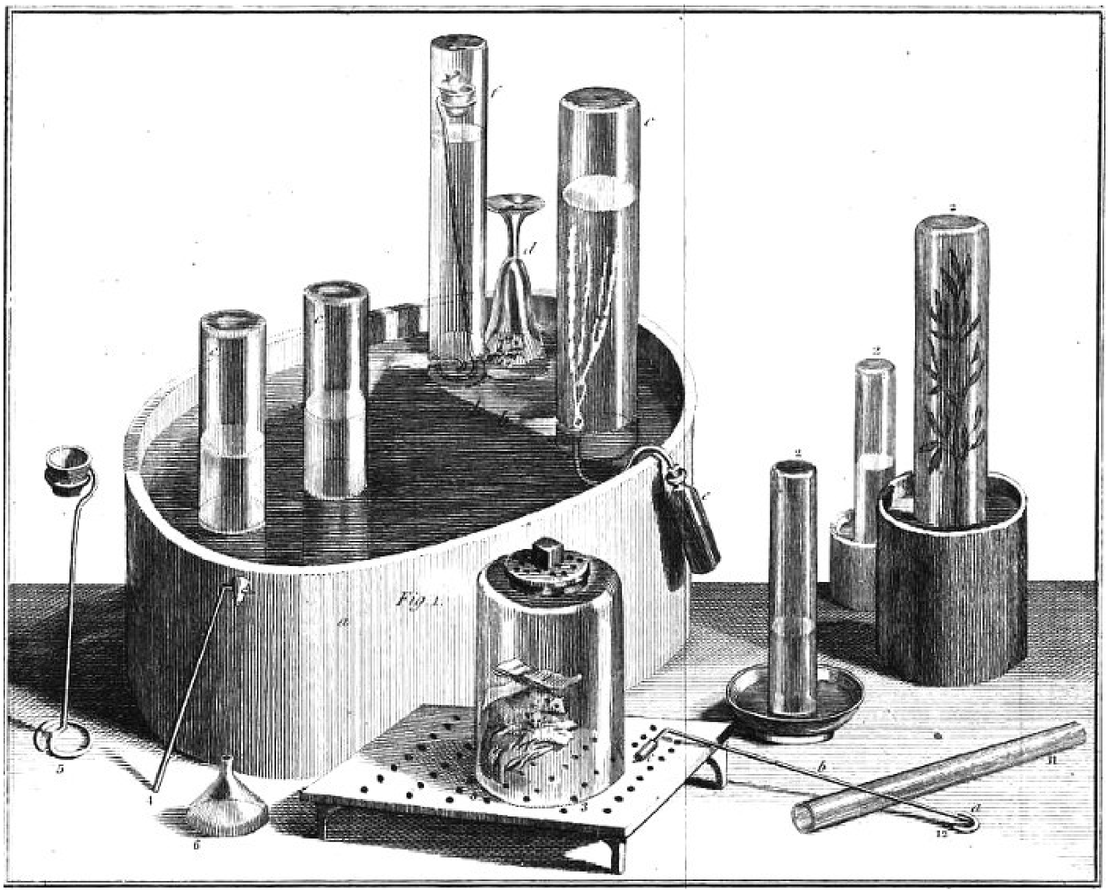

To face the Title.

EXPERIMENTS AND OBSERVATIONS ON DIFFERENT KINDS OF AIR.

[Price 5s. unbound.]

Quamobrem, si qua est erga Creatorem humilitas, si qua operum ejus reverentia et magnificatio, si qua charitas in homines, si erga necessitates et ærumnas humanas relevandas studium, si quis amor veritatis in naturalibus, et odium tenebrarum, et intellectus purificandi desiderium; orandi sunt homines iterum atque iterum, ut, missis philosophiis istis volaticis et preposteris, quæ theses hypothesibus anteposuerunt, et experientiam captivam duxerunt, atque de operibus dei triumpharunt, summisse, et cum veneratione quadam, ad volumen creaturarum evolvendum accedant; atque in eo moram faciant, meditentur, et ab opinionibus abluti et mundi, caste et integre versentur.——In interpretatione ejus eruenda nulli operæ parcant, sed strenue procedant, persistant, immoriantur.

LORD BACON IN INSTAURATIONE MAGNA.

EXPERIMENTS

AND

OBSERVATIONS

ON DIFFERENT KINDS OF

AIR.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

The SECOND EDITION Corrected.

Fert animus Causas tantarum expromere rerum;
Immensumque aperitur opus.

LUCAN

LONDON:

Printed for J. JOHNSON, No. 72, in St. Paul's Church-Yard.

MDCCLXXV.

TO THE RIGHT HONOURABLE
THE EARL OF SHELBURNE,
THIS TREATISE IS
WITH THE GREATEST GRATITUDE
AND RESPECT,
INSCRIBED,
BY HIS LORDSHIP's
MOST OBLIGED,
AND OBEDIENT
HUMBLE SERVANT,
J. PRIESTLEY.

Transcriber's Note: Footnotes have been moved to the end of the chapter. The errata listed at the end of the book have been corrected in the text.

THE PREFACE.

One reason for the present publication has been the favourable reception of those of my *Observations on different kinds of air*, which were published in the Philosophical Transactions for the year 1772, and the demand for them by persons who did not chuse, for the sake of those papers only, to purchase the whole volume in which they were contained. Another motive was the *additions* to my observations on this subject, in consequence of which my papers grew too large for such a publication as the *Philosophical Transactions*.

Contrary, therefore, to my intention, expressed Philosophical Transactions, vol. 64. p. 90, but with the approbation of the President, and of my friends in the society, I have determined to send them no more papers for the present on this subject, but to make a separate and immediate publication of all that I have done with respect to it.

Besides, considering the attention which, I am informed, is now given to this subject by philosophers in all parts of Europe, and the rapid progress that has already been made, and may be expected to be made in this branch of knowledge, all unnecessary delays in the publication of experiments relating to it are peculiarly unjustifiable.

When, for the sake of a little more reputation, men can keep brooding over a new fact, in the discovery of which they might, possibly, have very little real merit, till they think they can astonish the world with a system as complete as it is new, and give mankind a prodigious idea of their judgment and penetration; they are justly punished for their ingratitude to the fountain of all knowledge, and for their want of a genuine love of science and of mankind, in finding their boasted discoveries anticipated, and the field of honest fame pre-occupied, by men, who, from a natural ardour of mind, engage in philosophical pursuits, and with an ingenuous simplicity immediately communicate to others whatever occurs to them in their inquiries.

As to myself, I find it absolutely impossible to produce a work on this subject that shall be any thing like *complete*. My first publication I acknowledged to be very imperfect, and the present, I am as ready to acknowledge, is still more so. But, paradoxical as it may seem, this will ever be the case in the progress of natural science, so long as the works of God are, like himself, infinite and inexhaustible. In completing one discovery we never fail to get an imperfect knowledge of others, of which we could have no idea before; so that we cannot solve one doubt without creating several new ones.

Travelling on this ground resembles Pope's description of travelling among the Alps, with this difference, that here there is not only *succession*, but an *increase* of new objects and new difficulties.

> So pleas'd at first the tow'ring Alps we try,
> Mount o'er the vales, and seem to tread the sky.
> Th' eternal snows appear already past,
> And the first clouds and mountains seem the last,
> But those attain'd, we tremble to survey
> The growing labours of the lengthen'd way.
> Th' increasing prospect tires our wand'ring eyes,
> Hills peep o'er hills, and Alps on Alps arise.

ESSAY ON CRITICISM.

Newton, as he had very little knowledge of *air*, so he had few doubts concerning it. Had Dr. Hales, after his various and valuable investigations, given a list of all his *desiderata*, I am confident that he would not have thought of one in ten that had occurred to me at the time of my last publication; and my doubts, queries, and hints for new experiments are very considerably increased, after a series of investigations, which have thrown great light upon many things of which I was not able to give any explanation before.

I would observe farther, that a person who means to serve the cause of science effectually, must hazard his own reputation so far as to risk even *mistakes* in things of less moment. Among a multiplicity of new objects, and new relations, some will necessarily pass without sufficient attention; but if a man be not mistaken in the principal objects of his pursuits, he has no occasion to distress himself about lesser things.

In the progress of his inquiries he will generally be able to rectify his own mistakes; or if little and envious souls should take a malignant pleasure in detecting them for him, and endeavouring to expose him, he is not worthy of the name of a philosopher, if he has not strength of mind sufficient to enable him not to be disturbed at it. He who does not foolishly affect to be above the failings of humanity, will not be mortified when it is proved that he is but a man.

In this work, as well as in all my other philosophical writings, I have made it a rule not to conceal the *real views* with which I have made experiments; because though, by following a contrary maxim, I might have acquired a character of greater sagacity, I think that two very good ends are answered by the method that I have adopted. For it both tends to make a narrative of a course of experiments more interesting, and likewise encourages other adventurers in experimental philosophy; shewing them that, by pursuing even false lights, real and important truths may be discovered, and that in seeking one thing we often find another.

In some respects, indeed, this method makes the narrative *longer*, but it is by making it less tedious; and in other respects I have written much more concisely than is usual with those who publish accounts of their experiments. In this treatise the reader will often find the result of long processes expressed in a few lines, and of many such in a single paragraph; each of which, if I had, with the usual parade, described it at large (explaining first the *preparation*, then reciting the *experiment* itself, with the *result* of it, and lastly making suitable *reflections*) would have made as many sections or chapters, and have swelled my book to a pompous and respectable size. But I have the pleasure to think that those philosophers who have but little time to spare for *reading*, which is always the case with those who *do* much themselves, will thank me for not keeping them too long from their own pursuits; and that they will find rather more in the volume, than the appearance of it promises.

I do not think it at all degrading to the business of experimental philosophy, to compare it, as I often do, to the diversion of *hunting*, where it sometimes happens that those who have beat the ground the most, and are consequently the best acquainted with it, weary themselves without starting any game; when it may fall in the way of a mere passenger; so that there is but little room for boasting in the most successful termination of the chace.

The best founded praise is that which is due to the man, who, from a supreme veneration for the God of nature, takes pleasure in contemplating his *works*, and from a love of his fellow-creatures, as the offspring of the same all-wise and benevolent parent, with a grateful sense and perfect enjoyment of the means of happiness of which he is already possessed, seeks, with earnestness, but without murmuring or impatience, that greater *command of the powers of nature*, which can only be obtained by a more extensive and more accurate *knowledge* of them; and which alone can enable us to avail ourselves of the numerous advantages with which we are surrounded, and contribute to make our common situation more secure and happy.

Besides, the man who believes that there is a *governor* as well as a *maker* of the world (and there is certainly equal reason to believe both) will acknowledge his providence and favour at least as much in a successful pursuit of *knowledge*, as of *wealth*; which is a sentiment that entirely cuts off all boasting with respect to ourselves, and all envy and jealousy with respect to others; and disposes us mutually to rejoice in every new light that we receive, through whose hands soever it be conveyed to us.

I shall pass for an enthusiast with some, but I am perfectly easy under the imputation, because I am happy in those views which subject me to it; but considering the amazing improvements in natural knowledge which have been made within the last century, and the many ages, abounding with men who had no other object but study, in which, however, nothing of this kind was done, there appears to me to be a very particular providence in the concurrence of those circumstances which have produced so great a change; and I cannot help flattering myself that this will be instrumental in bringing about other changes in the state of the world, of much more consequence to the improvement and happiness of it.

This rapid progress of knowledge, which, like the progress of a *wave* of the sea, of *sound*, or of *light* from the sun, extends itself not this way or that way only, but *in all directions*, will, I doubt not, be the means, under God, of extirpating *all* error and prejudice, and of putting an end to all undue and usurped authority in the business of *religion*, as well as of *science*; and all the efforts of the interested friends of corrupt establishments of all kinds will be ineffectual for their support in this enlightened age: though, by retarding their downfal, they may make the final ruin of them more complete and glorious. It was ill policy in Leo the Xth to patronize polite literature. He was cherishing an enemy in disguise. And the English hierarchy (if there be any thing unsound in its constitution) has equal reason to tremble even at an air-pump, or an electrical machine.

There certainly never was any period in which *natural knowledge* made such a progress as it has done of late years, and especially in this country; and they who affect to speak with supercilious contempt of the publications of the present age in general, or of the Royal Society in particular, are only those who are themselves engaged in the most trifling of all literary pursuits, who are unacquainted with all real science, and are ignorant of the progress and present state of it.[1]

It is true that the rich and the great in this country give less attention to these subjects than, I believe, they were ever known to do, since the time of Lord Bacon, and much less than men of rank and fortune in other countries give to them. But with us this loss is made up by men of leisure, spirit, and ingenuity, in the middle ranks of life, which is a circumstance that promises better for the continuance of this progress in useful knowledge than any noble or royal patronage. With us, politics chiefly engage the attention of those who stand foremost in the community, which, indeed, arises from the *freedom* and peculiar *excellence* of our constitution, without which even the spirit of men of letters in general, and of philosophers in particular, who never directly interfere in matters of government, would languish.

It is rather to be regretted, however, that, in such a number of nobility and gentry, so very few should have any taste for scientifical pursuits, because, for many valuable purposes of science, *wealth* gives a decisive advantage. If extensive and lasting *fame* be at all an object, literary, and especially scientifical pursuits, are preferable to political ones in a variety of respects. The former are as much more favourable for the display of the human faculties than the latter, as the *system of nature* is superior to any *political system* upon earth.

If extensive *usefulness* be the object, science has the same advantage over politics. The greatest success in the latter seldom extends farther than one particular country, and one particular age; whereas a successful pursuit of science makes a man the benefactor of all mankind, and of every age. How trifling is the fame of any statesman that this country has ever produced to that of Lord Bacon, of Newton, or of Boyle; and how much greater are our obligations to such men as these, than to any other in the whole *Biographia Britannica*; and every country, in which science has flourished, can furnish instances for similar observations.

Here my reader will thank me, and the writer will, I hope, forgive me, if I quote a passage from the postscript of a letter which I happen to have just received from that excellent, and in my opinion, not too enthusiastical philosopher, father Beccaria of Turin.

> *Mi spiace che il mondo politico ch'è pur tanto passeggero, rubbi il grande Franklin al mondo della natura, che non sa ne cambiare, ne mancare.* In English. "I am sorry that the *political world*, which is so very transitory, should take the great Franklin from the *world of nature*, which can never change, or fail."

I own it is with peculiar pleasure that I quote this passage, respecting this truly great man, at a time when some of the infatuated politicians of this country are vainly thinking to build their wretched and destructive projects, on the ruins of his established reputation; a reputation as extensive as the spread of science itself, and of which it is saying very little indeed, to pronounce that it will last and flourish when the names of all his enemies shall be forgotten.

I think it proper, upon this occasion, to inform my friends, and the public, that I have, for the present, suspended my design of writing *the history and present state of all the branches of experimental philosophy*. This has arisen not from any dislike of the undertaking, but, in truth, because I see no prospect of being reasonably indemnified for so much labour and expence, notwithstanding the specimens I have already given of that work (in the *history of electricity*, and of the *discoveries relating to vision, light, and colours*) have met with a much more favourable reception from the best judges both at home and abroad, than I expected. Immortality, if I should have any view to it, is not the proper price of such works as these.

I propose, however, having given so much attention to the subject of *air*, to write, at my leisure, the history and present state of discoveries relating to it; in which case I shall, as a part of it, reprint this work, with such improvements as shall have occurred to me at that time; and I give this notice of it, that no person who intends to purchase it may have reason (being thus apprised of my intention) to complain of buying the same thing twice. If any person chuse it, he may save his five or six shillings for the present, and wait five or six years longer (if I should live so long) for the opportunity of buying the same thing, probably much enlarged, and at the same time a complete account of all that has been done by others relating to this subject.

Though for the plain, and I hope satisfactory reason above mentioned, I shall probably write no other *histories* of this kind, I shall, as opportunity serves, endeavour to provide *materials* for such histories, by continuing my experiments, keeping my eyes open to such new appearances as may present themselves, investigating them as far as I shall be able, and never failing to communicate to the public, by some channel or other, the result of my observations.

In the publication of this work I have thought that it would be agreeable to my readers to preserve, in some measure, the order of history, and therefore I have not thrown together all that I have observed with respect to each kind of air, but have divided the work into *two parts*; the former containing what was published before, in the Philosophical Transactions, with such observations and corrections as subsequent experience has suggested to me; and I have reserved for the latter part of the work an account of the experiments which I have made since that publication, and after a pretty long interruption in my philosophical pursuits, in the course of the last summer. Besides I am sensible that in the latter part of this work a different arrangement of the subjects will be more convenient, for their mutual illustration.

Some persons object to the term *air*, as applied to *acid*, *alkaline*, and even *nitrous air*; but it is certainly very convenient to have a common term by which to denote things which have so many common properties, and those so very striking; all of them agreeing with the air in which we breathe, and with *fixed air*, in *elasticity*, and *transparency*, and in being alike affected by heat or cold; so that to the eye they appear to have no difference at all. With much more reason, as it appears to me, might a person object to the common term *metal*, as applied to things so very different from one another as gold, quicksilver, and lead.

Besides, *acid* and *alkaline* air do not differ from *common air* (in any respect that can countenance an objection to their having a common appellation) except in such properties as are common to it with *fixed air*, though in a different degree; viz. that of being imbibed by water. But, indeed, all kinds of air, common air itself not excepted, are capable of being imbibed by water in some degree.

Some may think the terms acid and alkaline *vapour* more proper than acid and alkaline *air*. But the term *vapour* having always been applied to elastic matters capable of being condensed in the temperature of the atmosphere, especially the vapour of water, it seems harsh to apply it to any elastic substance, which at the same time that it is as transparent as the air we breathe, is no more affected by cold than it is.

As my former papers were immediately translated into several foreign languages, I may presume that this treatise, having a better title to it, will be translated also; and, upon this presumption, I cannot help expressing a wish, that it may be done by persons who have a competent knowledge of *subject*, as well as of the *English* *language*. The mistakes made by some foreigners, have induced me to give this caution.

London, Feb.
1774.

ADVERTISEMENT.

The *weights* mentioned in the course of this treatise are *Troy*, and what is called *an ounce measure of air*, is the space occupied by an ounce weight of water, which is equal to 480 grains, and is, therefore, almost two *cubic inches* of water; for one cubic inch weighs 254 grains.

FOOTNOTES:

[1] See Sir John Pringle's *Discourse on the different kinds of air*, p. 29, which, if it became me to do it, I would recommend to the reader, as containing a just and elegant account of the several discoveries that have been successively made, relating to the subject of this treatise.

THE CONTENTS.

THE APPENDIX.

THE INTRODUCTION.

SECTION I.

A general view of PRECEDING DISCOVERIES *relating to air.*

For the better understanding of the experiments and observations on different kinds of air contained in this treatise, it will be useful to those who are not acquainted with the history of this branch of natural philosophy, to be informed of those facts which had been discovered by others, before I turned my thoughts to the subject; which suggested, and by the help of which I was enabled to pursue, my inquiries. Let it be observed, however, that I do not profess to recite in this place *all* that had been discovered concerning air, but only those discoveries the knowledge of which is necessary, in order to understand what I have done myself; so that any person who is only acquainted with the general principles of natural philosophy, may be able to read this treatise, and, with proper attention, to understand every part of it.

That the air which constitutes the atmosphere in which we live has *weight*, and that it is *elastic*, or consists of a compressible and dilatable fluid, were some of the earliest discoveries that were made after the dawning of philosophy in this western part of the world.

That elastic fluids, differing essentially from the air of the atmosphere, but agreeing with it in the properties of weight, elasticity, and transparency, might be generated from solid substances, was discovered by Mr. Boyle, though two remarkable kinds of factitious air, at least the effects of them, had been known long before to all miners. One of these is heavier than common air. It lies at the bottom of pits, extinguishes candles, and kills animals that breathe it, on which account it had obtained the name of the *choke damp*. The other is lighter than common air, taking its place near the roofs of subterraneous places, and because it is liable to take fire, and explode, like gunpowder, it had been called the *fire damp*. The word *damp* signifies *vapour* or *exhalation* in the German and Saxon language.

Though the former of these kinds of air had been known to be noxious, the latter I believe had not been discovered to be so, having always been found in its natural state, so much diluted with common air, as to be breathed with safety. Air of the former kind, besides having been discovered in various caverns, particularly the *grotta del Cane* in Italy, had also been observed on the surface of fermenting liquors, and had been called *gas* (which is the same with *geist*, or *spirit*) by Van Helmont, and other German chymists; but afterwards it obtained the name of *fixed air*, especially after it had been discovered by Dr. Black of Edinburgh to exist, in a fixed state, in alkaline salts, chalk, and other calcareous substances.

This excellent philosopher discovered that it is the presence of the fixed air in these substances that renders them *mild*, and that when they are deprived of it, by the force of fire, or any other process, they are in that state which had been called *caustic*, from their corroding or burning animal and vegetable substances.

Fixed air had been discovered by Dr. Macbride of Dublin, after an observation of Sir John Pringle's, which led to it, to be in a considerable degree antiseptic; and since it is extracted in great plenty from fermenting vegetables, he had recommended the use of *wort* (that is an infusion of malt in water) as what would probably give relief in the sea-scurvy, which is said to be a putrid disease.

Dr. Brownrigg had also discovered that the same species of air is contained in great quantities in the water of the Pyrmont spring at Spa in Germany, and in other mineral waters, which have what is called an *acidulous* taste, and that their peculiar flavour, briskness, and medicinal virtues, are derived from this ingredient.

Dr. Hales, without seeming to imagine that there was any material difference between these kinds of air and common air, observed that certain substances and operations *generate* air, and others *absorb* it; imagining that the diminution of air was simply a taking away from the common mass, without any alteration in the properties of what remained. His experiments, however, are so numerous, and various, that they are justly esteemed to be the solid foundation of all our knowledge of this subject.

Mr. Cavendish had exactly ascertained the specific gravities of fixed and inflammable air, shewing the former of them to be 1-1/2 heavier than common air, and the latter ten times lighter. He also shewed that water would imbibe more than its own bulk of fixed air.

Lastly, Mr. Lane discovered that water thus impregnated with fixed air will dissolve a considerable quantity of iron, and thereby become a strong chalybeate.

These, I would observe, are by no means all the discoveries concerning air that have been made by the gentlemen whose names I have mentioned, and still less are they all that have been made by others; but they comprise all the previous knowledge of this subject that is necessary to the understanding of this treatise; except a few particulars, which will be mentioned in the course of the work, and which it is, therefore, unnecessary to recite in this place.

SECTION II.

An account of the APPARATUS *with which the following experiments were made.*

Rather than describe at large the manner in which every particular experiment that I shall have occasion to recite was made, which would both be very tedious, and require an unnecessary multiplicity of drawings, I think it more adviseable to give, at one view, an account of all my apparatus and instruments, or at least of every thing that can require a description, and of all the different operations and processes in which I employ them.

It will be seen that my apparatus for experiments on air is, in fact, nothing more than the apparatus of Dr. Hales, Dr. Brownrigg, and Mr. Cavendish, diversified, and made a little more simple. Yet notwithstanding the simplicity of this apparatus, and the ease with which all the operations are conducted, I would not have any person, who is altogether without experience, to imagine that he shall be able to select any of the following experiments, and immediately perform it, without difficulty or blundering. It is known to all persons who are conversant in experimental philosophy, that there are many little attentions and precautions necessary to be observed in the conducting of experiments, which cannot well be described in words, but which it is needless to describe, since practice will necessarily suggest them; though, like all other arts in which the hands and fingers are made use of, it is only *much practice* that can enable a person to go through complex experiments, of this or any other kind, with ease and readiness.

For experiments in which air will bear to be confined by water, I first used an oblong trough made of earthen ware, as *a* fig. 1. about eight inches deep, at one end of which I put thin flat stones, *b. b.* about an inch, or half an inch, under the water, using more or fewer of them according to the quantity of water in the trough. But I have since found it more convenient to use a larger wooden trough, of the same general shape, eleven inches deep, two feet long, and 1-1/2 wide, with a shelf about an inch lower than the top, instead of the flat stones above-mentioned. This trough being larger than the former, I have no occasion to make provision for the water being higher or lower, the bulk of a jar or two not making so great a difference as did before.

The several kinds of air I usually keep in *cylindrical jars*, as *c*, *c*, fig. 1, about ten inches long, and 2-1/2 wide, being such as I have generally used for electrical batteries, but I have likewise vessels of very different forms and sizes, adapted to particular experiments.

When I want to remove vessels of air from the large trough, I place them in *pots* or *dishes*, of various sizes, to hold more or less water, according to the time that I have occasion to keep the air, as fig. 2. These I plunge in water, and slide the jars into them; after which they may be taken out together, and be set wherever it shall be most convenient. For the purpose of merely removing a jar of air from one place to another, where it is not to stand longer than a few days, I make use of common *tea-dishes*, which will hold water enough for that time, unless the air be in a state of diminution, by means of any process that is going on in it.

If I want to try whether an animal will live in any kind of air, I first put the air into a small vessel, just large enough to give it room to stretch itself; and as I generally make use of *mice* for this purpose, I have found it very convenient to use the hollow part of a tall beer-glass, *d* fig. 1, which contains between two and three ounce measures of air. In this vessel a mouse will live twenty minutes, or half an hour.

For the purpose of these experiments it is most convenient to catch the mice in small wire traps, out of which it is easy to take them, and holding them by the back of the neck, to pass them through the water into the vessel which contains the air. If I expect that the mouse will live a considerable time, I take care to put into the vessel something on which it may conveniently sit, out of the reach of the water. If the air be good, the mouse will soon be perfectly at its ease, having suffered nothing by its passing through the water. If the air be supposed to be noxious, it will be proper (if the operator be desirous of preserving the mice for farther use) to keep hold of their tails, that they may be withdrawn as soon as they begin to shew signs of uneasiness; but if the air be thoroughly noxious, and the mouse happens to get a full inspiration, it will be impossible to do this before it be absolutely irrecoverable.

In order to *keep* the mice, I put them into receivers open at the top and bottom, standing upon plates of tin perforated with many holes, and covered with other plates of the same kind, held down by sufficient weights, as fig. 3. These receivers stand upon *a frame of wood*, that the fresh air may have an opportunity of getting to the bottoms of them, and circulating through them. In the inside I put a quantity of paper or tow, which must be changed, and the vessel washed and dried, every two or three days. This is most conveniently done by having another receiver, ready cleaned and prepared, into which the mice may be transferred till the other shall be cleaned.

Mice must be kept in a pretty exact temperature, for either much heat or much cold kills them presently. The place in which I have generally kept them is a shelf over the kitchen fire-place where, as it is usual in Yorkshire, the fire never goes out; so that the heat varies very little, and I find it to be, at a medium, about 70 degrees of Fahrenheit's thermometer. When they had been made to pass through the water, as they necessarily must be in order to a change of air, they require, and will bear a very considerable degree of heat, to warm and dry them.

I found, to my great surprize, in the course of these experiments, that mice will live intirely without water; for though I have kept them for three or four months, and have offered them water several times, they would never taste it; and yet they continued in perfect health and vigour. Two or three of them will live very peaceably together in the same vessel; though I had one instance of a mouse tearing another almost in pieces, and when there was plenty of provisions for both of them.

In the same manner in which a mouse is put into a vessel of any kind of air, a *plant*, or any thing else, may be put into it, viz. by passing it through the water; and if the plant be of a kind that will grow in water only, there will be no occasion to set it in a pot of earth, which will otherwise be necessary.

There may appear, at first sight, some difficulty in opening the mouth of a phial, containing any substance, solid or liquid, to which water must not be admitted, in a jar of any kind of air, which is an operation that I have sometimes had recourse to; but this I easily effect by means of *a cork cut tapering*, and a strong, wire thrust through it, as in fig. 4, for in this form it will sufficiently fit the mouth of any phial, and by holding the phial in one hand, and the wire in the other, and plunging both my hands into the trough of water, I can easily convey the phial through the water into the jar; which must either be held by an assistant, or be fastened by strings, with its mouth projecting over the shelf. When the phial is thus conveyed into the jar, the cork may easily be removed, and may also be put into it again at pleasure, and conveyed the same way out again.

When any thing, as a gallipot, &c. is to be supported at a considerable height within a jar, it is convenient to have such *wire stands* as are represented fig. 5. They answer better than any other, because they take up but little room, and may be easily bended to any shape or height.

If I have occasion to pour air from a vessel with a wide mouth into another with a very narrow one, I am obliged to make use of a funnel, fig. 6, but by this means the operation is exceedingly easy; first filling the vessel into which the air is to be conveyed with water, and holding the mouth of it, together with the funnel, both under water with one hand, while the other is employed in pouring the air; which, ascending through the funnel up into the vessel, makes the water descend, and takes its place. These funnels are best made of glass, because the air being visible through them, the quantity of it may be more easily estimated by the eye. It will be convenient to have several of these funnels of different sizes.

In order to expel air from solid substances by means of heat, I sometimes put them into a *gun-barrel*, fig. 7, and filling it up with dry sand, that has been well burned, so that no air can come from it, I lute to the open end the stem of a tobacco pipe, or a small glass tube. Then having put the closed end of the barrel, which contains the materials, into the fire, the generated air, issuing through the tube, may be received in a vessel of quicksilver, with its mouth immersed in a bason of the same, suspended all together in wires, in the manner described in the figure: or any other fluid substance may be used instead of quicksilver.

But the most accurate method of procuring air from several substances, by means of heat, is to put them, if they will bear it, into phials full of quicksilver, with the mouths immersed in the same, and then throw the focus of a burning mirror upon them. For this purpose the phials should be made with their bottoms round, and very thin, that they may not be liable to break with a pretty sudden application of heat.

If I want to expel air from any liquid, I nearly fill a phial with it, and having a cork perforated, I put through it, and secure with cement, a glass tube, bended in the manner represented at *e* fig. 1. I then put the phial into a kettle of water, which I set upon the fire and make to boil. The air expelled by the heat, from the liquor contained in the phial, issues through the tube, and is received in the bason of quicksilver, fig. 7. Instead of this suspended bason, I sometimes content myself with tying a flaccid bladder to the end of the tube, in both these processes, that it may receive the newly generated air.

In experiments on those kinds of air which are readily imbibed by water, I always make use of quicksilver, in the manner represented fig. 8, in which *a* is the bason of quicksilver, *b* a glass vessel containing quicksilver, with its mouth immersed in it, *c* a phial containing the ingredients from which the air is to be produced; and *d* is a small recipient, or glass vessel designed to receive and intercept any liquor that may be discharged along with the air, which is to be transmitted free from any moisture into the vessel *b*. If there be no apprehension of moisture, I make use of the glass tube only, without any recipient, in the manner represented *e* fig. 1. In order to invert the vessel *b*, I first fill it with quicksilver, and then carefully cover the mouth of it with a piece of soft leather; after which it may be turned upside down without any danger of admitting the air, and the leather may be withdrawn when it is plunged in the quicksilver.

In order to generate air by the solution of metals, or any process of a similar nature, I put the materials into a phial, prepared in the manner represented at *e* fig. 1, and put the end of the glass tube under the mouth of any vessel into which I want to convey the air. If heat be necessary I can easily apply to it a candle, or a red hot poker while it hangs in this position.

When I have occasion to transfer air from a jar standing in the trough of water to a vessel standing in quicksilver, or in any other situation whatever, I make use of the contrivance represented fig. 9, which consists of a bladder, furnished at one end with a small glass tube bended, and at the other with a cork, perforated so as just to admit the small end of a funnel. When the common air is carefully pressed out of this bladder, and the funnel is thrust tightly into the cork, it may be filled with any kind of air as easily as a glass jar; and then a string being tied above the cork in which the funnel is inserted, and the orifice in the other cork closed, by pressing the bladder against it, it may be carried to any place, and if the tube be carefully wiped, the air may be conveyed quite free from moisture through a body of quicksilver, or any thing else. A little practice will make this very useful manœuvre perfectly easy and accurate.

In order to impregnate fluids with any kind of air, as water with fixed air, I fill a phial with the fluid larger or less as I have occasion (as *a* fig. 10;) and then inverting it, place it with its mouth downwards, in a bowl *b*, containing a quantity of the same fluid; and having filled the bladder, fig. 9, with the air, I throw as much of it as I think proper into the phial, in the manner described above. To accelerate the impregnation, I lay my hand on the top of the phial, and shake it as much as I think proper.

If, without having any air previously generated, I would convey it into the fluid immediately as it arises from the proper materials, I keep the same bladder in connection with a phial *c* fig. 10, containing the same materials (as chalk, salt of tartar, or pearl ashes in diluted oil of vitriol, for the generation of fixed air) and taking care, lest, in the act of effervescence, any of the materials in the phial *c* should get into the vessel *a*, to place this phial on a stand lower than that on which the bason was placed, I press out the newly generated air, and make it ascend directly into the fluid. For this purpose, and that I may more conveniently shake the phial *c*, which is necessary in some processes, especially with chalk and oil of vitriol, I sometimes make use of a flexible leathern tube *d*, and sometimes only a glass tube. For if the bladder be of a sufficient length, it will give room for the agitation of the phial; or if not, it is easy to connect two bladders together by means of a perforated cork, to which they may both be fastened.

When I want to try whether any kind of air will admit a candle to burn in it, I make use of a cylindrical glass vessel, fig. 11. and a bit of wax candle *a* fig. 12, fastened to the end of a wire *b*, and turned up, in such a manner as to be let down into the vessel with the flame upwards. The vessel should be kept carefully covered till the moment that the candle is admitted. In this manner I have frequently extinguished a candle more than twenty times successively, in a vessel of this kind, though it is impossible to dip the candle into it without giving the external air an opportunity of mixing with the air in the inside more or less. The candle *c*, at the other end of the wire is very convenient for holding under a jar standing in water, in order to burn as long as the inclosed air can supply it; for the moment that it is extinguished, it may be drawn through the water before any smoke can have mixed with the air.

In order to draw air out of a vessel which has its mouth immersed in water, and thereby to raise the water to whatever height may be necessary, it is very convenient to make use of a glass *syphon*, fig. 13, putting one of the legs up into the vessel, and drawing the air out at the other end by the mouth. If the air be of a noxious quality, it may be necessary to have a syringe fastened to the syphon, the manner of which needs no explanation. I have not thought it safe to depend upon a valve at the top of the vessel, which Dr. Hales sometimes made use of.

If, however, a very small hole be made at the top of a glass vessel, it may be filled to any height by holding it under water, while the air is issuing out at the hole, which may then be closed with wax or cement.

If the generated air will neither be absorbed by water, nor diminish common air, it may be convenient to put part of the materials into a cup, supported by a stand, and the other part into a small glass vessel, placed on the edge of it, as at *f*, fig. 1. Then having, by means of a syphon, drawn the air to at convenient height, the small glass vessel may be easily pushed into the cup, by a wire introduced through the water; or it may be contrived, in a variety of ways, only to discharge the contents of the small vessel into the larger. The distance between the boundary of air and water, before and after the operation, will shew the quantity of the generated air. The effect of processes that *diminish* air may also be tried by the same apparatus.

When I want to admit a particular kind of air to any thing that will not bear wetting, and yet cannot be conveniently put into a phial, and especially if it be in the form of a powder, and must be placed upon a stand (as in those experiments in which the focus of a burning mirror is to be thrown upon it) I first exhaust a receiver, in which it is previously placed; and having a glass tube, bended for the purpose, as in fig. 14, I screw it to the stem of a transfer of the air pump on which the receiver had been exhausted, and introducing it through the water into a jar of that kind of air with which I would fill the receiver, I only turn the cock, and I gain my purpose. In this method, however, unless the pump be very good, and several contrivances, too minute to be particularly described, be made use of a good deal of common air will get into the receiver.

When I want to measure the goodness of any kind of air, I put two measures of it into a jar standing in water; and when I have marked upon the glass the exact place of the boundary of air and water, I put to it one measure of nitrous air; and after waiting a proper time, note the quantity of its diminution. If I be comparing two kinds of air that are nearly alike, after mixing them in a large jar, I transfer the mixture into a long glass tube, by which I can lengthen my scale to what degree I please.

If the quantity of the air, the goodness of which I want to ascertain, be exceedingly small, so as to be contained in a part of a glass tube, out of which water will not run spontaneously, as *a* fig. 15; I first measure with a pair of compasses the length of the column of air in the tube, the remaining part being filled with water, and lay it down upon a scale; and then, thrusting a wire of a proper thickness, *b*, into the tube, I contrive, by means of a thin plate of iron, bent to a sharp angle *c*, to draw it out again, when the whole of this little apparatus has been introduced through the water into a jar of nitrous air; and the wire being drawn out, the air from the jar must supply its place. I then measure the length of this column of nitrous air which I have got into the tube, and lay it also down upon the scale, so as to know the exact length of both the columns. After this, holding the tube under water, with a small wire I force the two separate columns of air into contact, and when they have been a sufficient time together, I measure the length of the whole, and compare it with the length of both the columns taken before. A little experience will teach the operator how far to thrust the wire into the tube, in order to admit as much air as he wants and no more.

In order to take the electric spark in a quantity of any kind of air, which must be very small, to produce a sensible effect upon it, in a short time, by means of a common machine, I put a piece of wire into the end of a small tube, and fasten it with hot cement, as in fig. 16; and having got the air I want into the tube by means of the apparatus fig. 15, I place it inverted in a bason containing either quicksilver, or any other fluid substance by which I chuse to have the air confined. I then, by the help of the air pump, drive out as much of the air as I think convenient, admitting the quicksilver, &c. to it, as at *a*, and putting a brass ball on the end of the wire, I take the sparks or shocks upon it, and thereby transmit them through the air to the liquor in the tube.

To take the electric sparks in any kind of fluid, as oil, &c. I use the same apparatus described above, and having poured into the tube as much of the fluid as I conjecture I can make the electric spark pass through, I fill the rest with quicksilver; and placing it inverted in a bason of quicksilver, I take the sparks as before.

If air be generated very fast by this process, I use a tube that is narrow at the top, and grows wider below, as fig. 17, that the quicksilver may not recede too soon beyond the striking distance.

Sometimes I have used a different apparatus for this purpose, represented fig. 18. Taking a pretty wide glass tube, hermetically sealed at the upper-end, and open below, at about an inch, or at what distance I think convenient from the top, I get two holes made in it, opposite to each other. Through these I put two wires, and fastening them with warm cement, I fix them at what distance I please from each other. Between these wires I take the sparks, and the bubbles of air rise, as they are formed, to the top of the tube.

PART I.

Experiments and Observations made in, and before the year 1772.

In writing upon the subject of *different kinds of air*, I find myself at a loss for proper *terms*, by which to distinguish them, those which have hitherto obtained being by no means sufficiently characteristic, or distinct. The only terms in common use are, *fixed air*, *mephitic*, and *inflammable*. The last, indeed, sufficiently characterizes and distinguishes that kind of air which takes fire, and explodes on the approach of flame; but it might have been termed *fixed* with as much propriety as that to which Dr. Black and others have given that denomination, since it is originally part of some solid substance, and exists in an unelastic state.

All these newly discovered kinds of air may also be called *factitious*; and if, with others, we use the term *fixable*, it is still obvious to remark, that it is applicable to them all; since they are all capable of being imbibed by some substance or other, and consequently of being *fixed* in them, after they have been in an elastic state.

