac{1}{2}}=\frac{407}{1000}$ of its weight of fixed air; and ſecondly, that the quantity of moiſture, which flies off along with the fixed air in effervefcence, is but trifling; as I imagine that the greateſt part of what did fly off muſt have been condenſed in the filtering paper.

By another experiment tried much in the ſame way, marble was found to contain $\frac{408}{1000}$ of its weight of fixed air.

Experiment XI.

Volatile ſal ammoniac diſſolves with too great rapidity in acids, and makes too violent an effervefcence, to allow one to try what quantity of fixed air it contains in the foregoing manner: I therefore made uſe of the following method.

Three ſmall phials were weighed together in the ſame ſcale. The firſt contained ſome weak ſpirit of ſalt, the ſecond contained ſome volatile ſal ammoniac in moderate ſized lumps without powder, corked up to prevent evaporation, and the third, intended for mixing the acid and alcali in, contained only a little water, and was covered with a paper cap, to prevent the ſmall jets of liquor, which are thrown up during the effervescence, from eſcaping out of the bottle. In order to prevent too violent an effervefcence, the acid and alcali were both added by a little at a time, care being taken that the acid ſhould always predominate in the mixture. Care was alſo taken always to cover the bottle with the paper cap, as ſoon as any of the acid or alcali were added. As ſoon as the mixture was finiſhed, the three phials were weighed again; whereby the loſs in effervefcence was found to be 134 grains. The weight of the volatile ſalt made uſe of was 254 grains, and was pretty exactly ſufficient to ſaturate the acid. The ſolution appeared, by pouring ſome of it into lime water, to contain ſcarce any fixed air. Therefore 254 grains of the volatile ſal ammoniac contain 134 grains of fixed air, *i.e.* $\frac{528}{1000}$ of their weight. It appeared from the ſame experiment, that 1680 grains of the volatile ſalt ſaturate as much acid as 1000 grains of marble.

By another experiment, tried with ſome of the ſame parcel of volatile ſalt, it was found to contain $\frac{538}{1000}$ of its weight of fixed air, and 1643 grains of it ſaturated as much acid as 1000 grains of marble. By a medium, the ſalt contained $\frac{533}{1000}$ of its weight

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of fixed air; and 1661 grains of it saturated as much acid as 1000 grains of marble.

One thousand grains of marble were found to contain $407\frac{1}{2}$ grains of air, and 1661 grains of volatile sal ammoniac contain 885 grains. Therefore this parcel of volatile sal ammoniac contains more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 885 to $407\frac{1}{2}$, or of 217 to 100.

N.B. It is not unlikely, that the quantity of fixed air may be found to differ considerably in different parcels of volatile sal ammoniac; so that any one, who was to repeat these experiments, ought not to be surprized if he was to find the result to differ considerably from that here laid down. The same thing may be said of pearl ashes.

Experiment XII.

This serves to account for a remarkable phenomenon, which I formerly met with, on putting a solution of volatile sal ammoniac in water into a solution of chalk in spirit of salt. The earth was precipitated hereby, as might naturally be expected: but what surprized me, was, that it was attended with a considerable effervescence; though I was well assured, that the acid in the solution of chalk was perfectly neutralized. This is very easily accounted for, from the above-mentioned circumstance of volatile sal ammoniac containing more fixed air in proportion to the quantity of acid that it can saturate, than calcareous earths do. For the volatile alcali, by uniting to the acid, was necessarily deprived of its fixed air. Part of this air united to the calcareous earth, which

was

was at the ſame time ſeparated from the acid; but, as the earth was not able to abſorb the whole of the fixed air, the remainder flew off in an elaſtic form, and thereby produced an efferveſcence.

EXPERIMENT XIII.

