Experiments

on

Factitious Air.

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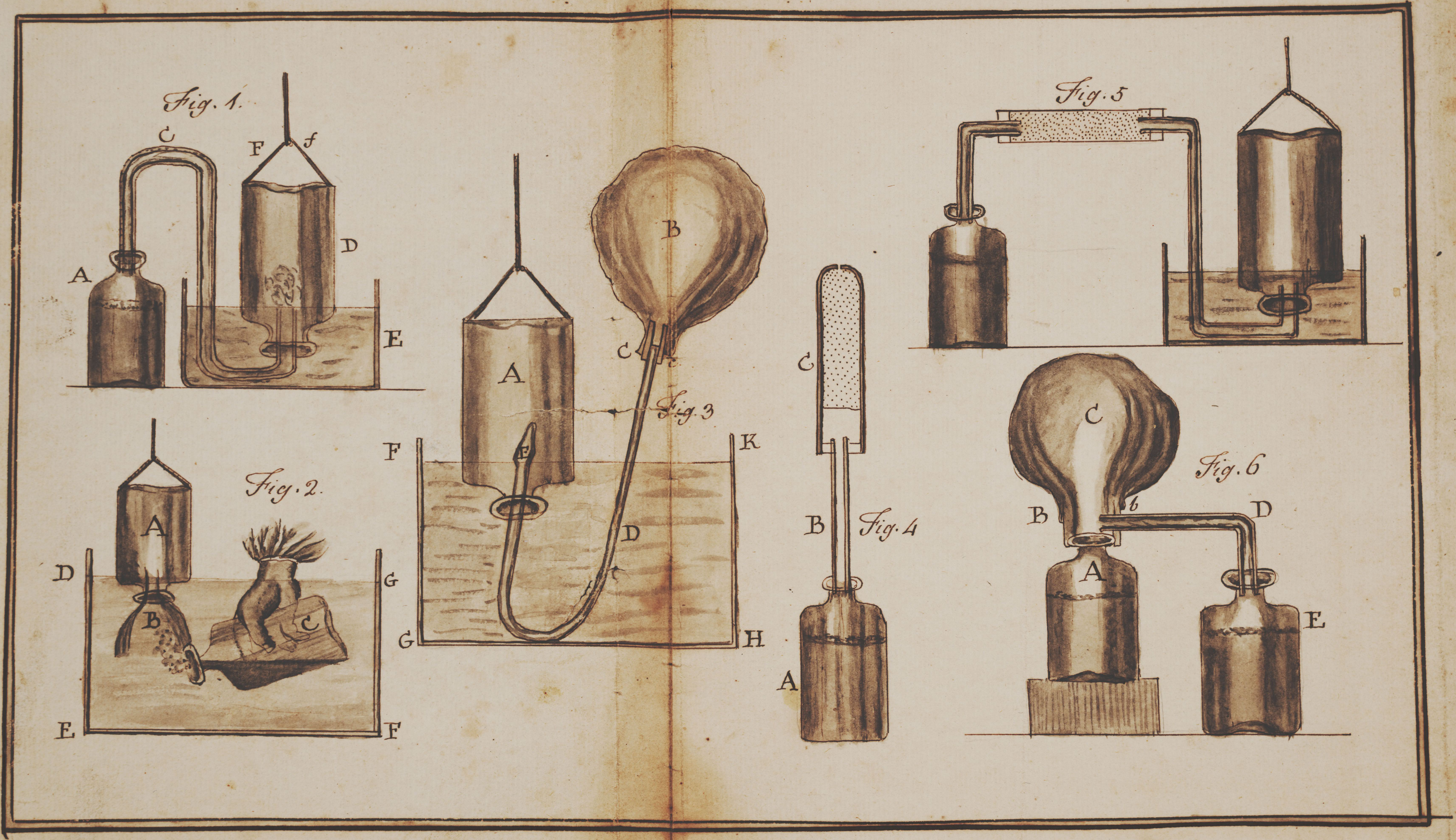

Fig. 1.
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D
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Fig. 2.
A
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Fig. 3
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K
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Fig. 4
A
Fig. 5
C
Fig. 6
B
b
D
A
E

Part 1st.