The term *mephitic* is equally applicable to what is called *fixed air*, to that which is *inflammable*, and to many other kinds; since they are equally noxious, when breathed by animals. Rather, however, than either introduce new terms, or change the signification of old ones, I shall use the term *fixed air*, in the sense in which it is now commonly used, and distinguish the other kinds by their properties, or some other periphrasis. I shall be under a necessity, however, of giving names to those kinds of air, to which no names had been given by others, as *nitrous*, *acid*, and *alkaline*.

SECTION I.

Of Fixed Air.

It was in consequence of living for some time in the neighbourhood of a public brewery, that I was induced to make experiments on fixed air, of which there is always a large body, ready formed, upon the surface of the fermenting liquor, generally about nine inches, or a foot in depth, within which any kind of substance may be very conveniently placed; and though, in these circumstances, the fixed air must be continually mixing with the common air, and is therefore far from being perfectly pure, yet there is a constant fresh supply from the fermenting liquor, and it is pure enough for many purposes.

A person, who is quite a stranger to the properties of this kind of air, would be agreeably amused with extinguishing lighted candles, or chips of wood in it, as it lies upon the surface of the fermenting liquor; for the smoke readily unites with this kind of air, probably by means of the water which it contains; so that very little or none of the smoke will escape into the open air, which is incumbent upon it. It is remarkable, that the upper surface of this smoke, floating in the fixed air, is smooth, and well defined; whereas the lower surface is exceedingly ragged, several parts hanging down to a considerable distance within the body of the fixed air, and sometimes in the form of balls, connected to the upper stratum by slender threads, as if they were suspended. The smoke is also apt to form itself into broad flakes, parallel to the surface of the liquor, and at different distances from it, exactly like clouds. These appearances will sometimes continue above an hour, with very little variation. When this fixed air is very strong, the smoke of a small quantity of gunpowder fired in it will be wholly retained by it, no part escaping into the common air.

Making an agitation in this air, the surface of it, (which still continues to be exactly defined) is thrown into the form of waves, which it is very amusing to look upon; and if, by this agitation, any of the fixed air be thrown over the side of the vessel, the smoke, which is mixed with it, will fall to the ground, as if it was so much water, the fixed air being heavier than common air.

The red part of burning wood was extinguished in this air, but I could not perceive that a red-hot poker was sooner cooled in it.

Fixed air does not instantly mix with common air. Indeed if it did, it could not be caught upon the surface of the fermenting liquor. A candle put under a large receiver, and immediately plunged very deep below the surface of the fixed air, will burn some time. But vessels with the smallest orifices, hanging with their mouths downwards in the fixed air, will *in time* have the common air, which they contain, perfectly mixed with it. When the fermenting liquor is contained in vessels close covered up, the fixed air, on removing the cover, readily affects the common air which is contiguous to it; so that, candles held at a considerable distance above the surface will instantly go out. I have been told by the workmen, that this will sometimes be the case, when the candles are held two feet above the mouth of the vessel.

Fixed air unites with the smoke of rosin, sulphur, and other electrical substances, as well as with the vapour of water; and yet, by holding the wire of a charged phial among these fumes, I could not make any electrical atmosphere, which surprized me a good deal, as there was a large body of this smoke, and it was so confined, that it could not escape me.

I also held some oil of vitriol in a glass vessel within the fixed air, and by plunging a piece of red-hot glass into it, raised a copious and thick fume. This floated upon the surface of the fixed air like other fumes, and continued as long.

Considering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeast of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and some other medicinal mineral waters. Accordingly, I found, that when the surface of the water was considerable, it always acquired the pleasant acidulous taste that Pyrmont water has. The readiest way of impregnating water with this virtue, in these circumstances, is to take two vessels, and to keep pouring the water from one into the other, when they are both of them held as near the yeast as possible; for by this means a great quantity of surface is exposed to the air, and the surface is also continually changing. In this manner, I have sometimes, in the space of two or three minutes, made a glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont, or rather Seltzer water.

But the *most effectual* way of impregnating water with fixed air is to put the vessels which contain the water into glass jars, filled with the purest fixed air made by the solution of chalk in diluted oil of vitriol, standing in quicksilver. In this manner I have, in about two days, made a quantity of water to imbibe more than an equal bulk of fixed air, so that, according to Dr. Brownrigg's experiments, it must have been much stronger than the best imported Pyrmont; for though he made his experiments at the spring-head, he never found that it contained quite so much as half its bulk of this air. If a sufficient quantity of quicksilver cannot be procured, *oil* may be used with sufficient advantage, for this purpose, as it imbibes the fixed air very slowly. Fixed air may be kept in vessels standing in water for a long time, if they be separated by a partition of oil, about half an inch thick. Pyrmont water made in these circumstances, is little or nothing inferior to that which has stood in quicksilver.

The *readiest* method of preparing this water for use is to agitate it strongly with a large surface exposed to the fixed air. By this means more than an equal bulk of air may be communicated to a large quantity of water in the space of a few minutes. But since agitation promotes the dissipation of fixed air from water, it cannot be made to imbibe so great a quantity in this method as in the former, where more time is taken.

Easy directions for impregnating water with fixed air I have published in a small pamphlet, designed originally for the use of seamen in long voyages, on the presumption that it might be of use for preventing or curing the sea scurvy, equally with wort, which was recommended by Dr. Macbride for this purpose, on no other account than its property of generating fixed air, by its fermentation in the stomach.

Water thus impregnated with fixed air readily dissolves iron, as Mr. Lane has discovered; so that if a quantity of iron filings be put to it, it presently becomes a strong chalybeate, and of the mildest and most agreeable kind.

I have recommended the use of *chalk* and oil of vitriol as the cheapest, and, upon the whole, the best materials for this purpose. But some persons prefer *pearl ashes*, *pounded marble*, or other calcareous or *alkaline substances*; and perhaps with reason. My own experience has not been sufficient to enable me to decide in this case.

Whereas some persons had suspected that a quantity of the oil of vitriol was rendered volatile by this process, I examined it, by all the chemical methods that are in use; but could not find that water thus impregnated contained the least perceivable quantity of that acid.

Mr. Hey, indeed, who assisted me in this examination, found that distilled water, impregnated with fixed air, did not mix so readily with soap as the distilled water itself; but this was also the case when the fixed air had passed through a long glass tube filled with alkaline salts, which, it may be supposed, would have imbibed any of the oil of vitriol that might have been contained in that air[2].

Fixed air itself may be said to be of the nature of an acid, though of a weak and peculiar sort.——Mr. Bergman of Upsal, who honoured me with a letter upon the subject, calls it the *aërial acid*, and, among other experiments to prove it to be an acid, he says that it changes the blue juice of tournesole into red. This Mr. Hey found to be true, and he moreover discovered that when water tinged blue with the juice of tournesole, and then red with fixed air, has been exposed to the open air, it recovers its blue colour again.

The heat of boiling water will expel all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

Dr. Percival, who is particularly attentive to every improvement in the medical art, and who has thought so well of this impregnation as to prescribe it in several cases, informs me that it seems to be much stronger, and sparkles more, like the true Pyrmont water, after it has been kept some time. This circumstance, however, shews that, in time, the fixed air is more easily disengaged from the water; and though, in this state, it may affect the taste more sensibly, it cannot be of so much use in the stomach and bowels, as when the air is more firmly retained by the water.

By the process described in my pamphlet, fixed air may be readily incorporated with wine, beer, and almost any other liquor whatever; and when beer, wine, or cyder, is become flat or dead (which is the consequence of the escape of the fixed air they contained) they may be revived by this means; but the delicate and agreeable flavour, or acidulous taste, communicated by fixed air, and which is very manifest in water, can hardly be perceived in wine, or any liquors which have much taste of their own.

I should think that there can be no doubt, but that water thus impregnated with fixed air must have all the medicinal virtues of genuine Pyrmont or Seltzer water; since these depend upon the fixed air they contain. If the genuine Pyrmont water derives any advantage from its being a natural chalybeate, this may also be obtained by providing a common chalybeate water, and using it in these processes, instead of common water.

Having succeeded so well with this artificial Pyrmont water, I imagined that it might be possible to give *ice* the same virtue, especially as cold is known to promote the absorption of fixed air by water; but in this I found myself quite mistaken. I put several pieces of ice into a quantity of fixed air, confined by quicksilver, but no part of the air was absorbed in two days and two nights; but upon bringing it into a place where the ice melted, the air was absorbed as usual.

I then took a quantity of strong artificial Pyrmont water, and putting it into a thin glass phial, I set it in a pot that was filled with snow and salt. This mixture instantly freezing the water that was contiguous to the sides of the glass, the air was discharged plentifully, so that I catched a considerable quantity, in a bladder tied to the mouth of the phial.

I also took two quantities of the same Pyrmont water, and placed one of them where it might freeze, keeping the other in a cold place, but where it would not freeze. This retained its acidulous taste, though the phial which contained it was not corked; whereas the other being brought into the same place, where the ice melted very slowly, had at the same time the taste of common water only. That quantity of water which had been frozen by the mixture of snow and salt, was almost as much like snow as ice, such a quantity of air-bubbles were contained in it, by which it was prodigiously increased in bulk.

The pressure of the atmosphere assists very considerably in keeping fixed air confined in water; for in an exhausted receiver, Pyrmont water will absolutely boil, by the copious discharge of its air. This is also the reason why beer and ale froth so much *in vacuo*. I do not doubt, therefore, but that, by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring; and it would not be difficult to contrive a method of doing it.

The manner in which I made several experiments to ascertain the absorption of fixed air by different fluid substances, was to put the liquid into a dish, and holding it within the body of the fixed air at the brewery, to set a glass vessel into it, with its mouth inverted. This glass being necessarily filled with the fixed air, the liquor would rise into it when they were both taken into the common air, if the fixed air was absorbed at all.

Making use of *ether* in this manner, there was a constant bubbling from under the glass, occasioned by this fluid easily rising in vapour, so that I could not, in this method, determine whether it imbibed the air or not. I concluded however, that they did incorporate, from a very disagreeable circumstance, which made me desist from making any more experiments of the kind. For all the beer, over which this experiment was made, contracted a peculiar taste; the fixed air impregnated with the ether being, I suppose, again absorbed by the beer. I have also observed, that water which remained a long time within this air has sometimes acquired a very disagreeable taste. At one time it was like tar-water. How this was acquired, I was very desirous of making some experiments to ascertain, but I was discouraged by the fear of injuring the fermenting liquor. It could not come from the fixed air only.

Insects and animals which breathe very little are stifled in fixed air, but are not soon quite killed in it. Butterflies and flies of other kinds will generally become torpid, and seemingly dead, after being held a few minutes over the fermenting liquor; but they revive again after being brought into the fresh air. But there are very great varieties with respect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large strong frog was much swelled, and seemed to be nearly dead, after being held about six minutes over the fermenting liquor; but it recovered upon being brought into the common air. A snail treated in the same manner died presently.

Fixed air is presently fatal to vegetable life. At least sprigs of mint growing in water, and placed over the fermenting liquor, will often become quite dead in one day, or even in a less space of time; nor do they recover when they are afterwards brought into the common air. I am told, however, that some other plants are much more hardy in this respect.

A red rose, fresh gathered, lost its redness, and became of a purple colour, after being held over the fermenting liquor about twenty-four hours; but the tips of each leaf were much more affected than the rest of it. Another red rose turned perfectly white in this situation; but various other flowers of different colours were very little affected. These experiments were not repeated, as I wish they might be done, in pure fixed air, extracted from chalk by means of oil of vitriol.

For every purpose, in which it was necessary that the fixed air should be as unmixed as possible, I generally made it by pouring oil of vitriol upon chalk and water, catching it in a bladder fastened to the neck of the phial in which they were contained, taking care to press out all the common air, and also the first, and sometimes the second, produce of fixed air; and also, by agitation, making it as quickly as I possibly could. At other times, I made it pass from the phial in which it was generated through a glass tube, without the intervention of any bladder, which, as I found by experience, will not long make a sufficient separation between several kinds of air and common air.

I had once thought that the readiest method of procuring fixed air, and in sufficient purity, would be by the simple process of burning chalk, or pounded lime-stone in a gun-barrel, making it pass through the stem of a tobacco-pipe, or a glass tube carefully luted to the orifice of it. In this manner I found that air is produced in great plenty; but, upon examining it, I found, to my very great surprise, that little more than one half of it was fixed air, capable of being absorbed by water; and that the rest was inflammable, sometimes very weakly, but sometimes pretty highly so.

Whence this inflammability proceeds, I am not able to determine, the lime or chalk not being supposed to contain any other than fixed air. I conjecture, however, that it must proceed from the iron, and the separation of it from the calx may be promoted by that small quantity of oil of vitriol, which I am informed is contained in chalk, if not in lime-stone also.

But it is an objection to this hypothesis, that the inflammable air produced in this manner burns blue, and not at all like that which is produced from iron, or any other metal, by means of an acid. It also has not the smell of that kind of inflammable air which is produced from mineral substances. Besides, oil of vitriol without water, will not dissolve iron; nor can inflammable air be got from it, unless the acid be considerably diluted; and when I mixed brimstone with the chalk, neither the quality nor the quantity of the air was changed by it. Indeed no air, or permanently elastic vapour, can be got from brimstone, or any oil.

Perhaps this inflammable principle may come from some remains of the animals, from which it is thought that all calcareous matter proceeds.

In the method in which I generally made the fixed air (and indeed always, unless the contrary be particularly mentioned, viz. by diluted oil of vitriol and chalk) I found by experiment that it was as pure as Mr. Cavendish made it. For after it had patted through a large body of water in small bubbles, still 1/50 or 1/60 part only was not absorbed by water. In order to try this as expeditiously as possible, I kept pouring the air from one glass vessel into another, immersed in a quantity of cold water, in which manner I found by experience, that almost any quantity may be reduced as far as possible in a very short time. But the most expeditious method of making water imbibe any kind of air, is to confine it in a jar; and agitate it strongly, in the manner described in my pamphlet on the impregnation of water with fixed air, and represented fig. 10.

At the same time that I was trying the purity of my fixed air, I had the curiosity to endeavour to ascertain whether that part of it which is not miscible in water, be equally diffused through the whole mass; and, for this purpose, I divided a quantity of about a gallon into three parts, the first consisting of that which was uppermost, and the last of that which was the lowest, contiguous to the water; but all these parts were reduced in about an equal proportion, by passing through the water, so that the whole mass had been of an uniform composition. This I have also found to be the case with several kinds of air, which will, not properly incorporate.

A mouse will live very well, though a candle will not burn in the residuum of the purest fixed air that I can make; and I once made a very large quantity for the sole purpose of this experiment. This, therefore, seems to be one instance of the generation of genuine common air, though vitiated in some degree. It is also another proof of the residuum of fixed air being, in part at least, common air, that it becomes turbid, and is diminished by the mixture of nitrous air, as will be explained hereafter.

That fixed air only wants some addition to make it permanent, and immiscible with water if not in all respects, common air, I have been led to conclude, from several attempts which I once made to mix it with air in which a quantity of iron filings and brimstone, made into a paste with water, had stood; for, in several mixtures of this kind, I imagined that not much more than half of the fixed air could be imbibed by water; but, not being able to repeat the experiment, I conclude that I either deceived myself in it, or that I overlooked some circumstance on which the success of it depended.

These experiments, however, whether they were fallacious or otherwise, induced me to try whether any alteration would be made in the constitution of fixed air, by this mixture of iron filings and brimstone. I therefore put a mixture of this kind into a quantity of as pure fixed air as I could make, and confined the whole in quicksilver, lest the water should absorb it before the effects of the mixture could take place. The consequence was, that the fixed air was diminished, and the quicksilver rose in the vessel, till about the fifth part was occupied by it; and, as near as I could judge, the process went on, in all respects, as if the air in the inside had been common air.

What is most remarkable, in the result of this experiment, is, that the fixed air, into which this mixture had been put, and which had been in part diminished by it, was in part also rendered insoluble in water by this means. I made this experiment four times, with the greatest care, and observed, that in two of them about one sixth, and in the other two about one fourteenth, of the original quantity, was such as could not be absorbed by water, but continued permanently elastic. Lest I should have made any mistake with respect to the purity of the fixed air, the last time that I made the experiment, I set part of the fixed air, which I made use of, in a separate vessel, and found it to be exceedingly pure, so as to be almost wholly absorbed by water; whereas the other part, to which I had put the mixture, was far from being so.

In one of these cases, in which fixed air was made immiscible with water, it appeared to be not very noxious to animals; but in another case, a mouse died in it pretty soon. This difference probably arose from my having inadvertently agitated the air in water rather more in one case than in the other.

As the iron is reduced to a calx by this process, I once concluded, that it is phlogiston that fixed air wants, to make it common air; and, for any thing I yet know this may be the case, though I am ignorant of the method of combining them; and when I calcined a quantity of lead in fixed air, in the manner which will be described hereafter, it did not seem to have been less soluble in water than it was before.

FOOTNOTES:

[2] An account of Mr. Hey's experiments will be found in the Appendix to these papers.

SECTION II.

Of Air in which a Candle, or Brimstone, has burned out.

It is well known that flame cannot subsist long without change of air, so that the common air is necessary to it, except in the case of substances, into the composition of which nitre enters, for these will burn *in vacuo*, in fixed air, and even under water, as is evident in some rockets, which are made for this purpose. The quantity of air which even a small flame requires to keep it burning is prodigious. It is generally said, that an ordinary candle *consumes*, as it is called, about a gallon in a minute. Considering this amazing consumption of air, by fires of all kinds, volcanos, &c. it becomes a great object of philosophical inquiry, to ascertain what change is made in the constitution of the air by flame, and to discover what provision there is in nature for remedying the injury which the atmosphere receives by this means. Some of the following experiments will, perhaps, be thought to throw light upon the subject.

The diminution of the quantity of air in which a candle, or brimstone, has burned out, is various; But I imagine that, at a medium, it may be about one fifteenth, or one sixteenth of the whole; which is one third as much as by animal or vegetable substances putrefying in it, by the calcination of metals, or by any of the other causes of the complete diminution of air, which will be mentioned hereafter.

I have sometimes thought, that flame disposes the common air to deposit the fixed air it contains; for if any lime-water be exposed to it, it immediately becomes turbid. This is the case, when wax candles, tallow candles, chips of wood, spirit of wine, ether, and every other substance which I have yet tried, except brimstone, is burned in a close glass vessel, standing in lime-water. This precipitation of fixed air (if this be the case) may be owing to something emitted from the burning bodies, which has a stronger affinity with the other constituent parts of the atmosphere[3].

If brimstone be burned in the same circumstances, the lime-water continues transparent, but still there may have been the same precipitation of the fixed part of the air; but that, uniting with the lime and the vitriolic acid, it forms a selenetic salt, which is soluble in water. Having evaporated a quantity of water thus impregnated, by burning brimstone a great number of times over it, a whitish powder remained, which had an acid taste; but repeating the experiment with a quicker evaporation, the powder had no acidity, but was very much like chalk. The burning of brimstone but once over a quantity of lime-water, will affect it in such a manner, that breathing into it will not make it turbid, which otherwise it always presently does.

Dr. Hales supposed, that by burning brimstone repeatedly in the same quantity of air, the diminution would continue without end. But this I have frequently tried, and not found to be the case. Indeed, when the ignition has been imperfect in the first instance, a second firing of the same substance will increase the effect of the first, &c. but this progress soon ceases.

In many cases of the diminution of air, the effect is not immediately apparent, even when it stands in water; for sometimes the bulk of air will not be much reduced, till it has passed several times through a quantity of water, which has thereby a better opportunity of absorbing that part of the air, which had not been perfectly detatched from the rest. I have sometimes found a very great reduction of a mass of air, in consequence of passing but once through cold water. If the air has stood in quicksilver, the diminution is generally inconsiderable, till it has undergone this operation, there not being any substance exposed to the air that could absorb any part of it.

I could not find any considerable alteration in the specific gravity of the air, in which candles, or brimstone, had burned out. I am satisfied, however, that it is not heavier than common air, which must have been manifest, if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose, I concluded that air, thus diminished in bulk, is rather lighter than common air, which favours the supposition of the fixed, or heavier part of the common air, having been precipitated.

An animal will live nearly, if not quite as long, in air in which candles have burned out, as in common air. This fact surprized me very greatly, having imagined that what is called the *consumption* of air by flame, or respiration, to have been of the same nature, and in the same degree; but I have since found, that this fact has been observed by many persons, and even so early as by Mr. Boyle. I have also observed, that air, in which brimstone has burned, is not in the least injurious to animals, after the fumes, which at first make it very cloudy, have intirely subsided.

I must, in this place, admonish my reader not to confound the simple *burning of brimstone*, or of matches (*i. e.* bits of wood dipped in it) and the burning of brimstone with a burning mirror, or any *foreign heat*. The effect of the former is nothing more than that of any other *flame*, or *ignited vapour*, which will not burn, unless the air with which it is surrounded be in a very pure state, and which is therefore extinguished when the air begins to be much vitiated. Lighted brimstone, therefore reduces the air to the same state as lighted wood. But the focus of a burning mirror thrown for a sufficient time either upon brimstone, or wood, after it has ceased to burn of its own accord, and has become *charcoal*, will have a much greater effect: of the same kind, diminishing the air to its utmost extent, and making it thoroughly noxious. In fact, as will be seen hereafter, more phlogiston is expelled from these substances in the latter case than in the former. I never, indeed, actually carried this experiment so far with brimstone; but from the diminution of air that I did produce by this means, I concluded that, by continuing the process some time longer, it would have been effected.

Having read, in the Memoirs of the Philosophical Society at Turin, vol. I. p. 41. that air in which candles had burned out was perfectly restored, so that other candles would burn in it again as well as ever, after having been exposed to a considerable degree of *cold*, and likewise after having been compressed in bladders, (for the cold had been supposed to have produced this effect by nothing but *condensation*) I repeated those experiments, and did, indeed, find, that when I compressed the air in *bladders*, as the Count de Saluce, who made the observation, had done, the experiment succeeded: but having had sufficient reason to distrust bladders, I compressed the air in a glass vessel standing in water; and then I found, that this process is altogether ineffectual for the purpose. I kept the air compressed much more, and much longer, than the Count had done, but without producing any alteration in it. I also find, that a greater degree of cold than that which he applied, and of longer continuance, did by no means restore this kind of air:

for when I had exposed the phials which contained it a whole night, in which the frost was very intense; and also when I kept it surrounded with a mixture of snow and salt, I found it, in all respects, the same as before.

It is also advanced, in the same Memoir, p. 41. that *heat* only, as the reverse of *cold*, renders air unfit for candles burning in it. But I repeated the experiment of the Count for that purpose, without finding any such effect from it. I also remember that, many years ago, I filled an exhausted receiver with air, which had passed through a glass tube made red-hot, and found that a candle would burn in it perfectly well. Also, rarefaction by the air-pump does not injure air in the least degree.

Though this experiment failed, I have been so happy, as by accident to have hit upon a method of restoring air, which has been injured by the burning of candles, and to have discovered at least one of the restoratives which nature employs for this purpose. It is *vegetation*. This restoration of vitiated air, I conjecture, is effected by plants imbibing the phlogistic matter with which it is overloaded by the burning of inflammable bodies. But whether there be any foundation for this conjecture or not, the fact is, I think, indisputable. I shall introduce the account of my experiments on this subject, by reciting some of the observations which I made on the growing of plants in confined air, which led to this discovery.

One might have imagined that, since common air is necessary to vegetable, as well as to animal life, both plants and animals had affected it in the same manner; and I own I had that expectation, when I first put a sprig of mint into a glass jar, standing inverted in a vessel of water: but when it had continued growing there for some months, I found that the air would neither extinguish a candle, nor was it at all inconvenient to a mouse, which I put into it.

The plant was not affected any otherwise than was the necessary consequence of its confined situation; for plants growing in several other kinds of air, were all affected in the very same manner. Every succession of leaves was more diminished in size than the preceding, till, at length, they came to be no bigger than the heads of pretty small pins. The root decayed, and the stalk also, beginning from the root; and yet the plant continued to grow upwards, drawing its nourishment through a black and rotten stem. In the third or fourth set of leaves, long and white hairy filaments grew from the insertion of each leaf and sometimes from the body of the stem, shooting out as far as the vessel in which it grew would permit, which, in my experiments, was about two inches. In this manner a sprig of mint lived, the old plant decaying, and new ones shooting up in its place, but less and less continually, all the summer season.

In repeating this experiment, care must be taken to draw away all the dead leaves from about the plant, lest they should putrefy, and affect the air. I have found that a fresh cabbage leaf, put under a glass vessel filled with common air, for the space of one night only, has so affected the air, that a candle would not burn in it the next morning, and yet the leaf had not acquired any smell of putrefaction.

Finding that candles would burn very well in air in which plants had grown a long time, and having had some reason to think, that there was something attending vegetation, which restored air that had been injured by respiration, I thought it was possible that the same process might also restore the air that had been injured by the burning of candles.

Accordingly, on the 17th of August 1771, I put a sprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the 27th of the same month, another candle burned perfectly well in it. This experiment I repeated, without the least variation in the event, not less than eight or ten times in the remainder of the summer.

Several times I divided the quantity of air in which the candle had burned out, into two parts, and putting the plant into one of them, left the other in the same exposure, contained, also, in a glass vessel immersed in water, but without any plant; and never failed to find, that a candle would burn in the former, but not in the latter.

I generally found that five or six days were sufficient to restore this air, when the plant was in its vigour; whereas I have kept this kind of air in glass vessels, immersed in water many months, without being able to perceive that the least alteration had been made in it. I have also tried a great variety of experiments upon it, as by condensing, rarefying, exposing to the light and heat, &c. and throwing into it the effluvia of many different substances, but without any effect.

Experiments made in the year 1772, abundantly confirmed my conclusion concerning the restoration of air, in which candles had burned out by plants growing in it. The first of these experiments was made in the month of May; and they were frequently repeated in that and the two following months, without a single failure.

For this purpose I used the flames of different substances, though I generally used wax or tallow candles. On the 24th of June the experiment succeeded perfectly well with air in which spirit of wine had burned out, and on the 27th of the same month it succeeded equally well with air in which brimstone matches had burned out, an effect of which I had despaired the preceding year.

This restoration of air, I found, depended upon the *vegetating state* of the plant; for though I kept a great number of the fresh leaves of mint in a small quantity of air in which candles had burned out, and changed them frequently, for a long space of time, I could perceive no melioration in the state of the air.

This remarkable effect does not depend upon any thing peculiar to *mint*, which was the plant that I always made use of till July 1772; for on the 16th of that month, I found a quantity of this kind of air to be perfectly restored by sprigs of *balm*, which had grown in it from the 7th of the same month.

That this restoration of air was not owing to any *aromatic effluvia* of these two plants, not only appeared by the *essential oil of mint* having no sensible effect of this kind; but from the equally complete restoration of this vitiated air by the plant called *groundsel*, which is usually ranked among the weeds, and has an offensive smell. This was the result of an experiment made the 16th of July, when the plant had been growing in the burned air from the 8th of the same month. Besides, the plant which I have found to be the most effectual of any that I have tried for this purpose is *spinach*, which is of quick growth, but will seldom thrive long in water. One jar of burned air was perfectly restored by this plant in four days, and another in two days. This last was observed on the 22d of July.

In general, this effect may be presumed to have taken place in much less time than I have mentioned; because I never chose to make a trial of the air, till I was pretty sure, from preceding observations, that the event which I had expected must have taken place, if it would succeed at all; lest, returning back that part of the air on which I made the trial, and which would thereby necessarily receive a small mixture of common air, the experiment might not be judged to be quite fair; though I myself might be sufficiently satisfied with respect to the allowance that was to be made for that small imperfection.

FOOTNOTES:

[3] The supposition, mentioned in this and other passages of the first part of this publication, viz. that the diminution of common air, by this and other processes is, in part at least, owing to the precipitation of the fixed air from it, the reader will find confirmed by the experiments and observations in the second part.

SECTION III.

Of INFLAMMABLE AIR.

I have generally made inflammable air in the manner described by Mr. Cavendish, in the Philosophical Transactions, from iron, zinc, or tin; but chiefly from the two former metals, on account of the process being the least troublesome: but when I extracted it from vegetable or animal substances, or from coals, I put them into a gun-barrel, to the orifice of which I luted a glass tube, or the stem of a tobacco-pipe, and to the end of this I tied a flaccid bladder in order to catch the generated air; or I received the air in a vessel of quicksilver, in the manner represented Fig. 7.

There is not, I believe, any vegetable or animal substance whatever, nor any mineral substance, that is inflammable, but what will yield great plenty of inflammable air, when they are treated in this manner, and urged with a strong heat; but, in order to get the most air, the heat must be applied as suddenly, and as vehemently, as possible. For, notwithstanding the same care be taken in luting, and in every other respect, six or even ten times more air may be got by a sudden heat than by a slow one, though the heat that is last applied be as intense as that which was applied suddenly. A bit of dry oak, weighing about twelve grains, will generally yield about a sheep's bladder full of inflammable air with a brisk heat, when it will only give about two or three ounce measures, if the same heat be applied to it very gradually. To what this difference is owing, I cannot tell. Perhaps the phlogiston being extricated more slowly may not be intirely expelled, but form another kind of union with its base; so that charcoal made with a heat slowly applied shall contain more phlogiston than that which is made with a sudden heat. It may be worth while to examine the properties of the charcoal with this view.

Inflammable air, when it is made by a quick process, has a very strong and offensive smell, from whatever substance it be generated; but this smell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal substances. The last is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, from soft muscular flesh; or any other part of the animal. The burning of any substance occasions the same smell: for the gross fume which arises from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The smell of inflammable air is the very same, as far as I am able to perceive, from whatever substance of the same kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was observed before, from any part of an animal.

If a quantity of inflammable air be contained in a glass vessel standing in water, and have been generated very fast, it will smell even through the water, and this water will also soon become covered with a thin film, assuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red okre, or the earth of iron, as I have found by collecting a considerable quantity of it; and if it have been generated from zinc, it is a whitish substance, which I suppose to be the calx of the metal. It likewise settles to the bottom of the vessel, and when the water is stirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this scum for a considerable time after the air is removed from it. This I have often observed with respect to iron.

Inflammable air, made by a violent effervescence, I have observed to be much more inflammable than that which is made by a weak effervescence, whether the water or the oil of vitriol prevailed in the mixture. Also the offensive smell was much stronger in the former case than in the latter. The greater degree of inflammability appeared by the greater number of successive explosions, when a candle was presented to the neck of a phial filled with it.[4] It is possible, however, that this diminution of inflammability may, in some measure, arise from the air continuing so much longer in the bladder when it is made very slowly; though I think the difference is too great for this cause to have produced the whole of it. It may, perhaps, deserve to be tried by a different process, without a bladder.

Inflammable air is not thought to be miscible with water, and when kept many months, seems, in general, to be as inflammable as ever. Indeed, when it is extracted from vegetable or animal substances, a part of it will be imbibed by the water in which it stands; but it may be presumed, that in this case, there was a mixture of fixed air extracted from the substance along with it. I have indisputable evidence, however, that inflammable air, standing long in water, has actually lost all its inflammability, and even come to extinguish flame much more than that air in which candles have burned out. After this change it appears to be greatly diminished in quantity, and it still continues to kill animals the moment they are put into it.

This very remarkable fact first occurred to my observation on the twenty-fifth of May 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately set by a common quart-bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly mistaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was set by. That which had been produced from zinc was not altered, and filled the bottle as at first.

Another instance of this kind occurred to my observation on the 19th of June 1772, when a quantity of air, half of which had been inflammable air from zinc, and half air in which mice had died, and which had been put together the 30th of July 1771, appeared not to be in the least inflammable, but extinguished flame, as much as any kind of air that I had ever tried. I think that, in all, I have had four instances of inflammable air losing its inflammability, while it stood in water.

Though air tainted with putrefaction extinguishes flame, I have not found that animals or vegetables putrefying in inflammable air render it less inflammable. But one quantity of inflammable air, which I had set by in May 1771, along with the others above mentioned, had had some putrid flesh in it; and this air had lost its inflammability, when it was examined at the same time with the other in the December following. The bottle in which this air had been kept, smelled exactly like very strong Harrogate water. I do not think that any person could have distinguished them.

I have made plants grow for several months in inflammable air made from zinc, and also from oak; but, though the plants grew pretty well, the air still continued inflammable. The former, indeed, was not so highly inflammable as when it was fresh made, but the latter was quite as much so; and the diminution of inflammability in the former case, I attribute to some other cause than the growth of the plant.

No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of an electric spark is remarkably different in some different kinds of air, which seems to shew that they are not equally good non-conductors. In fixed air, the electric spark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, since the most vigorous sparks are always the whitest, and, in other cases, when the spark is red, there is reason to think that the electric matter passes with difficulty, and with less rapidity: it is possible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteness of the spark in the fixed air, may be owing to its meeting with no conducting particles at all. When an explosion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were still tinged with a beautiful purple. The degree of whiteness in this case was probably owing to the electric matter rushing with more violence in an explosion than in a common spark.

Inflammable air kills animals as suddenly as fixed air, and, as far as can be perceived, in the same manner, throwing them into convulsions, and thereby occasioning present death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become less noxious; but this did not appear to be the case; for I killed great number of mice in a small quantity of this air; which I kept several months for this purpose, without its being at all sensibly mended; the last, as well as the first mouse, dying the moment it was put into it.

I once imagined that, since fixed and inflammable air are the reverse of one another, in several remarkable properties, a mixture of them would make common air; and while I made the mixtures in bladders, I imagined that I had succeeded in my attempt; but I have since found that thin bladders do not sufficiently prevent the air that is contained in them from mixing with the external air. Also corks will not sufficiently confine different kinds of air, unless the phials in which they are confined be set with their mouths downwards, and a little water lie in the necks of them, which, indeed, is equivalent to the air standing in vessels immersed in water. In this manner, however, I have kept different kinds of air for several years.

Whatever methods I took to promote the mixture of fixed and inflammable air, they were all ineffectual. I think it my duty, however, to recite the issue of an experiment or two of this kind, in which equal mixtures of these two kinds of air had stood near three years, as they seem to shew that they had in part affected one another, in that long space of time. These mixtures I examined April 27, 1771. One of them had stood in quicksilver, and the other in a corked phial, with a little water in it. On opening the latter in water, the water instantly rushed in, and filled almost half of the phial, and very little more was absorbed afterwards. In this case the water in the phial had probably absorbed a considerable part of the fixed air, so that the inflammable air was exceedingly rarefied; and yet the whole quantity that must have been rendered non-elastic was ten times more than the bulk of the water, and it has not been found that water can contain much more than its own bulk of fixed air. But in other cases I have found the diminution of a quantity of air, and especially of fixed air, to be much greater than I could well account for by any kind of absorption.

The phial which had stood immersed in quicksilver had lost very little of its original quantity of air; and being now opened in water, and left there, along with another phial, which was just then filled, as this had been three years before, viz. with air half inflammable and half fixed, I observed that the quantity of both was diminished, by the absorption of the water, in the same proportion.

Upon applying a candle to the mouths of the phials which had been kept three years, that which had stood in quicksilver went off at one explosion, exactly as it would have done if there had been a mixture of common air with the inflammable. As a good deal depends upon the apertures of the vessels in which the inflammable air is mixed, I mixed the two kinds of air in equal proportions in the same phial, and after letting the phial stand some days in water, that the fixed air might be absorbed, I applied a candle to it, but it made ten or twelve explosions (stopping the phial after each of them) before the inflammable matter was exhausted.

The air which had been confined in the corked phial exploded in the very same manner as an equal and fresh mixture of the two kinds of air in the same phial, the experiment being made as soon as the fixed air was absorbed, as before; so that in this case, the two kinds of air did not seem to have affected one another at all.

Considering inflammable air as air united to, or loaded with phlogiston, I exposed to it several substances, which are said to have a near affinity with phlogiston, as oil of vitriol, and spirit of nitre (the former for above a month), but without making any sensible alteration in it.

I observed, however, that inflammable air, mixed with the fumes of smoking spirit of nitre, goes off at one explosion, exactly like a mixture of half common and half inflammable air. This I tried several times, by throwing the inflammable air into a phial full of spirit of nitre, with its mouth immersed in a bason containing some of the same spirit, and then applying the flame of a candle to the mouth of the phial, the moment that it was uncovered, after it had been taken out of the bason.

This remarkable effect I hastily concluded to have arisen from the inflammable air having been in part deprived of its inflammability, by means of the stronger affinity, which the spirit of nitre had with phlogiston, and therefore I imagined that by letting them stand longer in contact, and especially by agitating them strongly together, I should deprive the air of all its inflammability; but neither of these operations succeeded, for still the air was only exploded at once, as before.

And lastly, when I passed a quantity of inflammable air, which had been mixed with the fumes of spirit of nitre, through a body of water, and received it in another vessel, it appeared not to have undergone any change at all, for it went off in several successive explosions, like the purest inflammable air. The effect above-mentioned must, therefore, have been owing to the fumes of the spirit of nitre supplying the place of common air for the purpose of ignition, which is analogous to other experiments with nitre.

Having had the curiosity, on the 25th of July 1772, to expose a great variety of different kinds of air to water out of which the air it contained had been boiled, without any particular view; the result was, in several respects, altogether unexpected, and led to a variety of new observations on the properties and affinities of several kinds of air with respect to water. Among the rest three fourths of that which was inflammable was absorbed by the water in about two days, and the remainder was inflammable, but weakly so.

Upon this, I began to agitate a quantity of strong inflammable air in a glass jar, standing in a pretty large trough of water, the surface of which was exposed to the common air, and I found that when I had continued the operation about ten minutes, near one fourth of the quantity of air had disappeared; and finding that the remainder made an effervescence with nitrous air, I concluded that it must have become fit for respiration, whereas this kind of air is, at the first, as noxious as any other kind whatever. To ascertain this, I put a mouse into a vessel containing 2-1/2 ounce measures of it, and observed that it lived in it twenty minutes, which is as long as a mouse will generally live in the same quantity of common air. This mouse was even taken out alive, and recovered very well. Still also the air in which it had breathed so long was inflammable, though very weakly so. I have even found it to be so when a mouse has actually died in it. Inflammable air thus diminished by agitation in water, makes but one explosion on the approach of a candle, exactly like a mixture of inflammable air with common air.

From this experiment I concluded that, by continuing the same process, I should deprive inflammable air of all its inflammability, and this I found to be the case; for, after a longer agitation, it admitted a candle to burn in it, like common air, only more faintly; and indeed by the test of nitrous air it did not appear to be near so good as common air. Continuing the same process still farther, the air which had been most strongly inflammable a little before, came to extinguish a candle, exactly like air in which a candle had burned out, nor could they be distinguished by the test of nitrous air.

I found, by repeated trials, that it was difficult to catch the time in which inflammable air obtained from metals, in coming to extinguish flame, was in the state of common air, so that the transition from the one to the other must be very short. Indeed I think that in many, perhaps in most cases, there may be no proper medium at all, the phlogiston passing at once from that mode of union with its base which constitutes inflammable air, to that which constitutes an air that extinguishes flame, being so much overloaded as to admit of no more. I readily, however, found this middle state in a quantity of inflammable air extracted from oak, which air I had kept a year, and in which a plant had grown, though very poorly, for some part of the time. A quantity of this air, after being agitated in water till it was diminished about one half, admitted a candle to burn in it exceedingly well, and was even hardly to be distinguished from common air by the test of nitrous air.