The ſame ſolution of volatile ſal ammoniac made no precipitate, when mixed with a ſolution of Epſom ſalt; though a mixture thereof with a little ſpirit of ſal ammoniac, made with lime, immediately precipitated the magneſia from the ſame ſolution of Epſom ſalt; as it ought to do according to Dr. Black's account of the affinity of magneſia and volatile alcalies to acids. This experiment is not ſo eaſily accounted for as the laſt; but I imagine, that the magneſia is really ſeparated from the acid by the volatile alcali; but that it is ſoluble in water, when united to ſo great a proportion of fixed air, as is contained in a portion of volatile ſal ammoniac, ſufficient to ſaturate the ſame quantity of acid. The reaſon, why the mixture of the ſolution of volatile ſal ammoniac, with the ſpirits of ſal ammoniac made with lime, precipitates the magneſia from the Epſom ſalt, is that, as the ſpirits made with lime contain no fixed air, the mixture of theſe ſpirits with the ſolution of volatile ſal ammoniac contains leſs air in proportion to the quantity of acid which it can ſaturate, than the ſolution of volatile ſal ammoniac by itſelf does.

Volatile ſal ammoniac requires a great deal of water to diſſolve it, and the ſolution has not near ſo ſtrong a ſmell as the ſpirits of ſal ammoniac made with fixed alcali; the reaſon of which is, that the latter contain much leſs fixed air. But volatile ſal

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ammoniac diſſolves in conſiderable quantity in weak ſpirits of ſal ammoniac made with lime, and the ſolution differs in no reſpect from the ſpirits made with fixed alcali. This is a convenient way of procuring the mild ſpirits of ſal ammoniac, as thoſe made with fixed alcali are ſeldom to be met with in the ſhops.

Experiment XIV.

The quantity of fixed air contained in pearl aſhes was tried, by mixing a ſolution of pearl aſhes with diluted oil of vitriol, in the ſame manner as was uſed for volatile ſal ammoniac. As much of the ſolution was uſed as contained $328\frac{1}{4}$ grains of dry pearl aſhes. The loſs of efferveſcence was 90 grains. The mixture, which was perfectly neutralized, being then added to a ſufficient quantity of lime water, in order to ſee whether it contained any fixed air, a precipitate was made, which being dried weighed $8\frac{1}{2}$ grains. Therefore, if we ſuppoſe this precipitate to contain as much fixed air as an equal weight of marble, which I am well aſſured cannot differ very conſiderably from the truth, the fixed air therein is $3\frac{1}{2}$ grains, and conſequently the air in $328\frac{1}{2}$ grains of the pearl aſhes, is $93\frac{1}{2}$ grains, *i. e.* $\frac{284}{1000}$ of their weight.

By another experiment tried in the ſame way, they appeared to contain $\frac{287}{1000}$ of their weight of fixed air.

1558 grains of the pearl aſhes were found to ſaturate as much acid as 1000 grains of marble. Therefore this parcel of pearl aſhes contains more air in proportion to the quantity of acid that it can ſaturate, than marble does, in the proportion of 109 to 100.

Experiment XV.

Dr. Black ſays, that, by expoſing a ſolution of ſalt of tartar for a long time to the open air, ſome cryſtals were formed in it, which ſeemed to be nothing elſe than the vegetable alcali united to more than its uſual proportion of fixed air. This induced me to try, whether I could not perform the ſame thing more expeditiouſly, by furniſhing the alcali with fixed air artificially; which I did in the manner repreſented in Fig. 6: where A repreſents a wide-mouthed bottle, containing a ſolution of pearl aſhes; B*b* repreſents a round wooden ring faſtened over the mouth of the bottle, and ſecured with luting; C is a bladder bound tight over the wooden ring. This bladder, being firſt preſſed cloſe together, ſo as to drive out as much of the included air as poſſible, was filled with fixed air, by means of the bent tube D; one end of which is fixed into the wooden ring, and the other faſtened into the mouth of the bottle E, containing marble and ſpirit of ſalt. By this means the fixed air thrown into the bladder mixed with the air in the bottle, and came in contact with the fixed alcali. The fixed air was by degrees abſorbed, and cryſtals were formed on the ſurface of the fixed alcali, which were thrown to the bottom by ſhaking the bottle. When the alcali had abſorbed as much fixed air as it would readily do, the cryſtals were taken out and dryed on filtered paper, and the remaining ſolution evaporated; by which means ſome more cryſtals were procured.