Containing Experiments on Inflammable Air.

I know of only three metallic substances, namely, zinc, iron & 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 1½, 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

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. One ounce of tinfoil yields 202 ounce measure of inflammable air.

These experiments were made, when the Thermometer was at 50°. & 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 effervescence, 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 & tin are in some measure acted on by it, while cold.

It seems likely from hence, that, when

either

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 undilated 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 to Phlogiston.* It is highly probable too, that the

same

* 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.

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, & 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 shewn 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

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 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

 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 Linen; 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

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

With equal quantities of inflammable & 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, 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 7 parts of inflammable to 3 of

 common air, there was a very gentle bounce or rather puff: it continued 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 of 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 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, & which had been kept about a forthnight; 2. some of the same

M 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 7/10 of common air, & their inflammability tried in the same bottle, that was used in the former experiment, & with the same precautions. They each went off with a pretty loud noise, & without any difference in the sound that I could be sure 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 3/5 of common air, and tried in the same way as before. They each of them went off with a gentle bounce, and burnt some time in the bottle, without my being able to perceive any difference between them.

In order to avoid being hurt, in case the bottle should burst by the explosion, I have commonly, in making these sorts of experiments, made use of an apparatus contrived in such manner, that, by pulling a string, I drew the flame of a lamp over the mouth of the bottle, and at the same time pulled off the cap, while I stood out of the reach of danger. I believe, however,
that

 that this precaution is not very necessary, as I have never known a bottle to burst in any of the trials I have made.

The specific gravity of each of the above mentioned sorts of inflammable air, except the first, was tried in the following manner. A Bladder holding about 100 ounce measures was filled with inflammable air, in the manner represented in Fig. 3, and the air pressed out again as perfectly as possible. By this means the small quantity of air remaining in the bladder was almost entirely of the inflammable kind. 80 ounce measures of the inflammable air, produced from zinc by the vitriolic acid, were then forced into the bladder in the same manner: after which, the pewter pipe was taken out of the wooden cap of the Bladder, the orifice of the cap stopt up with a bit of lute, and the bladder weighed. A hole was then made in the lute, the air pressed out as perfectly as possible, and the bladder weighed again. It was found to have increased in weight 40 3/4 grains. Therefore the air pressed out of the bladder weighs 40 3/4 grains less than an equal quantity of common air: but the quantity of air pressed out of the Bladder must be nearly

the

the same as that which was forced into it, i. e. 80 ounce measures: consequently 80 ounce measures of this sort of inflammable air weigh 40 3/4 less than an equal bulk of common air. The three other sorts of inflammable air were then tried in the same way, in the same bladder, immediately one after the other. In the trial of the air from Zinc by spirit of salt, the bladder increased 40 1/2 grains on forcing out the air. In the trial with the air from iron, it increased 41 1/2 grains, and in that with the air from tin, it increased 41 grains. The heat of the air when this experiment was made, was 50.°; the Barometer stood at 29 3/4 Inches.

There seems no reason to imagine from these experiments, that there is any difference in point of specific gravity between these four sorts of inflammable air; as the small difference observed in these trials is in all probability less than what may arise from the unavoidable errors of the experiment. Taking a medium therefore of the different trials, 80 ounce measures of inflammable air weigh 41 grains less than an equal bulk of common air. Therefore if the density of + common air, at the time when this

experiment

 experiment was tried, was 800 times less than that of water, which, I imagine, must be near the truth,* inflammable air must be 5490 times lighter than water, or near 7 times lighter than common air.
But

* Mr. Hawksbee, whose determination is usually followed as the most exact, makes air to be 850 times lighter than water; vid. Hawksbee's experiments. p. 94. or Cotes's Hydrostatics, p. 159. But his method of trying the experiment must in all probability make it appear lighter than it really is. For 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 it is in reality not so much; for the bottle, when exhausted, must necessarily be compressed, & on that account weigh heavier in water than it would otherwise do. Suppose, for example that air is really 800 times lighter than water, & that the water 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 $\frac{1}{12000}$ part of the weight of a quantity of water of the same bulk, or more than 1/15 of the

But if the density of common air was 850 times less than that of water, then would inflammable air be 9200 times lighter than water, or $10\frac{8}{10}$ lighter than common air.