I took some pains to ascertain the quantity of diminution, in fresh made and very highly-inflammable air from iron, at which it ceased to be inflammable, and, upon the whole, I concluded that it was so when it was diminished a little more than one half; for a quantity which was diminished exactly one half had something inflammable in it, but in the slightest degree imaginable. It is not improbable, however, but there may be great differences in the result of this experiment.

Finding that water would imbibe inflammable air, I endeavoured to impregnate water with it, by the same process by which I had made water imbibe fixed air; but though I found that distilled water would imbibe about one fourteenth of its bulk of inflammable air, I could not perceive that the taste of it was sensibly altered.

FOOTNOTES:

[4] To try this, after every explosion, which immediately follows the presenting of the flame, the mouth of the phial should be closed (I generally do it with a finger of the hand in which I hold the phial) for otherwise the inflammable air will continue burning, though invisibly in the day time, till the whole be consumed.

SECTION IV.

Of Air infected with animal respiration, or Putrefaction.

That candles will burn only a certain time, in a given quantity of air is a fact not better known, than it is that animals can live only a certain time in it; but the cause of the death of the animal is not better known than that of the extinction of flame in the same circumstances; and when once any quantity of air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been discovered of rendering it fit for breathing again. It is evident, however, that there must be some provision in nature for this purpose, as well as for that of rendering the air fit for sustaining flame; for without it the whole mass of the atmosphere would, in time, become unfit for the purpose of animal life; and yet there is no reason to think that it is, at present, at all less fit for respiration than it has ever been. I flatter myself, however, that I have hit upon two of the methods employed by nature for this great purpose. How many others there may be, I cannot tell.

When animals die upon being put into air in which other animals have died, after breathing in it as long as they could, it is plain that the cause of their death is not the want of any *pabulum vitæ,* which has been supposed to be contained in the air, but on account of the air being impregnated with something stimulating to their lungs; for they almost always die in convulsions, and are sometimes affected so suddenly, that they are irrecoverable after a single inspiration, though they be withdrawn immediately, and every method has been taken to bring them to life again. They are affected in the same manner, when they are killed in any other kind of noxious air that I have tried, viz. fixed air, inflammable air, air filled with the fumes of brimstone, infected with putrid matter, in which a mixture of iron filings and brimstone has stood, or in which charcoal has been burned, or metals calcined, or in nitrous air, &c.

As it is known that *convulsions* weaken, and exhaust the vital powers, much more than the most vigorous *voluntary* action of the muscles, perhaps these universal convulsions may exhaust the whole of what we may call the *vis vitæ* at once, at least that the lungs may be rendered absolutely incapable of action, till the animal be suffocated, or be irrecoverable for want of respiration.

If a mouse (which is an animal that I have commonly made use of for the purpose of these experiments) can stand the first shock of this stimulus, or has been habituated to it by degrees, it will live a considerable time in air in which other mice will die instantaneously. I have frequently found that when a number of mice have been confined in a given quantity of air, less than half the time that they have actually lived in it, a fresh mouse being introduced to them has been instantly thrown into convulsions, and died. It is evident, therefore, that if the experiment of the Black Hole were to be repeated, a man would stand the better chance of surviving it, who should enter at the first, than at the last hour.

I have also observed, that young mice will always live much longer than old ones, or than those which are full grown, when they are confined in the same quantity of air. I have sometimes known a young mouse to live six hours in the same circumstances in which an old mouse has not lived one. On these accounts, experiments with mice, and, for the same reason, no doubt, with other animals also, have a considerable degree of uncertainty attending them; and therefore, it is necessary to repeat them frequently, before the result can be absolutely depended upon. But every person of feeling will rejoice with me in the discovery of *nitrous air*, to be mentioned hereafter, which supersedes many experiments with the respiration of animals, being a much more accurate test of the purity of air.

The discovery of the provision in nature for restoring air, which has been injured by the respiration of animals, having long appeared to me to be one of the most important problems in natural philosophy, I have tried a great variety of schemes in order to effect it. In these my guide has generally been to consider the influences to which the atmosphere is, in fact, exposed; and, as some of my unsuccessful trials may be of use to those who are disposed to take pains in the farther investigation of this subject, I shall mention the principal of them.

The noxious effluvium with which air is loaded by animal respiration, is not absorbed by standing, without agitation; in fresh or salt water. I have kept it many months in fresh water, when, instead of being meliorated, it has seemed to become even more deadly, so as to require more time to restore it, by the methods which will be explained hereafter, than air which has been lately made noxious. I have even spent several hours in pouring this air from one glass vessel into another, in water, sometimes as cold, and sometimes as warm, as my hands could bear it, and have sometimes also wiped the vessels many times, during the course of the experiment, in order to take off that part of the noxious matter, which might adhere to the glass vessels, and which evidently gave them an offensive smell; but all these methods were generally without any sensible effect. The *motion*, also, which the air received in these circumstances, it is very evident, was of no use for this purpose. I had not then thought of the simple, but most effectual method of agitating air in water, by putting it into a tall jar and shaking it with my hand.

This kind of air is not restored by being exposed to the *light*, or by any other influence to which it is exposed, when confined in a thin phial, in the open air, for some months.

Among other experiments, I tried a great variety of different *effluvia*, which are continually exhaling into the air, especially of those substances which are known to resist putrefaction; but I could not by these means effect any melioration of the noxious quality of this kind of air.

Having read, in the memoirs of the Imperial Society, of a plague not affecting a particular village, in which there was a large sulphur-work, I immediately fumigated a quantity of this kind of air; or (which will hereafter appear to be the very same thing) air tainted with putrefaction, with the fumes of burning brimstone, but without any effect.

I once imagined, that the *nitrous acid* in the air might be the general restorative which I was in quest of; and the conjecture was favoured, by finding that candles would burn in air extracted from saltpetre. I therefore spent a good deal of time in attempting, by a burning glass, and other means, to impregnate this noxious air, with some effluvium of saltpetre, and, with the same view, introduced into it the fumes of the smoaking spirit of nitre; but both these methods were altogether ineffectual.

In order to try the effect of *heat*, I put a quantity of air, in which mice had died, into a bladder, tied to the end of the stem of a tobacco-pipe, at the other end of which was another bladder, out of which the air was carefully pressed. I then put the middle part of the stem into a chafing-dish of hot coals, strongly urged with a pair of bellows; and, pressing the bladders alternately, I made the air pass several times through the heated part of the pipe. I have also made this kind of air very hot, standing in water before the fire. But neither of these methods were of any use.

Rarefaction and *condensation* by instruments were also tried, but in vain.

Thinking it possible that the *earth* might imbibe the noxious quality of the air, and thence supply the roots of plants with such putrescent matter as is known to be nutritive to them, I kept a quantity of air, in which mice had died, in a phial, one half of which was filled with fine garden-mould; but, though it stood two months in these circumstances, it was not the better for it.

I once imagined that, since several kinds of air cannot be long separated from common air, by being confined in bladders, in bottles well corked; or even closed with ground stopples, the affinity between this noxious air and the common air might be so great, that they would mix through a body of water interposed between them; the water continually receiving from the one, and giving to the other, especially as water receives some kind of impregnation from, I believe, every kind of air to which it is contiguous; but I have seen no reason to conclude, that a mixture of any kind of air with the common air can be produced in this manner.

I have kept air in which mice have died, air in which candles have burned out, and inflammable air, separated from the common air, by the slightest partition of water that I could well make, so that it might not evaporate in a day or two, if I should happen not to attend to them; but I found no change in them after a month or six weeks. The inflammable air was still inflammable, mice died instantly in the air in which other mice had died before, and candles would not burn where they had burned out before.

Since air tainted with animal or vegetable putrefaction is the same thing with air rendered noxious by animal respiration, I shall now recite the observations which I have made upon this kind of air, before I treat of the method of restoring them.

That these two kinds of air are, in fact, the same thing, I conclude from their having several remarkable common properties, and from their differing in nothing that I have been able to observe. They equally extinguish flame, they are equally noxious to animals, they are equally, and in the same way, offensive to the smell, and they are restored by the same means.

Since air which has passed through the lungs is the same thing with air tainted with animal putrefaction, it is probable that one use of the lungs is to carry off a *putrid effluvium*, without which, perhaps, a living body might putrefy as soon as a dead one.

When a mouse putrefies in any given quantity of air, the bulk of it is generally increased for a few days; but in a few days more it begins to shrink up, and in about eight or ten days, if the weather be pretty warm, it will be found to be diminished 1/6, or 1/5 of its bulk. If it do not appear to be diminished after this time, it only requires to be passed through water, and the diminution will not fail to be sensible. I have sometimes known almost the whole diminution to take place, upon once or twice passing through the water. The same is the case with air, in which animals have breathed as long as they could. Also, air in which candles have burned out may almost always be farther reduced by this means.

All these processes, as I observed before, seem to dispose the compound mass of air to part with some constituent part belonging to it (which appears to be the *fixed air* that enters into its constitution) and this being miscible with water, must be brought into contact with it, in order to mix with it to the most advantage, especially when its union with the other constituent principles of the air is but partially broken.

I have put mice into vessels which had their mouths immersed in quicksilver, and observed that the air was not much contracted after they were dead or cold; but upon withdrawing the mice, and admitting lime water to the air, it immediately became turbid, and was contracted in its dimensions as usual.

I tried the same thing with air tainted with putrefaction, putting a dead mouse to a quantity of common air, in a vessel which had its mouth immersed in quicksilver, and after a week I took the mouse out, drawing it through the quicksilver, and observed that, for some time, there was an apparent increase of the air perhaps about 1/20. After this, it stood two days in the quicksilver, without any sensible alteration; and then admitting water to it, it began to be absorbed, and continued so, till the original quantity was diminished about 1/6. If, instead of common water, I had made use of lime-water in this experiment, I make no doubt but it would have become turbid.

If a quantity of lime-water in a phial be put under a glass vessel standing in water, it will not become turbid, and provided the access of the common air be prevented, it will continue lime-water, I do not know how long; but if a mouse be left to putrefy in the vessel, the water will deposit all its lime in a few days. This is owing to the fixed air deposited by the common air, and perhaps also from more fixed air discharged from the putrefying substances in some part of the process of putrefaction.

The air that is discharged from putrefying substances seems, in some cases, to be chiefly fixed air, with the addition of some other effluvium, which has the power of diminishing common air. The resemblance between the true putrid effluvium and fixed air in the following experiment, which is as decisive as I can possibly contrive it, appeared to be very great; indeed much greater than I had expected. I put a dead mouse into a tall glass vessel, and having filled the remainder with quicksilver, and set it, inverted, in a pot of quicksilver, I let it stand about two months, in which time the putrid effluvium issuing from the mouse had filled the whole vessel, and part of the dissolved blood, which lodged upon the surface of the quicksilver, began to be thrown out. I then filled another glass vessel, of the same size and shape, with as pure fixed air as I could make, and exposed them both, at the same time, to a quantity of lime-water. In both cases the water grew turbid alike, it rose equally fast in both the vessels, and likewise equally high; so that about the same quantity remained unabsorbed by the water. One of these kinds of air, however, was exceedingly sweet and pleasant, and the other insufferably offensive; one of them also would have made an addition to any quantity of common air, with which it had been mixed, and the other would have diminished it. This, at least, would have been the consequence, if the mouse itself had putrefied in any quantity of common air.

It seems to depend, in some measure, upon the *time*, and other circumstances, in the dissolution of animal or vegetable substances, whether they yield the proper putrid effluvium, or fixed, or inflammable air; but the experiments which I have made upon this subject, have not been numerous enough to enable me to decide with certainty concerning those circumstances.

Putrid cabbage, green or boiled, infects the air in the very same manner as putrid animal substances. Air thus tainted is equally contracted in its dimensions, it equally extinguishes flame, and is equally noxious to animals; but they affect the air very differently, if the heat that is applied to them be considerable.

If beef or mutton, raw or boiled, be placed so near to the fire, that the heat to which it is exposed shall equal, or rather exceed, that of the blood, a considerable quantity of air will be generated in a day or two, about 1/7th of which I have generally found to be absorbed by water, while all the rest was inflammable; but air generated from vegetables, in the same circumstances, will be almost all fixed air, and no part of it inflammable. This I have repeated again and again, the whole process being in quicksilver; so that neither common air nor water, had any access to the substance on which the experiment was made; and the generation of air, or effluvium of any kind, except what might be absorbed by quicksilver, or resorbed by the substance itself, might be distinctly noted.

A vegetable substance, after standing a day or two in these circumstances, will yield nearly all the air that can be extracted from it, in that degree of heat; whereas an animal substance will continue to give more air, or effluvium, of some kind or other, with very little alteration, for many weeks. It is remarkable, however, that though a piece of beef or mutton, plunged in quicksilver, and kept in this degree of heat, yield air, the bulk of which is inflammable, and contracts no putrid smell (at least, in a day or two) a mouse treated in the same manner, yields the proper putrid effluvium, as indeed the smell sufficiently indicates.

That the putrid effluvium will mix with water seems to be evident from the following experiment. If a mouse be put into a jar full of water, standing with its mouth inverted in another vessel of water, a considerable quantity of elastic matter (and which may, therefore, be called *air*) will soon be generated, unless the weather be so cold as to check all putrefaction. After a short time, the water contracts an extremely fetid and offensive smell, which seems to indicate that the putrid effluvium pervades the water, and affects the neighbouring air; and since, after this, there is often no increase of the air, that seems to be the very substance which is carried off through the water, as fast as it is generated; and the offensive smell is a sufficient proof that it is not fixed air. For this has a very agreeable flavour, whether it be produced by fermentation, or extracted from chalk by oil of vitriol; affecting not only the mouth, but even the nostrils; with a pungency which is peculiarly pleasing to a certain degree, as any person may easily satisfy himself, who will chuse to make the experiment.

If the water in which the mouse was immersed, and which is saturated with the putrid air, be changed, the greater part of the putrid air, will, in a day or two, be absorbed, though the mouse continues to yield the putrid effluvium as before; for as soon as this fresh water becomes saturated with it, it begins to be offensive to the smell, and the quantity of the putrid air upon its surface increases as before. I kept a mouse producing putrid air in this manner for the space of several months.

Six ounce measures of air not readily absorbed by water, appeared to have been generated from one mouse, which had been putrefying eleven days in confined air, before it was put into a jar which was quite filled with water, for the purpose of this observation.

Air thus generated from putrid mice standing in water, without any mixture of common air, extinguishes flame, and is noxious to animals, but not more so than common air only tainted with putrefaction. It is exceedingly difficult and tedious to collect a quantity of this putrid air, not miscible in water, so very great a proportion of what is collected being absorbed by the water in which it is kept; but what that proportion is, I have not endeavoured to ascertain. It is probably the same proportion that that part of fixed air, which is not readily absorbed by water, bears to the rest; and therefore this air, which I at first distinguished by the name of *the putrid* *effluvium*, is probably the same with fixed air, mixed with the phlogistic matter, which, in this and other processes, diminishes common air.

Though a quantity of common air be diminished by any substance putrefying in it, I have not yet found the same effect to be produced by a mixture of putrid air with common air; but, in the manner in which I have hitherto made the experiment, I was obliged to let the putrid air pass through a body of water, which might instantly absorb the phlogistic matter that diminished the common air.

Insects of various kinds live perfectly well in air tainted with animal or vegetable putrefaction, when a single inspiration of it would have instantly killed any other animal. I have frequently tried the experiment with flies and butterflies. The *aphides* also will thrive as well upon plants growing in this kind of air, as in the open air. I have even been frequently obliged to take plants out of the putrid air in which they were growing, on purpose to brush away the swarms of these insects which infected them; and yet so effectually did some of them conceal themselves, and so fast did they multiply, in these circumstances, that I could seldom keep the plants quite clear of them.

When air has been freshly and strongly tainted with putrefaction, so as to smell through the water, sprigs of mint have presently died, upon being put into it, their leaves turning black; but if they do not die presently, they thrive in a most surprizing manner. In no other circumstances have I ever seen vegetation so vigorous as in this kind of air, which is immediately fatal to animal life. Though these plants have been crouded in jars filled with this air, every leaf has been full of life; fresh shoots have branched out in various directions, and have grown much faster than other similar plants, growing in the same exposure in common air.

This observation led me to conclude, that plants, instead of affecting the air in the same manner with animal respiration, reverse the effects of breathing, and tend to keep the atmosphere sweet and wholesome, when it is become noxious, in consequence of animals either living and breathing, or dying and putrefying in it.

In order to ascertain this, I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water; and to the other (which was contained in a glass jar, standing in water) I put a sprig of mint. This was about the beginning of August 1771, and after eight or nine days, I found that a mouse lived perfectly well in that part of the air, in which the sprig of mint had grown, but died the moment it was put into the other part of the same original quantity of air; and which I had kept in the very same exposure, but without any plant growing in it.

This experiment I have several times repeated; sometimes using air in which animals had breathed and died, and at other times using air, tainted with vegetable or animal putrefaction; and generally with the same success.

Once, I let a mouse live and die in a quantity of air which had been noxious, but which had been restored by this process, and it lived nearly as long as I conjectured it might have done in an equal quantity of fresh air; but this is so exceedingly various, that it is not easy to form any judgment from it; and in this case the symptom of *difficult respiration* seemed to begin earlier than it would have done in common air.

Since the plants that I made use of manifestly grow and thrive in putrid air; since putrid matter is well known to afford proper nourishment for the roots of plants; and since it is likewise certain that they receive nourishment by their leaves as well as by their roots, it seems to be exceedingly probable, that the putrid effluvium is in some measure extracted from the air, by means of the leaves of plants, and therefore that they render the remainder more fit for respiration.

Towards the end of the year some experiments of this kind did not answer so well as they had done before, and I had instances of the relapsing of this restored air to its former noxious state. I therefore suspended my judgment concerning the efficacy of plants to restore this kind of noxious air, till I should have an opportunity of repeating my experiments, and giving more attention to them. Accordingly I resumed the experiments in the summer of the year 1772, when I presently had the most indisputable proof of the restoration of putrid air by vegetation; and as the fact is of some importance, and the subsequent variation in the state of this kind of air is a little remarkable, I think it necessary to relate some of the facts pretty circumstantially.

The air, on which I made the first experiments, was rendered exceedingly noxious by mice dying in it on the 20th of June. Into a jar nearly filled with one part of this air, I put a sprig of mint, while I kept another part of it in a phial, in the same exposure; and on the 27th of the same month, and not before, I made a trial of them, by introducing a mouse into a glass vessel, containing 2-1/2 ounce measures filled with each kind of air; and I noted the following facts.

When the vessel was filled with the air in which the mint had grown, a very large mouse lived five minutes in it, before it began to shew any sign of uneasiness. I then took it out, and found it to be as strong and vigorous as when it was first put in; whereas in that air which had been kept in the phial only, without a plant growing in it, a younger mouse continued not longer than two or three seconds, and was taken out quite dead. It never breathed after, and was immediately motionless. After half an hour, in which time the larger mouse (which I had kept alive, that the experiment might be made on both the kinds of air with the very same animal) would have been sufficiently recruited, supposing it to have received any injury by the former experiment, was put into the same vessel of air; but though it was withdrawn again, after being in it hardly one second, it was recovered with difficulty, not being able to stir from the place for near a minute. After two days, I put the same mouse into an equal quantity of common air, and observed that it continued seven minutes without any sign of uneasiness; and being very uneasy after three minutes longer, I took it out. Upon the whole, I concluded that the restored air wanted about one fourth of being as wholesome as common air. The same thing also appeared when I applied the test of nitrous air.

In the seven days, in which the mint was growing in this jar of noxious air, three old shoots had extended themselves about three inches, and several new ones had made their appearance in the same time. Dr. Franklin and Sir John Pringle happened to be with me, when the plant had been three or four days in this state, and took notice of its vigorous vegetation, and remarkably healthy appearance in that confinement.

On the 30th of the same month, a mouse lived fourteen minutes, breathing naturally all the time, and without appearing to be much uneasy, till the last two minutes, in the vessel containing two ounce measures and a half of air which had been rendered noxious, by mice breathing in it almost a year before, and which, I had found to be most highly noxious on the 19th of this month, a plant having grown in it, but not exceedingly well, these eleven days; on which account I had deferred making the trial so long. The restored air was affected by a mixture of nitrous air, almost as much as common air.

As this putrid air was thus easily restored to a considerable degree of fitness for respiration, by plants growing in it, I was in hopes that by the same means it might in time be so much more perfectly restored, that a candle would burn in it; and for this purpose I kept plants growing in the jars which contained this air till the middle of August following, but did not take sufficient care to pull out all the old and rotten leaves. The plants, however, had grown, and looked so well upon the whole, that I had no doubt but that the air must constantly have been in a mending state; when I was exceedingly surprized to find, on the 24th of that month, that though the air in one of the jars had not grown worse, it was no better; and that the air in the other jar was so much worse than it had been, that a mouse would have died in it in a few seconds. It also made no effervescence with nitrous air, as it had done before.

Suspecting that the same plant might be capable of restoring putrid air to a certain degree only, or that plants might have a contrary tendency in some stages of their growth, I withdrew the old plant, and put a fresh one in its place; and found that, after seven days, the air was restored to its former wholesome state. This fact I consider as a very remarkable one, and well deserving of a farther investigation, as it may throw more light upon the principles of vegetation. It is not, however, a single fact; for I had several instances of the same kind in the preceding year; but it seemed so very extraordinary, that air should grow worse by the continuance of the same treatment by which it had grown better, that, whenever I observed it, I concluded that I had not taken sufficient care to satisfy myself of its previous restoration.

That plants are capable of perfectly restoring air injured by respiration, may, I think, be inferred with certainty from the perfect restoration, by this means, of air which had passed through my lungs, so that a candle would burn in it again, though it had extinguished flame before, and apart of the same original quantity of air still continued to do so. Of this one instance occurred in the year 1771, a sprig of mint having grown in a jar of this kind of air, from the 25th of July to the 17th of August following; and another trial I made, with the same success, the 7th of July 1772, the plant having grown in it from the 29th of June preceding. In this case also I found that the effect was not owing to any virtue in the leaves of mint; for I kept them constantly changed in a quantity of this kind of air, for a considerable time, without making any sensible alteration in it.

These proofs of a partial restoration of air by plants in a state of vegetation, though in a confined and unnatural situation, cannot but render it highly probable, that the injury which is continually done to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation. And, notwithstanding the prodigious mass of air that is corrupted daily by the above-mentioned causes; yet, if we consider the immense profusion of vegetables upon the face of the earth, growing in places, suited to their nature, and consequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a sufficient counterbalance to it, and that the remedy is adequate to the evil.

Dr. Franklin, who, as I have already observed, saw some of my plants in a very flourishing state, in highly noxious air, was pleased to express very great satisfaction with the result of the experiments. In his answer to the letter in which I informed him of it, he says,

"That the vegetable creation should restore the air which is spoiled by the animal part of it, looks like a rational system, and seems to be of a piece with the rest. Thus fire purifies water all the world over. It purifies it by distillation, when it raises it in vapours, and lets it fall in rain; and farther still by filtration, when, keeping it fluid, it suffers that rain to percolate the earth. We knew before that putrid animal substances were converted into sweet vegetables, when mixed with the earth, and applied as manure; and now, it seems, that the same putrid substances, mixed with the air, have a similar effect. The strong thriving state of your mint in putrid air seems to shew that the air is mended by taking something from it, and not by adding to it." He adds, "I hope this will give some check to the rage of destroying trees that grow near houses, which has accompanied our late improvements in gardening, from an opinion of their being unwholesome. I am certain, from long observation, that there is nothing unhealthy in the air of woods; for we Americans have every where our country habitations in the midst of woods, and no people on earth enjoy better health, or are more prolific."

Having rendered inflammable air perfectly innoxious by continued *agitation in a trough of water*, deprived of its air, I concluded that other kinds of noxious air might be restored by the same means; and I presently found that this was the case with putrid air, even of more than a year's standing. I shall observe once for all, that this process has never failed to restore any kind of noxious air on which I have tried it, viz. air injured by respiration or putrefaction, air infected with the fumes of burning charcoal, and of calcined metals, air in which a mixture of iron filings and brimstone, that in which paint made of white lead and oil has stood, or air which has been diminished by a mixture of nitrous air. Of the remarkable effect which this process has on nitrous air itself, an account will be given in its proper place.

If this process be made in water deprived of air, either by the air-pump, by boiling, or by distillation, or if fresh rain-water be used, the air will always be diminished by the agitation; and this is certainly the fairest method of making the experiment. If the water be fresh pump-water, there will always be an increase of the air by agitation, the air contained in the water being set loose, and joining that which is in the jar. In this case, also, the air has never failed to be restored; but then it might be suspected that the melioration was produced by the addition of some more wholesome ingredient. As these agitations were made in jars with wide mouths, and in a trough which had a large surface exposed to the common air, I take it for granted that the noxious effluvia, whatever they be, were first imbibed by the water, and thereby transmitted to the common atmosphere. In some cases this was sufficiently indicated by the disagreeable smell which attended the operation.

After I had made these experiments, I was informed that an ingenious physician and philosopher had kept a fowl alive twenty-four hours, in a quantity of air in which another fowl of the same size had not been able to live longer than an hour, by contriving to make the air, which it breathed, pass through no very large quantity of acidulated water, the surface of which was not exposed to the common air; and that even when the water was not acidulated, the fowl lived much longer than it could have done, if the air which it breathed had not been drawn through the water.

As I should not have concluded that this experiment would have succeeded so well, from any observations that I had made upon the subject, I took a quantity of air in which mice had died, and agitated it very strongly, first in about five times its own quantity of distilled water, in the manner in which I had impregnated water with fixed air; but though the operation was continued a long time, it made no sensible change in the properties of the air. I also repeated the operation with pump-water, but with as little effect. In this case, however, though the air was agitated in a phial, which had a narrow neck, the surface of the water in the bason was considerably large, and exposed to the common atmosphere, which must have tended a little to favour the experiment.

In order to judge more precisely of the effect of these different methods of agitating air, I transferred the very noxious air, which I had hot been able to amend in the least degree by the former method, into an open jar, standing in a trough of water; and when I had agitated it till it was diminished about one third, I found it to be better than air in which candles had burned out, as appeared by the test of the nitrous air; and a mouse lived in 2-1/2 ounce measures of it a quarter of an hour, and was not sensibly affected the first ten or twelve minutes.

In order to determine whether the addition of any *acid* to the water, would make it more capable of restoring putrid air, I agitated a quantity of it in a phial containing very strong vinegar; and after that in *aqua fortis*, only half diluted with water; but by neither of these processes was the air at all mended, though the agitation was repeated, at intervals, during a whole day, and it was moreover allowed to stand in that situation all night.

Since, however, water in these experiments must have imbibed and retained a certain portion of the noxious effluvia, before they could be transmitted to the external air, I do not think it improbable but that the agitation of the sea and large lakes may be of some use for the purification of the atmosphere, and the putrid matter contained in water may be imbibed by aquatic plants, or be deposited in some other manner.

Having found, by several experiments above-mentioned that the proper putrid effluvium is something quite distinct from fixed air, and finding, by the experiments of Dr. Macbride, that fixed air corrects putrefaction; it occured to me, that fixed air, and air tainted with putrefaction, though equally, noxious when separate, might make a wholesome mixture, the one, correcting the other; and I was confirmed in this opinion by, I believe, not less than fifty or sixty instances, in which air, that had been made in the highest degree noxious, by respiration or putrefaction, was so far sweetened, by a mixture of about four times as much fixed air, that afterwards mice lived in it exceedingly well, and in some cases almost as long as in common air. I found it, indeed, to be more difficult to restore *old* putrid air by this means; but I hardly ever failed to do it, when the two kinds of air had stood a long time together; by which I mean about a fortnight or three weeks.

The reason why I do not absolutely conclude that the restoration of air in these cases was the effect of fixed air, is that, when I made a trial of the mixture, I sometimes agitated the two kinds of air pretty strongly together, in a trough of water, or at least passed it several times through water, from one jar to another, that the superfluous fixed air might be absorbed, not suspecting at that time that the agitation could have any other effect. But having since found that very violent, and especially long-continued agitation in water, without any mixture of fixed air, never failed to render any kind of noxious air in some measure fit for respiration (and in one particular instance the mere transferring of the air from one vessel to another through the water, though for a much longer time than I ever used for the mixtures of air, was of considerable use for the same purpose) I began to entertain some doubt of the efficacy of fixed air in this case. In some cases also the mixture of fixed air had by no means so much effect on the putrid air as, from the generality of my observations, I should have expected.

I was always aware, indeed, that it might be said, that, the residuum of fixed air not being very noxious, such an addition must contribute to mend the putrid air; but, in order to obviate this objection, I once mixed the residuum of as much fixed air as I had found, by a variety of trials, to be sufficient to restore a given quantity of putrid air, with an equal quantity of that air, without making any sensible melioration of it.

Upon the whole, I am inclined to think that this process could hardly have succeeded so well as it did with me, and in so great a number of trials, unless fixed air have some tendency to correct air tainted with respiration or putrefaction; and it is perfectly agreeable to the analogy of Dr. Macbride's discoveries, and may naturally be expected from them, that it should have such an effect.

By a mixture of fixed air I have made wholesome the residuum of air generated by putrefaction only, from mice plunged in water. This, one would imagine, *à priori*, to be the most noxious of all kinds of air. For if common air only tainted with putrefaction be so deadly, much more might one expect that air to be so, which was generated from putrefaction only; but it seems to be nothing more than common air (or at least that kind of fixed air which is not absorbed by water) tainted with putrefaction, and therefore requires no other process to sweeten it. In this case, however, we seem to have an instance of the generation of genuine common air, though mixed with something that is foreign to it. Perhaps the residuum of fixed air may be another instance of the same nature, and also the residuum of inflammable air, and of nitrous air, especially nitrous air loaded with phlogiston, after long agitation in water.

Fixed air is equally diffused through the whole mass of any quantity of putrid air with which it is mixed: for dividing the mixture into two equal parts, they were reduced in the same proportion by passing through water. But this is also the case with some of the kinds of air which will not incorporate, as inflammable air, and air in which brimstone has burned.

If fixed air tend to correct air which has been injured by animal respiration or putrefaction, *lime kilns*, which discharge great quantities of fixed air, may be wholesome in the neighbourhood of populous cities, the atmosphere of which must abound with putrid effluvia. I should think also that physicians might avail themselves of the application of fixed air in many putrid disorders, especially as it may be so easily administered by way of *clyster*, where it would often find its way to much of the putrid matter.

Nothing is to be apprehended from the distention of the bowels by this kind of air, since it is so readily absorbed by any fluid or moist substance.

Since fixed air is not noxious *per se*, but, like fire, only in excess, I do not think it at all hazardous to attempt to *breathe* it. It is however easily conveyed into the *stomach*, in natural or artificial Pyrmont water, in briskly-fermenting liquors, or a vegetable diet. It is even possible, that a considerable quantity of fixed air might be imbibed by the absorbing vessels of the skin, if the whole body, except the head, should be suspended over a vessel of strongly-fermenting liquor; and in some putrid disorders this treatment might be very salutary. If the body was exposed quite naked, there would be very little danger from the cold in this situation, and the air having freer access to the skin might produce a greater effect. Being no physician, I run no risk by throwing out these random, and perhaps whimsical proposals.[5]

Having communicated my observations on fixed air, and especially my scheme of applying it by way of *clyster* in putrid disorders, to Mr. Hey, an ingenious surgeon in Leeds a case presently occurred, in which he had an opportunity of giving it a trial; and mentioning it to Dr. Hird and Dr. Crowther, two physicians who attended the patient, they approved the scheme, and it was put in execution; both by applying the fixed air by way of clyster, and at the same time making the patient drink plentifully of liquors strongly impregnated with it. The event was such, that I requested Mr. Hey to draw up a particular account of the case, describing the whole of the treatment, that the public might be satisfied that this new application of fixed air is perfectly safe, and also, have an opportunity of judging how far it had the effect which I expected from it; and as the application is new, and not unpromising, I shall subjoin his letter to me on the subject, by way of *Appendix* to these papers.

When I began my inquires into the properties of different kinds of air, I engaged my friend Dr. Percival to attend to the *medicinal uses* of them, being sensible that his knowledge of philosophy as well as of medicine would give him a singular advantage for this purpose. The result of his observations I shall also insert in the Appendix.

FOOTNOTES:

[5] Some time after these papers were first printed, I was pleased to find the same proposal in *Dr. Alexander's Experimental Essays*.

SECTION V.

Of Air in which a mixture of Brimstone and Filings of Iron has stood.

Reading in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which *a mixture of powdered brimstone and filings of iron, made into a paste with water*, had stood, I repeated the experiment, and found the diminution greater than I had expected. This diminution of air is made as effectually, and as expeditiously, in quicksilver as in water; and it may be measured with the greatest accuracy, because there is neither any previous expansion or increase of the quantity of air, and because it is some time before this process begins to have any sensible effect. This diminution of air is various; but I have generally found it to be between one fifth and one fourth of the whole.

Air thus diminished is not heavier, but rather lighter than common air; and though lime-water does not become turbid when it is exposed to this air, it is probably owing to the formation of a selenitic salt, as was the case with the simple burning of brimstone above-mentioned. That something proceeding from the brimstone strongly affects the water which is confined in the same place with this mixture, is manifest from the very strong smell that it has of the volatile spirit of vitriol.

I conclude that the diminution of air by this, process is of the same kind with the diminution of it in the other cases, because when this mixture is put into air which has been previously diminished, either by the burning of candles, by respiration, or putrefaction, though it never fails to diminish it something more, it is, however, no farther than this process alone would have done it. If a fresh mixture be introduced into a quantity of air which had been reduced by a former mixture, it has little or no farther effect.

I once observed, that when a mixture of this kind was taken out of a quantity of air in which a candle had before burned out, and in which it had stood for several days, it was quite cold and black, as it always becomes in a confined place; but it presently grew very hot, smoaked copously, and smelled very offensively; and when it was cold, it was brown, like the rust of iron.

I once put a mixture of this kind to a quantity of inflammable air, made from iron, by which means it was diminished 1/9 or 1/10 in its bulk; but, as far as I could judge, it was still as inflammable as ever. Another quantity of inflammable air was also reduced in the same proportion, by a mouse putrefying in it; but its inflammability was not seemingly lessened.

Air diminished by this mixture of iron filings and brimstone, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The smell of it is very pungent and offensive.

The quantity of this mixture which I made use of in the preceding experiments, was from two to four ounce measures; but I did not perceive, but that the diminution of the quantity of air (which was generally about twenty ounce measures) was as great with the smallest, as with the largest quantity. How small a quantity is necessary to diminish a given quantity of air to a *maximum*, I have made no experiments to ascertain.

As soon as this mixture of iron filings with, brimstone and water, begins to ferment, it also turns black, and begins to swell, and it continues to do so, till it occupies twice as much space as it did at first. The force with which it expands is great; but how great it is I have not endeavoured to determine.

When this mixture is immersed in water, it generates no air, though it becomes black, and swells.

SECTION VI.

Of NITROUS AIR.

Ever since I first read Dr. Hales's most excellent *Statical Essays*, I was particularly struck with that experiment of his, of which an account is given, VOL. I, p. 224. and VOL. II, p. 280. in which common air, and air generated from the Walton pyrites, by spirit of nitre, made a turbid red mixture, and in which part of the common air was absorbed; but I never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to that particular mineral. Happening to mention this subject to the Hon. Mr. Cavendish, when I was in London, in the spring of the year 1772, he said that he did not imagine but that other kinds of pyrites, or the metals might answer as well, and that probably the red appearance of the mixture depended upon the spirit of nitre only. This encouraged me to attend to the subject; and having no pyrites, I began with the solution of the different metals in spirit of nitre, and catching the air which was generated in the solution, I presently found what I wanted, and a good deal more.

Beginning with the solution of brass, on the 4th of June 1772, I first found this remarkable species of air, only one effect of which, was casually observed by Dr. Hales; and he gave so little attention to it, and it has been so much unnoticed since his time, that, as far as I know, no name has been given to it. I therefore found myself, contrary to my first resolution, under an absolute necessity of giving a name to this kind of air myself. When I first began to speak and write of it to my friends, I happened to distinguish it by the name of *nitrous air*, because I had procured it by means of spirit of nitre only; and though I cannot say that I altogether like the term, neither myself nor any of my friends, to whom I have applied for the purpose, have been able to hit upon a better; so that I am obliged, after all, to content myself with it.

I have found that this kind of air is readily procured from iron, copper, brass, tin, silver, quicksilver, bismuth, and nickel, by the nitrous acid only, and from gold and the regulus of antimony by *aqua regia*. The circumstances attending the solution of each of these metals are various, but hardly worth mentioning, in treating of the properties of the *air* which they yield; which, from what metal soever it is extracted, has, as far as I have been able to observe, the very same properties.

One of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. The *smell* of it, also, is very strong, and remarkable, but very much resembling that of smoking spirit of nitre.

The diminution of a mixture of this and common air is not an equal diminution of both the kinds, which is all that Dr. Hales could observe, but of about one fifth of the common air, and as much of the nitrous air as is necessary to produce that effect; which, as I have found by many trials, is about one half as much as the original quantity of common air. For if one measure of nitrous air be put to two measures of common air, in a few minutes (by which time the effervescence will be over, and the mixture will have recovered its transparency) there will want about one ninth of the original two measures; and if both the kinds of air be very pure, the diminution will still go on slowly, till in a day or two, the whole will be reduced to one fifth less than the original quantity of common air. This farther diminution, by long standing, I had not observed at the time of the first publication of these papers.

I hardly know any experiment that is more adapted to amaze and surprize than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is considerably diminished by it. If, after this full saturation of common air with nitrous air, more nitrous air be put to it, it makes an addition equal to its own bulk, without producing the least redness, or any other visible effect.

If the smallest quantity of common air be put to any larger quantity of nitrous air, though the two together will not occupy so much space as they did separately, yet the quantity will still be larger than that of the nitrous air only. One ounce measure of common air being put to near twenty ounce measures of nitrous air, made an addition to it of about half an ounce measure. This being a much greater proportion than the diminution of common air, in the former experiment, proves that part of the diminution in the former case is in the nitrous air. Besides, it will presently appear, that nitrous air is subject to a most remarkable diminution; and as common air, in a variety of other cases, suffers a diminution from one fifth to one fourth, I conclude, that in this case also it does not exceed that proportion, and therefore that the remainder of the diminution respects the nitrous air.

In order to judge whether the *water* contributed to the diminution of this mixture of nitrous and common air, I made the whole process several times in quicksilver, using one third of nitrous, and two thirds of common air, as before. In this case the redness continued a very long time, and the diminution was not so great as when the mixtures had been made in water, there remaining one seventh more than the original quantity of common air.

This mixture stood all night upon the quicksilver; and the next morning I observed that it was no farther diminished upon the admission of water to it, nor by pouring it several times through the water, and letting it stand in water two days.

Another mixture, which had stood about six hours on the quicksilver, was diminished a little more upon the admission of water, but was never less than the original quantity of common air. In another case however, in which the mixture had stood but a very short time in quicksilver, the farther diminution, which took place upon the admission of water, was much more considerable; so that the diminution, upon the whole, was very nearly as great as if the process had been intirely in water.