N. B. It ſeemed, as, if not all the air diſcharged from the marble was of a nature proper to be abſorbed by the alcali, but only part of it; for when the alcali had

abſorbed

abſorbed ſomewhat more than $\frac{1}{2}$ of the air firſt thrown into the bladder, it would not abſorb any more: but, on preſſing the remaining air out of the bladder, and ſupplying its place with freſh fixed air, a good deal of this new air was abſorbed. I cannot, however, ſpeak poſitively as to this point; as I am not certain whether the apparatus was perfectly air-tight*.

Theſe cryſtals do not in the leaſt attract the moiſture of the air; as I have kept ſome, during a whole winter, expoſed to the air in a room without a fire, without their growing at all moiſt or increaſing in weight.

Being held over the fire in a glaſs veſſel, they did not melt as many ſalts do, but rather grew white and calcined.

They diſſolve in about four times their weight of water when the weather is temperate, and diſſolve in greater quantity in hot water than cold.

It was found, by the ſame method, that was made uſe of for the volatile ſal ammoniac, that theſe cryſtals contain $\frac{423}{1000}$ of their weight of fixed air, and that 2035 grains of them ſaturate as much acid as 1000 grains of marble. Therefore theſe cryſtals contain more air in proportion to the quantity of acid they

* Pearl aſhes deprived of their fixed air, *i. e.* ſope leys, will abſorb the whole of the air diſcharged from marble; as I know by experience. But yet it is not improbable, but that the ſame alcali, when near ſaturated with fixed air, may be able to abſorb only ſome particular part of it. For as it has been already ſhewn, that part of the air diſcharged from marble is more ſoluble in water than the reſt; ſo it is not unlikely, but that part of it may have a greater affinity to fixed alcali, and be abſorbed by it in greater quantity than the reſt.

ſaturate,

ſaturate, than marble does, in the ratio of 211 to 100.

Experiment XVI.

As theſe cryſtals contain about as much fixed air in proportion to the quantity of acid, that they can ſaturate, as volatile ſal ammoniac does, it was natural to expect, that they ſhould produce the ſame effects with a ſolution of Epſom ſalt, or a ſolution of chalk in ſpirit of ſalt; as thoſe effects ſeemed owing only to the great quantity of fixed air contained in volatile ſal ammoniac. This was found to be the real caſe: for a ſolution of theſe cryſtals in five times their weight of water, being dropt into a ſolution of chalk in ſpirit of ſalt, the earth was precipitated, and an efferveſcence was produced. No precipitate was made on dropping ſome of the ſame ſolution into a ſolution of Epſom ſalt, though the mixture was kept upwards of twelve hours. But, upon heating this mixture over the fire, a great deal of air was diſcharged, and the magneſia was precipitated.

Experiments on Factitious Air.

PART III.

Containing Experiments on the Air, produced by Fermentation and Putrefaction.

MR. M'Bride has already ſhewn, that vegetable and animal ſubſtances yield fixed air by fermentation and putrefaction. The following experiments were made chiefly with a view of ſeeing, whether they yield any other ſort of air beſides that.

Experi-

EXPERIMENT I.