This

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; 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 arising 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 29¾, 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 1/500 part by 1°. of heat,

whence

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

whence its density in any other state of the At-mosphere 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 mouvements celestes, annee 1762. To make that rule agree accurately with observation, the density of air, whose heat is the same with 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 29¾.—

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 to be 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 watery vapours that could have

ascended

 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 effervescence, 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 11¾ grains in weight;* which loss is principally owing to the weight of the inflammable air discharged. But it must be observed, that, before the effervescence, that part of the bottle and cylinder, which was not occupied by other more solid matter, was filled with common air; whereas, after the effervescence, 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

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

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 3/4 grains. The quantity of zinc used was 254 grains, and consequently the weight of the air discharged is 1/23 or 1/24 of the weight of the zinc.

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 3/4 grains; therefore inflammable air is about 8410 times lighter than water, or 10 3/4 times lighter than common air.

The quantity of moisture condensed in the Pearl ashes was found to be about 1 1/4 grains.

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

18. dissolved in spirit of salt diluted with about four times its weight of water, the loss in effervescence was 10 3/4 grains; the empty space in the bottle & cylinder was 914 grain measures; whence the weight of the inflammable air was 9/4 grains, & 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 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 3/8 grains, i. e. about 1/22 the weight of the Iron, and its density was 8973 times less than that of water. The moisture condensed was 1 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 3/4 grains, the empty space 873 grain measures: therefore the weight of the inflammable air was 13 3/4 grains, i. e. 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

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 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 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 & inflammable air is 1/4 of a grain. Therefore the real quantity of moisture condensed in the pearl-ashes is 1 1/4 grain. The weight of 192 ounce measures of inflammable air deprived of its moisture appears from the former experiments to be 10 1/2 grains; therefore its weight when saturated would be 11 3/4 grains. Therefore inflammable air, in that state in which it is in, when kept under the inverted bottles, contains near 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

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 effervescence 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,

 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, & 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

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 1st

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

days

days 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.*

Experiment 2d.

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

* Lime, as Dr. Black has shewn, is no more than a calcarious earth rendered soluble in water by being deprived of its fixed air. Lime water is a solution of lime in water. therefore, in 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, & then making a precipitate with Lime water, has been taken notice of by Dr. Macbride.

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

Experiment 3.d

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, & 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

 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 intro-ducing 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 & 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 triffling. 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

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

 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 1000 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 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.

Experiment 4th

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 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 5th

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

 the end of that time to make no clouds with lime water.

Experiment 6th.

In like manner it was tried how much of the same sort of fixed air was absorbed by spirit 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

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 2¼ times its bulk of the more soluble part of this air.

Experiment 7th.

After the same manner it was tried how much fixed air is absorbed by oil. Some olive oil, equal in bulk to ⅓ 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 & 50. The experiment was not carried any farther. The oil was found to absorb the air very

very slowly.

Experiment 8th

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 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

 fixed air seemed to be about 563 times lighter than water.

Experiment 9th.

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 1/20 of the whole mixture.

When the fixed air was 3/40 of the whole mixture, the candle burnt 23".

When the fixed air was 1/10 of the whole, it burnt 11".

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

Hence it should seem, that, when the air contains near 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 & a larger receiver, it might have burnt in a mixture

containing

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 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. 10th

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 effervescence, 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 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 311½ grains: The loss of weight in effervescence was 125½ grains: The whole empty space in the bottle & cylinder was about 2700 grain measures: the excess of weight of that quantity of fixed air, above an equal quantity of common air is 1$\frac{3}{4}$ grains. The cylinder with the filtering paper was found to have increased 1$\frac{3}{4}$ grains in weight during the effervescence. The empty space 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 1 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 with 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

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 effervescence, is but triffling, 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. II.

Volatile Sal ammoniac dissolves with too great rapidity in acids, and makes too violent an effervescence, 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

 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 effervescence, 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 either the acid or alcali were added. As soon as the mixture was finished, the three phials were weighed again; whereby the loss in effervescence 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

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 $\frac{528}{1000}$ 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 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 407½ 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 407½, 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 12th.