It is evident from these experiments, that the diminution is in part owing to the absorption by the water; but that when the mixture is kept a long time, in a situation in which there is no water to absorb any part of it, it acquires a constitution, by which it is afterwards incapable of being absorbed by water, or rather, there is an addition to the quantity of air by nitrous air produced by the solution of the quicksilver.

It will be seen, in the second part of this work, that, in the decomposition of nitrous air by its mixture with common air, there is nothing at hand when the process is made in quicksilver, with which the acid that entered into its composition can readily unite.

In order to determine whether the fixed part of common air was deposited in the diminution of it by nitrous air, I inclosed a vessel full of lime-water in the jar in which the process was made, but it occasioned no precipitation of the lime; and when the vessel was taken out, after it had been in that situation a whole day, the lime was easily precipitated by breathing into it as usual.

But though the precipitation of the lime was not sensible in this method of making the experiment, it is sufficiently so when the whole process is made in lime-water, as will be seen in the second part of this work; so that we have here another evidence of the deposition of fixed air from common air. I have made no alteration, however, in the preceding paragraph, because it may not be unuseful, as a caution to future experimenters.

It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, or *air fit for respiration*; and, as far as I can judge, from a great number of observations, is at least very nearly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by putting mice, or any other animals, to breathe in it.

This was a most agreeable discovery to me, as I hope it may be an useful one to the public; especially as, from this time, I had no occasion for so large a stock of mice as I had been used to keep for the purpose of these experiments, using them only in those which required to be very decisive; and in these cases I have seldom failed to know beforehand in what manner they would be affected.

It is also remarkable that, on whatever account air is unfit for respiration, this same test is equally applicable. Thus there is not the least effervescence between nitrous and fixed air, or inflammable air, or any species of diminished air. Also the degree of diminution being from nothing at all to more than one third of the whole of any quantity of air, we are, by this means, in possession of a prodigiously large *scale*, by which we may distinguish very small degrees of difference in the goodness of air.

I have not attended much to this circumstance, having used this test chiefly for greater differences; but, if I did not deceive myself, I have perceived a real difference in the air of my study, after a few persons have been with me in it, and the air on the outside of the house. Also a phial of air having been sent me, from the neighbourhood of York, it appeared not to be so good as the air near Leeds; that is, it was not diminished so much by an equal mixture of nitrous air, every other circumstance being as nearly the same as I could contrive. It may perhaps be possible, but I have not yet attempted it, to distinguish some of the different winds, or the air of different times of the year, &c. &c. by this test.

By means of this test I was able to determine what I was before in doubt about, viz. the *kind* as well as the *degree* of injury done to air by candles burning in it. I could not tell with certainty, by means of mice, whether it was at all injured with respect to respiration; and yet if nitrous air may be depended upon for furnishing an accurate test, it must be rather more than one third worse than common air, and have been diminished by the same general cause of the other diminutions of air. For when, after many trials, I put one measure of thoroughly putrid and highly noxious air, into the same vessel with two measures of good wholesome air, and into another vessel an equal quantity, viz. three measures of air in which a candle had burned out; and then put equal quantities of nitrous air to each of them, the latter was diminished rather more than the former.

It agrees with this observation, that *burned air* is farther diminished both by putrefaction, and a mixture of iron filings and brimstone; and I therefore take it for granted by every other cause of the diminution of air. It is probable, therefore, that burned air is air so far loaded with phlogiston, as to be able to extinguish a candle, which it may do long before it is fully saturated.

Inflammable air with a mixture of nitrous air burns with a green flame. This makes a very pleasing experiment when it is properly conducted. As, for some time, I chiefly made use of *copper* for the generation of nitrous air, I first ascribed this circumstance to that property of this metal, by which it burns with a green flame; but I was presently satisfied that it must arise from the spirit of nitre, for the effect is the very same from which ever of the metals the nitrous air is extracted, all of which I tried for this purpose, even silver and gold.

A mixture of oil of vitriol and spirit of nitre in equal proportions dissolved iron, and the produce was nitrous air; but a less degree of spirit of nitre in the mixture produced air that was inflammable, and which burned with a green flame. It also tinged common air a little red, and diminished it, though not much.

The diminution of common air by a mixture of nitrous air, is not so extraordinary as the diminution which nitrous air itself is subject to from a mixture of iron filings and brimstone, made into a paste with water. This mixture, as I have already observed, diminishes common air between one fifth and one fourth, but has no such effect upon any kind of air that has been diminished, and rendered noxious by any other process; but when it is put to a quantity of nitrous air, it diminishes it so much, that no more than one fourth of the original quantity will be left.

The effect of this process is generally perceived in five or six hours, about which time the visible effervesence of the mixture begins; and in a very short time it advances so rapidly, that in about an hour almost the whole effect will have taken place. If it be suffered to stand a day or two longer, the air will still be diminished farther, but only a very little farther, in proportion to the first diminution. The glass jar, in which the air and this mixture have been confined, has generally been so much heated in this process, that I have not been able to touch it.

Nitrous air thus diminished has not so strong a smell as nitrous air itself, but smells just like common air in which the same mixture has stood; and it is not capable of being diminished any farther, by a fresh mixture of iron and brimstone.

Common air saturated with nitrous air is also no farther diminished by this mixture of iron filings and brimstone, though the mixture ferments with great heat, and swells very much in it.

Plants die very soon, both in nitrous air, and also in common air saturated with nitrous air, but especially in the former.

Neither nitrous air, nor common air saturated with nitrous air, differ in specific gravity from common air. At least, the difference is so small, that I could not be sure there was any; sometimes about three pints of it seeming to be about half a grain heavier, and at other times as much lighter than common air.

Having, among other kinds of air, exposed a quantity of nitrous air to water out of which the air had been well boiled, in the experiment to which I have more than once referred (as having been the occasion of several new and important observations) I found that 19/20 of the whole was absorbed. Perceiving, to my great surprize, that so very great a proportion of this kind of air was miscible with water, I immediately began to agitate a considerable quantity of it, in a jar standing in a trough of the same kind of water; and, with about four times as much agitation as fixed air requires, it was so far absorbed by the water, that only about one fifth remained. This remainder extinguished flame, and was noxious to animals.

Afterwards I diminished a pretty large quantity of nitrous air to one eighth of its original bulk, and the remainder still retained much of its peculiar smell, and diminished common air a little. A mouse also died in it, but not so suddenly as it would have done in pure nitrous air. In this operation the peculiar smell of nitrous air is very manifest, the water being first impregnated with the air, and then transmitting it to the common atmosphere.

This experiment gave me the hint of impregnating water with nitrous air, in the manner in which I had before done it with fixed air; and I presently found that distilled water would imbibe about one tenth of its bulk of this kind of air, and that it acquired a remarkably acid and astringent taste from it. The smell of water thus impregnated is at first peculiarly pungent. I did not chuse to swallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in some cases, salutary.

This kind of air is retained very obstinately by water. In an exhausted receiver a quantity of water thus saturated emitted a whitish fume, such as sometimes issues from bubbles of this air when it is first generated, and also some air-bubbles; but though it was suffered to stand a long time in this situation, it still retained its peculiar taste; but when it had stood all night pretty near the fire, the water was become quite vapid, and had deposited a filmy kind of matter, of which I had often collected a considerable quantity from the trough in which jars containing this air had stood. This I suppose to be a precipitate of the metal, by the solution of which the nitrous air was generated. I have not given so much attention to it as to know, with certainty, in what circumstances this *deposit* is made, any more than I do the matter deposited from inflammable air above-mentioned; for I cannot get it, at least in any considerable quantity, when I please; whereas I have often found abundance of it, when I did not expect it at all.

The nitrous air with which I made the first impregnation of water was extracted from copper; but when I made the impregnation with air from quicksilver, the water had the very same taste, though the matter deposited from it seemed to be of a different kind; for it was whitish, whereas the other had a yellowish tinge. Except the first quantity of this impregnated water, I could never deprive any more that I made of its peculiar taste. I have even let some of it stand more than a week, in phials with their mouths open, and sometimes very near the fire, without producing any alteration in it[6].

Whether any of the spirit of nitre contained in the nitrous air be mixed with the water in this operation, I have not yet endeavoured to determine. This, however, may probably be the case, as the spirit of nitre is, in a considerable degree, volatile[7].

It will perhaps be thought, that the most *useful*, if not the most remarkable, of all the properties of this extraordinary kind of air, is its power of preserving animal substances from putrefaction, and of restoring those that are already putrid, which it possesses in a far greater degree than fixed air. My first observation of this was altogether casual. Having found nitrous air to suffer so great a diminution as I have already mentioned by a mixture of iron filings and brimstone, I was willing to try whether it would be equally diminished by other causes of the diminution of common air, especially by putrefaction; and for this purpose I put a dead mouse into a quantity of it, and placed it near the fire, where the tendency to putrefaction was very great. In this case there was a considerable diminution, viz. from 5-1/4 to 3-1/4; but not so great as I had expected, the antiseptic power of the nitrous air having checked the tendency to putrefaction; for when, after a week, I took the mouse out, I perceived, to my very great surprize, that it had no offensive smell.

Upon this I took two other mice, one of them just killed, and the other soft and putrid, and put them both into the same jar of nitrous air, standing in the usual temperature of the weather, in the months of July and August of 1772; and after twenty-five days, having observed that there was little or no change in the quantity of the air, I took the mice out; and, examining them, found them both perfectly sweet, even when cut through in several places. That which had been put into the air when just dead was quite firm; and the flesh of the other, which had been putrid and soft, was still soft, but perfectly sweet.

In order to compare the antiseptic power of this kind of air with that of fixed air, I examined a mouse which I had inclosed in a phial full of fixed air, as pure as I could make it, and which I had corked very close; but upon opening this phial in water about a month after, I perceived that a large quantity of putrid effluvium had been generated; for it rushed with violence out of the phial; and the smell that came from it, the moment the cork was taken out, was insufferably offensive. Indeed Dr. Macbride says, that he could only restore very thin pieces of putrid flesh by means of fixed air. Perhaps the antiseptic power of these kinds of air may be in proportion to their acidity.

If a little pains were taken with this subject, this remarkable antiseptic power of nitrous air might possibly be applied to various uses, perhaps to the preservation of the more delicate birds, fishes, fruits, &c. mixing it in different proportions with common or fixed air. Of this property of nitrous air anatomists may perhaps avail themselves, as animal substances may by this means be preserved in their natural soft state; but how long it will answer for this purpose, experience only can shew.

I calcined lead and tin in the manner hereafter described in a quantity of nitrous air, but with very little sensible effect; which rather surprized me; as, from the result of the experiment with the iron filings and brimstone, I had expected a very great diminution of the nitrous air by this process; the mixture of iron filings and brimstone, and the calcination of metals, having the same effect upon common air, both of them diminishing it in nearly the same proportion. But though I made the metals *fume* copiously in nitrous air, there might be no real *calcination*, the phlogiston not being separated, and the proper calcination prevented by there being no *fixed air*, which is necessary to the formation of the calx, to unite with it.

Nitrous air is procured from all the proper metals by spirit of nitre, except lead, and from all the semi-metals that I have tried, except zinc. For this purpose I have used bismuth and nickel, with spirit of nitre only, and regulus of antimony and platina, with *aqua regia*.

I got little or no air from lead by spirit of nitre, and have not yet made any experiments to ascertain the nature of this solution. With zinc I have taken a little pains.

Four penny-weights and seventeen grains of zinc dissolved in spirit of nitre, to which as much water was added, yielded about twelve ounce measures of air, which had, in some degree, the properties of nitrous air, making a slight effervescence with common air, and diminishing it about as much as nitrous air, which had been itself diminished one half by washing in water. The smell of them both was also the same; so that I concluded it to be the same thing, that part of the nitrous air, which is imbibed by water, being retained in this solution.

In order to discover whether this was the case, I made the solution boil in a sand-heat. Some air came from it in this state, which seemed to be the same thing, with nitrous air diminished about one sixth, or one eighth, by washing in water. When the fluid part was evaporated, there remained a brown fixed substance, which was observed by Mr. Hellot, who describes it, Ac. Par. 1735, M. p. 35. A part of this I threw into a small red-hot crucible; and covering it immediately with a receiver, standing in water, I observed that very dense red fumes rose from it, and filled the receiver. This redness continued about as long as that which is occasioned by a mixture of nitrous and common air; the air was also considerably diminished within the receiver. This substance, therefore, must certainly have contained within it the very same thing, or principle, on which the peculiar properties of nitrous air depend.

It is remarkable, however, that though the air within the receiver was diminished about one fifth by this process, it was itself as much affected with a mixture of nitrous air, as common air is, and a candle burned in it very well. This may perhaps be attributed to some effect of the spirit of nitre, in the composition of that brown substance.

Nitrous air, I find, will be considerably diminished in its bulk by standing a long time in water, about as much as inflammable air is diminished in the same circumstances. For this purpose I kept for some months a quart-bottle full of each of these kinds of air; but as different quantities of inflammable air vary very much in this respect, it is not improbable but that nitrous air may vary also.

From one trial that I made, I conclude that nitrous air may be kept in a bladder much better than most other kinds of air. The air to which I refer was kept about a fortnight in a bladder, through which the peculiar smell of the nitrous air was very sensible for several days. In a day or two the bladder became red, and was much contracted in its dimensions. The air within it had lost very little of its peculiar property of diminishing common air.

I did not endeavour to ascertain the exact quantity of nitrous air produced from given quantities, of all the metals which yield it; but the few observations which I did make for this purpose I shall recite in this place:

dwt.	gr.			
6	0	of silver	yielded	17-1/2 ounce measures.
5	19	of quicksilver		4-1/2
1	2-1/2	of copper		14-1/2
2	0	of brass		21
0	20	of iron		16
1	5	of bismuth		6
0	12	of nickel		4

FOOTNOTES:

[6] I have since found, that nitrous air has never failed to escape from the water, which has been impregnated with it, by long exposure to the open air.

[7] This suspicion has been confirmed by the ingenious Mr. Bewley, of Great Massingham in Norfolk, who has discovered that the acid taste of this water is not the necessary consequence of its impregnation with nitrous air, but is the effect of the *acid vapour*, into which part of this air is resolved, when it is decomposed by a mixture with common air. This, it will be seen, exactly agrees with my own observation on the constitution of nitrous air, in the second part of this work. A more particular account of Mr. Bewley's observation will be given in the *Appendix*.

SECTION VII.

Of Air infected with the fumes of burning Charcoal.

Air infected with the fumes of burning charcoal is well known to be noxious; and the Honourable Mr. Cavendish favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce measures, by passing through a red-hot iron tube filled with the dust of charcoal. This diminution he ascribed to such a *destruction* of common air as Dr. Hales imagined to be the consequence of burning. Mr. Cavendish also observed, that there had been a generation of fixed air in this process, but that it was absorbed by sope leys. This experiment I also repeated, with a small variation of circumstances, and with nearly the same result.

Afterwards, I endeavoured to ascertain, by what appears to me to be an easier and more certain method, in what manner air is affected with the fumes of charcoal, viz. by suspending bits of charcoal within glass vessels, filled to a certain height with water, and standing inverted in another vessel of water, while I threw the focus of a burning mirror, or lens, upon them. In this manner I diminished a given quantity of air one fifth, which is nearly in the same proportion with other diminutions of air.

If, instead of pure water, I used *lime-water* in this process, it never failed to become turbid by the precipitation of the lime, which could only be occasioned by fixed air, either discharged from the charcoal, or deposited by the common air. At first I concluded that it came from the charcoal; but considering that it is not probable that fixed air, confined in any substance, can bear so great a degree of heat as is necessary to make charcoal, without being wholly expelled; and that in other diminutions of common air, by phlogiston only, there appears to be a deposition of fixed air, I have now no doubt but that, in this case also, it is supplied from the same source.

This opinion is the more probable, from there being the same precipitation of lime, in this process, with whatever degree of heat the charcoal had been made. If, however, the charcoal had not been made with a very considerable degree of heat, there never failed to be a permanent addition of inflammable air produced; which agrees with what I observed before, that, in converting dry wood into charcoal, the greatest part is changed into inflammable air.

I have sometimes found, that charcoal which was made with the most intense heat of a smith's fire, which vitrified part of a common crucible in which the charcoal was confined, and which had been continued above half an hour, did not diminish the air in which the focus of a burning mirror was thrown upon it; a quantity of inflammable air equal to the diminution of the common air being generated in the process: whereas, at other times, I have not perceived that there was any generation of inflammable air, but a simple diminution of common air, when the charcoal had been made with a much less degree of heat. This subject deserves to be farther investigated.

To make the preceding experiment with still more accuracy, I repeated it in quicksilver; when I perceived that there was a small increase of the quantity of air, probably from a generation of inflammable air. Thus it stood without any alteration a whole night, and part of the following day; when lime-water, being admitted to it, it presently became turbid, and, after some time, the whole quantity of air, which was about four ounce measures, was diminished one fifth, as before. In this case, I carefully weighed the piece of charcoal, which was exactly two grains, and could not find that it was sensibly diminished in weight by the operation.

Air thus diminished by the fumes of burning charcoal not only extinguishes flame, but is in the highest degree noxious to animals; it makes no effervescence with nitrous air, and is incapable of being diminished any farther by the fumes of more charcoal, by a mixture of iron filings and brimstone, or by any other cause of the diminution of air that I am acquainted with.

This observation, which respects all other kinds of diminished air, proves that Dr. Hales was mistaken in his notion of the *absorption* of air in those circumstances in which he observed it. For he supposed that the remainder was, in all cases, of the same nature with that which had been absorbed, and that the operation of the same cause would not have failed to produce a farther diminution; whereas all my observations shew that air, which has once been fully diminished by any cause whatever, is not only incapable of any farther diminution, either from the same or from any other cause, but that it has likewise acquired *new properties*, most remarkably different from those which it had before, and that they are, in a great measure, the same in all the cases. These circumstances give reason to suspect, that the cause of diminution is, in reality, the same in all the cases. What this cause is, may, perhaps, appear in the next course of observations.

SECTION VIII.

Of the effect of the CALCINATION of METALS, and of the EFFLUVIA of PAINT made with WHITE-LEAD and OIL, on AIR.

Having been led to suspect, from the experiments which I had made with charcoal, that the diminution of air in that case, and perhaps in other cases also, was, in some way or other the consequence of its having more than its usual quantity of phlogiston, it occurred to me, that the calcination of metals, which are generally supposed to consist of nothing but a metallic earth united to phlogiston, would tend to ascertain the fact, and be a kind of *experimentum crucis* in the case.

Accordingly, I suspended pieces of lead and tin in given quantities of air, in the same manner as I had before treated the charcoal; and throwing the focus of a burning mirror or lens upon them, so as to make them fume copiously. I presently perceived a diminution of the air. In the first trial that I made, I reduced four ounce measures of air to three, which is the greatest diminution of common air that I had ever observed before, and which I account for, by supposing that, in other cases, there was not only a cause of diminution, but causes of addition also, either of fixed or inflammable air, or some other permanently elastic matter, but that the effect of the calcination of metals being simply the escape of phlogiston, the cause of diminution was alone and uncontrouled.

The air, which I had thus diminished by calcination of lead, I transferred into another clean phial, but found that the calcination of more lead in it (or at least the attempt to make a farther calcination) had no farther effect upon it. This air also, like that which had been infected with the fumes of charcoal, was in the highest degree noxious, made no effervescence with nitrous air, was no farther diminished by the mixture of iron filings and brimstone, and was not only rendered innoxious, but also recovered, in a great measure, the other properties of common air, by washing in water.

It might be suspected that the noxious quality of air in which *lead* was calcined, might be owing to some fumes peculiar to that metal; but I found no sensible difference between the properties of this air, and that in which *tin* was calcined.

The *water* over which metals are calcined acquires a yellowish tinge, and an exceedingly pungent smell and taste, pretty much (as near as I can recollect, for I did not compare them together) like that over which brimstone has been frequently burned. Also a thin and whitish pellicle covered both the surface of the water, and likewise the sides of the phial in which the calcination was made; insomuch that, without frequently agitating the water, it grew so opaque by this constantly accumulating incrustation, that the sun-beams could not be transmitted through it in a quantity sufficient to produce the calcination.

I imagined, however, that, even when this air was transferred into a clean phial, the metals were not so easily melted or calcined as they were in fresh air; for the air being once fully saturated with phlogiston, may not so readily admit any more, though it be only to transmit it to the water. I also suspected that metals were not easily melted or calcined in inflammable, fixed, or nitrous air, or any kind of diminished air.[8] None of these kinds of air suffered any change by this operation; nor was there any precipitation of lime, when charcoal was heated in any of these kinds of air standing in lime-water. This furnishes another, and I think a pretty decisive proof, that, in the precipitation of lime by charcoal, the fixed air does not come from the charcoal, but from the common air. Otherwise it is hard to assign a reason, why the same degree of heat (or at least a much greater) should not expel the fixed air from this substance, though surrounded by these different kinds of air, and why the fixed air might not be transmitted through them to the lime-water.

Query. May not water impregnated with phlogiston from calcined metals, or by any other method, be of some use in medicine? The effect of this impregnation is exceedingly remarkable; but the principle with which it is impregnated is volatile, and intirely escapes in a day or two, if the surface of the water be exposed to the common atmosphere.

It should seem that phlogiston is retained more obstinately by charcoal than it is by lead or tin; for when any given quantity of air is fully saturated with phlogiston from charcoal, no heat that I have yet applied has been able to produce any more effect upon it; whereas, in the same circumstances, lead and tin may still be calcined, at least be made to emit a copious fume, in which some part of the phlogiston may be set loose. The air indeed, can take no more; but the water receives it, and the sides of the phial also receive an addition of incrustation. This is a white powdery substance, and well deserves to be examined. I shall endeavour to do it at my leisure.

Lime-water never became turbid by the calcination of metals over it, the calx immediately seizing the precipitated fixed air, in preference to the lime in the water; but the colour, smell, and taste of the water was always changed and the surface of it became covered with a yellow pellicle, as before.

When this process was made in quicksilver, the air was diminished only one fifth; and upon water being admitted to it, no more was absorbed; which is an effect similar to that of a mixture of nitrous and common air, which was mentioned before.

The preceding experiments on the calcination of metals suggested to me a method of explaining the cause of the mischief which is known to arise from fresh *paint*, made with white-lead (which I suppose is an imperfect calx of lead) and oil.

To verify my hypothesis, I first put a small pot full of this kind of paint, and afterwards (which answered much better, by exposing a greater surface of the paint) I daubed several pieces of paper with it, and put them under a receiver, and observed, that in about twenty-four hours, the air was diminished between one fifth and one fourth, for I did not measure it very exactly. This air also was, as I expected to find, in the highest degree noxious; it did not effervesce with nitrous air, it was no farther diminished by a mixture of iron filings and brimstone, and was made wholesome by agitation in water deprived of all air.

I think it appears pretty evident, from the preceding experiments on the calcination of metals that air is, some way or other, diminished in consequence of being highly charged with phlogiston; and that agitation in water restores it, by imbibing a great part of the phlogistic matter.

That water has a considerable affinity with phlogiston, is evident from the strong impregnation which it receives from it. May not plants also restore air diminished by putrefaction by absorbing part of the phlogiston with which it is loaded? The greater part of a dry plant, as well as of a dry animal substance, consists of inflammable air, or something that is capable of being converted into inflammable air; and it seems to be as probable that this phlogistic matter may have been imbibed by the roots and leaves of plants, and afterwards incorporated into their substance, as that it is altogether produced by the power of vegetation. May not this phlogistic matter be even the most essential part of the food and support of both vegetable and animal bodies?

In the experiments with metals, the diminution of air seems to be the consequence of nothing but a saturation with phlogiston; and in all the other cases of the diminution of air, I do not see but that it may be effected by the same means. When a vegetable or animal substance is dissolved by putrefaction, the escape of the phlogistic matter (which, together with all its other constituent parts, is then let loose from it) may be the circumstance that produces the diminution of the air in which it putrefies. It is highly improbable that what remains after an animal body has been thoroughly dissolved by putrefaction, should yield so great a quantity of inflammable air, as the dried animal substance would have done. Of this I have not made an actual trial, though I have often thought of doing it, and still intend to do it; but I think there can be no doubt of the result.

Again, iron, by its fermentation with brimstone and water, is evidently reduced to a calx, so that phlogiston must have escaped from it. Phlogiston also must evidently be set loose by the ignition of charcoal, and is not improbably the matter which flies off from paint, composed of white-lead and oil. Lastly, since spirit of nitre is known to have a very remarkable affinity with phlogiston, it is far from being improbable that nitrous air may also produce the same effect by the same means.

To this hypothesis it may be objected, that, if diminished air be air saturated with phlogiston, it ought to be inflammable. But this by no means follows;

since its inflammability may depend upon some particular *mode of combination*, or degree of affinity, with which we are not acquainted. Besides, inflammable air seems to consist of some other principle, or to have some other constituent part, besides phlogiston and common air, as is probable from that remarkable deposit, which, as I have observed, is made by inflammable air, both from iron and zinc.

It is not improbable, however, but that a greater degree of heat may inflame that air which extinguishes a common candle, if it could be conveniently applied. Air that is inflammable, I observe, extinguishes red-hot wood; and indeed inflammable substances can only be those which, in a certain degree of heat, have a less affinity with the phlogiston they contain, than the air, or some other contiguous substance, has with it; so that the phlogiston only quits one substance, with which it was before combined, and enters another, with which it may be combined in a very different manner. This substance, however, whether it be air or any thing else, being now fully saturated with phlogiston, and not being able to take any more, in the same circumstances, must necessarily extinguish fire, and put a stop to the ignition of all other bodies, that is, to the farther escape of phlogiston from them.

That plants restore noxious air, by imbibing the phlogiston with which it is loaded, is very agreeable to the conjectures of Dr. Franklin, made many years ago, and expressed in the following extract from the last edition of his Letters, p. 346.

"I have been inclined to think that the fluid *fire*, as well as the fluid *air*, is attracted by plants in their growth, and becomes consolidated with the other materials of which they are formed, and makes a great part of their substance; that, when they come to be digested, and to suffer in the vessels a kind of fermentation, part of the fire, as well as part of the air, recovers its fluid active state again, and diffuses itself in the body, digesting and separating it; that the fire so re-produced, by digestion and separation, continually leaving the body, its place is supplied by fresh quantities, arising from the continual separation; that whatever quickens the motion of the fluids in an animal, quickens the separation, and re-produces more of the fire, as exercise; that all the fire emitted by wood, and other combustibles, when burning, existed in them before in a solid state, being only discovered when separating; that some fossils, as sulphur, sea-coal,

&c. contain a great deal of solid fire; and that, in short, what escapes and is dissipated in the burning of bodies, besides water and earth, is generally the air and fire, that before made parts of the solid."

FOOTNOTES:

[8] I conclude from the experiments of M. Lavoisier, which were made with a much better burning lens than I had an opportunity of making use of, that there was no *real calcination* of the metals, though they were made to *fume* in inflammable or nitrous air; because he was not able to produce more than a slight degree of calcination in any given quantity of common air.

SECTION IX.

Of Marine Acid Air.

Being very much struck with the result of an experiment of the Hon. Mr. Cavendish, related Phil. Trans. Vol. LVI. p. 157, by which, though, he says, he was not able to get any inflammable air from copper, by means of spirit of salt, he got a much more remarkable kind of air, viz. one that lost its elasticity by coming into contact with water, I was exceedingly desirous of making myself acquainted with it. On this account, I began with making the experiment in quicksilver, which I never failed to do in any case in which I suspected that air might either be absorbed by water, or be in any other manner affected by it; and by this means I presently got a much more distinct idea of the nature and effects of this curious solution.

Having put some copper filings into a small phial, with a quantity of spirit of salt; and making the air (which was generated in great plenty, on the application of heat) ascend into a tall glass vessel full of quicksilver, and standing in quicksilver, the whole produce continued a considerable time without any change of dimensions. I then introduced a small quantity of water to it; when about three fourths of it (the whole being about four ounce measures) presently, but gradually, disappeared, the quicksilver rising in the vessel. I then introduced a considerable quantity of water; but there was no farther diminution of the air, and the remainder I found to be inflammable.

Having frequently continued this process a long time after the admission of the water, I was much amused with observing the large bubbles of the newly generated air, which came through the quicksilver, the sudden diminution of them when they came to the water, and the very small bubbles which went through the water. They made, however, a continual, though slow, increase of inflammable air.

Fixed air, being admitted to the whole produce of this air from copper, had no sensible effect upon it. Upon the admission of water, a great part of the mixture presently disappeared; another part, which I suppose to have been the fixed air, was absorbed slowly; and in this particular case the very small permanent residuum did not take fire; but it is very possible that it might have done so, if the quantity had been greater.

The solution of *lead* in the marine acid is attended with the very same phænomena as the solution of copper in the same acid; about three fourths of the generated air disappearing on the admission of water; and the remainder being inflammable.

The solutions of iron, tin, and zinc, in the marine acid, were all attended with the same phænomena as the solutions of copper and lead, but in a less degree; for in iron one eighth, in tin one sixth, and in zinc one tenth of the generated air disappeared on the admission of water. The remainder of the air from iron, in this case, burned with a green, or very light blue flame.

I had always thought it something extraordinary that a species of air should *lose its elasticity* by the mere *contact* of any thing, and from the first suspected that it must have been *imbibed* by the water that was admitted to it; but so very great a quantity of this air disappeared upon the admission of a very small quantity of water, that at first I could not help concluding that appearances favoured the former hypothesis. I found, however, that when I admitted a much smaller quantity of water, confined in a narrow glass tube, a part only of the air disappeared, and that very slowly, and that more of it vanished upon the admission of more water. This observation put it beyond a doubt, that this air was properly *imbibed* by the water, which, being once fully saturated with it, was not capable of receiving any more.

The water thus impregnated tasted very acid, even when it was much diluted with other water, through which the tube containing it was drawn. It even dissolved iron very fast, and generated inflammable air. This last observation, together with another which immediately follows, led me to the discovery of the true nature of this remarkable kind of air.

Happening, at one time, to use a good deal of copper and a small quantity of spirit of salt, in the generation of this kind of air, I was surprized to find that air was produced long after, I could not but think that the acid must have been saturated with the metal; and I also found that the proportion of inflammable air to that which was absorbed by the water continually diminished, till, instead of being one fourth of the whole, as I had first observed, it was not so much as one twentieth. Upon this, I concluded that this subtle air did not arise from the copper, but from the spirit of salt; and presently making the experiment with the acid only, without any copper, or metal of any kind, this air was immediately produced in as great plenty as before; so that this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt, which appear to be of such a nature, that they are not liable to be condensed by cold, like the vapour of water, and other fluids, and therefore may be very properly called an *acid air*, or more restrictively, the *marine acid air*.

This elastic acid vapour, or acid air, extinguishes flame, and is much heavier than common air; but how much heavier, will not be easy to ascertain. A cylindrical glass vessel, about three fourths of an inch in diameter, and four inches deep, being filled with it, and turned upside down, a lighted candle may be let down into it more than twenty times before it will burn at the bottom. It is pleasing to observe the colour of the flame in this experiment; for both before the candle goes out, and also when it is first lighted again, it burns with a beautiful green, or rather light-blue flame, such as is seen when common salt is thrown into the fire.

When this air is all expelled from any quantity of spirit of salt, which is easily perceived by the subsequent vapour being condensed by cold, the remainder is a very weak acid, barely capable of dissolving iron.

Being now in the possession of a new subject of experiments, viz. an elastic acid vapour, in the form of a permanent air, easily procured, and effectually confined by glass and quicksilver, with which it did not seem to have any affinity; I immediately began to introduce a variety of substances to it; in order to ascertain its peculiar properties and affinities, and also the properties of those other bodies with respect to it.

Beginning with *water*, which, from preceding observations, I knew would imbibe it, and become impregnated with it; I found that 2-1/2 grains of rain-water absorbed three ounce measures of this air, after which it was increased one third in its bulk, and weighed twice as much as before; so that this concentrated vapour seems to be twice as heavy as rain-water: Water impregnated with it makes the strongest spirit of salt that I have seen, dissolving iron with the most rapidity. Consequently, two thirds of the best spirit of salt is nothing more than mere phlegm or water.

Iron filings, being admitted to this air, were dissolved by it pretty fast, half of the air disappearing, and the other half becoming inflammable air, not absorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many substances to this air, before I discovered that it had an affinity with *phlogiston*, so that it would deprive other substances of it, and form with it such an union as constitutes inflammable air; which seems to shew, that inflammable air universally consists of the union of some acid vapour with phlogiston.

Inflammable air was produced, when to this acid air I put spirit of wine, oil of olives, oil of turpentine, charcoal, phosphorus, bees-wax, and even sulphur. This last observation, I own, surprized me; for, the marine acid being reckoned the weakest of the three mineral acids, I did not think that it had been capable of dislodging the oil of vitriol from this substance; but I found that it had the very same effect both upon alum and nitre; the vitriolic acid in the former case, and the nitrous in the latter, giving place to the stronger vapour of spirit of salt.

The rust of iron, and the precipitate of nitrous air made from copper, also imbibed this air very fast, and the little that remained of it was inflammable air; which proves, that these calces contain phlogiston. It seems also to be pretty evident, from this experiment, that the precipitate above mentioned is a real calx of the metal, by the solution of which the nitrous air is generated.

As some remarkable circumstances attend the absorption of this acid air, by the substances above-mentioned, I shall briefly mention them.

Spirit of wine absorbs this air as readily as water itself, and is increased in bulk by that means. Also, when it is saturated, it dissolves iron with as much rapidity, and still continues inflammable.

Oil of olives absorbs this air very slowly, and at the same time, it turns almost black, and becomes glutinous. It is also less miscible with water, and acquires a very disagreeable smell. By continuing upon the surface of the water, it became white, and its offensive smell went off in a few days.

Oil of turpentine absorbed this air very fast, turning brown, and almost black. No inflammable air was formed, till I raised more of the acid air than the oil was able to absorb, and let it stand a considerable time; and still the air was but weakly inflammable. The same was the case with the oil of olives, in the last mentioned experiment; and it seems to be probable, that, the longer this acid air had continued in contact with the oil, the more phlogiston it would have extracted from it. It is not wholly improbable, but that, in the intermediate state, before it becomes inflammable air, it may be nearly of the nature of common air.

Bees-wax absorbed this air very slowly. About the bigness of a hazel-nut of the wax being put to three ounce measures of the acid air, the air was diminished one half in two days, and, upon the admission of water, half of the remainder also disappeared. This air was strongly inflammable.

Charcoal absorbed this air very fast. About one fourth of it was rendered immiscible in water, and was but weakly inflammable.

A small bit of *phosphorus*, perhaps about half a grain, smoked, and gave light in the acid air, just as it would have done in common air confined. It was not sensibly wasted after continuing about twelve hours in that state, and the bulk of the air was very little diminished. Water being admitted to it absorbed it as before, except about one fifth of the whole. It was but weakly inflammable.

Putting several pieces of *sulphur* to this air, it was absorbed but slowly. In about twenty-four hours about one fifth of the quantity had disappeared; and water being admitted to the remainder, very little more was absorbed. The remainder was inflammable, and burned with a blue flame.

Notwithstanding the affinity which this acid air appears to have with phlogiston, it is not capable of depriving all bodies of it. I found that dry wood, crusts of bread, and raw flesh, very readily imbibed this air, but did not part with any of their phlogiston to it. All these substances turned very brown, after they had been some time exposed to this air, and tasted very strongly of the acid when they were taken out; but the flesh, when washed in water, became very white, and the fibres easily separated from one another, even more than they would have done if it had been boiled or roasted[9].

When I put a piece of *saltpetre* to this air it was presently surrounded with a white fume, which soon filled the whole vessel, exactly like the fume which bursts from the bubbles of nitrous air, when it is generated by a vigorous fermentation, and such as is seen when nitrous air is mixed with this acid air. In about a minute, the whole quantity of air was absorbed, except a very little, which might be the common air that had lodged upon the surface of the spirit of salt within the phial.

A piece of *alum* exposed to this air turned yellow, absorbed it as fast as the saltpetre had done, and was reduced by it to the form of a powder. Common salt, as might be expected, had no effect whatever on this marine acid air.

I had also imagined, that if air diminished by the processes above-mentioned was affected in this manner, in consequence of its being saturated with phlogiston, a mixture of this acid air might imbibe that phlogiston, and render it wholesome again; but I put about one fourth of this air to a quantity of air in which metals had been calcined, without making any sensible alteration in it. I do not, however, infer from this, that air is not diminished by means of phlogiston, since the common air, like some other substances, may hold the phlogiston too fast, to be deprived of it by this acid air.

I shall conclude my account of these experiments with observing, that the electric spark is visible in acid air, exactly as it is in common air; and though I kept making this spark a considerable time in a quantity of it, I did not perceive that any sensible alteration was made in it. A little inflammable air was produced, but not more than might have come from the two iron nails which I made use of in taking the sparks.

FOOTNOTES:

[9] It will be seen, in the second part of this work, that, in some of these processes, I had afterwards more success.

SECTION X.

Miscellaneous Observations.

1. As many of the preceding observations relate to the *vinous* and *putrefactive* fermentations, I had the curiosity to endeavour to ascertain in what manner the air would be affected by the *acetous* fermentation. For this purpose I inclosed a phial full of small beer in a jar standing in water; and observed that, during the first two or three days, there was an increase of the air in the jar, but from that time it gradually decreased, till at length there appeared to be a diminution of about one tenth of the whole quantity.

During this time the whole surface of it was gradually covered with a scum, beautifully corrugated. After this there was an increase of the air till there was more than the original quantity; but this must have been fixed air, not incorporated with the rest of the mass; for, withdrawing the beer, which I found to be sour, after it had stood 18 or 20 days under the jar, and passing the air several times through cold water, the original quantity was diminished about one ninth. In the remainder a candle would not burn, and a mouse would have died presently.

The smell of this air was exceedingly pungent, but different from that of the putrid effluvium. A mouse lived perfectly well in this air, thus affected with the acetous fermentation; after it had stood several days mixed with four times the quantity of fixed air.

2. All the kinds of factitious air on which I have yet made the experiment are highly noxious, except that which is extracted from saltpetre, or alum; but in this even a candle burned just as in common air[10]. In one quantity which I got from saltpetre a candle not only burned, but the flame was increased, and something was heard like a hissing, similar to the decrepitation of nitre in an open fire. This experiment was made when the air was fresh made, and while it probably contained some particles of nitre, which would have been deposited afterwards. The air was extracted from these substances by heating them in a gun-barrel, which was much corroded

and soon spoiled by the experiment. What effect this circumstance may have had upon the air I have not considered.

November 6, 1772, I had the curiosity to examine the state of a quantity of this air which had been extracted from saltpetre above a year, and which at first was perfectly wholesome; when, to my very great surprize, I found that it was become, in the highest degree, noxious. It made no effervescence with nitrous air, and a mouse died the moment it was put into it. I had not, however, washed it in rain-water quite ten minutes (and perhaps less time would have been sufficient) when I found, upon trial, that it was restored to its former perfectly wholesome state. It effervesced with nitrous air as much as the best common air ever does; and even a candle burned in it very well, which I had never before observed of any kind of noxious air meliorated by agitation in water. This series of facts, relating to air extracted from nitre, appear to me to be very extraordinary and important, and, in able hands, may lead to considerable discoveries.