The air produced from brown ſugar and water, by fermentation, was caught in an inverted bottle of ſope leys in the uſual manner, and which is repreſented in Fig. 1. As the weather was too cold to ſuffer the ſugar and water to ferment freely, the bottle containing it was immerſed in water, which, by means of a lamp, was kept conſtantly at about 80° of heat. The quantity of ſugar put into the bottle was 931 grains: it was diſſolved in about $6\frac{1}{2}$ times its weight of water, and mixed with 100 grains of yeaſt, by way of ferment. The empty ſpace left in the fermenting bottle and tube together meaſured 1920 grains. The mixture fermented freely, and generated a great deal of air, which was forced up in bubbles into the inverted bottle, but was abſorbed by the ſope leys, as faſt as it roſe up. It frothed greatly; but none of the froth or liquor ran over. In about ten days, the fermentation ſeeming almoſt over, the veſſels were ſeparated. The bottle with the fermented liquor was found to weigh 412 grains leſs than it did, before the fermentation began. As none of the liquor ran over, and as little or no moiſture condenſed within the bent tube, I think one may be well aſſured, that the loſs of weight was owing intirely to the air forced into the inverted bottle; for the matter diſcharged, during the fermentation, muſt have conſiſted either of air, or of ſome other ſubſtance, changed into vapour: if this laſt was the caſe, I think it could hardly have failed, but that great part of thoſe vapours muſt have condenſed in the tube. The air remaining unabſorbed in the inverted bottle of ſope leys was meaſured, and was

 found

found to be exactly equal to the empty ſpace left in the bent tube and fermenting bottle. It appears therefore, that there is not the leaſt air of any kind diſcharged from the ſugar and water by fermentation, but what is abſorbed by the ſope leys, and which may therefore be reaſonably ſuppoſed to be fixed air. It ſeems alſo, that no part of the common air left in the fermenting bottle was abſorbed by the fermenting mixture, or ſuffered any change in its nature from thence: for a ſmall phial being filled with one part of this air, and two of inflammable air; the mixture went off with a bounce, on applying a piece of lighted paper to the mouth, with exactly the ſame appearances, as far as I could perceive, as when the phial was filled with the ſame quantities of common and inflammable air.

The ſugar uſed in this experiment was moiſt, and was found to loſe $\frac{228}{1000}$ parts of its weight by drying gently before a fire. Therefore the quantity of dry ſugar uſed was 715 grains; and the weight of the air diſcharged by fermentation appears to be near 412 grains, *i. e.* near $\frac{57}{100}$ parts of the weight of the dry ſugar in the mixture.

The fermented liquor was found to have intirely loſt its ſweetneſs; ſo that the vinous fermentation ſeemed to be compleated; but it was not grown at all ſour.

Experiment II.

The air, diſcharged from apple-juice by fermentation, was tried exactly in the ſame manner. The quantity ſet to ferment was 7060 grains, and was mixed with 100 grains of yeaſt. Some of the ſame parcel of

 apple-

apple-juice, being evaporated gently to the confiftence of a moderately hard extract, was reduced to $\frac{1}{7}$ of its weight; fo that the quantity of extract, in the 7060 grains of juice employed, was 1009 grains. The liquor fermented much fafter than the fugar and water. The lofs of weight during the fermentation was 384 grains. The air remaining unabforbed in the inverted bottle of fope leys was loft by accident, fo that it could not be meafured; but, from the fpace it took up in the inverted bottle, I think I may be certain that it could not much exceed the empty fpace in the bent tube and fermenting bottle, if it did at all. Therefore there is no reafon to think that the apple-juice, any more than the fugar and water, produced any kind of air during the fermentation, except fixed air. It appears too, that the fixed air was near $\frac{381}{1000}$ of the weight of the extract contained in the apple-juice. The fermented liquor was very four; fo that it had gone beyond the vinous fermentation, and made fome progrefs in the acetous fermentation.

In order to compare more exactly the nature of the air produced from fugar by fermentation, with that produced from marble by folution in acids, I made the three following experiments.

Experiment III.

I firft tried in what quantity the air from fugar was abforbed by water, and at the fame time made a like experiment on the air difcharged from marble, by folution in fpirit of falt. This was done exactly in the fame way as the former experiments of this kind. The refult is as follows, beginning with the air from fugar and water.

Air

Air from ſugar and water let up = 1000.

Bulk of water let up each time.	Bulk of air abſorbed each time.	Wholebulk of water let up.	Wholebulk of air abſorbed.	Bulk of air remaining.	Height of thermometer when obſerv. was made.
375	517	375	517	483	40
143	164	518	681	319	45
153	164	673	845	154	45
82	103	755	948	52	46

Air from marble let up = 1000.