This serves to account for a remarkable phenomenon

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 surprised 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 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 13th

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 and volatile

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 volatile sal ammoniac in solution, with the mixture 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 ammoniac dissolves in considerable quantity in weak spirits of sal ammoniac made with lime, & the solution differs in no respect from the spirits made with

fixed

 fixed alcali. This is a convenient way of procuring the mild spirit of sal ammoniac, as those made with fixed alcali are seldom to be met with in the shops.

Experiment 14th

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 328¼ 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 8½ 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 3½ grains, & consequently the air in 328½ grains of the pearl-ashes, is 93½ 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

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 15.

Dr. Black says, that, by exposing a solution of salt of Tartar for a long time to the open air, some chrystals 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; Bb. represents a round wooden ring fastened over the mouth of the bottle, & secured with luting: C is a bladder bound fast 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 in the wooden ring, and the other fastened into the mouth of the bottle E. containing marble & spirit of salt. By this means the fixed air thrown into the bladder mixed with

the

 the air in the bottle, and came in contact with the fixed alcali. The fixed air was by degrees absorbed, and chrystals 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 Chrystals were taken out and dried on filtered paper, and the remaining solution evaporated; by which means some more chrystals were procured.

N.B. It seemed, as, if not all the air dis-charged 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 somewhat more than ½ 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

* 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

air by Fermentation and Putrefaction. The following experiments were made chiefly with a view of seeing whether they yield any sort of air besides that.

Experiment 1st.

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 6½ times its weight of water, and mixed with 100 grains of yeast by way of ferment. The empty space left in the fermenting bottle & tube together measured 1920 grains. The mixture fer-mented 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 it did, before the fermentation began. As none of the liquor ran over, and as little or no liquor con-densed 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, ~~must have con-sisted 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 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 & 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

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 entirely lost its sweetness; so that the vinous fermentation seemed to be compleated; but it was not grown at all sour.

Experiment 2^d^.

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-juice, being evaporated gently to the consistence of a moderately hard extract, was reduced to $\frac{1}{7}$ of its weight; so that the quantity of extract, in the 7060 grains of juice employed, was 1009 grains. The liquor

fermented

 fermented much faster than the sugar and water. The loss of weight during the fermentation was 384 grains. The air remaining unabsorbed in the inverted bottle of sope leys was lost by accident, so that it could not be measured; but, from the space it took up in the inverted bottle, I think I may be certain that it could not much exceed the empty space in the bent tube & fermenting bottle, if it did at all. Therefore there is no reason to think that the apple-juice, any more than the sugar and water, produced any kind of air during the fermentation, except fixed air. It appears too, that the fixed air was near $\frac{181}{1000}$ of the weight of the extract contained in the apple juice. The fermented liquor was very sour; so that it had gone beyond the vinous fermentation, and made some progress in the acetous fermentation.

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

Experiment. 3^d

I first tried in what quantity the air from

from sugar was absorbed by water, and at the same time made a like experiment on the air discharged from marble, by solution in spirit of salt. This was done exactly in the same way as the former experiments of this kind. The result is as follows, beginning with the air from sugar and water.

Air from sugar and water let up = 1000

Bulk of water let up each time.	Bulk of air absorbed each time	Whole bulk of water let up	Whole bulk of air absorbed	Bulk of air remaining	Height of Thermometer when ob. 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	812	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 285 parts of air. The bulk of the air from marble not absorbed, was 194; so that 85 parts were

 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 in substances of different nature: part being absorbed by water in greater quantity than the rest. But, in 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 & water in fermentation.

Experiment 4th.

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 $29\frac{7}{8}$ 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

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 5th.

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 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 6th.

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 two days the fermentation seemed intirely over. The liquor smelt very putrid, and was found to have lost 11½ grains of its weight.

weight. The sopeleys 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 sopeleys, 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

5500 grain measures of this a[illegible] i. e. 4540 of pure factitious 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 1/2 grains in weight on 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 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

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. Macbride'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 & 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 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.