3. There are many substances which impregnate common air in a very remarkable manner, but without making it noxious to animals. Among other things I tried volatile alkaline salts, and camphor; the latter of which I melted with a burning-glass, in air inclosed in a phial. The mouse, which was put into this air, sneezed and coughed very much, especially after it was taken out; but it presently recovered, and did not appear to have been sensibly injured.

4. Having made several experiments with a mixture of iron filings and brimstone, kneaded to a paste with water, I had the curiosity to try what would be the effect of substituting *brass dust* in the place of the iron filings. The result was, that when this mixture had stood about three weeks, in a given quantity of air, it had turned black, but was not increased in bulk. The air also was neither sensibly increased nor decreased, but the nature of it was changed; for it extinguished flame, it would have killed a mouse presently, and was not restored by fixed air, which had been mixed with it several days.

5. I have frequently mentioned my having, at one time, exposed equal quantities of different kinds of air in jars standing in boiled water. *Common air* in this experiment was diminished four sevenths, and the remainder

extinguished flame. This experiment demonstrates that water does not absorb air equally, but that it decomposes it, taking one part, and leaving the rest. To be quite sure of this fact, I agitated a quantity of common air in boiled water, and when I had reduced it from eleven ounce measures to seven, I found that it extinguished a candle, but a mouse lived in it very well. At another time a candle barely went out when the air was diminished one third, and at other times I have found this effect lake place at other very different degrees of diminution.

This difference I attribute to the differences in the state of the water with respect to the air contained in it; for sometimes it had stood longer than at other times before I made use of it. I also used distilled-water, rain-water, and water out of which the air had been pumped, promiscuously with rain water. I even doubt, not but that, in a certain state of the water, there might be no sensible difference in the bulk of the agitated air, and yet at the end of the process it would extinguish a candle, air being supplied from the water in the place of that part of the common air which had been absorbed.

It is certainly a little extraordinary that the very same process should so far mend putrid air, as to reduce it to the standard of air in which candles have burned out; and yet that it should so far injure common and wholesome air as to reduce it to about the same standard: but so the fact certainly is. If air extinguish flame in consequence of its being previously saturated with phlogiston, it must, in this case, have been transferred from the water to the air, and it is by no means inconsistent with this hypothesis to suppose, that, if the air be over saturated with phlogiston, the water will imbibe it, till it be reduced to the same proportion that agitation in water would have communicated to it.

To a quantity of common air, thus diminished by agitation in water, till it extinguished a candle, I put a plant, but it did not so far restore it as that a candle would burn in it again; which to me appeared not a little extraordinary, as it did not seem to be in a worse state than air in which candles had burned out, and which had never failed to be restored by the same means.

I had no better success with a quantity of permanent air which I had collected from my pump-water. Indeed these experiments were begun before I was acquainted with that property of nitrous air, which makes it so accurate a measure of the goodness of other kinds of air; and it might perhaps be rather too late in the year when I made the experiments. Having neglected these two jars of air, the plants died and putrefied in both of them; and then I found the air in them both to be highly noxious, and to make no effervescence with nitrous air.

I found that a pint of my pump-water contained about one fourth of an ounce measure of air, one half of which was afterwards absorbed by standing in fresh pump-water. A candle would not burn in this air, but a mouse lived in it very well. Upon the whole, it seemed to be in about the same state as air in which a candle had burned out.

6. I once imagined that, by mere *stagnation*, air might become unfit for respiration, or at least the burning of candles; but if this be the case, and the change be produced gradually, it must require a long time for the purpose. For on the 22d of September 1772, I examined a quantity of common air, which had been kept in a phial, without agitation, from May 1771, and found it to be in no respect worse than fresh air, even by the test of the nitrous air.

7. The crystallization of nitre makes no sensible alteration in the air in which the process is made. For this purpose I dissolved as much nitre as a quantity of hot water would contain, and let it cool under a receiver, standing in water.

8. November 6, 1772, a quantity of inflammable air, which, by long keeping, had come to extinguish flame, I observed to smell very much like common air in which a mixture of iron filings and brimstone had stood. It was not, however, quite so strong, but it was equally noxious.

9. Bismuth and nickel are dissolved in the marine acid with the application of a considerable degree of heat; but little or no air is got from either of them; but, what I thought a little remarkable, both of them smelled very much like Harrowgate water, or liver of sulphur. This smell I have met with several times in the course of my experiments, and in processes very different from one another.

FOOTNOTES:

[10] Experiments, of which an account will be given in the second part of this work, make it probable, that though a candle burned even *more than well* in this air, an animal would not have lived in it. At the time of this first publication, however, I had no idea of this being possible in nature.

PART II.

Experiments and Observations made in the Year 1773, and the Beginning of 1774.

SECTION I.

Observations on ALKALINE AIR.

After I had made the discovery of the *marine acid air*, which the vapour of spirit of salt may properly enough be called, and had made those experiments upon it, of which I have given an account in the former part of this work, and others which I propose to recite in this part; it occurred to me, that, by a process similar to that by which this *acid* air is expelled from the spirit of salt, an *alkaline* air might be expelled from substances containing volatile alkali.

Accordingly I procured some volatile spirit of sal ammoniac, and having put it into a thin phial, and heated it with the flame of a candle, I presently found that a great quantity of vapour was discharged from it; and being received in a vessel of quicksilver, standing in a bason of quicksilver, it continued in the form of a transparent and permanent air, not at all condensed by cold; so that I had the same opportunity of making experiments upon it, as I had before on the acid air, being in the same favourable circumstances.

With the same ease I also procured this air from *spirit of hartshorn*, and *sal volatile* either in a fluid or solid form, i. e. from those volatile alkaline salts which are produced by the distillation of sal ammoniac with fixed alkalis. But in this case I soon found that the alkaline air I procured was not pure; for the fixed air, which entered into the composition of my materials, was expelled along with it. Also, uniting again with the alkaline air, in the glass tube through which they were conveyed, they stopped it up, and were often the means of bursting my vessels.

While these experiments were new to me, I imagined that I was able to procure this air with peculiar advantage and in the greatest abundance, either from the salts in a dry state, when they were just covered with water, or in a perfectly fluid state; for, upon applying a candle to the phials in which they were contained, there was a most astonishing production of air;

but having examined it, I found it to be chiefly fixed air, especially after the first or second produce from the same materials; and removing my apparatus to a trough of water and using the water instead of quicksilver, I found that it was not presently absorbed by it.

This, however, appears to be an easy and elegant method of procuring fixed air, from a small quantity of materials, though there must be a mixture of alkaline air along with it; as it is by means of its combination with this principle only, that it is possible, that so much fixed air should be retained in any liquid. Water, at least, we know, cannot be made to contain much more than its own bulk of fixed air.

After this disappointment, I confined myself to the use of that volatile spirit of sal ammoniac which is procured by a distillation with slaked lime, which contains no fixed air; and which seems, in a general state, to contain about as much alkaline air, as an equal quantity of spirit of salt contains of the acid air.

Wanting, however, to procure this air in greater quantities, and this method being rather expensive, it occurred to me, that alkaline air might, probably, be procured, with the most ease and convenience, from the original materials, mixed in the same proportions that chemists had found by experience to answer the best for the production of the volatile spirit of sal ammoniac. Accordingly I mixed one fourth of pounded sal ammoniac, with three fourths of slaked lime; and filling a phial with the mixture, I presently found it completely answered my purpose. The heat of a candle expelled from this mixture a prodigious quantity of alkaline air; and the same materials (as much as filled an ounce phial) would serve me a considerable time, without changing; especially when, instead of a glass phial, I made use of a small iron tube, which I find much more convenient for the purpose.

As water soon begins to rise in this process, it is necessary, if the air is intended to be conveyed perfectly *dry* into the vessel of quicksilver, to have a small vessel in which this water (which is the common volatile spirit of sal ammoniac) may be received. This small vessel must be interposed between the vessel which contains the materials for the generation of the air, and that in which it is to be received, as *d* fig. 8.

This *alkaline* air being perfectly analogous to the *acid* air, I was naturally led to investigate the properties of it in the same manner, and nearly in the same order. From this analogy I concluded, as I presently found to be the fact, that this alkaline air would be readily imbibed by water, and, by its union with it, would form a volatile spirit of sal ammoniac. And as the water, when admitted to the air in this manner, confined by quicksilver, has an opportunity of fully saturating itself with the alkaline vapour, it is made prodigiously stronger than any volatile spirit of sal ammoniac that I have ever seen; and I believe stronger than it can be made in the common way.

In order to ascertain what addition, with respect to quantity and weight, water would acquire by being saturated with alkaline air, I put 1-1/4 grains of rain-water into a small glass tube, closed at one end with cement, and open at the other, the column of water measuring 7/10 of an inch; and having introduced it through the quicksilver into a vessel containing alkaline air, observed that it absorbed 7/8 of an ounce measure of the *air*, and had then gained about half a grain in weight, and was increased to 8-1/2 tenths of an inch in length. I did not make a second experiment of this kind, and therefore will not answer for the exactness of these proportions in future trials. What I did sufficiently answered my purpose, in a general view of the subject.

When I had, at one time, saturated a quantity of distilled water with alkaline air, so that a good deal of the air remained unabsorbed on the surface of the water, I observed that, as I continued to throw up more air, a considerable proportion of it was imbibed, but not the whole; and when I had let the apparatus stand a day, much more of the air that lay on the surface was imbibed. And after the water would imbibe no more of the *old* air, it imbibed *new*. This shews that water requires a considerable time to saturate itself with this kind of air, and that part of it more readily unites with water than the rest.

The same is also, probably, the case with all the kinds of air with which water can be impregnated. Mr. Cavendish made this observation with respect to fixed air, and I repeated the whole process above-mentioned with acid air, and had precisely the same result. The alkaline water which I procured in this experiment was, beyond comparison, stronger to the smell, than any spirit of sal ammoniac that I had seen.

This experiment led me to attempt the making of spirit of sal ammoniac in a larger quantity, by impregnating distilled water with this alkaline air. For this purpose I filled a piece of a gun-barrel with the materials above-mentioned, and luted to the open end of it a small glass tube, one end of which was bent, and put within the mouth of a glass vessel, containing a quantity of distilled water upon quicksilver, standing in a bason of quicksilver, as in fig. 7. In these circumstances the heat of the fire, applied gradually, expelled the alkaline air, which, passing through the tube, and the quicksilver, came at last to the water, which, in time, became fully saturated with it.

By this means I got a very strong alkaline liquor, from which I could again expel the alkaline air which I had put into it, whenever it happened to be more convenient to me to get it in that manner. This process may easily be performed in a still larger way; and by this means a liquor of the same nature with the volatile spirit of sal ammoniac, might be made much stronger, and much cheaper, than it is now made.

Having satisfied myself with respect to the relation that alkaline air bears to water, I was impatient to find what would be the consequence of mixing this new air with the other kinds with which I was acquainted before, and especially with *acid* air; having a notion that these two airs, being of opposite natures, might compose a *neutral air*, and perhaps the very same thing with common air. But the moment that these two kinds of air came into contact, a beautiful white cloud was formed, and presently filled the whole vessel in which they were contained. At the same time the quantity of air began to diminish, and, at length, when the cloud was subsided, there appeared to be formed a solid *while salt*, which was found to be the common *sal ammoniac*, or the marine acid united to the volatile alkali.

The first quantity that I produced immediately deliquesced, upon being exposed to the common air; but if it was exposed in a very dry and warm place, it almost all evaporated, in a white cloud. I have, however, since, from the same materials, produced the salt above-mentioned in a state not subject to deliquesce or evaporate. This difference, I find, is owing to the proportion of the two kinds of air in the compound. It is only volatile when there is more than a due proportion of either of the constituent parts. In these cases the smell of the salts is extremely pungent, but very different from one another; being manifestly acid, or alkaline, according to the prevalence of each of these airs respectively.

Nitrous air admitted to alkaline air likewise occasioned a whitish cloud, and part of the air was absorbed; but it presently grew clear again; leaving only a little dimness on the sides of the vessel. This, however, might be a kind of salt, formed by the union of the two kinds of air. There was no other salt formed that I could perceive. Water being admitted to this mixture of nitrous and alkaline air presently absorbed the latter, and left the former possessed of its peculiar properties.

Fixed air admitted to alkaline air formed oblong and slender crystals, which crossed one another, and covered the sides of the vessel in the form of net-work. These crystals must be the same thing with the volatile alkalis which chemists get in a solid form, by the distillation of sal ammoniac with fixed alkaline salts.

Inflammable air admitted to alkaline air exhibited no particular appearance. Water, as in the former experiment, absorbed the alkaline air, and left the inflammable air as it was before. It was remarkable, however, that the water which was admitted to them became whitish, and that this white cloud settled, in the form of a white powder, to the bottom of the vessel.

Alkaline air mixed with *common air*, and standing together several days, first in quicksilver, and then in water (which absorbed the alkaline air) it did not appear that there was any change produced in the common air: at least it was as much diminished by nitrous air as before. The same was the case with a mixture of acid air and common air.

Having mixed air that had been diminished by the fermentation of a mixture of iron filings and brimstone with alkaline air, the water absorbed the latter, but left the former, with respect to the test of nitrous air (and therefore, as I conclude, with respect to all its properties) the same that it was before.

Spirit of wine imbibes alkaline air as readily as water, and seems to be as inflammable afterwards as before.

Alkaline air contracts no union with *olive oil*. They were in contact almost two days, without any diminution of the air. Oil of turpentine, and essential oil of mint, absorbed a very small quantity of alkaline air, but were not sensibly changed by it.

Ether, however, imbibed alkaline air pretty freely; but it was afterwards as inflammable as before, and the colour was not changed. It also evaporated as before, but I did not attend to this last circumstance very accurately.

Sulphur, *nitre*, *common salt*, and *flints*, were put to alkaline air without imbibing any part of it; but *charcoal*, *spunge*, bits of *linen cloth*, and other substances of that nature, seemed to condense this air upon their surfaces; for it began to diminish immediately upon their being put to it; and when they were taken out the alkaline smell they had contracted was so pungent as to be almost intolerable, especially that of the spunge. Perhaps it might be of use to recover persons from swooning. A bit of spunge, about as big as a hazel nut, presently imbibed an ounce measure of alkaline air.

A piece of the inspissated juice of *turnsole* was made very dry and warm, and yet it imbibed a great quantity of the air; by which it contracted a most pungent smell, but the colour of it was not changed.

Alum undergoes a very remarkable change by the action of alkaline air. The outward shape and size remain the same, but the internal structure is quite changed, becoming opaque, beautifully white, and, to appearance, in all respects, like alum which had been roasted; and so as not to be at all affected by a degree of heat that would have reduced it to that state by roasting. This effect is produced slowly; and if a piece of alum be taken out of alkaline air before the operation is over, the inside will be transparent, and the outside, to an equal thickness, will be a white crust.

I imagine that the alkaline vapour seizes upon the water that enters into the constitution of crude alum, and which would have been expelled by heat. Roasted alum also imbibes alkaline air, and, like the raw alum that has been exposed to it, acquires a taste that is peculiarly disagreeable.

Phosphorus gave no light in alkaline air, and made no lasting change in its dimensions. It varied, indeed, a little, being sometimes increased and sometimes diminished, but after a day and a night, it was in the same state as at the first. Water absorbed this air just as if nothing had been put to it.

Having put some *spirit of salt* to alkaline air, the air was presently absorbed, and a little of the white salt above-mentioned was formed. A little remained unabsorbed, and transparent, but upon the admission of common air to it, it instantly became white.

Oil of vitriol, also formed a white salt with alkaline air, and this did not rise in white fumes.

Acid air, as I have observed in my former papers, extinguishes a candle. Alkaline air, on the contrary, I was surprized to find, is slightly inflammable; which, however, seems to confirm the opinion of chemists, that the volatile alkali contains phlogiston.

I dipped a lighted candle into a tall cylindrical vessel, filled with alkaline air, when it went out three or four times successively; but at each time the flame was considerably enlarged, by the addition of another flame, of a pale yellow colour; and at the last time this light flame descended from the top of the vessel to the bottom. At another time, upon presenting a lighted candle to the mouth of the same vessel, filled with the same kind of air, the yellowish flame ascended two inches higher than the flame of the candle. The electric spark taken in alkaline air is red, as it is in common inflammable air.

Though alkaline air be inflammable, it appeared, by the following experiment, to be heavier than the common inflammable air, as well as to contract no union with it. Into a vessel containing a quantity of inflammable air, I put half as much alkaline air, and then about the same quantity of acid air. These immediately formed a white cloud, but it did not rise within the space that was occupied by the inflammable air; so that this latter had kept its place above the alkaline air, and had not mixed with it.

That alkaline air is lighter than acid air is evident from the appearances that attend the mixture, which are indeed very beautiful. When acid air is introduced into a vessel containing alkaline air, the white cloud which they form appears at the bottom only, and ascends gradually. But when the alkaline air is put to the acid, the whole becomes immediately cloudy, quite to the top of the vessel.

In the last place, I shall observe that alkaline air, as well as acid, dissolves *ice* as fast as a hot fire can do it. This was tried when both the kinds of air, and every instrument made use of in the experiment, had been exposed to a pretty intense frost several hours. In both cases, also, the water into which the ice was melted dissolved more ice, to a considerable quantity.

SECTION II.

Of COMMON AIR diminished and made noxious by various processes.

It will have been observed that, in the first publication of my papers, I confined myself chiefly to the narration of the new *facts* which I had discovered, barely mentioning any *hypotheses* that occurred to me, and never seeming to lay much stress upon them. The reason why I was so much upon my guard in this respect was, left, in consequence of attaching myself to any hypothesis too soon, the success of my future inquiries might be obstructed. But subsequent experiments having thrown great light upon the preceding ones and having confirmed the few conjectures I then advanced, I may now venture to speak of my hypotheses with a little less diffidence. Still, however, I shall be ready to relinquish any notions I may now entertain, if new facts should hereafter appear not to favour them.

In a great variety of cases I have observed that there is a remarkable *diminution* of common, or respirable air, in proportion to which it is always rendered unfit for respiration, indisposed to effervesce with nitrous air, and incapable of farther diminution from any other cause. The circumstances which produce this effect I had then observed to be the burning of candles, the respiration of animals, the putrefaction of vegetables or animal substances, the effervescence of iron filings and brimstone, the calcination of metals, the fumes of charcoal, the effluvia of paint made of white-lead and oil, and a mixture of nitrous air.

All these processes, I observed, agree in this one circumstance, and I believe in no other, that the principle which the chemists call *phlogiston* is set loose; and therefore I concluded that the diminution of the air was, in some way or other, the consequence of the air becoming overcharged with phlogiston,[11] and that water, and growing vegetables, tend to restore this air to a state fit for respiration, by imbibing the superfluous phlogiston. Several experiments which I have since made tend to confirm this supposition.

Common air, I find, is diminished, and rendered noxious, by *liver of sulphur*, which the chemists say exhales phlogiston, and nothing else. The diminution in this case was one fifth of the whole, and afterwards, as in other similar cases, it made no effervescence with nitrous air.

I found also, after Dr. Hales, that air is diminished by *Homberg's pyrophorus*.

The same effect is produced by firing *gunpowder* in air. This I tried by firing the gunpowder in a receiver half exhausted, by which the air was rather more injured than it would have been by candles burning in it.

Air is diminished by a cement made with one half common coarse turpentine and half bees-wax. This was the result of a very casual observation. Having, in an air-pump of Mr. Smeaton's construction, closed that end of the syphon-gage, which is exposed to the outward air, with this cement (which I knew would make it perfectly air-light) instead of sealing it hermetically; I observed that, in a course of time, the quicksilver in that leg kept continually rising, so that the measures I marked upon it were of no use to me; and when I opened that end of the tube, and closed it again, the same consequence always took place. At length, suspecting that this effect must have arisen from the bit of *cement* diminishing the air to which it was exposed, I covered all the inside of a glass tube with it, and one end of it being quite closed with the cement, I set it perpendicular, with its open end immersed in a bason of quicksilver; and was presently satisfied that my conjecture was well founded: for, in a few days, the quicksilver rose so much within the tube, that the air in the inside appeared to be diminished about one sixth.

To change this air I filled the tube with quicksilver, and pouring it out again, I replaced the tube in its former situation; when the air was diminished again, but not so fast as before. The same lining of cement diminished the air a third time. How long it will retain this power I cannot tell. This cement had been made several months before I made this experiment with it. I must observe, however, that another quantity of this kind of cement, made with a finer and more liquid turpentine, had not the power of diminishing air, except in a very small proportion. Also the common red cement has this property in the same small degree. Common air, however, which had been confined in a glass vessel lined with this cement about a month, was so far injured that a candle would not burn in it. In a longer time it would, I doubt not, have become thoroughly noxious.

Iron that has been suffered to rust in nitrous air diminishes common air very fast, as I shall have occasion to mention when I give a continuation of my experiments on nitrous air.

Lastly, the same effect, I find, is produced by the *electric spark*, though I had no expectation of this event when I made the experiment.

This experiment, however, and those which I have made in pursuance of it, has fully confirmed another of my conjectures, which relates to the *manner* in which air is diminished by being overcharged with phlogiston, viz. the phlogiston having a nearer affinity with some of the constituent parts of the air than the fixed air which enters into the composition of it, in consequence of which the fixed air is precipitated.

This I first imagined from perceiving that lime-water became turbid by burning candles over it, p. 44. This was also the case with lime-water confined in air in which an animal substance was putrefying, or in which an animal died, p. 79. and that in which charcoal was burned, p. 81. But, in all these cases, there was a possibility of the fixed air being discharged from the candle, the putrefying substance, the lungs of the animal, or the charcoal. That there is a precipitation of lime when nitrous air is mixed with common air, I had not then observed, but I have since found it to be the case.

That there was no precipitation of lime when brimstone was burned, I observed, p. 45. might be owing to the fixed air and the lime uniting with the vitriolic acid, and making a salt, which was soluble in water; which salt I, indeed, discovered by the evaporation of the water.

I also observed, p. 46, 105. that diminished air being rather lighter than common air is a circumstance in favour of the fixed, or the heavier part of the common air, having been precipitated.

It was upon this idea, together with others similar to it, that I took so much pains to mix fixed air with air diminished by respiration or putrefaction, in order to make it fit for respiration again; and I thought that I had, in general, succeeded to a considerable degree, p. 99, &c. I will add, also, what I did not mention before, that I once endeavoured, but without effect, to preserve mice alive in the same unchanged air, by supplying them with fixed air, when the air in which they were confined began to be injured by their respiration. Without effect, also, I confined for some months, a quantity of quick lime in a given quantity of common air, thinking it might extract the fixed air from it.

The experiments which I made with electricity were solely intended to ascertain what has often been attempted, but, as far as I know, had never been fully accomplished, viz. to change the blue colour of liquors, tinged with vegetable juices, red.

For this purpose I made use of a glass tube, about one tenth of an inch diameter in the inside, as in fig. 16. In one end of this I cemented a piece of wire *b*, on which I put a brass ball. The lower part from *a* was filled with water tinged blue, or rather purple, with the juice of turnsole, or archil. This is easily done by an air-pump, the tube being set in a vessel of the tinged water.

Things being thus prepared, I perceived that, after I had taken the electric spark, between the wire *b*, and the liquor at *a*, about a minute, the upper part of it began to look red, and in about two minutes it was very manifestly so; and the red part, which was about a quarter of an inch in length, did not readily mix with the rest of the liquor. I observed also, that if the tube lay inclined while I took the sparks, the redness extended twice as far on the lower side as on the upper.

The most important, though the least expected observation, however, was that, in proportion as the liquor became red, it advanced nearer to the wire, so that the space of air in which the sparks were taken was diminished; and at length I found that the diminution was about one fifth of the whole space; after which more electrifying produced no sensible effect.

To determine whether the cause of the change of colour was in the *air*, or in the *electric matter*, I expanded the air which had been diminished in the tube by means of an air-pump, till it expelled all the liquor, and admitted fresh blue liquor into its place; but after that, electricity produced no sensible effect, either on the air, or on the liquor; so that it was evident that the electric matter had decomposed the air, and had made it deposite something that was of an acid nature.

In order to determine whether the *wire* had contributed any thing to this effect, I used wires of different metals, iron, copper, brass, and silver; but the result was the very same with them all.

It was also the same when, by means of a bent glass tube, I made the electric spark without any wire at all, in the following manner. Each leg of the tube, fig. 19. stood in a bason of quicksilver; which, by means of an air-pump, was made to ascend as high as *a, a*, in each leg, while the space between *a* and *b* in each contained the blue liquor, and the space between *b* and *b* contained common air. Things being thus disposed, I made the electric spark perform the circuit from one leg to the other, passing from the liquor in one leg of the tube to the liquor in the other leg, through the space of air. The effect was, that the liquor, in both the legs, became red, and the space of air between them was contracted, as before.

Air thus diminished by electricity makes no effervescence with, and is no farther diminished by a mixture of nitrous air; so that it must have been in the highest degree noxious, exactly like air diminished by any other process.

In order to determine what the *acid* was, which was deposited by the air, and which changed the colour of the blue liquor, I exposed a small quantity of the liquor so changed to the common air, and found that it recovered its blue colour, exactly as water, tinged with the same blue, and impregnated with fixed air, will do. But the following experiment was still more decisive to this purpose. Taking the electric spark upon *lime-water*, instead of the blue liquor, the lime was precipitated as the air diminished.

From these experiments it pretty clearly follows, that the electric matter either is, or contains phlogiston; since it does the very same thing that phlogiston does. It is also probable, from these experiments, that the sulphureous smell, which is occasioned by electricity, being very different from that of fixed air, the phlogiston in the electric matter itself may contribute to it.

It was now evident that common air diminished by any one of the processes above-mentioned being the same thing, as I have observed, with air diminished by any other of them (since it is not liable to be farther diminished by any other) the loss which it sustains, in all the cases, is, in part, that of the *fixed air* which entered into its constitution. The fixed air thus precipitated from common air by means of phlogiston unites with lime, if any lime water be ready to receive it, unless there be some other substance at hand, with which it has a greater affinity, as the *calces of metals*.

If the whole of the diminution of common air was produced by the deposition of fixed air, it would be easy to ascertain the quantity of fixed air that is contained in any given quantity of common air. But it is evident that the whole of the diminution of common air by phlogiston is not owing to the precipitation of fixed air, because a mixture of nitrous air will make a great diminution in all kinds of air that are fit for respiration, even though they never were common air, and though nothing was used in the process for generating them that can be supposed to yield fixed air.

Indeed, it appears, from some of the experiments, that the diminution of some of these kinds of air by nitrous air is so great, and approaches so nearly to the quantity of the diminution of common air by the same process, as to shew that, unless they be very differently affected by phlogiston, very little is to be allowed to the loss of fixed air in the diminution of common air by nitrous air.

The kinds of air on which this experiment was made were inflammable air, nitrous air diminished by iron filings and brimstone, and nitrous air itself; all of which are produced by the solution of metals in acids; and also on common air diminished and made noxious, and therefore deprived of its fixed air by phlogistic processes; and they were restored to a great degree of purity by agitation in water, out of which its own air had been carefully boiled.

To five parts of inflammable air, which had been agitated in water till it was diminished about one half (at which time part of it fired with a weak explosion) I put one part of nitrous air, which diminished it one eighth of the whole. This was done in lime-water, without any precipitation of lime.

To compare this with common air, I mixed the same quantity, viz. five parts of this, and one part of nitrous air: when considerable crust of lime was formed upon the surface of the lime water, though the diminution was very little more than in the former process. It is possible, however, that the common air might have taken more nitrous air before it was fully saturated, so as to begin to receive an addition to its bulk.

I agitated in water a quantity of nitrous air phlogisticated with iron filings and brimstone, and found it to be so far restored, that three fourths of an ounce measure of nitrous air being put to two ounce measures of it, made no addition to it.

But the most remarkable of these experiments is that which I made with *nitrous air* itself which I had no idea of the possibility of reducing to a state fit for respiration by any process whatever, at the time of my former publication on this subject. This air, however, itself, without any previous phlogistication, is purified by agitation in water till it is diminished by fresh nitrous air, and to a very considerable degree.

In a pretty long time I agitated nitrous air in water, supplying it from time to time with more, as the former quantity diminished, till only one eighteenth of the whole quantity remained; in which state it was so wholesome, that a mouse lived in two ounce measures of it more than ten minutes, without shewing any sign of uneasiness; so that I concluded it must have been about as good as air in which candles had burned out. After agitating it again in water, I put one part of fresh nitrous air to five parts of this air, and it was diminished one ninth part. I then agitated it a third time, and putting more nitrous air to it, it was diminished again in the same proportion, and so a fourth time; so that, by continually repeating the process, it would, I doubt not, have been all absorbed. These processes were made in lime-water, without forming any incrustation on the surface of it.

Lastly, I took a quantity of common air, which had been diminished and made noxious by phlogistic processes; and when it had been agitated in water, I found that it was diminished by nitrous air, though not so much as it would have been at the first. After cleansing it a second time, it was diminished again by the same means; and, after that, a third time; and thus there can be no doubt but that, in time, the whole quantity would have disappeared. For I have never found that agitation in water, deprived of its own air, made any addition to a quantity of noxious air; though, *a priori*, it might have been imagined that, as a saturation with phlogiston diminishes air, the extraction of phlogiston would increase the bulk of it. On the contrary, agitation in water always diminished noxious air a little; indeed, if water be deprived of all its own air, it is impossible to agitate any kind of air in it without some loss. Also, when noxious air has been restored by plants, I never perceived that it gained any addition to its bulk by that means. There was no incrustation of the lime-water in the above-mentioned experiment.

It is not a little remarkable, that those kinds of air which never had been common air, as inflammable air, phlogisticated nitrous air, and nitrous air itself, when rendered wholesome by agitation in water, should be more diminished by fresh nitrous air, than common air which had been made noxious, and restored by the same process; and yet, from the few trials that I have made, I could not help concluding that this is the case.

In this course of experiments I was very near deceiving myself, in consequence of transferring the nitrous air which I made use of in a bladder, in the manner described, p. 15. fig. 9. so as to conclude that there was a precipitation of lime in all the above-mentioned cases, and that even nitrous air itself produced that effect. But after repeated trials, I found that there was no precipitation of lime, except, in the first diminution of common air, when the nitrous air was transferred in a glass vessel.

That the calces of metals contain air, of some kind or other, and that this air contributes to the additional weight of the calces, above that of the metals from which they are made, had been observed by Dr. Hales; and Mr. Hartley had informed me, that when red-lead is boiled in linseed oil, there is a prodigious discharge of air before they incorporate. I had likewise found, that no weight is either gained or lost by the calcination of tin in a close glass vessel; but I purposely deferred making any more experiments on the subject, till we should have some weather in which I could make use of a large burning lens, which I had provided for that and other purposes; but, in the mean time, I was led to the discovery in a different manner.

Having, by the last-recited experiments, been led to consider the electric matter as phlogiston, or something containing phlogiston, I was endeavouring to revivify the calx of lead with it; when I was surprized to perceive a considerable generation of air. It occurred to me, that possibly this effect might arise from the *heat* communicated to the red-lead by the electric sparks, and therefore I immediately filled a small phial with the red-lead, and heating it with a candle, I presently expelled from it a quantity of air about four or five times the bulk of the lead, the air being received in a vessel of quicksilver. How much more air it would have yielded, I did not try.

Along with the air, a small quantity of *water* was likewise thrown out; and it immediately occurred to me, that this water and air together must certainly be the cause of the addition of weight in the calx. It still remained to examine what kind of air this was; but admitting water to it, I found that it was imbibed by it, exactly like *fixed air*, which I therefore immediately concluded it must be[12].

After this, I found that Mr. Lavoisier had completely discovered the same thing, though his apparatus being more complex, and less accurate than mine, he concluded that more of the air discharged from the calces of metals was immiscible with water than I found it to be. It appeared to me that I had never obtained fixed air more pure.

It being now pretty clearly determined, that common air is made to deposit the fixed air which entered into the constitution of it, by means of phlogiston, in all the cases of diminished air, it will follow, that in the precipitation of lime, by breathing into lime-water the fixed air, which incorporates with lime, comes not from the lungs, but from the common air, decomposed by the phlogiston exhaled from them, and discharged, after having been taken in with the aliment, and having performed its function in the animal system.

Thus my conjecture is more confirmed, that the cause of the death of animals in confined air is not owing to the want of any *pabulum vitæ*, which the air had been supposed to contain, but to the want of a discharge of the phlogistic matter, with which the system was loaded; the air, when once saturated with it, being no sufficient *menstruum* to take it up.

The instantaneous death of animals put into air so vitiated, I still think is owing to some *stimulus*, which, by causing immediate, universal and violent convulsions, exhausts the whole of the *vis vitæ* at once; because, as I have observed, the manner of their death is the very same in all the different kinds of noxious air.

To this section on the subject of diminished, and noxious air, or as it might have been called *phlogisticated air*, I shall subjoin a letter which I addressed to Sir John Pringle, on the noxious quality of the effluvia of putrid marshes, and which was read at a meeting of the Royal Society, December 16, 1773.

This letter which is printed in the Philosophical Transactions, Vol. 74, p. 90. is immediately followed by another paper, to which I would refer my reader. It was written by Dr. Price, who has so greatly distinguished himself, and done such eminent service to his country, and to mankind, by his calculations relating to the probabilities of human life, and was suggested by his hearing this letter read at the Royal Society. It contains a confirmation of my observations on the noxious effects of stagnant waters by deductions from Mr. Muret's account of the Bills of Mortality for a parish situated among marshes, in the district of Vaud, belonging to the Canton of Bern in Switzerland.

To Sir JOHN PRINGLE, Baronet.

DEAR SIR,

Having pursued my experiments on different kinds of air considerably farther, in several respects, than I had done when I presented the last account of them to the Royal Society; and being encouraged by the favourable notice which the Society has been pleased to take of them, I shall continue my communications on this subject; but, without waiting for the result of a variety of processes, which I have now going on, or of other experiments, which I propose to make, I shall, from time to time, communicate such detached articles, as I shall have given the most attention to, and with respect to which, I shall have been the most successful in my inquiries.

Since the publication of my papers, I have read two treatises, written by Dr. Alexander, of Edinburgh, and am exceedingly pleased with the spirit of philosophical inquiry, which they discover. They appear to me to contain many new, curious, and valuable observations; but one of the *conclusions*, which he draws from his experiments, I am satisfied, from my own observations, is ill founded, and from the nature of it, must be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marshes.

I was particularly surprised, to meet with such an opinion as this, in a book inscribed to yourself, who have so clearly explained the great mischief of such a situation, in your excellent treatise *on the diseases of the army*. On this account, I have thought it not improper, to address to you the following observations and experiments, which I think clearly demonstrate the fallacy of Dr. Alexander's reasoning, indisputably establish your doctrine, and indeed justify the apprehensions of all mankind in this case.

I think it probable enough, that putrid matter, as Dr. Alexander has endeavoured to prove, will preserve other substances from putrefaction; because, being already saturated with the putrid effluvium, it cannot readily take any more; but Dr. Alexander was not aware, that air thus loaded with putrid effluvium is exceedingly noxious when taken into the lungs. I have lately, however, had an opportunity of fully ascertaining how very noxious such air is.

Happening to use at Calne, a much larger trough of water, for the purpose of my experiments, than I had done at Leeds, and not having fresh water so near at hand as I had there, I neglected to change it, till it turned black, and became offensive, but by no means to such a degree, as to deter me from making use of it. In this state of the water, I observed bubbles of air to rise from it, and especially in one place, to which some shelves, that I had in it, directed them; and having set an inverted glass vessel to catch them, in a few days I collected, a considerable quantity of this air, which issued spontaneously from the putrid water; and putting nitrous air to it, I found that no change of colour or diminution ensued, so that it must have been, in the highest degree, noxious. I repeated the same experiment several times afterwards, and always with the same result.

After this, I had the curiosity to try how wholesome air would be affected by this water; when, to my real surprise, I found, that after only one minute's agitation in it, a candle would not burn in it; and, after three or four minutes, it was in the same state with the air, which had issued spontaneously from the same water.

I also found, that common air, confined in a glass vessel, in *contact* only with this water, and without any agitation, would not admit a candle to burn in it after two days.

These facts certainly demonstrate, that air which either arises from stagnant and putrid water, or which has been for some time in contact with it, must be very unfit for respiration; and yet Dr. Alexander's opinion is rendered so plausible by his experiments, that it is very possible that many persons may be rendered secure, and thoughtless of danger, in a situation in which they must necessarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. Alexander appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

That air issuing from water, or rather from the soft earth, or mud, at the bottom of pits containing water, is not always unwholesome, I have also had an opportunity of ascertaining. Taking a walk, about two years ago, in the neighbourhood of Wakefield, in Yorkshire, I observed bubbles of air to arise, in remarkably great plenty, from a small pool of water, which, upon inquiry, I was informed had been the place, where some persons had been boring the ground, in order to find coal. These bubbles of air having excited my curiosity, I presently returned, with a bason, and other vessels proper for my purpose, and having stirred the mud with a long stick, I soon got about a pint of this air; and, examining it, found it to be good, common air; at least a candle burned in it very well. I had not then discovered the method of ascertaining the goodness of common air, by a mixture of nitrous air. Previous to the trial, I had suspected that this air would have been found to be inflammable.

I shall conclude this letter with observing, that I have found a remarkable difference in different kinds of water, with respect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitate common air in the water of a deep well, near my house in Calne, which is hard, but clear and sweet, a candle will not burn in it after three minutes. The same is the case with the rain-water, which I get from the roof of my house. But in distilled water, or the water of a spring-well near the house, I must agitate the air about twenty minutes, before it will be so much injured. It may be worth while, to make farther experiments with respect to this property of water.

In consequence of using the rain-water, and the well-water above mentioned, I was very near concluding, contrary to what I have asserted in this treatise, that common air suffers a decomposition by great rarefaction. For when I had collected a considerable quantity of air, which had been rarefied about four hundred times, by an excellent pump made for me by Mr. Smeaton, I always found, that if I filled my receivers with the water above mentioned, though I did it so gradually as to occasion as little agitation as possible, a candle would not burn in the air that remained in them. But when I used distilled water, or fresh spring-water, I undeceived myself.

I think myself honoured by the attention, which, from the first, you have given to my experiments, and am, with the greatest respect,

Dear Sir,

Your most obliged

Humble Servant,

London, 7 Dec. 1773.

J. PRIESTLEY.

POSTSCRIPT.

I cannot help expressing my surprize, that so clear and intelligible an account, of Mr. SMEATON'S air-pump, should have been before the public so long, as ever since the publication of the forty-seventh volume of the Philosophical Transactions, printed in 1752, and yet that none of our philosophical instrument-makers should use the construction. The superiority of this pump, to any that are made upon the common plan, is, indeed, prodigious. Few of them will rarefy more than 100 times, and, in a general way, not more than 60 or 70 times; whereas this instrument must be in a poor state indeed, if it does not rarefy 200 or 300 times; and when it is in good order, it will go as far as 1000 times, and sometimes even much farther than that; besides, this instrument is worked with much more ease, than a common air-pump, and either exhausts or condenses at pleasure. In short, to a person engaged in philosophical pursuits, this instrument is an invaluable acquisition. I shall have occasion to recite some experiments, which I could not have made, and which, indeed, I should hardly have dared to attempt, if I had not been possessed of such an air-pump as this. It is much to be wished, that some person of spirit in the trade would attempt the construction of an instrument, which would do great credit to himself, as well as be of eminent service to philosophy.