391	473	391	473	527	40
143	133	534	606	394	45
284	115	818	811	189	45
194	80	1.012	891	109	46

The apparatus uſed in this experiment was ſuffered to remain in the ſame ſituation till ſummer, when the thermometer ſtood at 65°. The bulk of the air from ſugar, not abſorbed by the water, was then found to be 287; ſo that the matter had remitted 235 parts of air. The bulk of the air from marble not abſorbed, was 194; ſo that 85 parts were remitted; which is therefore a proof, that water abſorbs leſs fixed air in warm weather than cold.

It appears from this experiment, that the air produced from ſugar by fermentation, as well as that diſcharged from marble by ſolution in acids, conſiſts of ſubſtances of different nature: part being abſorbed by water in greater quantity than the reſt. But, in

 general,

general, the air from ſugar is abſorbed in greater quantity than that from marble.

In forcing the air from ſugar into the cylindrical glaſs, no ſenſible quantity of moiſture was found to condenſe on the ſurface of the quickſilver, or ſides of the glaſs; which is a proof that no conſiderable quantity of any thing except air could fly off from the ſugar and water in fermentation.

Experiment IV.

The ſpecific gravity of the air produced from ſugar was found in the ſame way as that produced from marble. A bladder holding 102 ounce meaſures, being filled with this kind of air, loſt $29\frac{1}{8}$ grains on forcing out the air, the thermometer ſtanding at 62°, and the barometer at $29\frac{1}{2}$ inches. Whence, ſuppoſing the outward air during the trial of this experiment to be 826 times lighter than water, as it ſhould be, according to the ſuppoſition made uſe of in the former parts of this paper, the air from ſugar ſhould be 554 times lighter than water. Its denſity therefore appears to be much the ſame as that of the air contained in marble; as that air appeared to be 511 times lighter than water, by a trial made when the thermometer was at 45°; and 563 times lighter, by another trial when the thermometer was at 65°.

This air ſeems alſo to poſſeſs the property of extinguiſhing flame, in much the ſame degree as that produced from marble; as appears from the following experiment.

Experiment V.

A ſmall wax candle burnt 15″ in a receiver filled with $\frac{1}{10}$ of air from ſugar, the reſt common air.

In a mixture containing $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of air from ſugar, the reſt common air, the candle went out immediately. When the receiver was filled with common air only, the ſame candle burnt 72″.

The receiver was the ſame as that uſed in the former experiment of this kind, and the experiment tried in the ſame way, except that the air from ſugar was firſt received in an empty bladder, and thence transferred into the inverted bottles of water, in which it was meaſured: for the air is produced from the ſugar ſo ſlowly, that, if it had been received in the inverted bottles immediately, it would have been abſorbed almoſt as faſt as it was generated.

It appears from theſe experiments, that the air produced from ſugar by fermentation, and in all probability that from all the other ſweet juices of vegetables, is of the ſame kind as that produced from marble by ſolution in acids, or at leaſt does not differ more from it than the different parts of that air do from each other, and may therefore juſtly be called fixed air. I now proceed to the air generated by putrefying animal ſubſtances.

Experiment VI.

The air produced from gravy broth by putrefaction, was forced into an inverted bottle of ſope leys, in the ſame way as in the former experiment. The quantity of broth uſed, was 7640 grains, and was found, by evaporating ſome of the ſame to the conſiſtence of a dry extract, to contain 163 grains of ſolid matter. The fermenting bottle was immerſed in water kept conſtantly to the heat of about 96°. In about

about two days the fermentation ſeemed intirely over. The liquor ſmelt very putrid, and was found to have loſt $11\frac{1}{2}$ grains of its weight. The ſope leys had acquired a browniſh colour from the putrid vapours, and a muſty ſmell. The air forced into the inverted bottle, and not abſorbed by the ſope leys, meaſured 6280 grains: the air left in the bent tube and fermenting bottle was 1100 grains; almoſt all of which muſt have been forced into the inverted bottles: ſo that this unabſorbed air is a mixture of about one part of common air and $4\frac{7}{10}$ of factitious air.