FOOTNOTES:

[11] On this account, if it was thought convenient to introduce a new term (or rather make a new application of a term already in use among chemists) it might not be amiss to call air that has been diminished, and made noxious by any of the processes above mentioned, or others similar to them, by the common appellation of *phlogisticated air*; and, if it was necessary, the particular process by which it was phlogisticated might be added; as common air phlogisticated by charcoal, air phlogisticated by the calcination of metals, nitrous air phlogisticated with the liver of sulphur, &c.

[12] Here it becomes me to ask pardon of that excellent philosopher Father Beccaria of Turin, for conjecturing that the phlogiston, with which he revivified metals, did not come from the electric matter itself, but from what was discharged from other pieces of metal with which he made the experiment. See History of Electricity, p. 277, &c. This *revivification of metals* by electricity completes the proof of the electric matter being, or containing phlogiston.

SECTION III.

Of Nitrous Air.

Since the publication of my former papers I have given more attention to the subject of nitrous air than to any other species of air; and having been pretty fortunate in my inquiries, I shall be able to lay before my reader a more satisfactory account of the curious phenomena occasioned by it, and also of its nature and constitution, than I could do before, though much still remains to be investigated concerning it, and many new objects of inquiry are started.

With a view to discover where the power of nitrous air to diminish common air lay, I evaporated to dryness a quantity of the solution of copper in diluted spirit of nitre; and having procured from it a quantity of a *green precipitate*, I threw the focus of a burning-glass upon it, when it was put into a vessel of quicksilver, standing inverted in a bason of quicksilver. In this manner I procured air from it, which appeared to be, in all respects, nitrous air; so that part of the same principle which had escaped during the solution, in the form of *air*, had likewise been retained in it, and had not left it in the evaporation of the water.

With great difficulty I also procured a small quantity of the same kind of air from a solution of *iron* in spirit of nitre, by the same process.

Having, for a different purpose, fired some paper, which had been dipped in a solution of copper in diluted spirit of nitre, in nitrous air, I found there was a considerable addition to the quantity of it; upon which I fired some of the same kind of paper in quicksilver and presently observed that air was produced from it in great plenty. This air, at the first, seemed to have some singular properties, but afterwards I found that it was nothing more than a mixture of nitrous air, from the precipitate of the solution, and of inflammable air, from the paper; but that the former was predominant.

In the mixture of this kind of air with common air, in a trough of water which had been putrid, but which at that time seemed to have recovered its former sweetness (for it was not in the least degree offensive to the smell) a phenomenon sometimes occurred, which for a long time exceedingly delighted and puzzled me; but which was afterwards the means of letting me see much farther into the constitution of nitrous air than I had been able to see before.

When the diminution of the air was nearly completed, the vessel in which the mixture was made began to be filled with the most beautiful *white fumes*, exactly resembling the precipitation of some white substance in a transparent menstruum, or the falling of very fine snow; except that it was much thicker below than above, as indeed is the case in all chemical precipitations. This appearance continued two or three minutes.

At other times I went over the same process, as nearly as possible in the same manner, but without getting this remarkable appearance, and was several times greatly disappointed and chagrined, when I baulked the expectations of my friends, to whom I had described, and meant to have shewn it. This made me give all the attention I possibly could to this experiment, endeavouring to recollect every circumstance, which, though unsuspected at the time, might have contributed to produce this new appearance; and I took a great deal of pains to procure a quantity of this air from the paper above mentioned for the purpose, which, with a small burning lens, and an uncertain sun, is not a little troublesome. But all that I observed for some time was, that I stood the best chance of succeeding when I *warmed* the vessel in which the mixture was made, and *agitated* the air during the effervescence.

Finding, at length, that, with the same preparation and attentions, I got the same appearance from a mixture of nitrous and common air in the same trough of water, I concluded that it could not depend upon any thing peculiar to the precipitate of the *copper* contained in the *paper* from which the air was procured, as I had at first imagined, but upon what was common to it, and pure nitrous air.

Afterwards, having, (with a view to observe whether any crystals would be formed by the union of volatile alkali, and nitrous air, similar to those formed by it and fixed air, as described by Mr. Smeth in his *Dissertation on fixed Air*) opened the mouth of a phial which was half filled with a volatile alkaline liquor, in a jar of nitrous air (in the manner described p. 11. fig. 4.) I had an appearance which perfectly explained the preceding. All that part of the phial which was above the liquor, and which contained common air, was filled with beautiful *white clouds*, as if some fine white powder had been instantly thrown into it, and some of these clouds rose within the jar of nitrous air. This appearance continued about a minute, and then intirely disappeared, the air becoming transparent.

Withdrawing the phial, and exposing it to the common air, it there also became turbid, and soon after the transparency returned. Introducing it again into the nitrous air, the clouds appeared as before. In this manner the white fumes, and transparency, succeeded each other alternately, as often as I chose to repeat the experiment, and would no doubt have continued till the air in the jar had been thoroughly diluted with common air. These appearances were the same with any substance that contained *volatile alkali*, fluid or solid.

When, instead of the small phial, I used a large and tall glass jar, this appearance was truly fine and striking, especially when the water in the trough was very transparent. For I had only to put the smallest drop of a volatile alkaline liquor, or the smallest bit of the solid salt, into the jar, and the moment that the mouth of it was opened in a jar of nitrous air, the white clouds above mentioned began to be formed at the mouth, and presently descended to the bottom, so as to fill the whole, were it ever so large, as with fine snow.

In considering this experiment, I soon perceived that this curious appearance must have been occasioned by the mixture of the nitrous and common air, and therefore that the white clouds must be *nitrous ammoniac*, formed by the acid of the nitrous air, set loose in the decomposition of it by common air, while the phlogiston, which must be another constituent part of nitrous air, entering the common air, is the cause of the diminution it suffers in this process; as it is the cause of a similar diminution, in a variety of other processes.

I would observe, that it is not peculiar to nitrous air to be a test of the fitness of air for respiration. Any other process by which air is diminished and made noxious answers the same purpose. Liver of sulphur for instance, the calcination of metals, or a mixture of iron filings and brimstone will do just the same thing; but the application of them is not so easy, or elegant, and the effect is not so soon perceived. In fact, it is *phlogiston* that is the test. If the air be so loaded with this principle that it can take no more, which is seen by its not being diminished in any of the processes above mentioned, it is noxious; and it is wholesome in proportion to the quantity of phlogiston that it is able to take.

This, I have no doubt, is the true theory of the diminution of common air by nitrous air, the redness of the appearance being nothing more than the usual colour of the fumes, of spirit of nitre, which is now disengaged from the superabundant phlogiston with which it was combined in the nitrous air, and ready to form another union with any thing that is at hand, and capable of it.

With the volatile alkali it forms nitrous ammoniac, water imbibes it like any other acid, even quicksilver is corroded by it; but this action being slow, the redness in this mixture of nitrous and common air continues much longer when the process is made in quicksilver, than when it is made in water, and the diminution, as I have also observed; is by no means so great.

I was confirmed in this opinion when I put a bit of volatile alkaline salt into the jar of quicksilver in which I made the mixture of nitrous and common air. In these circumstances, the vessel being previously filled with the alkaline fumes, the acid immediately joined them, formed the white clouds above mentioned, and the diminution proceeded almost as far as when the process was made in water. That it did not proceed quite so far, I attribute chiefly to the small quantity of calx formed by the slight solution of mercury with the acid fumes not being able to absorb all the fixed air that is precipitated from the common air by the phlogiston.

In part, also, it may be owing to the small quantify of surface in the quicksilver in the vessels that I made use of; in consequence of which the acid fumes could act upon it only in a slow succession, so that part of them, as well as of the fixed air, had an opportunity of forming another union with the diminished air.

This, as I have observed before, was so much the case when the process was made in quicksilver, without any volatile alkali, that when water was admitted to it, after some time, it was not capable of dissolving that union, tho' it would not have taken place if the process had been in water from the first.

In diversifying this experiment, I found that it appeared to very great advantage when I suspended a piece of volatile salt in the common air, previous to the admission of nitrous air to it, inclosing it in a bit of gauze, muslin, or a small net of wire. For, presently after the redness of the mixture begins to go off, the white cloud, like snow, begins to descend from the salt, as if a white powder was shaken out of the bag that contains it. This white cloud presently fills the whole vessel, and the appearance will last about five minutes.

If the salt be not put to the mixture of these two kinds of air till it has perfectly recovered its transparency, the effervescence being completely over, no white cloud will be formed; and, what is rather more remarkable, there is nothing of this appearance when the salt is put into the nitrous air itself. The reason of this must be, that the acid of the nitrous air has a nearer affinity with its phlogiston than with the volatile alkali; though the phlogiston having a nearer affinity with something in the common air, the acid being thereby set loose, will unite with the alkaline vapour, if it be at hand to unite with it.

There is also very little, if any white cloud formed upon holding a piece of the volatile salt within the mouth of a phial containing smoking spirit of nitre. Also when I threw the focus of a burning mirror upon some sal ammoniac in nitrous air, and filled the whole vessel with white fumes which arose from it, they were soon dispersed, and the air was neither diminished nor altered.

I was now fully convinced, that the white cloud which I casually observed, in the first of these experiments, was occasioned by the volatile alkali emitted from the water, which was in a slight degree putrid; and that the warming, and agitation of the vessels, had promoted the emission of the putrid, or alkaline effluvium.

I could not perceive that the diminution of common air by the mixture of nitrous air was sensibly increased by the presence of the volatile alkali. It is possible, however, that, by assisting the water to take up the acid, something less of it may be incorporated with the remaining diminished air than would otherwise have been; but I did not give much attention to this circumstance.

When the phial in which I put the alkaline salts contained any kind of noxious air, the opening of it in nitrous air was not followed by any thing of the appearance above mentioned. This was the case with inflammable air. But when, after agitating the inflammable air in water, I had brought it to a state in which it was diminished a little by the mixture of nitrous air, the cloudy appearance was in the same proportion; so that this appearance seems to be equally a test of the fitness of air for respiration, with the redness which attends the mixture of it with nitrous air only.

Having generally fastened the small bag which contained the volatile salt to a piece of brass wire in the preceding experiment, I commonly found the end of it corroded, and covered with a blue substance. Also the salt itself, and sometimes the bag was died blue. But finding that this was not the case when I used an iron wire in the same circumstances, but that it became *red*, I was satisfied that both the metals had been dissolved by the volatile alkali. At first I had a suspicion that the blue might have come from the copper, out of which the nitrous air had been made. But when the nitrous air was made from iron, the appearances were, in all respects, the same.

I have observed, in the preceding section, that if nitrous air be mixed with common air in *lime-water*, the surface of the water, where it is contiguous to that mixture, will be covered with an incrustation of lime, shewing that some fixed air had been deposited in the process. It is remarkable, however, as I there also just mentioned, that this is the case when nitrous air alone is put to a vessel of lime-water, after it has been kept in a *bladder*, or only transferred from one vessel to another by a bladder, in the manner described, p. 15. fig. 9.

As I had used the same bladder for transferring various kinds of air, and among the rest *fixed air*, I first imagined that this effect might have been occasioned by a mixture of this fixed air with the nitrous air, and therefore took a fresh bladder; but still the effect was the same. To satisfy myself farther, that the bladder had produced this effect, I put one into a jar of nitrous air, and after it had continued there a day and a night, I found that the nitrous air in this jar, though it was transferred in a glass vessel, made lime-water turbid.

Whether there was any thing in the preparation of these bladders that occasioned their producing this effect, I cannot tell. They were such as I procure from the apothecaries. The thing seems to deserve farther examination, as there seems, in this case, to be the peculiar effect of fixed air from other causes, or else a production of fixed air from materials that have not been supposed to yield it, at least not in circumstances similar to these.

As fixed air united to water dissolves iron, I had the curiosity to try whether fixed air alone would do it; and as nitrous air is of an *acid* nature, as well as fixed air, I, at the same time, exposed a large surface of iron to both the kinds; first filling two eight ounce phials with nails, and then with quicksilver, and after that displacing the quicksilver in one of the phials by fixed air, and in the other by nitrous air; then inverting them, and leaving them with their mouths immersed in basons of quicksilver.

In these circumstances the two phials stood about two months, when no sensible change at all was produced in the fixed air, or in the iron which had been exposed to it, but a most remarkable, and most unexpected change was made in the nitrous air; and in pursuing the experiment, it was transformed into a species of air, with properties which, at the time of my first publication on this subject, I should not have hesitated to pronounce impossible, viz. air in which a candle burns quite naturally and freely, and which is yet in the highest degree noxious to animals, insomuch that they die the moment they are put into it; whereas, in general, animals live with little sensible inconvenience in air in which candles have burned out. Such, however, is nitrous air, after it has been long exposed to a large surface of iron.

It is not less extraordinary, that a still longer continuance of nitrous air in these circumstances (but *how long* depends upon too many, and too minute circumstances to be ascertained with exactness) makes it not only to admit a candle to burn in it, but enables it to burn with an *enlarged flame*, by another flame (extending every where to an equal distance from that of the candle, and often plainly distinguishable from it) adhering to it. Sometimes

I have perceived the flame of the candle, in these circumstances, to be twice as large as it is naturally, and sometimes not less than five or six times larger; and yet without any thing like an *explosion*, as in the firing of the weakest inflammable air.

Nor is the farther progress in the transmutation of nitrous air, in these circumstances, less remarkable. For when it has been brought to the state last mentioned, the agitation of it in fresh water almost instantly takes off that peculiar kind of inflammability, so that it extinguishes a candle, retaining its noxious quality. It also retains its power of diminishing common air in a very great degree.

But this noxious quality, like the noxious quality of all other kinds of air that will bear agitation in water, is taken out of it by this operation, continued about five minutes; in which process it suffers a farther and very considerable diminution. It is then itself diminished by fresh nitrous air, and animals live in it very well, about as well as in air in which candles have burned out.

Lastly, One quantity of nitrous air, which had been exposed to iron in quicksilver, from December 18 to January 20, and which happened to stand in water till January 31 (the iron still continuing in the phial) was fired with an explosion, exactly like a weak inflammable air. At the same time another quantity of nitrous air, which had likewise been exposed to iron, standing in quicksilver, till about the same time, and had then stood in water only, without iron, only admitted a candle to burn in it with an enlarged flame, as in the cases above mentioned. But whether the difference I have mentioned in the circumstances of these experiments contributed to this difference in the result, I cannot tell.

Nitrous air treated in the manner above mentioned is diminished about one fourth by standing in quicksilver; and water admitted to it will absorb about half the remainder; but if water only, and no quicksilver, be used from the beginning, the nitrous air will be diminished much faster and farther; so that not more than one fourth, one sixth, or one tenth of the original quantity will remain. But I do not know that there is any difference in the constitution of the air which remains in these two cases.

The water which has imbibed this nitrous air exposed to iron is remarkably green, also the phial containing it becomes deeply, and, I believe, indelibly tinged with green; and if the water be put into another vessel, it presently deposits a considerable quantity of matter, which when dry appears to be the earth or ochre of iron; from which it is evident, that the acid of the nitrous air dissolves the iron; while the phlogiston, being set loose, diminishes nitrous air, as in the process of the iron filings and brimstone.

Upon this hint, instead of using *iron*, I introduced a pot of *liver of sulphur* into a jar of nitrous air, and presently found, that what I had before done by means of iron in six weeks, or two months, I could do by liver of sulphur (in consequence, no doubt, of its giving its phlogiston more freely) in less than twenty-four hours, especially when the process was kept warm.

It is remarkable, however, that if the process with liver of sulphur be suffered to proceed, the nitrous air will be diminished much farther. At one time not more than one twentieth of the original quantity remained, and how much farther it right have been diminished, I cannot tell. In this great diminution, it does not admit a candle to burn in it at all; and I generally found this to be the case whenever the diminution had proceeded beyond three fourths of the original quantity[13].

It is something remarkable, that though the diminution of nitrous air by iron filings and brimstone very much resembles the diminution of it by iron only, or by liver of sulphur, yet the iron filings and brimstone never bring it to such a state as that a candle will burn in it; and also that, after this process, it is never capable of diminishing common air. But when it is considered that these properties are destroyed by agitation in water, this difference in the result of processes, in other respects similar, will appear less extraordinary; and they agree in this, that long agitation in water makes both these kinds of nitrous air equally fit for respiration, being equally diminished by fresh nitrous air. It is possible that there would have been a more exact agreement in the result of these processes, if they had been made in equal degrees of *heat*; but the process with iron was made in the usual temperature of the atmosphere, and that with liver of sulphur generally near a fire.

It may clearly, I think, be inferred from these experiments, that all the difference between fresh nitrous air, that state of it in which it is partially inflammable, or wholly so, that in which it again extinguishes candles, and that in which it finally becomes fit for respiration, depends upon some difference in the *mode of the combination* of its acid with phlogiston, or on the *proportion* between these two ingredients in its composition; and it is not improbable but that, by a little more attention to these experiments, the whole mystery of this proportion and combination may be explained.

I must not omit to observe that there was something peculiar in the result of the first experiment which I made with nitrous air exposed to iron; which was that, without any agitation in water, it was diminished by fresh nitrous air, and that a candle burned in it quite naturally. To what this difference was owing I cannot tell. This air, indeed, had been exposed to the iron a week or two longer than in any of the other cases, but I do not imagine that this circumstance could have produced that difference.

When the process is in water with iron, the time in which the diminution is accomplished is exceedingly various; being sometimes completed in a few days, whereas at other times it has required a week or a fortnight. Some kinds of iron also produced this effect much sooner than others, but on what circumstances this difference depends I do not know. What are the varieties in the result of this experiment when it is made in quicksilver I cannot tell, because, on account of its requiring more time, I have not repeated it so often; but I once found that nitrous air was not sensibly changed by having been exposed to iron in quicksilver nine days; whereas in water a very considerable alteration was always made in much less than half that time.

It may just deserve to be mentioned, that nitrous air extremely rarified in an air-pump dissolves iron, and is diminished by it as much as when it is in its native state of condensation.

It is something remarkable, though I never attended to it particularly before I made these last experiments, and it may tend to throw some light upon them, that when a candle is extinguished, as it never fails to be, in nitrous air, the flame seems to be a little enlarged at its edges, by another bluish flame added to it, just before its extinction.

It is proper to observe in this place, that the electric spark taken in nitrous air diminishes it to one fourth of its original quantity, which is about the quantity of its diminution by iron filings and brimstone, and also by liver of sulphur without heat. The air is also brought by electricity to the same state as it is by iron filings and brimstone, not diminishing common air. If the electric spark be taken in it when it is confined by water tinged with archil, it is presently changed from blue to red, and that to a very great degree.

When the iron nails or wires, which I have used to diminish nitrous air, had done their office, I laid them aside, not suspecting that they could be of any other philosophical use; but after having lain exposed to the open air almost a fortnight; having, for some other purpose, put some of them into a vessel containing common air, standing inverted, and immersed in water, I was surprized to observe that the air in which they were confined was diminished. The diminution proceeded so fast, that the process was completed in about twenty-four hours; for in that time the air was diminished about one fifth, so that it made no effervescence with nitrous air, and was, therefore, no doubt, highly noxious, like air diminished by any other process.

This experiment I have repeated a great number of times, with the same phials, filled with nails or wires that have been suffered to rust in nitrous air, but their power of diminishing common air grows less and less continually. How long it will be before it is quite exhausted I cannot tell. This diminution of air I conclude must arise from the phlogiston, either of the nitrous air or the iron, being some way entangled in the rust, in which the wires were encrusted, and afterwards getting loose from it.

To the experiments upon iron filings and brimstone in nitrous air, I must add, that when a pot full of this mixture had absorbed as much as it could of a jar of nitrous air (which is about three fourths of the whole) I put fresh nitrous air to it, and it continued to absorb, till three or four jars full of it disappeared; but the absorption was exceedingly slow at the last. Also when I drew this pot through the water, and admitted fresh nitrous air to it, it absorbed another jar full, and then ceased. But when I scraped off the outer surface of this mixture, which had been so long exposed to the nitrous air, the remainder absorbed more of the air.

When I took the top of the mixture which I had scraped off and threw upon it the focus of a burning-glass, the air in which it was confined was diminished, and became quite noxious; yet when I endeavoured to get air from this matter in a jar full of quicksilver, I was able to procure little or nothing.

It is not a little remarkable that nitrous air diminished by iron filings and brimstone, which is about one fourth, cannot, by agitation in water, be diminished much farther; whereas pure nitrous air may, by the same process, be diminished to one twentieth of its whole bulk, and perhaps much more. This is similar to the effect of the same mixture, and of phlogiston in other cases, on fixed air; for it so far changes its constitution, that it is afterwards incapable of mixing with water. It is similar also to the effect of phlogiston in acid air, which of itself is almost instantly absorbed by water; but by this addition it is first converted into inflammable air, which does not readily mix with water, and which, by long agitation in water, becomes of another constitution, still less miscible with water.

I shall close this section with a few other observations of a miscellaneous nature.

Nitrous air is as much diminished both by iron filings, and also by liver of sulphur, when confined in quicksilver, as when it is exposed to water.

Distilled water tinged blue with the juice of turnsole becomes red on being impregnated with nitrous air; but by being exposed a week or a fortnight to the common atmosphere, in open and shallow vessels, it recovers its blue colour; though, in that time, the greater part of the water will be evaporated. This shews that in time nitrous air escapes from the water with which it is combined, just as fixed air does, though by no means so readily[14].

Having dissolved silver, copper, and iron in equal quantities of spirit of nitre diluted with water, the quantities of nitrous air produced from them were in the following proportion; from iron 8, from copper 6-1/4, from silver 6. In about the same proportion also it was necessary to mix water with the spirit of nitre in each case, in order to make it dissolve these metals with equal rapidity, silver requiring the least water, and iron the most.

Phosphorus gave no light in nitrous air, and did not take away from its power of diminishing common air; only when the redness of the mixture went off, the vessel in which it was made was filled with white fumes, as if there had been some volatile alkali in it. The phosphorus itself was unchanged.

There is something remarkable in the effect of nitrous air on *insects* that are put into it. I observed before that this kind of air is as noxious as any whatever, a mouse dying the moment it is put into it; but frogs and snails (and therefore, probably, other animals whose respiration is not frequent) will bear being exposed to it a considerable time, though they die at length. A frog put into nitrous air struggled much for two or three minutes, and moved now and then for a quarter of an hour, after which it was taken out, but did not recover. *Wasps* always died the moment they were put into the nitrous air. I could never observe that they made the least motion in it, nor could they be recovered to life afterwards. This was also the case in general with *spiders*, *flies*, and *butterflies*. Sometimes, however, spiders would recover after being exposed about a minute to this kind of air.

Considering how fatal nitrous air is to insects, and likewise its great antiseptic power, I conceived that considerable use might be made of it in medicine, especially in the form of *clysters*, in which fixed air had been applied with some success; and in order to try whether the bowels of an animal would bear the injection of it, I contrived, with the help of Mr. Hey, to convey a quantity of it up the anus of a dog. But he gave manifest signs of uneasiness, as long as he retained it, which was a considerable time, though in a few hours afterwards he was as lively as ever, and seemed to have suffered nothing from the operation.

Perhaps if nitrous air was diluted either with common air, or fixed air, the bowels might bear it better, and still it might be destructive to *worms* of all kinds, and be of use to check or correct putrefaction in the intestinal canal, or other parts of the system. I repeat it once more that, being no physician, I run no risk by such proposals as these; and I cannot help flattering myself that, in time, very great medicinal use will be made of the application of these different kinds of air to the animal system. Let ingenious physicians attend to this subject, and endeavour to lay hold of the new *handle* which is now presented them, before it be seized by rash empiricks; who, by an indiscriminate and injudicious application, often ruin the credit of things and processes which might otherwise make an useful addition to the *materia* and *ars medica*.

In the first publication of my papers, having experienced the remarkable antiseptic power of nitrous air, I proposed an attempt to preserve anatomical preparations, &c. by means of it; but Mr. Hey, who made the trial, found that, after some months, various animal substances were shriveled, and did not preserve their natural forms in this kind of air.

FOOTNOTES:

[13] The result of several of these experiments I had the pleasure of trying in the presence of the celebrated Mr. De Luc of Geneva, when he was upon a visit to Lord Shelburne in Wiltshire.

[14] I have not repeated this experiment with that variation of circumstances which an attention to Mr. Bewley's observation will suggest.

SECTION IV.

Of Marine Acid Air.

In my former experiments on this species of air I procured it from spirit of salt, but I have since hit upon a much less expensive method of getting it, by having recourse to the process by which the spirit of salt is itself originally made. For this purpose I fill a small phial with common salt, pour upon it a small quantity of concentrated oil of vitriol, and receive the fumes emitted by it in a vessel previously filled with quicksilver, and standing in a bason of quicksilver, in which it appears in the form of a perfectly *transparent air*, being precisely the same thing with that which I had before expelled from the spirit of salt.

This method of procuring acid air is the more convenient, as a phial, once prepared in this manner, will suffice, for common experiments, many weeks; especially if a little more oil of vitriol be occasionally put to it. It only requires a little more heat at the last than at the first. Indeed, at the first, the heat of a person's hand will often be sufficient to make it throw out the vapour. In warm weather it will even keep smoking many days without the application of any other heat.

On this account, it should be placed where there are no instruments, or any thing of metal, that can be corroded by this acid vapour. It is from dear-bought experience that I give this advice. It may easily be perceived when this phial is throwing out this acid vapour, as it always appears, in the open air, in the form of a light cloud; owing, I suppose, to the acid attracting to itself, and uniting with, the moisture that is in the common atmosphere.

By this process I even made a stronger spirit of salt than can be procured in any other way. For having a little water in the vessel which contains the quicksilver, it imbibes the acid vapour, and at length becomes truly saturated with it. Having, in this manner, impregnated pure water with acid air, I could afterwards expel the same air from it, as from common spirit of salt.

I observed before that this acid vapour, or air, has a strong affinity with *phlogiston*, so that it decomposes many substances which contain it, and with them forms a permanently inflammable air, no more liable to be imbibed by water than inflammable air procured by any other process, being in fact the very same thing; and that, in some cases, it even dislodges spirit of nitre and oil of vitriol, which in general appear to be stronger acids than itself. I have since observed that, by giving it more time, it will extract phlogiston from substances from which I at first concluded that it was not able to do it, as from dry wood, crusts of bread not burnt, dry flesh, and what is more extraordinary from flints. As there was something peculiar to itself in the process or result of each of these experiments, it may not be improper to mention them distinctly.

Pieces of dry *cork wood* being put to the acid air, a small quantity remained not imbibed by water, and was inflammable.

Very dry pieces of *oak*, being exposed to this air a day and a night, after imbibing a considerable quantity of it, produced air which was inflammable indeed, but in the slightest degree imaginable. It seemed to be very nearly in the state of common air.

A piece of *ivory* imbibed the acid vapour very slowly. In a day and a night, however, about half an ounce measure of permanent air was produced, and it was pretty strongly inflammable. The ivory was not discoloured, but was rendered superficially soft, and clammy, tasting very acid.

Pieces of *beef*, roasted, and made quite dry, but not burnt, absorbed the acid vapour slowly; and when it had continued in this situation all night, from five ounce measures of the air, half a measure was permanent, and pretty strongly inflammable. This experiment succeeded a second time exactly in the same manner; but when I used pieces of white dry *chicken-flesh* though I allowed the same time, and in other respects the process seemed to go on in the same manner, I could not perceive that any part of the remaining air was inflammable.

Some pieces of a whitish kind of *flint*, being put into a quantity of acid air, imbibed but a very little of it in a day and a night; but of 2-1/2 ounce measures of it, about half a measure remained unabsorbed by water, and this was strongly inflammable, taking fire just like an equal mixture of inflammable and common air. At another time, however, I could not procure any inflammable air by this means, but to what circumstance these different results were owing I cannot tell.

That inflammable air is produced from *charcoal* in acid air I observed before. I have since found that it may likewise be procured from *pit coal*, without being charred.

Inflammable air I had also observed to arise from the exposure of spirit of wine, and various *oily* substances, to the vapour of spirit of salt. I have since made others of a similar nature, and as peculiar circumstances attended some of these experiments, I shall recite them more at large.

Essential oil of mint absorbed this air pretty fast, and presently became of a deep brown colour. When it was taken out of this air it was of the consistence of treacle, and sunk in water, smelling differently from what it did before; but still the smell of the mint was predominant. Very little or none of the air was fixed, so as to become inflammable; but more time would probably have produced this effect.

Oil of turpentine was also much thickened, and became of a deep brown colour, by being saturated with acid air.

Ether absorbed acid air very fast, and became first of a turbid white, and then of a yellow and brown colour. In one night a considerable quantity of permanent air was produced, and it was strongly inflammable.

Having, at one time, fully saturated a quantity of ether with acid air, I admitted bubbles of common air to it, through the quicksilver, by which it was confined, and observed that white fumes were made in it, at the entrance of every bubble, for a considerable time.

At another time, having fully saturated a small quantity of ether with acid air, and having left the phial in which it was contained nearly full of the air, and inverted, it was by some accident overturned; when, instantly, the whole room was filled with a visible fume, like a white cloud, which had very much the smell of ether, but peculiarly offensive. Opening the door and window of the room, this light cloud filled a long passage, and another room. In the mean time the ether was seemingly all vanished, but some time after the surface of the quicksilver in which the experiment had been made was covered with a liquor that tasted very acid; arising, probably, from the moisture in the atmosphere attracted by the acid vapour with which the ether had been impregnated.

This visible cloud I attribute to the union of the moisture in the atmosphere with the compound of the acid air and ether. I have since saturated other quantities of ether with acid air, and found it to be exceedingly volatile, and inflammable. Its exhalation was also visible, but not in so great a degree as in the case above mentioned.

Camphor was presently reduced into a fluid state by imbibing acid air, but there seemed to be something of a whitish sediment in it. After continuing two days in this situation I admitted water to it; immediately upon which the camphor resumed its former solid state, and, to appearance, was the very same substance that it had been before; but the taste of it was acid, and a very small part of the air was permanent, and slightly inflammable.

The acid air seemed to make no impression upon a piece of Derbyshire *spar*, of a very dark colour, and which, therefore, seemed to contain a good deal of phlogiston.

As the acid air has so near an affinity with phlogiston, I expected that the fumes of *liver of sulphur*, which chemists agree to be phlogistic, would have united with it, so as to form inflammable air; but I was disappointed in that expectation. This substance imbibed half of the acid air to which it was introduced: one fourth of the remainder, after standing one day in quicksilver, was imbibed by water, and what was left extinguished a candle. This experiment, however, seems to prove that acid air and phlogiston may form a permanent kind of air that is not inflammable. Perhaps it may be air in such a state as common air loaded with phlogiston, and from which the fixed air has been precipitated. Or rather, it may be the same thing with inflammable air, that has lost its inflammability by long standing in water. It well deserves a farther examination.

The following experiments are those in which the *stronger acids* were made use of, and therefore they may assist us farther to ascertain their affinities with certain substances, with respect to this marine acid in the form of air.

I put a quantity of strong concentrated *oil of vitriol* to acid air, but it was not at all affected by it in a day and a night. In order to try whether it would not have more power in a more condensed state, I compressed it with an additional atmosphere; but upon taking off this pressure, the air expanded again, and appeared to be not at all diminished. I also put a quantity of strong *spirit of nitre* to it without any sensible effect. We may conclude, therefore, that the marine acid, in this form of air, is not able to dislodge the other acids from their union with water.

Blue vitriol, which is formed by the union of the vitriolic acid with copper, turned to a dark green the moment that it was put to the acid air, which it absorbed, though slowly. Two pieces, as big as small nuts, absorbed three ounce measures of the air in about half an hour. The green colour was very superficial; for it was easily wiped or washed off.

Green copperas turned to a deeper green upon being put into acid air, which it absorbed slowly. *White copperas* absorbed this air very fast, and was dissolved in it.

Sal ammoniac, being the union of spirit of salt with volatile alkali, was no more affected with the acid air than, as I have observed before, common salt was.

I also introduced to the acid air various other substances, without any particular expectation; and it may be worth while to give an account of the results, that the reader may draw from them such conclusions as he shall think reasonable.

Borax absorbed acid air about as fast as blue vitriol, but without any thing else that was observable.

Fine white *sugar* absorbed this air slowly, was thoroughly penetrated with it, became of a deep brown colour, and acquired a smell that was peculiarly pungent.

A piece of *quick lime* being put to about twelve or fourteen ounce measures of acid air, and continuing in that situation about two days, there remained one ounce measure of air that was not absorbed by water, and it was very strongly inflammable, as much so as a mixture of half inflammable and half common air. Very particular care was taken that no common air mixed with the acid air in this process. At another time, from about half the quantity of acid air above mentioned, with much less quick-lime, and in the space of one day, I got half an ounce measure of air that was inflammable in a slight degree only. This experiment proves that some part of the phlogiston which escapes from the fuel, in contact with which the lime is burned, adheres to it. But I am very far from thinking that the causticity of quick-lime is at all owing to this circumstance.

I have made a few more experiments on the mixture of acid air with *other kinds of air*, and think that it may be worth while to mention them, though nothing of consequence, at least nothing but negative conclusions, can be drawn from them.

A quantity of common air saturated with nitrous air was put to a quantity of acid air, and they continued together all night, without any sensible effect. The quantity of both remained the same, and water being admitted to them, it absorbed all the acid air, and left the other just as before.

A mixture of two thirds of air diminished by iron filings and brimstone, and one third acid air, were mixed together, and left to stand four weeks in quicksilver. But when the mixture was examined, water presently imbibed all the acid air, and the diminished air was found to be just the same that it was before. I had imagined that the acid air might have united with the phlogiston with which the diminished air was overcharged, so as to render it wholsome; and I had read an account of the stench arising from putrid bodies being corrected by acid fumes.

The remaining experiments, in which the acid air was principally concerned, are of a miscellaneous nature.

I put a piece of dry *ice* to a quantity of acid air (as was observed in the section concerning *alkaline* air) taking it with a forceps, which, as well as the air itself, and the quicksilver by which it had been confined; had been exposed to the open air for an hour, in a pretty strong frost. The moment it touched the air it was dissolved as fast as it would have been by being thrown into a hot fire, and the air was presently imbibed. Putting fresh pieces of ice to that which was dissolved before, they were also dissolved immediately, and the water thus procured did not freeze again, though it was exposed a whole night, in a very intense frost.

Flies and spiders die in acid air, but not so quickly as in nitrous air. This surprized me very much; as I had imagined that nothing could be more speedily fatal to all animal life than this pure acid vapour.

As inflammable air, I have observed, fires at one explosion in the vapour of smoking spirit of nitre, just like an equal mixture of inflammable and common air, I thought it was possible that the fume which naturally rises from common spirit of salt might have the same effect, but it had not. For this purpose I treated the spirit of salt, as I had before done the smoking spirit of nitre; first filling a phial with it, then inverting it in a vessel containing a quantity of the same acid; and having thrown the inflammable air into it, and thereby driven out all the acid, turning it with its mouth upwards, and immediately applying a candle to it.

Acid air not being so manageable as most of the other kinds of air, I had recourse to the following peculiar method, in order to ascertain its *specific gravity*. Having filled an eight ounce phial with this air, and corked it up, I weighed it very accurately; and then, taking out the cork, I blew very strongly into it with a pair of bellows, that the common air might take place of the acid; and after this I weighed it again, together with the cork, but I could not perceive the least difference in the weight. I conclude, however, from this experiment, that the acid air is heavier than the common air, because the mouth of the phial and the inside of it were evidently moistened by the water which the acid vapour had attracted from the air, which moisture must have added to the weight of the phial.

SECTION V.

Of Inflammable Air.

It will have appeared from my former experiments, that inflammable air consists chiefly, if not wholly, of the union of an acid vapour with phlogiston; that as much of the phlogiston as contributes to make air inflammable is imbibed by the water in which it is agitated; that in this process it soon becomes fit for respiration, and by the continuance of it comes at length to extinguish flame. These observations, and others which I have made upon this kind of air, have been confirmed by my later experiments, especially those in which I have connected *electrical experiments* with those on air.

The electric spark taken in any kind of *oil* produces inflammable air, as I was led to observe in the following manner. Having found, as will be mentioned hereafter, that ether doubles the quantity of any kind of air to which it is admitted; and being at that time engaged in a course of experiments to ascertain the effect of the electric matter on all the different kinds of air, I had the curiosity to try what it would do with *common air*, thus increased by means of ether. The very first spark, I observed, increased the quantity of this air very considerably, so that I had very soon six or eight times as much as I began with; and whereas water imbibes all the ether that is put to any kind of air, and leaves it without any visible change, with respect to quantity or quality, this air, on the contrary, was not imbibed by water. It was also very little diminished by the mixture of nitrous air. From whence it was evident, that it had received an addition of some other kind of air, of which it now principally consisted.

In order to determine whether this effect was produced by the *wire*, or the *cement* by which the air was confined (as I thought it possible that phlogiston might be discharged from them) I made the experiment in a glass syphon, fig. 19, and by that means I contrived to make the electric spark pass from quicksilver through the air on which I made the experiment, and the effect was the same as before. At one time there happened to be a bubble of common air, without any ether, in one part of the syphon, and another bubble with ether in another part of it; and it was very amusing to observe how the same electric sparks diminished the former of these bubbles, and increased the latter.

It being evident that the *ether* occasioned the difference that was observable in these two cases, I next proceeded to take the electric spark in a quantity of ether only, without any air whatever; and observed that every spark produced a small bubble; and though, while the sparks were taken in the ether itself, the generation of air was slow, yet when so much air was collected, that the sparks were obliged to pass through it, in order, to come to the ether and the quicksilver on which it rested, the increase was exceedingly rapid; so that, making the experiment in small tubes, as fig. 16, the quicksilver soon receded beyond the striking distance. This air, by passing through water, was diminished to about one third, and was inflammable.

One quantity of air produced in this manner from ether I suffered to stand two days in water, and after that I transferred it several times through the water, from one vessel to another, and still found that it was very strongly inflammable; so that I have no doubt of its being genuine inflammable air, like that which is produced from metals by acids, or by any other chemical process.

Air produced from ether, mixed both with common and nitrous air, was likewise inflammable; but in the case of the nitrous air, the original quantity bore a very small proportion to the quantity generated.

Concluding that the inflammable matter in this air came from the ether, as being of the class of *oils*, I tried other kinds of oil, as *oil of olives*, *oil of turpentine*, and *essential oil of mint*, taking the electric spark in them, without any air to begin with, and found that inflammable air was produced in this manner from them all. The generation of air from oil of turpentine was the quickest, and from the oil of olives the slowest in these three cases.

By the same process I got inflammable air from *spirit of wine*, and about as copiously as from the essential oil of mint. This air continued in water a whole night, and when it was transferred into another vessel was strongly inflammable.

In all these cases the inflammable matter might be supposed to arise from the inflammable substances on which the experiments were made. But finding that, by the same process I could get inflammable air from the *volatile spirit of sal ammoniac*, I conclude that the phlogiston was in part supplied by the electric matter itself. For though, as I have observed before, the alkaline air which is expelled from the spirit of sal ammoniac be inflammable, it is so in a very slight degree, and can only be perceived to be so when there is a considerable quantity of it.

Endeavouring to procure air from a caustic alkaline liquor, accurately made for me by Mr. Lane, and also from spirit of salt, I found that the electric spark could not be made visible in either of them; so that they must be much more perfect conductors of electricity than water, or other fluid substances. This experiment well deserves to be prosecuted.

I observed before that inflammable air, by standing long in water, and especially by agitation in water, loses its inflammability; and that in the latter case, after passing through a state in which it makes some approach to common air (just admitting a candle to burn in it) it comes to extinguish a candle. I have since made another observation of this kind, which well deserves to be recited. It relates to the inflammable air generated from oak the 27th of July 1771, of which I have made mention before.

This air I have observed to have been but weakly inflammable some months after it was generated, and to have been converted into pretty good or wholesome air by no great degree of agitation in water; but on the 27th of March 1773, I found the remainder of it to be exceedingly good air. A candle burned in it perfectly well, and it was diminished by nitrous air almost as much as common air.

I shall conclude this section with a few miscellaneous observations of no great importance.

Inflammable air is not changed by being made to pass many times through a red-hot iron tube. It is also no more diminished or changed by the fumes of liver of sulphur, or by the electric spark, than I have before observed it to have been by a mixture of iron filings and brimstone. When the electric spark was taken in it, it was confined by a quantity of water tinged blue with the juice of archil, but the colour remained unchanged.

I put two *wasps* into inflammable air, and let them remain there a considerable time, one of them near an hour. They presently ceased to move, and seemed to be quite dead for about half an hour after they were taken into the open air; but then they came to life again, and presently after seemed to be as well as ever they had been.

SECTION VI.

Of Fixed Air.

The additions I have made to my observations on *fixed air* are neither numerous nor considerable.

The most important of them is a confirmation of my conjecture, that fixed air is capable of forming an union with phlogiston, and thereby becoming a kind of air that is not miscible with water. I had produced this effect before by means of iron filings and brimstone, fermenting in this kind of air; but I have since had a much more decisive and elegant proof of it by *electricity*. For after taking a small electric explosion, for about an hour, in the space of an inch of fixed air, confined in a glass tube one tenth of an inch in diameter, fig. 16, I found that when water was admitted to it, only one fourth of the air was imbibed. Probably the whole of it would have been rendered immiscible in water, if the electrical operation had been continued a sufficient time. This air continued several days in water, and was even agitated in water without any farther diminution. It was not, however, common air, for it was not diminished by nitrous air.

By means of iron filings and brimstone I have, since my former experiments, procured a considerable quantity of this kind of air in a method something different from that which I used before. For having placed a pot of this mixture under a receiver, and exhausted it with a pump of Mr. Smeaton's construction, I filled it with fixed air, and then left it plunged under water; so that no common air could have access to it. In this manner, and in about a week, there was, as near as I can recollect, one sixth, or at least one eighth of the whole converted into a permanent air, not imbibed by water.

From this experiment I expected that the same effect would have been produced on fixed air by the fumes of *liver of sulphur*; but I was disappointed in that expectation, which surprised me not a little; though this corresponds in some measure, to the effect of phlogiston exhaled from this substance on acid air. Perhaps more time may be requisite for this purpose, for this process was not continued more than a day and a night.

Iron filings and brimstone, I have observed, ferment with great heat in nitrous air, and I have since observed that this process is attended with greater heat in fixed air than in common air.

Though fixed air incorporated with water dissolves iron, fixed air without water has no such power, as I observed before. I imagined that, if it could have dissolved iron, the phlogiston would have united with the air, and have made it immiscible with water, as in the former instances; but after being confined in a phial full of nails from the 15th of December to the 4th of October following, neither the iron nor the air appeared to have been affected by their mutual contact.

Having exposed equal quantities of common and fixed air, in equal and similar cylindrical glass vessels, to equal degrees of heat, by placing them before a fire, and frequently changing their situations, I observed that they were expanded exactly alike, and when removed from the fire they both recovered their former dimensions.

Having had some small suspicion that liver of sulphur, besides emitting phlogiston, might also yield some fixed air (which is known to be contained in the salt of tartar from which it is made) I mixed the two ingredients, viz. salt of tartar and brimstone, and putting them into a thin phial, and applying the flame of a candle to it, so as to form the liver of sulphur, I received the air that came from it in this process in a vessel of quicksilver. In this manner I procured a very considerable quantity of fixed air, so that I judged it was all discharged from the tartar. But though it is possible that a small quantity of it may remain in liver of sulphur, when it is made in the most perfect manner, it is not probable that it can be expelled without heat.

SECTION VII.

Miscellaneous Experiments.

1. It is something extraordinary that, though ether, as I found, cannot be made to assume the form of air (the vapour arising from it by heat, being soon condensed by cold, even in quicksilver) yet that a very small quantity of ether put to any kind of air, except the acid, and alkaline, which it imbibes, almost instantly doubles the apparent quantity of it; but upon passing this air through water, it is presently reduced to its original quantity again, with little or no change of quality.

I put about the quantity of half a nut-shell full of ether, inclosed in a glass tube, through a body of quicksilver, into an ounce measure of common air, confined by quicksilver; upon which it presently began to expand, till it occupied the space of two ounce measures. It then gradually contracted about one sixth of an ounce measure. Putting more ether to it, it again expanded to two ounce measures; but no more addition of ether would make it expand any farther. Withdrawing the quicksilver, and admitting water to this air, without any agitation, it began to be absorbed; but only about half an ounce measure had disappeared after it had stood an hour in the water. But by once passing it through water the air was reduced to its original dimensions. Being tried by a mixture of nitrous air, it appeared not to be so good as fresh air, though the injury it had received was not considerable.

All the phenomena of dilatation and contraction were nearly the same, when, instead of common air, I used nitrous air, fixed air, inflammable air, or any species of phlogisticated common air. The quantity of each of these kinds of air was nearly doubled while they were kept in quicksilver, but fixed air was not so much increased as the rest, and phlogisticated air less; but after passing through the water, they appeared not to have been sensibly changed by the process.

2. Spirit of wine yields no air by means of heat, the vapours being soon condensed by cold, like the vapour of water; yet when, in endeavouring to procure air from it, I made it boil, and catched the air which had rested on the surface of the spirit, and which had been expelled by the heat together with the vapour, in a vessel of quicksilver, and afterwards admitted acid air to it, the vessel was filled with white fumes, as if there had been a mixture of alkaline air along with it. To what this appearance was owing I cannot tell, and indeed I did not examine into it.

3. Having been informed by Dr. Small and Mr. Bolton of Birmingham, that paper dipped in a solution of copper in spirit of nitre would take fire with a moderate heat (a fact which I afterwards found mentioned in the Philosophical Transactions) it occurred to me that this would be very convenient for experiments relating to *ignition* in different kinds of air; and indeed I found that it was easily fired, either by a burning lens, or the approach of red-hot iron on the outside of the phial in which it was contained, and that any part of it being once fired, the whole was presently reduced to ashes; provided it was previously made thoroughly dry, which, however, it is not very easy to do.

With this preparation, I found that this paper burned freely in all kinds of air, but not in *vacuo*, which is also the case with gunpowder; and, as I have in effect observed before, all the kinds of air in which this paper was burned received an addition to their bulk, which consisted partly of nitrous air, from the nitrous precipitate, and partly of inflammable air, from the paper. As some of the circumstances attending the ignition of this paper in some of the kinds of air were a little remarkable, I shall just recite them.

Firing this paper in *inflammable* air, which it did without any ignition of the inflammable air itself, the quantity increased regularly, till the phial in which the process was made was nearly full; but then it began to decrease, till one third of the whole quantity disappeared.

A piece of this paper being put to three ounce measures of *acid* air, a great part of it presently turned yellow, and the air was reduced to one third of the original quantity, at the same time becoming reddish, exactly like common air in a phial containing smoking spirit of nitre. After this, by the approach of hot iron, I set fire to the paper; immediately upon which there was a production of air which more than filled the phial. This air appeared, upon examination, to be very little different from pure nitrous air. I repeated this experiment with the same event.

Paper dipped in a solution of mercury, zinc, or iron, in nitrous acid, has, in a small degree, the same property with paper dipped in a solution of copper in the same acid.

4. Gunpowder is also fired in all kinds of air, and, in the quantity in which I tried it, did not make any sensible change in them, except that the common air in which it was fired would not afterwards admit a candle to burn in it. In order to try this experiment I half exhausted a receiver, and then with a burning-glass fired the gunpowder which had been previously put into it. By this means I could fire a greater quantity of gunpowder in a small quantity of air, and avoid the hazard of blowing up, and breaking my receiver.

I own that I was rather afraid of firing gunpowder in inflammable air, but there was no reason for my fear; for it exploded quite freely in this air, leaving it, in all respects, just as it was before.

In order to make this experiment, and indeed almost all the experiments of firing gunpowder in different kinds of air, I placed the powder upon a convenient stand within my receiver, and having carefully exhausted it by a pump of Mr. Smeaton's construction, I filled the receiver with any kind of air by the apparatus described, p. 19, fig. 14, taking the greatest care that the tubes, &c. which conveyed the air should contain little or no common air. In the experiment with inflammable air a considerable mixture of common air would have been exceedingly hazardous: for, by that assistance, the inflammable air might have exploded in such a manner, as to have been dangerous to the operator. Indeed, I believe I should not have ventured to have made the experiment at all with any other pump besides Mr. Smeaton's.

Sometimes, I filled a glass vessel with quicksilver, and introduced the air to it, when it was inverted in a bason of quicksilver. By this means I intirely avoided any mixture of common air; but then it was not easy to convey the gunpowder into it, in the exact quantity that was requisite for my purpose. This, however, was the only method by which I could contrive to fire gunpowder in acid or alkaline air, in which it exploded just as it did in nitrous or fixed air.

I burned a considerable quantity of gunpowder in an exhausted receiver (for it is well known that it will not explode in it) but the air I got from it was very inconsiderable, and in these circumstances was necessarily mixed with common air. A candle would not burn in it.

SECTION VIII.

Queries, Speculations, and Hints.

I begin to be apprehensive lest, after being considered as a *dry experimenter*, I should pass, with many of my readers, into the opposite character of a *visionary theorist*. A good deal of theory has been interspersed in the course of this work, but, not content with this, I am now entering upon a long section, which contains nothing else.

The conjectures that I have ventured to advance in the body of the work will, I hope, be found to be pretty well supported by facts; but the present section will, I acknowledge, contain many *random thoughts*. I have, however, thrown them together by themselves, that readers of less imagination, and who care not to advance beyond the regions of plain fact, may, if they please, proceed no farther, that their delicacy be not offended.

In extenuation of my offence, let it, however, be considered, that *theory* and *experiment* necessarily go hand in hand, every process being intended to ascertain some particular *hypothesis*, which, in fact, is only a conjecture concerning the circumstances or the cause of some natural operation; consequently that the boldest and most original experimenters are those, who, giving free scope to their imaginations, admit the combination of the most distant ideas; and that though many of these associations of ideas, will be wild and chimerical, yet that others will have the chance of giving rise to the greatest and most capital discoveries; such as very cautious, timid, sober, and slow-thinking people would never have come at.

Sir Isaac Newton himself, notwithstanding the great advantage which he derived from a habit of *patient thinking*, indulged bold and excentric thoughts, of which his Queries at the end of his book of Optics are a sufficient evidence. And a quick conception of distant analogies, which is the great key to unlock the secret of nature, is by no means incompatible with the spirit of *perseverance*, in investigations calculated to ascertain and pursue those analogies.

§ 1. *Speculations concerning the* CONSTITUENT PRINCIPLES *of the different kinds of* AIR*, and the* CONSTITUTION *and* ORIGIN *of the* ATMOSPHERE*, &c.*

All the kinds of air that appear to me to be essentially distinct from each other are *fixed air*, *acid* and *alkaline*; for these, and another principle, called *phlogiston*, which I have not been able to exhibit in the form of *air*, and which has never yet been exhibited by itself in *any form*, seem to constitute all the kinds of air that I am acquainted with.

Acid air and phlogiston constitute an air which either extinguishes flame, or is itself inflammable, according, probably, to the quantity of phlogiston combined in it, or the mode of combination. When it extinguishes flame, it is probably so much charged with the phlogistic matter, as to take no more from a burning candle, which must, therefore, necessarily go out in it. When it is inflammable, it is probably so much charged with phlogiston, that the heat communicated by a burning candle makes it immediately separate itself from the other principle with which it was united, in which separation *heat* is produced, as in other cases of ignition; the action and reaction, which necessarily attends the separation of the constituent principles, exciting probably a vibratory motion in them.

Since inflammable, air, by agitation in water, first comes to lose its inflammability, so as to be fit for respiration, and even to admit a candle to burn in it, and then comes to extinguish a candle; it seems probable that water imbibes a great part of this extraordinary charge of phlogiston. And that water *can* be impregnated with phlogiston, is evident from many of my experiments, especially those in which metals were calcined over it.

Water having this affinity with phlogiston, it is probable that it always contains a considerable portion of it; which phlogiston having a stronger affinity with the acid air, which is perhaps the basis of common air, may by long agitation be communicated to it, so as to leave it over saturated, in consequence of which it will extinguish a candle.

It is possible, however, that inflammable air and air which extinguishes a candle may differ from one another in the *mode* of the combination of these two constituent principles, as well as in the proportional quantity of each; and by agitation in water, or long standing, that mode of combination may change. This we know to be the case with other substances, as with *milk*, from which, by standing only, *cream* is separated; which by agitation becomes *butter*. Also many substances, being at rest, putrefy, and thereby become quite different from what they were before. If this be the case with inflammable air, the water may imbibe either of the constituent parts, whenever any proportion of it is spontaneously separated from the rest; and should this ever be that phlogiston, with which air is but slightly overcharged, as by the burning of a candle, it will be recovered to a state in which a candle may burn in it again.

It will be observed, however, that it was only in one instance that I found that strong inflammable air, in its transition to a state in which it extinguishes a candle, would admit a candle to burn in it, and that was very faintly; that then the air was far from being pure, as appeared by the test of nitrous air; and that it was only from a particular quantity of inflammable air which I got from oak, and which had stood a long time in water, that I ever got air which was as pure as common air. Indeed, it is much more easy to account for the passing of inflammable air into a state in which it extinguishes candles, without any intermediate state, in which it will admit a candle to burn in it, than otherwise. This subject requires and deserves farther investigation. It will also be well worth while to examine what difference the agitation of air in natural or artificial *sea-water* will occasion.

Since acid air and phlogiston make inflammable air, and since inflammable air is convertible into air fit for respiration, it seems not to be improbable, that these two ingredients are the only essential principles of common air. For this change is produced by agitation in water only, without the addition of any fixed air, though this kind of air, like various other things of a foreign nature, may be combined with it.

Considering also what prodigious quantities of inflammable air are produced by the burning of small pieces of wood or pit-coal, it may not be improbable but that the *volcanos*, with which there are evident traces of almost the whole surface of the earth having been overspread, may have been the origin of our atmosphere, as well as (according to the opinion of some) of all the solid land.

The superfluous phlogiston of the air, in the state in which it issues from volcanos, may have been imbibed by the waters of the sea, which it is probable originally covered the surface of the earth, though part of it might have united with the acid vapour exhaled from the sea, and by this union have made a considerable and valuable addition to the common mass of air; and the remainder of this over-charge of phlogiston may have been imbibed by plants as soon as the earth was furnished with them.

That an acid vapour is really exhaled from the sea, by the heat of the sun, seems to be evident from the remarkably different states of the atmosphere, in this respect, in hot and cold climates. In Hudson's bay, and also in Russia, it is said, that metals hardly ever rust, whereas they are remarkably liable to rust in Barbadoes, and other islands between the tropics. See Ellis's Voyage, p. 288. This is also the case in places abounding with salt-springs, as Nantwich in Cheshire.

That mild air should consist of parts of so very different a nature as an acid vapour and phlogiston, one of which is so exceedingly corrosive, will not appear surprising to a chemist, who considers the very strong affinity which these two principles are known to have with each other, and the exceedingly different properties which substances composed by them possess. This is exemplified in common *sulphur*, which is as mild as air, and may be taken into the stomach with the utmost safety, though nothing can be more destructive than one of its constituent parts, separately taken, viz. oil of vitriol. Common air, therefore, notwithstanding its mildness, may be composed of similar principles, and be a real *sulphur*.

That the fixed air which makes part of the atmosphere is not presently imbibed by the waters of the sea, on which it rests, may be owing to the union which this kind of air also appears to be capable of forming with phlogiston. For fixed air is evidently of the nature of an acid; and it appears, in fact, to be capable of being combined with phlogiston, and thereby of constituting a species of air not liable to be imbibed by water. Phlogiston, however, having a stronger affinity with acid air, which I suppose to be the basis of common air, it is not surprising that, uniting with this, in preference to the fixed air, the latter should be precipitated, whenever a quantity of common air is made noxious by an over-charge of phlogiston.

The fixed air with which our atmosphere abounds may also be supplied by volcanos, from the vast masses of calcareous matter lodged in the earth, together with inflammable air. Also a part of it may be supplied from the fermentation of vegetables upon the surface of it. At present, as fast as it is precipitated and imbibed by one process, it may be set loose by others.

Whether there be, upon, the whole, an increase or a decrease of the general mass of the atmosphere is not easy to conjecture, but I should imagine that it rather increases. It is true that many processes contribute to a great visible diminution of common air, and that when by other processes it is restored to its former wholesomeness, it is not increased in its dimensions; but volcanos and fires still supply vast quantities of air, though in a state not yet fit for respiration; and it will have been seen in my experiments, that vegetable and animal substances, dissolved by putrefaction, not only emit phlogiston, but likewise yield a considerable quantity of permanent elastic air, overloaded indeed with phlogiston, as might be expected, but capable of being purified by those processes in nature by which other noxious air is purified.

That particles are continually detaching themselves from the surfaces of all solid bodies, even the metallic ones, and that these particles constitute the most permanent part of the atmosphere, as Sir Isaac Newton supposed, does not appear to me to be at all probable.

My readers will have observed, that not only is common air liable to be diminished by a mixture of nitrous air, but likewise air originally produced from inflammable air, and even from nitrous air itself, which never contained any fixed air. From this it may be inferred, that the whole of the diminution of common air by phlogiston is not owing to the precipitation of fixed air, but from a real contraction of its dimensions, in consequence of its union with phlogiston. Perhaps an accurate attention to the specific gravity of air procured from these different materials, and in these different states, may determine this matter, and assist us in investigating the nature of phlogiston.

In what *manner* air is diminished by phlogiston, independent of the precipitation of any of its constituent parts, is not easy to conceive; unless air thus diminished be heavier than air not diminished, which I did not find to be the case. It deserves, however, to be tried with more attention. That phlogiston should communicate absolute *levity* to the bodies with which it is combined, is a supposition that I am not willing to have recourse to, though it would afford an easy solution of this difficulty.

I have likewise observed, that a mouse will live almost as long in inflammable air, when it has been agitated in water, and even before it has been deprived of all its inflammability, as in common air; and yet that in this state it is not, perhaps, so much diminished by nitrous air as common air is. In this case, therefore, the diminution seems to have been occasioned by a contraction of dimensions, and not by a loss of any constituent part; so that the air is really better, that is, more fit for respiration, than, by the test of nitrous air, it would seem to be.

If this be the case (for it is not easy to judge with accuracy by experiments with small animals) nitrous air will be an accurate test of the goodness of *common air* only, that is, air containing a considerable proportion of fixed air. But this is the most valuable purpose for which a test of the goodness of air can be wanted. It will still, indeed, serve for a measure of the goodness of air that does not contain fixed air; but, a smaller degree of diminution in this case, must be admitted to be equivalent to a greater diminution in the other.

As I could never, by means of growing vegetables, bring air which had been thoroughly noxious to so pure a state as that a candle would burn in it, it may be suspected that something else besides *vegetation* is necessary to produce this effect. But it should be considered, that no part of the common atmosphere can ever be in this highly noxious state, or indeed in a state in which a candle will not burn in it; but that even air reduced to this state, either by candles actually burning out in it, or by breathing it, has never failed to be perfectly restored by vegetation, at least so far that candles would burn in it again, and, to all appearance, as well, and as long as ever; so that the growing vegetables, with which the surface of the earth is overspread, may, for any thing that appears to the contrary, be a cause of the purification of the atmosphere sufficiently adequate to the effect.

It may likewise be suspected, that since *agitation in water* injures pure common air, the agitation of the sea may do more harm than good in this respect. But it requires a much more violent and longer continued agitation of air in water than is ever occasioned by the waves of the sea to do the least sensible injury to it. Indeed a light agitation of air in *putrid water* injures it very materially; but if the water be sweet, this effect is not produced, except by a long and tedious operation, whereas it requires but a very short time, in comparison, to restore a quantity of any of the most noxious kinds of air to a very great degree of wholesomeness by the same process.

Dr. Hales found that he could breathe the same air much longer when, in the course of his respiration, it was made to pass through several folds of cloth dipped in vinegar, in a solution of sea-salt, or in salt of tartar, especially the last. Statical Essays, vol. 1. p. 266. The experiment is valuable, and well deserves to be repeated with a greater variety of circumstances. I imagine that the effect was produced by those substances, or by the *water* which they attracted from the air, imbibing the phlogistic matter discharged from the lungs. Perhaps the phlogiston may unite with the watery part of the atmosphere, in preference to any other part of it, and may by that means be more easily transferred to such salts as imbibe moisture.

Sir Isaac Newton defines *flame* to be *fumus candens*, considering all *smoke* as being of the same nature, and capable of ignition. But the smoke of common fuel consists of two very different things. That which rises first is mere *water*, loaded with some of the grosser parts of the fuel, and is hardly more capable of becoming red hot than water itself; but the other kind of smoke, which alone is capable of ignition, is properly *inflammable air*, which is also loaded with other heterogeneous matter, so as to appear like a very dense smoke. A lighted candle soon shews them to be essentially different from each other. For one of them instantly takes fire, whereas the other extinguishes a candle.

It is remarkable that gunpowder will take fire, and explode in all kinds of air, without distinction, and that other substances which contain *nitre* will burn freely in those circumstances. Now since nothing can burn, unless there be something at hand to receive the phlogiston, which is set loose in the act of ignition, I do not see how this fact can be accounted for, but by supposing that the acid of nitre, being peculiarly formed to unite with phlogiston, immediately receives it. And if the sulphur, which is thereby formed, be instantly decomposed again, as the chemists in general say, thence comes the explosion of gunpowder, which, however, requires the reaction of some incumbent atmosphere, and without which the materials will only *melt*, and be *dispersed* without explosion.

Nitrous air seems to consist of the nitrous acid vapour united to phlogiston, together, perhaps, with some small portion of the metallic calx; just as inflammable air consists of the vitriolic or marine acid, and the same phlogistic principle. It should seem, however, that phlogiston has a stronger affinity with the marine acid, if that be the basis of common air; for nitrous air being admitted to common air, it is immediately decomposed; probably by the phlogiston joining with the acid principle of the common air, while the fixed air which it contained is precipitated, and the acid of the nitrous air is absorbed by the water in which the mixture is made, or unites with any volatile alkali that happens to be at hand.

This, indeed, is hardly agreeable to the hypothesis of most chemists, who suppose that the nitrous acid is stronger than the marine, so as to be capable of dislodging it from any base with which it may be combined; but it agrees with my own experiments on marine acid air, which shew that, in many cases, this *weaker acid*, as it is called, is capable of separating both the vitriolic and the nitrous acids from the phlogiston with which they are combined.

On the other hand, the solution of metals in the different acids seems to shew, that the nitrous acid forms a closer union with phlogiston than the other two; because the air which is formed by the nitrous acid is not inflammable, like that which is produced by the oil of vitriol, or the spirit of salt. Also, the same weight of iron does not yield half the quantity of nitrous air that it does of inflammable.

The great diminution of nitrous air by phlogiston is not easily accounted for, unless we suppose that its superabundant acid, uniting more intimately with the phlogiston, constitutes a species of *sulphur* that is not easily perceived or catched; though, in the process with iron, and also in that with liver of sulphur, part of the redundant phlogiston forms such an union with the acid as gives it a kind of inflammability.

It appears to me to be very probable, that the spirit of nitre might be exhibited in the form of *air*, if it were possible to find any fluid by which it could be confined; but it unites with quicksilver as well as with water, so that when, by boiling the spirit of nitre, the fumes are driven through the glass tube, fig. 8, they instantly seize upon the quicksilver through which they are to be conveyed, and uniting with it, form a substance that stops up the tube: a circumstance which has more than once exposed me to very disagreeable accidents, in consequence of the bursting of the phials.

I do not know any inquiry more promising than the investigation of the properties of *nitre*, the *nitrous acid*, and *nitrous air*. Some of the most wonderful phenomena in nature are connected with them, and the subject seems to be fully within our reach.

§ 2. *Speculations arising from the consideration of the similarity of the ELECTRIC MATTER and PHLOGISTON.*

There is nothing in the history of philosophy more striking than the rapid progress of *electricity*. Nothing ever appeared more trifling than the first effects which were observed of this agent in nature, as the attraction and repulsion of straws, and other light substances. It excited more attention by the flashes of *light* which it exhibited. We were more seriously alarmed at the electrical *shock*, and the effects of the electrical *battery*; and we were astonished to the highest degree by the discovery of the similarity of electricity with *lightning*, and the *aurora borealis*, with the connexion it seems to have with *water-spouts*, *hurricanes*, and *earthquakes*, and also with the part that is probably assigned to it in the system of *vegetation*, and other the most important processes in nature.

Yet, notwithstanding all this, we have been, within a few years, more puzzled than ever with the electricity of the *torpedo*, and of the *anguille temblante* of Surinam, especially since that most curious discovery of Mr. Walsh's, that the former of these wonderful fishes has the power of giving a proper electrical shock; the electrical matter which proceeds from it performing a real circuit from one part of the animal to the other; while both the fish which performs this experiment and all its apparatus are plunged in water, which is known to be a conducting substance.

Perhaps, however, by considering this fact in connexion with a few others, and especially with what I have lately observed concerning the identity of electricity and phlogiston, a little light may be thrown upon this subject, in consequence of which we may be led to consider electricity in a still more important light. Many of my readers, I am aware, will smile at what I am going to advance; but the apprehension of this shall not interrupt my speculations, how chimerical soever they may be thought to be.

The facts, the consideration of which I would combine with that of the electricity of the torpedo, are the following.

First, The remarkable electricity of the feathers of a paroquet, observed by Mr. Hartmann, an account of which may be seen in Mr. Rozier's Journal for Sept. 1771. p. 69. This bird never drinks, but often washes itself; but the person who attended it having neglected to supply it with water for this purpose, its feathers appeared to be endued with a proper electrical virtue, repelling one another, and retaining their electricity a long time after they were plucked from the body of the bird, just as they would have done if they had received electricity from an excited glass tube.

Secondly, The electric matter directed through the body of any muscle forces it to contract. This is known to all persons who attend to what is called the electrical shock; which certainly occasions a proper *convulsion*, but has been more fully illustrated by Father Beccaria. See my *History of Electricity*, p. 402.

Lastly, Let it be considered that the proper nourishment of an animal body, from which the source and materials of all muscular motion must be derived, is probably some modification of phlogiston. Nothing will nourish that does not contain phlogiston, and probably in such a state as to be easily separated from it by the animal functions.

That the source of muscular motion is phlogiston is still more probable, from the consideration of the well known effects of vinous and spirituous liquors, which consist very much of phlogiston, and which instantly brace and strengthen the whole nervous and muscular system; the phlogiston in this case being, perhaps, more easily extricated, and by a less tedious animal process, than in the usual method of extracting it from mild aliments. Since, however, the mildest aliments do the same thing more slowly and permanently, that spirituous liquors do suddenly and transiently, it seems probable that their operation is ultimately the same.

This conjecture is likewise favoured by my observation, that respiration and putrefaction affect common air in the same manner, and in the same manner in which all other processes diminish air and make it noxious, and which agree in nothing but the emission of phlogiston. If this be the case, it should seem that the phlogiston which we take in with our aliment, after having discharged its proper function in the animal system (by which it probably undergoes some unknown alteration) is discharged as *effete* by the lungs into the great common *menstruum*, the atmosphere.

My conjecture suggested (whether supported or not) by these facts, is, that animals have a power of converting phlogiston, from the state in which they receive it in their nutriment, into that state in which it is called the electrical fluid; that the brain, besides its other proper uses, is the great laboratory and repository for this purpose; that by means of the nerves this great principle, thus exalted, is directed into the muscles, and forces them to act, in the same manner as they are forced into action when the electric fluid is thrown into them *ab extra*.

I farther suppose, that the generality of animals have no power of throwing this generated electricity any farther than the limits of their own system; but that the *torpedo*, and animals of a similar construction, have likewise the power, by means of an additional apparatus, of throwing it farther, so as to affect other animals, and other substances at a distance from them.

In this case, it should seem that the electric matter discharged from the animal system (by which it is probably more exhausted and fatigued than by ordinary muscular motion) would never return to it, at least so as to be capable of being made use of a second time, and yet if the structure of these animals be such as that the electric matter shall dart from one part of them only, while another part is left suddenly deprived of it, it may make a circuit, as in the Leyden phial.

As to the *manner* in which the electric matter makes a muscle contract, I do not pretend to have any conjecture worth mentioning. I only imagine that whatever can make the muscular fibres recede from one another farther than the parts of which they consist, must have this effect.

Possibly, the *light* which is said to proceed from some animals, as from cats and wild beasts, when they are in pursuit of their prey in the night, may not only arise, as it has hitherto been supposed to do, from the friction of their hairs or bristles, &c. but that violent muscular exertion may contribute to it. This may assist them occasionally to catch their prey; as glow-worms, and other insects, are provided with a constant light for that purpose, to the supply of which light their nutriment may also contribute.

I would not even say that the light which is said to have proceeded from some human bodies, of a particular temperament, and especially on some extraordinary occasions, may not have been of the electrical kind, that is, produced independently of friction, or with less friction than would have produced it in other persons; as in those cases related by Bartholin in his treatice *De luce animalium*. See particularly what he says concerning Theodore king of the Goths, p. 54, concerning Gonzaga duke of Mantua, p. 57, and Gothofred Antonius, p. 123: But I would not have my readers suppose that I lay much stress upon stories no better authenticated than these.

The electric matter in passing through non-conducting substances always emits *light*. This light I have been sometimes inclined to suspect might have been supplied from the substance through which it passes. But I find that after the electric spark has diminished a quantity of air as much as it possibly can, so that it has no more visible effect upon it, the electric light in that air is not at all lessened. It is probable, therefore, that electric light comes from the electric matter itself; and this being a modification of phlogiston, it is probable that *all light* is a modification of phlogiston also. Indeed, since no other substances besides such as contain phlogiston are capable of ignition, and consequently of becoming luminous, it was on this account pretty evident, prior to these deductions from electrical phenomena, that light and phlogiston are the same thing, in different forms or states.

It appears to me that *heat* has no more proper connexion with phlogiston than it has with water, or any other constituent part of bodies; but that it is a state into which the parts of bodies are thrown by their action and reaction with respect to one another; and probably (as the English philosophers in general have supposed) the heated state of bodies may consist of a subtle vibratory motion of their parts. Since the particles which constitute light are thrown from luminous bodies with such amazing velocity, it is evident that, whatever be the cause of such a projection, the reaction consequent upon it must be considerable. This may be sufficient not only to keep up, but also to increase the vibration of the parts of those bodies in which the phlogiston is not very firmly combined; and the difference between the substances which are called *inflammable* and others which also contain phlogiston may be this, that in the former the heat, or the vibration occasioned by the emission of their own phlogiston, may be sufficient to occasion the emission of more, till the whole be exhausted; that is, till the body be reduced to ashes. Whereas in bodies which are not inflammable, the heat occasioned by the emission of their own phlogiston may not be sufficient for this purpose, but an additional heat *ab extra* may be necessary.

Some philosophers dislike the term *phlogiston*; but, for my part, I can see no objection to giving that, or any other name, to a *real something*, the presence or absence of which makes so remarkable difference in bodies, as that of *metallic calces* and *metals*, *oil of vitriol* and *brimstone*, &c. and which may be transferred from one substance to another, according to certain known laws, that is, in certain definite circumstances. It is certainly hard to conceive how any thing that answers this description can be only a mere *quality*, or mode of bodies, and not *substance* itself, though incapable of being exhibited alone. At least, there can be no harm in giving this name to any *thing*, or any *circumstance* that is capable of producing these effects. If it should hereafter appear not to be a substance, we may change our phraseology, if we think proper.

On the other hand I dislike the use of the term *fire*, as a constituent principle of natural bodies, because, in consequence of the use that has generally been made of that term, it includes another thing or circumstance, viz. *heat*, and thereby becomes ambiguous, and is in danger of misleading us. When I use the term phlogiston, as a principle in the constitution of bodies, I cannot mislead myself or others, because I use one and the same term to denote only one and the same *unknown cause* of certain well-known effects. But if I say that *fire* is a principle in the constitution of bodies, I must, at least, embarrass myself with the distinction of fire *in a state of action*, and fire *inactive*, or quiescent. Besides I think the term phlogiston preferable to that of fire, because it is not in common use, but confined to philosophy; so that the use of it may be more accurately ascertained.

Besides, if phlogiston and the electric matter be the same thing, though it cannot be exhibited alone, in a *quiescent state*, it may be exhibited alone under one of its modifications, when it is in *motion*. And if light be also phlogiston, or some modification or subdivision of phlogiston, the same thing is capable of being exhibited alone in this other form also.

In my paper on the *conducting power of charcoal*, (See Philosophical Transactions, vol. 60. p. 221) I observed that there is a remarkable resemblance between metals and charcoal; as in both these substances there is an intimate union of phlogiston with an earthy base; and I said that, had there been any phlogiston in *water*, I should have concluded, that there had been no conducting power in nature, but in consequence of an union of this principle with some base; for while metals have phlogiston they conduct electricity, but when they are deprived of it they conduct no longer. Now the affinity which I have observed between phlogiston and water leads me to conclude that water, in its natural state, does contain some portion of phlogiston; and according to the hypothesis just now mentioned they must be intimately united, because water is not inflammable.

I think, therefore, that after this state of hesitation and suspence, I may venture to lay it down as a characteristic distinction between conducting and non-conducting substances, that the former contain phlogiston intimately united with some base, and that the latter, if they contain phlogiston at all, retain it more loosely. In what manner this circumstance facilitates the passing of the electric matter through one substance, and obstructs its passage through another, I do not pretend to say. But it is no inconsiderable thing to have advanced but *one step* nearer to an explanation of so very capital a distinction of natural bodies, as that into conductors and non-conductors of electricity.

I beg leave to mention in this place, as favourable to this hypothesis, a most curious discovery made very lately by Mr. Walsh, who being assisted by Mr. De Luc to make a more perfect vacuum in the double or arched barometer, by boiling the quicksilver in the tube, found that the electric spark or shock would no more pass through it, than through a stick of solid glass. He has also noted several circumstances that affect this vacuum in a very extraordinary manner. But supposing that vacuum to be perfect, I do not see how we can avoid inferring from the fact, that some *substance* is necessary to conduct electricity; and that it is not capable, by its own expansive power, of extending itself into spaces void of all matter, as has generally been supposed, on the idea of there being nothing to obstruct its passage.

Indeed if this was the case, I do not see how the electric matter could be retained within the body of the earth, or any of the planets, or solid orbs of any kind. In nature we see it make the most splendid appearance in the upper and thinner regions of the atmosphere, just as it does in a glass tube nearly exhausted; but if it could expand itself beyond that degree of rarity, it would necessarily be diffused into the surrounding vacuum, and continue and be condensed there, at least in a greater proportion than in or near any solid body, as Newton supposed concerning his *ether*.

If that mode of vibration which constitutes heat be the means of converting phlogiston from that state in which it makes a part of solid bodies, and eminently contributes to the firmness of their texture into that state in which it diminishes common air; may not that peculiar kind of vibration by which Dr. Hartley supposes the brain to be affected, and by which he endeavours to explain all the phenomena of sensation, ideas, and muscular motion, be the means by which the phlogiston, which is conveyed into the system by nutriment, is converted into that form or modification of it of which the electric fluid consists.

These two states of phlogiston may be conceived to resemble, in some measure, the two states of fixed air, viz. elastic, or non-elastic; a solid, or a fluid.

THE APPENDIX.

In this Appendix I shall present the reader with the communications of several of my friends on the subject of the preceding work. Among them I should with pleasure have inserted some curious experiments, made by Dr. Hulme of Halifax, on the air extracted from Buxton water, and on the impregnation of several fluids, with different kinds of air; but that he informs me he proposes to make a separate publication on the subject.

NUMBER I.

EXPERIMENTS made by Mr. Hey to prove that there is no OIL of VITRIOL in water impregnated with FIXED AIR.

It having been suggested, that air arising from a fermenting mixture of chalk and oil of vitriol might carry up with it a small portion of the vitriolic acid, rendered volatile by the act of fermentation; I made the following experiments, in order to discover whether the acidulous taste, which water impregnated with such air affords, was owing to the presence of any acid, or only to the fixed air it had absorbed.

EXPERIMENT I.

I mixed a tea-spoonful of syrup of violets with an ounce of distilled water, saturated with fixed air procured from chalk by means of the vitriolic acid; but neither upon the first mixture, nor after standing 24 hours, was the colour of the syrup at all changed, except by its simple dilution.

EXPERIMENT II.

A portion of the same distilled water, unimpregnated with fixed air, was mixed with the syrup in the same proportion: not the least difference in colour could be perceived betwixt this and the above-mentioned mixture.

EXPERIMENT III.

One drop of oil of vitriol being mixed with a pint of the same distilled water, an ounce of this water was mixed with a tea-spoonful of the syrup. This mixture was very distinguishable in colour from the two former, having a purplish cast, which the others wanted.

EXPERIMENT IV.

The distilled water impregnated with so small a quantity of vitriolic acid, having a more agreeable taste than when alone, and yet manifesting the presence of an acid by means of the syrup of violets; I subjected it to some other tests of acidity. It formed curds when agitated with soap, lathered with difficulty, and very imperfectly; but not the least ebullition could be discovered upon dropping in spirit of sal ammoniac, or solution of salt of tartar, though I had taken care to render the latter free from causticity by impregnating it with fixed air.

Experiment V.

The distilled water saturated with fixed air neither effervesced, nor shewed any clouds, when mixed with the fixed or volatile alkali.

Experiment VI.

No curd was formed by pouring this water upon an equal quantity of milk, and boiling them together.

Experiment VII.

When agitated with soap, this water produced curds, and lathered with some difficulty; but not so much as the distilled water mixed with vitriolic acid in the very small proportion above-mentioned. The same distilled water without any impregnation of fixed air lathered with soap without the least previous curdling. River-water, and a pleasant pump-water not remarkably hard, were compared with these. The former produced curds before it lathered, but not quite in so great a quantity as the distilled water impregnated with fixed air: the latter caused a stronger curd than any of the others above-mentioned.