This air was found to be inflammable; for a ſmall phial being filled with 109 grain meaſures of it, and 301 of common air, which comes to the ſame thing as 90 grains of pure factitious air, and 320 of common air, it took fire on applying a piece of lighted paper, and went off with a gentle bounce, of much the ſame degree of loudneſs as when the phial was filled with the laſt mentioned quantities of inflammable air from zinc and common air. When the phial was filled with 297 grains of this air, and 113 of common air, *i. e.* with 245 of pure factitious air, and 165 of common air, it went off with a gentle bounce on applying the lighted paper; but I think not ſo loud as when the phial was filled with the laſt-mentioned quantities of air from zinc and common air.

5500 grain meaſures of this air, *i. e.* 4540 of pure factitious air, and 960 of common air, were forced into a piece of ox-gut furniſhed with a ſmall braſs cock, which I find more convenient for trying the ſpecific gravity of ſmall quantities of air, than a bladder: the gut increaſed $4\frac{1}{2}$ grains in weight on

2 forcing

forcing out the air. A mixture of 4540 grains of air from zinc and 960 of common air being then forced into the ſame gut, it increaſed $4\frac{3}{4}$ grains on forcing out the air. So that this factitious air ſhould ſeem to be rather heavier than air from zinc; but the quantity tried was too ſmall to afford any great degree of certainty.

N.B. The weight of 4540 grain meaſures of inflammable air, is $\frac{58}{100}$ grains, and the weight of the ſame quantity of common air is $5\frac{7}{10}$ grains.

On the whole it ſeems that this ſort of inflammable air is nearly of the ſame kind as that produced from metals. It ſhould ſeem, however, either to be not exactly the ſame, or elſe to be mixed with ſome air heavier than it, and which has in ſome degree the property of extinguiſhing flame, like fixed air.

The weight of the inflammable air diſcharged from the gravy appears to be about one grain, which is but a ſmall part of the loſs of weight which it ſuffered in putrefaction. Part of the remainder, according to Mr. M'Bride's experiments, muſt have been fixed air. But the colour and ſmell, communicated to the ſope leys, ſhew, that it muſt have diſcharged ſome other ſubſtance beſides fixed and inflammable air.

Raw meat alſo yields inflammable air by putrefaction, but not in near ſo great a quantity, in proportion to the loſs of weight which it ſuffers, as gravy does. Four ounces of raw meat mixed with water, and treated in the ſame manner as the gravy, loſt about 100 grains in putrefaction; but it yielded hardly more inflammable air than the gravy. This air ſeemed

ſeemed of the ſame kind as the former; but, as the experiments were not tried ſo exactly, they are not ſet down.

I endeavoured to collect in the ſame manner the air diſcharged from bread and water by fermentation, but I could not get it to ferment, or yield any ſenſible quantity of air; though I added a little putrid gravy by way of ferment.

Received May 21, 1766.

XX. *A farther Account of the* Poliſh *Cochineal: from Dr.* Wolfe, *of* Warſaw. *Communicated by* Henry Baker, *F. R. S.*

Read June 5, 1766.

IN the LIVth volume of the Philoſophical Tranſactions, for the year 1764, Art. XV. the Royal Society has been pleaſed to publiſh two curious papers, communicated by Mr. Baker, from Dr. Wolfe of Warſaw, deſcribing the Poliſh Cochineal, the plants on whoſe roots it is found, the manner of collecting and curing it, the method of dying therewith, and alſo the doctor's own experiments on theſe curious inſects; the figures whereof are there given as engraven on a copper plate.

Since that time, the doctor has been very induſtrious in breeding and obſerving theſe inſects, and has thereby diſcovered the male fly, about which he was before uncertain; and has ſent to Mr. Baker an elegant picture

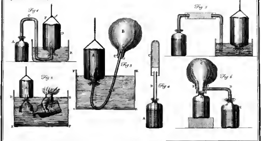
Fig 1
Fig 2
Fig 3
Fig 4
Fig 5
Fig 6