Experiment VIII.

Apprehending that the fixed air in the distilled water occasioned the coagulation, or separation of the oily part of the soap, only by destroying the causticity of the *lixivium*, and thereby rendering the union less perfect betwixt that and the tallow, and not by the presence of any acid; I impregnated a fresh quantity of the same distilled water with fixed air, which had passed through half a yard of a wide barometer-tube filled with salt of tartar; but this water caused the same curdling with soap as the former had done, and appeared in every respect to be exactly the same.

Experiment IX.

Distilled water saturated with fixed air formed a white cloud and precipitation, upon being mixed with a solution of *saccharum saturni*. I found likewise, that fixed air, after passing through the tube filled with alkaline salt, upon being let into a phial containing a solution of the metalic salt in distilled water, caused a perfect separation of the lead, in the form of a white powder; for the water, after this precipitation, shewed no cloudiness upon a fresh mixture of the substances which had before rendered it opaque.

NUMBER II.

A Letter from Mr. Hey to Dr. Priestley, concerning the Effects of fixed Air applied by way of Clyster.

Leeds, Feb. 15th, 1772.

Reverend Sir,

Having lately experienced the good effects of fixed air in a putrid fever, applied in a manner, I believe not heretofore made use of, I thought it proper to inform you of the agreeable event, as the method of applying this powerful corrector of putrefaction took its rise principally from your observations and experiments on factitious air; and now, at your request, I send the particulars of the case I mentioned to you, as far as concerns the administration of this remedy.

January 8, 1772, Mr. Lightbowne, a young gentleman who lives with me, was seized with a fever, which, after continuing about ten days, began to be attended with those symptoms that indicate a putrescent state of the fluids.

18th, His tongue was black in the morning when I first visited him, but the blackness went off in the day-time upon drinking: He had begun to doze much the preceding day, and now he took little notice of those that were about him: His belly was loose, and had been so for some days: his pulse beat 110 strokes in a minute, and was rather low: he was ordered to take twenty-five grains of Peruvian bark with five of tormentil-root in powder every four hours, and to use red wine and water cold as his common drink.

19th, I was called to visit him early in the morning, on account of a bleeding at the nose which had come on: he lost about eight ounces of blood, which was of a loose texture: the hæmorrhage was suppressed, though not without some difficulty, by means of tents made of soft lint, dipped in cold water strongly impregnated with tincture of iron, which were introduced within the nostrils quite through to their posterior apertures; a method which has never yet failed me in like cases. His tongue was now covered with a thick black pellicle, which was not diminished by drinking: his teeth were furred with the same kind of sordid matter, and even the roof of his mouth and sauces were not free from it: his looseness and stupor continued, and he was almost incessantly muttering to himself: he took this day a scruple of the Peruvian bark with ten grains of tormentil every two or three hours: a starch clyster, containing a drachm of the compound powder of bole, without opium, was given morning and evening: a window was set open in his room, though it was a severe frost, and the floor was frequently sprinkled with vinegar.

20th, He continued nearly in the same state: when roused from his dozing, he generally gave a sensible answer to the questions asked him; but he immediately relapsed, and repeated his muttering. His skin was dry, and harsh, but without *petechiæ*. He sometimes voided his urine and *fæces* into the bed, but generally had sense enough to ask for the bed-pan: as he now nauseated the bark in substance, it was exchanged for Huxham's tincture, of which he took a table spoonful every two hours in a cup full of cold water: he drank sometimes a little of the tincture of roses, but his common liquors were red wine and water, or rice-water and brandy acidulated with elixir of vitriol: before drinking, he was commonly requested to rinse his mouth with water to which a little honey and vinegar had been added. His looseness rather increased, and the stools were watery, black, and fœtid: It was judged necessary to moderate this discharge, which seemed to sink him, by mixing a drachm of the *theriaca Andromachi* with each clyster.

21st. The same putrid symptoms remained, and a *subsultus tendinum* came on: his stools were more fœtid; and so hot, that the nurse assured me she could not apply her hand to the bed-pan, immediately after they were discharged, without feeling pain on this account: The medicine and clysters were repeated.

Reflecting upon the disagreeable necessity we seemed to lie under of confining this putrid matter in the intestines, lest the evacuation should destroy the *vis vitæ* before there was time to correct its bad quality, and overcome its bad effects, by the means we were using; I considered, that, if this putrid ferment could be more immediately corrected, a stop would probably be put to the flux, which seemed to arise from, or at least to be encreased by it; and the *fomes* of the disease would likewise be in a great measure removed. I thought nothing was so likely to effect this, as the introduction of fixed air into the alimentary canal, which, from the experiments of Dr. Macbride, and those you have made since his publication, appears to be the most powerful corrector of putrefaction hitherto known. I recollected what you had recommended to me as deserving to be tried in putrid diseases, I mean, the injection of this kind of air by way of clyster, and judged that in the present case such a method was clearly indicated.

The next morning I mentioned my reflections to Dr. Hird and Dr. Crowther, who kindly attended this young gentleman at my request, and proposed the following method of treatment, which, with their approbation, was immediately entered upon. We first gave him five grains of ipecacuanha, to evacuate in the most easy manner part of the putrid *colluvies*: he was then allowed to drink freely of brisk orange-wine, which contained a good deal of fixed air, yet had not lost its sweetness. The tincture of bark was continued as before; and the water which he drank along with it, was impregnated with fixed air from the atmosphere of a large vat of fermenting wort, in the manner I had learned from you. Instead of the astringent clyster, air alone was injected, collected from a fermenting mixture of chalk and oil of vitriol: he drank a bottle of orange-wine in the course of this day, but refused any other liquor except water and his medicine: two bladders full of air were thrown up in the afternoon.

23d. His stools were less frequent; their heat likewise and peculiar *fœtor* were considerably diminished; his muttering was much abated, and the *subsultus tendinum* had left him. Finding that part of the air was rejected when given with a bladder in the usual way, I contrived a method of injecting it which was not so liable to this inconvenience. I took the flexible tube of that instrument which is used for throwing up the fume of tobacco, and tied a small bladder to the end of it that is connected with the box made for receiving the tobacco, which I had previously taken off from the tube: I then put some bits of chalk into a six ounce phial until it was half filled; upon these I poured such a quantity of oil of vitriol as I thought capable of saturating the chalk, and immediately tied the bladder, which I had fixed to the tube, round the neck of the phial: the clyster-pipe, which was fastened to the other end of the tube, was introduced into the *anus* before the oil of vitriol was poured upon the chalk. By this method the air passed gradually into the intestines as it was generated; the rejection of it was in a great measure prevented; and the inconvenience of keeping the patient uncovered during the operation was avoided.

24th, He was so much better, that there seemed to be no necessity for repeating the clysters: the other means were continued. The window of his room was now kept shut.

25th, All the symptoms of putrescency had left him; his tongue and teeth were clean; there remained no unnatural blackness or *fœtor* in his stool, which had now regained their proper consistence; his dozing and muttering were gone off; and the disagreeable odour of his breath and perspiration was no longer perceived. He took nourishment to-day, with pleasure; and, in the afternoon, sat up an hour in his chair.

His fever, however, did not immediately leave him; but this we attributed to his having caught cold from being incautiously uncovered, when the window was open, and the weather extremely severe; for a cough, which had troubled him in some degree from the beginning, increased, and he became likewise very hoarse for several days, his pulse, at the same time, growing quicker: but these complaints also went off, and he recovered, without any return of the bad symptoms above-mentioned.

I am, Reverend Sir,

Your obliged humble Servant,

WM. HEY.

POSTSCRIPT

October 29, 1772.

Fevers of the putrid kind have been so rare in this town, and in its neighbourhood, since the commencement of the present year, that I have not had an opportunity of trying again the effects of fixed air, given by way of clyster, in any case exactly similar to Mr. Lightbowne's. I have twice given water saturated with fixed air in a fever of the putrescent kind, and it agreed very well with the patients. To one of them the aërial clysters were administred, on account of a looseness, which attended the fever, though the stools were not black, nor remarkably hot or fœtid.

These clysters did not remove the looseness, though there was often a greater interval than usual betwixt the evacuations, after the injection of them. The patient never complained of any uneasy distention of the belly from the air thrown up, which, indeed, is not to be wondered at, considering how readily this kind of air is absorbed by aqueous and other fluids, for which sufficient time was given, by the gradual manner of injecting it. Both those patients recovered though the use of fixed air did not produce a crisis before the period at which such fevers usually terminate. They had neither of them the opportunity of drinking such wine as Mr. Lightbowne took, after the use of fixed air was entered upon; and this, probably, was some disadvantage to them.

I find the methods of procuring fixed air, and impregnating water with it, which you have published, are preferable to those I made use of in Mr. Lightbowne's case.

The flexible tube used for conveying the fume of tobacco into the intestines, I find to be a very convenient instrument in this case, by the method before-mentioned (only adding water to the chalk, before the oil of vitriol is instilled, as you direct) the injection of air may be continued at pleasure, without any other inconvenience to the patient, than what may arise from his continuing in one position during the operation, which scarcely deserves to be mentioned, or from the continuance of the clyster-pipe within the anus, which is but trifling, if it be not shaken much, or pushed against the rectum.

When I said in my letter, that fixed air appeared to be the greatest corrector of putrefaction hitherto known, your philosophical researches had not then made you acquainted with that most remarkably antiseptic property of nitrous air. Since you favoured me with a view of some astonishing proofs of this, I have conceived hopes, that this kind of air may likewise be applied medicinally to great advantage.

W. H.

NUMBER III.

Observations on the MEDICINAL USES of FIXED AIR. By THOMAS PERCIVAL, M. D. Fellow of the ROYAL SOCIETY, and of the SOCIETY of ANTIQUARIES in LONDON.

These Observations on the MEDICINAL USES OF FIXED AIR have been before published in the Second Volume of my Essays; but are here reprinted with considerable additions. They form a part of an experimental inquiry into this interesting and curious branch of Physics; in which the friendship of Dr. Priestley first engaged me, in concert with himself.

Manchester, March 16, 1774.

In a course of Experiments, which is yet unfinished, I have had frequent opportunities of observing that FIXED AIR may in no inconsiderable quantity be breathed without danger or uneasiness. And it is a confirmation of this conclusion, that at Bath, where the waters copiously exhale this mineral spirit,[15] the bathers inspire it with impunity. At Buxton also, where the Bath is in a close vault, the effects of such *effluvia*, if noxious, must certainly be perceived.

Encouraged by these considerations, and still more by the testimony of a very judicious Physician at Stafford, in favour of this powerful antiseptic remedy, I have administered fixed air in a considerable number of cases of the PHTHISIS PULMONALIS, by directing my patients to inspire the steams of an effervescing mixture of chalk and vinegar; or what I have lately preferred, of vinegar and potash. The hectic fever has in several instances been considerably abated, and the matter expectorated has become less offensive, and better digested. I have not yet been so fortunate in any one case, as to effect a cure; although the use of mephitic air has been accompanied with proper internal medicines. But Dr. Withering, the gentleman referred to above, informs me, that he has been more successful. One Phthisical patient under his care has by a similar course intirely recovered; another was rendered much better; and a third, whose case was truly deplorable, seemed to be kept alive by it more than two months. It may be proper to observe that fixed air can only be employed with any prospect of success, in the latter stages of the *phthisis pulmonalis*, when a purulent expectoration takes place. After the rupture and discharge of a VOMICA also, such a remedy promises to be a powerful palliative. Antiseptic fumigations and vapours have been long employed, and much extolled in cases of this kind. I made the following experiment, to determine whether their efficacy, in any degree, depends on the separation of fixed air from their substance.

One end of a bent tube was fixed in a phial full of lime-water; the other end in a bottle of the tincture of myrrh. The junctures were carefully luted, and the phial containing the tincture of myrrh was placed in water, heated almost to the boiling point, by the lamp of a tea-kettle. A number of air-bubbles were separated, but probably not of the mephitic kind, for no precipitation ensued in the lime water. This experiment was repeated with the *tinct. tolutanæ, ph. ed.* and with *sp, vinos. camp.* and the result was entirely the same. The medicinal action therefore of the vapours raised from such tinctures, cannot be ascribed to the extrication of fixed air; of which it is probable bodies are deprived by *chemical solution* as well as by *mixture*.

If mephitic air be thus capable of correcting purulent matter in the lungs, we may reasonably infer it will be equally useful when applied externally to foul ULCERS. And experience confirms the conclusion. Even the sanies of a CANCER, when the carrot poultice failed, has been sweetened by it, the pain mitigated, and a better digestion produced. The cases I refer to are now in the Manchester infirmary, under the direction of my friend Mr. White, whose skill as a surgeon, and abilities as a writer are well known to the public.

Two months have elapsed since these observations were written,[16] and the same remedy, during that period, has been assiduously applied, but without any further success. The progress of the cancers seems to be checked by the fixed air; but it is to be feared that a cure will not be effected. A palliative remedy, however, in a disease so desperate and loathsome, may be considered as a very valuable acquisition. Perhaps NITROUS AIR might be still more efficacious. This species of factitious air is obtained from all the metals except zinc, by means of the nitrous acid; and Dr. Priestley informs me, that as a sweetener and antiseptic it far surpasses fixed air. He put two mice into a quantity of it, one just killed, the other offensively putrid. After twenty-five days they were both perfectly sweet.

In the ULCEROUS SORE THROAT much advantage has been experienced from the vapours of effervescing mixtures drawn into the *fauces*[17]. But this remedy should not supersede the use of other antiseptic applications.[18]

A physician[19] who had a very painful APTHOUS ULCER at the point of his tongue, found great relief, when other remedies failed, from the application of fixed air to the part affected. He held his tongue over an effervescing mixture of potash and vinegar; and as the pain was always mitigated, and generally removed by this vaporisation, he repeated it, whenever the anguish arising from the ulcer was more than usually severe. He tried a combination of potash and oil of vitriol well diluted with water; but this proved stimulant and increased his pain; probably owing to some particles of the acid thrown upon the tongue, by the violence of the effervescence. For a paper stained with the purple juice of radishes, when held at an equal distance over two vessels, the one containing potash and vinegar, the other the same alkali and *Spiritus vitrioli tenuis*, was unchanged by the former, but was spotted with red, in various parts, by the latter.

In MALIGNANT FEVERS wines abounding with fixed air may be administered, to check the septic ferment, and sweeten the putrid *colluvies* in the *primæ viæ*. If the laxative quality of such liquors be thought an objection to the use of them, wines of a greater age may be given, impregnated with mephitic air, by a simple but ingenious contrivance of my friend Dr. Priestley.[20]

The patient's common drink might also be medicated in the same way. A putrid DIARRHŒA frequently occurs in the latter stage of such disorder, and it is a most alarming and dangerous symptom. If the discharge be stopped by astringents, a putrid *fomes* is retained in the body, which aggravates the delirium and increases the fever. On the contrary, if it be suffered to take its course, the strength of the patient must soon be exhausted, and death unavoidably ensue. The injection of mephitic air into the intestines, under these circumstances, bids fair to be highly serviceable. And a case of this deplorable kind, has lately been communicated to me, in which the vapour of chalk and oil of vitriol conveyed into the body by the machine employed for tobacco clysters, quickly restrained the *diarrhœa*, corrected the heat and fœtor of the stools, and in two days removed every symptom of danger[21]. Two similar instances of the salutary effects of mephitic air, thus administered, have occurred also in my own practice, the history of which I shall briefly lay before the reader. May we not presume that the same remedy would be equally useful in the DYSENTERY? The experiment is at least worthy of trial.

Mr. W——, aged forty-four years, corpulent, inactive, with a short neck, and addicted to habits of intemperance, was attacked on the 7th of July 1772, with symptoms which seemed to threaten an apoplexy. On the 8th, a bilious looseness succeeded, with a profuse hœmorrhage from the nose. On the 9th, I was called to his assistance. His countenance was bloated, his eyes heavy, his skin hot, and his pulse hard, full, and oppressed. The diarrhœa continued; his stools were bilious and very offensive; and he complained of griping pains in his bowels. He had lost, before I saw him, by the direction of Mr. Hall, a surgeon of eminence in Manchester, eight ounces of blood from the arm, which was of a lax texture; and he had taken a saline mixture every sixth hour. The following draught was prescribed, and a dose of rhubarb directed to be administered at night.

> ℞. *Aq. Cinnam. ten.* ℥j.
>
> *Succ. Limon. recent.* ℥ß.
>
> *Salis Nitri gr. xij. Syr. è Succo Limon. ʒj. M. f. Haust.*
> *4tis horis sumendus.*

July 11. The *Diarrhœa* was more moderate; his griping pains were abated; and he had less stupor and dejection in his countenance. Pulse 90, not so hard or oppressed. As his stools continued to be fœtid, the dose of rhubarb was repeated; and instead of simple cinnamon-water, his draughts were prepared with an infusion of columbo root.

12. The *Diarrhœa* continued; his stools were involuntary; and he discharged in this way a quantity of black, grumous, and fœtid blood. Pulse hard and quick; skin hot; tongue covered with a dark fur; abdomen swelled; great stupor. Ten grains of columbo root, and fifteen of the *Gummi rubrum astringens* were added to each draught. Fixed air, under the form of clysters, was injected every second or third hour; and directions were given to supply the patient plentifully with water, artificially impregnated with mephitic air. A blister was also laid between his shoulders.

13. The Diarrhœa continued, with frequent discharges of blood; but the stools had now lost their fœtor. Pulse 120; great flatulence in the bowels, and fulness in the belly. The clysters of fixed air always diminished the tension of the *Abdomen*, abated flatulence, and made the patient more easy and composed for some time after their injection. They were directed to be continued, together with the medicated water. The nitre was omitted, and a scruple of the *Confect. Damocratis* was given every fourth hour, in an infusion of columbo root.

14. The Diarrhœa was how checked, His other symptoms continued as before. Blisters were applied to the arms; and a drachm and a half of the *Tinctura Serpentariæ* was added to each draught.

15. His pulse was feeble, quicker and more irregular. He dosed much; talked incoherently; and laboured under a slight degree of *Dyspnœa*. His urine, which had hitherto assumed no remarkable appearance, now became pale. Though he discharged wind very freely, his belly was much swelled, except for a short time after the injection of the air-clysters. The following draughts were then prescribed.

> ℞ *Camphoræ mucilag. G. Arab, solutæ gr. viij. Infus. Rad. Columbo Tinct, Serpent. ʒij Confect. Card. Syr. è Cort. Aurant ʒi m. f. Haust. 4tis horis sumendus.*

Directions were given to foment his feet frequently with vinegar and warm water.

16. He has had no stools since the 14th. His *Abdomen* is tense. No change in the other symptoms. The *Tinct. Serpent.* was omitted in his draughts, and an equal quantity of *Tinct. Rhœi Sp.* substituted in its place.

In the evening he had a motion to stool, of which he was for the first time so sensible, as to give notice to his attendants. But the discharge, which was considerable and slightly offensive, consisted almost entirely of blood, both in a coagulated and in a liquid state. His medicines were therefore varied as follows:

> ℞. *Decoct. Cort. Peruv. iss Tinct. Cort. ejusd. ʒij. Confect. Card. j Gum. Rubr. Astring. gr. xv. Pulv. Alnmin. gr. vij. m. f. Haustus 4tis horis fumendus.*

Red Port wine was now given more freely in his medicated water; and his nourishment consisted of sago and salep.

In this state, with very little variation, he continued for several days; at one time ostive, and at another discharging small quantities of fæces, mixed with grumous blood. The air-clysters were continued, and the astringents omitted.

20. His urine was now of an amber colour, and deposited a slight sediment. His pulse was more regular, and although still very quick, abated in number ten strokes in a minute. His head was less confused, and his sleep seemed to be refreshing. No blood appeared in his stools, which were frequent, but small in quantity; and his *Abdomen* was less tense than usual. He was extremely deaf; but gave rational answers to the few questions which were proposed to him; and said he felt no pain.

21. He passed a very restless night; his delirium recurred; his pulse beat 125 strokes in a minute; his urine was of a deep amber colour when first voided; but when cold assumed the appearance of cow's whey. The *Abdomen* was not very tense, nor had he any further discharge of blood.

Directions were given to shave his head, and to wash it with a mixture of vinegar and brandy; the quantity of wine in his drink was diminished; and the frequent use of the pediluvium was enjoined. The air-clysters were discontinued, as his stools were not offensive, and his *Abdomen* less distended.

22. His pulse was now small, irregular, and beat 130 strokes in a minute. The *Dyspnœa* was greatly increased; his skin was hot, and bedewed with a clammy moisture; and every symptom seemed to indicate the approach of death. In this state he continued till evening, when he recruited a little. The next day he had several slight convulsions. His urine which was voided plentifully, still put on the appearance of whey when cold. Cordial and antispasmodic draughts, composed of camphor, tincture of castor, and *Sp. vol. aromat.* were now directed; and wine was liberally administered.

24. He rose from his bed, and by the assistance of his attendants walked across the chamber. Soon after he was seized with a violent convulsion, in which he expired.

To adduce a case which terminated fatally as a proof of the efficacy of any medicine, recommended to the attention of the public, may perhaps appear singular; but cannot be deemed absurd, when that remedy answered the purposes for which it was intended. For in the instance before us; fixed air was employed, not with an expectation that it would cure the fever, but to obviate the symptoms of putrefaction, and to allay the uneasy irritation in the bowels. The disease was too malignant, the nervous system too violently affected, and the strength of the patient too much exhausted by the discharges of blood which he suffered, to afford hopes of recovery from the use of the most powerful antiseptics.

But in the succeeding case the event proved more fortunate.

Elizabeth Grundy, aged seventeen, was attacked on the 10th of December 1772, with the usual symptoms of a continued fever. The common method of cure was pursued; but the disease increased, and soon assumed a putrid type.

On the 23d I found her in a constant delirium, with a *subsultus tendinum*. Her skin was hot and dry, her tongue black, her thirst immoderate, and her stools frequent, extremely offensive, and for the most part involuntary. Her pulse beat 130 strokes in a minute; she dosed much; and was very deaf. I directed wine to be administered freely; a blister to be applied to her back;

the *pediluvium* to be used several times in the day; and mephitic air to be injected under the form of a clyster every two hours. The next day her stools were less frequent, had lost their fœtor, and were no longer discharged involuntarily; her pulse was reduced to 110 strokes in the minute; and her delirium was much abated. Directions were given to repeat the clysters, and to supply the patient liberally with wine. These means were assiduously pursued several days; and the young woman was so recruited by the 28th, that the injections were discontinued. She was now quite rational, and not averse to medicine. A decoction of Peruvian bark was therefore prescribed, by the use of which she speedily recovered her health.

I might add a third history of a putrid disease, in which the mephitic air is now under trial, and which affords the strongest proof both of the *antiseptic*, and of the *tonic* powers of this remedy; but as the issue of the case remains yet undetermined (though it is highly probable, alas! that it will be fatal) I shall relate only a few particulars of it. Master D. a boy of about twelve years of age, endowed with an uncommon capacity, and with the most amiable dispositions, has laboured many months under a hectic fever, the consequence of several tumours in different parts of his body. Two of these tumours were laid open by Mr. White, and a large quantity of purulent matter was discharged from them. The wounds were very properly treated by this skilful surgeon, and every suitable remedy, which my best judgment could suggest, was assiduously administered. But the matter became sanious, of a brown colour, and highly putrid. A *Diarrhœa* succeeded; the patient's stools were intolerably offensive, and voided without his knowledge. A black fur collected about his teeth; his tongue was covered with *Aphthœ*; and his breath was so fœtid, as scarcely to be endured. His strength was almost exhausted; a *subsultus tendinum* came on; and the final period of his sufferings seemed to be rapidly approaching. As a last, but almost hopeless, effort, I advised the injection of clysters of mephitic air. These soon corrected the fœtor of the patient's stools; restrained his *Diarrhœa*; and seemed to recruit his strength and spirits. Within the space of twenty-four hours his wounds assumed a more favourable appearance; the matter discharged from them became of a better colour and consistence; and was no longer so offensive to the smell. The use of this remedy has been continued several days, but is now laid aside. A large tumour is suddenly formed under the right ear; swallowing is rendered difficult and painful; and the patient refuses all food and medicine. Nourishing clysters are directed; but it is to be feared that these will renew the looseness, and that this amiable youth will quickly sink under his disorder[22].

The use of *wort* from its saccharine quality, and disposition to ferment, has lately been proposed as a remedy for the Sea Scurvy. Water or other liquors, already abounding with fixed air in a separate state, should seem to be better adapted to this purpose; as they will more quickly correct the putrid disposition of the fluids, and at the same time, by their gentle stimulus[23] increase the powers of digestion, and give new strength to the whole system.

Dr. Priestley, who suggested both the idea and the means of executing it, has under the sanction of the College of Physicians, proposed the scheme to the Lords of the Admiralty, who have ordered trial to be made of it, on board some of his Majesty's ships of war. Might it not however give additional efficacy to this remedy, if instead of simple water, the infusion of malt were to be employed?

I am persuaded such a medicinal drink might be prescribed also with great advantage in scrophulous complaints, when not attended with a hectic fever; and in other disorders in which a general acrimony prevails, and the crasis of the blood is destroyed. Under such circumstances, I have seen *vibices* which spread over the body, disappear in a few days from the use of wort.

A gentleman who is subject to a scorbutic eruption in his face, for which he has used a variety of remedies with no very beneficial effect, has lately applied the fumes of chalk and oil of vitriol to the parts affected. The operation occasions great itching and pricking in the skin, and some degree of drowsiness, but evidently abates the serous discharge, and diminishes the eruption. This patient has several symptoms which indicate a genuine scorbutic Diathesis; and it is probable that fixed air, taken internally, would be an useful medicine in this case.

The saline draughts of Riverius are supposed to owe their antiemetic effects to the air, which is separated from the salt of wormwood during the act of effervescence. And the tonic powers of many mineral waters seem to depend on this principle. I was lately desired to visit a lady who had most severe convulsive REACHINGS. Various remedies had been administered without effect, before I saw her. She earnestly desired a draught of malt liquor, and was indulged with half a pint of Burton beer in brisk effervescence. The vomitings ceased immediately, and returned no more. Fermenting liquors, it is well known, abound with fixed air. To this, and to the cordial quality of the beer, the favourable effect which it produced, may justly be ascribed. But I shall exceed my design by enlarging further on this subject. What has been advanced it is hoped, will suffice to excite the attention of physicians to a remedy which is capable of being applied to so many important medicinal purposes.

NUMBER IV.

Extract of a Letter from William Falconer, M.D. of Bath.

Jan 6, 1774,

Reverend Sir,

I once observed the same taste you mention (Philosophical Transactions, p. 156. of this Volume, p. 35.) viz. like tar water, in some water that I impregnated with fixed air about three years ago. I did not then know to what to attribute it, but your experiment seems to clear it up. I happened to have spent all my acid for raising effervescence, and to supply its place I used a bottle of dulcified spirit of nitre, which I knew was greatly under-saturated with spirit of wine; from which, as analogous to your observation, I imagine the effect proceeded.

As[24] to the coagulation of the blood of animals by fixed Air, I fear it will scarce stand the test of experiment, as I this day gave it, I think, a fair trial, in the following manner.

A young healthy man, at 20 years old, received a contusion by a fall, was instantly carried to a neighbouring surgeon, and, at my request, bled in the following manner.

I inserted a glass funnel into the neck of a large clear phial about ℥x. contents, and bled him into it to about ℥viii. By these means the blood was exposed to the air as little a time as possible, as it flowed into the bottle as it came from the orifice.

As soon as the quantity proposed was drawn, the bottle was carefully corked, and brought to me. It was then quite fluid, nor was there the least separation of its parts.

On the surface of this I conveyed several streams of fixed air (having first placed the bottle with the blood in a bowl of water, heated as nearly to the human heat as possible) from the mixture of the vitriolic acid and lixiv. tartar, which I use preferably to other alkalines, as being (as Dr. Cullen observes) in the mildest state, and therefore most likely to generate most air.

I shook the phial often, and threw many streams of air on the blood, as I have often practised with success for impregnating water; but could not perceive the smallest signs of coagulation, although it stood in an atmosphere of fixed air 20 minutes or more. I then uncorked the bottles, and poured off about ℥ii to which I added about 6 or 7 gtts of spirit of vitriol, which coagulated it immediately. I set the remainder in a cold place and it coagulated, as near as I could judge, in the same time that blood would have done newly drawn from the vein.

P. 82. Perhaps the circumilance of putrid vegetables yielding all fixed and no inflammable air may be the causes of their proving so antiseptic, even when putrid, as appears by Mr. Alexander's Experiments.

P. 86. Perhaps the putrid air continually exhaled may be one cause of the luxuriancy of plants growing on dunghills or in very rich soils.

P. 146. Your observation that inflammable air consists of the union of some acid vapour with phlogiston, puts me in mind of an old observation of Dr. Cullen, that the oil separated from soap by an acid was much more inflammable than before, resembling essential oil, and soluble in V. sp.

I have tried fixed air as an antiseptic taken in by respiration, but with no great success. In one case it seemed to be of service, in two it seemed indifferent, and in one was injurious, by exciting a cough.

NUMBER V.

Extract of a Letter from Mr. William Bewley, of Great Massingham, Norfolk.

March 23, 1774.

Dear Sir,

When I first received your paper, I happened to have a process going on for the preparation of *nitrous ether*, without distillation.[25] I had heretofore always taken for granted that the elastic fluid generated in that preparation was *fixed* air: but on examination I found this combination of the nitrous acid with inflammable spirits, produced an elastic fluid that had the same general properties with the air that you unwillingly, though very properly, in my opinion, term *nitrous*; as I believe it is not to be procured without employing the *nitrous* acid, either in a simple state, or compounded, as in *aqua regia*. I shall suggest, however, by and by some doubts with respect to it's title to the appellation of *air*.

Water impregnated with your nitrous air *certainly*, as you suspected from it's taste, contains the nitrous acid. On saturating a quantity of this water with a fixed alcali, and then evaporating, &c. I have procured two chrystals of nitre. But the principal observations that have occurred to me on the subject of nitrous air are the following. My experiments have been few and made by snatches, under every disadvantage as to apparatus, &c. and with frequent interruptions; and yet I think they are to be depended upon.

My first remark is, that nitrous air does not give water a sensibly acid impregnation, unless it comes into contact, or is mixed with a portion of common or atmospherical air: and my second, that nitrous air principally consists of the nitrous acid itself, reduced to the state of a *permanent* vapour not condensable by cold, like other vapours, but which requires the presence and admixture of common air to restore it to its primitive state of a liquid. I am beholden for this idea, you will perceive, to your own very curious discovery of the true nature of Mr. Cavendish's *marine* vapour.

When I first repeated your experiment of impregnating water with nitrous air, the water, I must own tasted acid; as it did in one, or perhaps two trials afterwards; but, to my great astonishment, in all the following experiments, though some part of the factitious air, or vapour, was visibly absorbed by the water, I could not perceive the latter to have acquired any sensible acidity. I at length found, however, that I could render this same water *very* acid, by means only of the nitrous air already included in the phial with it. Taking the inverted phial out of the water, I remove my finger from the mouth of it, to admit a little of the common air, and instantly replace my finger. The redness, effervescence, and diminution take place. Again taking off my finger, and instantly replacing it, more common, air rushes in, and the same phenomena recur. The process sometimes requires to be seven or eight times repeated, before the whole of the nitrous *vapour* (as I shall venture to call it) is condensed into nitrous *acid*, by the successive entrance of fresh parcels of common air after each effervescence; and the water becomes evidently more and more acid after every such fresh admission of the external air, which at length ceases to enter, when the whole of the vapour has been condensed. No agitation of the water is requisite, except a gentle motion, just sufficient to rince the sides of the phial, in order to wash off the condensed vapour.

The acidity which you (and I likewise, at first) observed in the water agitated with nitrous air *alone*, I account for thus. On bringing the phial to the mouth, the common air meeting with the nitrous vapour in the neck of the phial, condenses it, and impregnates the water with the acid, in the very act of receiving it upon the tongue. On stopping the mouth of the phial with my tongue for a short time and afterwards withdrawing it a very little, to suffer the common air to rush past it into the phial, the sensation of acidity has been sometimes intolerable: but taking a large gulph of the water at the same time, it has been found very slightly acid.—The following is one of the methods by which I have given water a very strong acid impregnation, by means of a mixture of nitrous and common air.

Into a small phial, containing only common air, I force a quantity of nitrous air at random, out of a bladder, and instantly clap my finger on the mouth of the bottle. I then immerse the neck of it into water, a small quantity of which I suffer to enter, which squirts into it with violence; and immediately replacing my finger, remove the phial. The water contained in it is already *very* acid, and it becomes more and more so (if a sufficient quantity of nitrous air was at first thrown in) on alternately stopping the mouth of the phial, and opening it, as often as fresh air will enter.

Since I wrote the above, I have frequently converted a small portion of water in an ounce phial into a weak *Aqua fortis*, by repeated mixtures of common and nitrous air; throwing in alternately the one or the other, according to the circumstances; that is, as long as there was a superabundance of nitrous air, suffering the common air to enter and condense it; and, when that was effected, forcing in more nitrous air from the bladder, to the common air which now predominated in the phial—and so alternately. I have wanted leisure, and conveniences, to carry on this process to its *maximum*, or to execute it in a different and better manner; but from what I have done, I think we may conclude that nitrous air consists principally of the nitrous acid, phlogisticated, or otherwise so modified, by a previous commenstruation with metals, inflammable spirits, &c. as to be reduced into a durably elastic vapour: and that, in order to deprive it of its elasticity, and restore it to its former state, an addition of common air is requisite, and, as I suspect, of water likewise, or some other fluid: as in the course of my few trials, I have not yet been able to condense it in a perfectly dry bottle.

NUMBER VI.

A Letter from Dr. Franklin.

Craven Street, April 10, 1774.

Dear Sir,

In compliance with your request, I have endeavoured to recollect the circumstances of the American experiments I formerly mentioned to you, of raising a flame on the surface of some waters there.

When I passed through New Jersey in 1764, I heard it several times mentioned, that by applying a lighted candle near the surface of some of their rivers, a sudden flame Would catch and spread on the water, continuing to burn for near half a minute. But the accounts I received were so imperfect that I could form no guess at the cause of such an effect, and rather doubted the truth of it. I had no opportunity of seeing the experiment; but calling to see a friend who happened to be just returned home from making it himself, I learned from him the manner of it; which was to choose a shallow place, where the bottom could be reached by a walking-stick, and was muddy; the mud was first to be stirred with the stick, and when a number of small bubbles began to arise from it, the candle was applied. The flame was so sudden and so strong, that it catched his ruffle and spoiled it, as I saw. New-Jersey having many pine-trees in different parts of it, I then imagined that something like a volatile oil of turpentine might be mixed with the waters from a pine-swamp, but this supposition did not quite satisfy me. I mentioned the fact to some philosophical friends on my return to England, but it was not much attended to. I suppose I was thought a little too credulous.

In 1765, the Reverend Dr. Chandler received a letter from Dr. Finley, President of the College in that province, relating the same experiment. It was read at the Royal Society, Nov. 21, of that year, but not printed in the Transactions; perhaps because it was thought too strange to be true, and some ridicule might be apprehended if any member should attempt to repeat it in order to ascertain or refute it. The following is a copy of that account.

"A worthy gentleman, who lives at a few miles distance, informed me that in a certain small cove of a mill-pond, near his house, he was surprized to see the surface of the water blaze like inflamed spirits. I soon after went to the place, and made the experiment with the same success. The bottom of the creek was muddy, and when stirred up, so as to cause a considerable curl on the surface, and a lighted candle held within two or three inches of it, the whole surface was in a blaze, as instantly as the vapour of warm inflammable spirits, and continued, when strongly agitated, for the space of several seconds. It was at first imagined to be peculiar to that place; but upon trial it was soon found, that such a bottom in other places exhibited the same phenomenon. The discovery was accidentally made by one belonging to the mill."

I have tried the experiment twice here in England, but without success. The first was in a slow running water with a muddy bottom. The second in a stagnant water at the bottom of a deep ditch. Being some time employed in stirring this water, I ascribed an intermitting fever, which seized me a few days after, to my breathing too much of that foul air which I stirred up from the bottom, and which I could not avoid while I stooped in endeavouring to kindle it.—The discoveries you have lately made of the manner in which inflammable air is in some cases produced, may throw light on this experiment, and explain its succeeding in some cases, and not in others. With the highest esteem and respect,

I am, Dear Sir,

Your most obedient humble servant,

B. FRANKLIN.

NUMBER VII.

Extract of a Letter from Mr. HENRY *of* Manchester.

It is with great pleasure I hear of your intended publication *on air*, and I beg leave to communicate to you an experiment or two which I lately made.

Dr. Percival had tried, without effect, to dissolve lead in water impregnated with fixed air. I however thought it probable, that the experiment might succeed with nitrous air. Into a quantity of water impregnated with it, I put several pieces of sheet-lead, and suffered them, after agitation, to continue immersed about two hours. A few drops of vol. tincture of sulphur changed the water to a deep orange colour, but not so deep as when the same tincture was added to a glass of the same water, into which one drop of a solution of sugar of lead had been instilled. The precipitates of both in the morning, were exactly of the same kind; and the water in which the lead had been infused all night, being again tried by the same test, gave signs of a still stronger saturnine impregnation—Whether the nitrous air acts as an acid on the lead, or in the same manner that fixed air dissolves iron, I do not pretend to determine. Syrup of violets added to the nitrous water became of a pale red, but on standing about an hour, grew of a turbid brown cast.

Though the nitrous acid is not often found, except produced by art, yet as there is a probability that nitre may be formed in the earth in large towns, and indeed fossile nitre has been actually found in such situations, it should be an additional caution against the use of leaden pumps.

I tried to dissolve mercury by the same means, but without success.

I am, with the most sincere esteem,

Dear Sir,

Your obliged and obedient servant,

THO. HENRY.

FINIS.

FOOTNOTES:

[15] See Dr. Falconer's very useful and ingenious treatise on the Bath water, 2d edit. p. 313.

[16] May, 1772.

[17] Vid. Mr. White's useful treatise on the management of pregnant and lying-in women, p. 279.

[18] See the author's observations on the efficacy of external applications in the ulcerous sore throats, Essays medical and experimental, Vol. I. 2d edit. p. 377.

[19] The author of these observations.

[20] Directions for impregnating water with fixed air, in order to communicate to it the peculiar spirit and virtues of Pyrmont water, and other mineral waters of a similar nature.

[21] Referring to the case communicated by Mr. Hey.

[22] He languished about a week, and then died.

[23] The vegetables which are most efficacious in the cure of the scurvy, possess some degree of a stimulating power.

[24] This refers, to an experiment mentioned in the first publication of these papers in the Philosophical Transactions, but omitted in this volume.

[25] The first account of this curious process was, I believe, given in the Mem. de l'Ac. de Sc. de Paris for 1742. Though seemingly less volatile than the vitriolic ether, it boils with a much smaller degree of heat. One day last summer, it boiled in the coolest room of my house; as it gave me notice by the explosion attending its driving out the cork. To save the bottle, and to prevent the total loss of the liquor by evaporation, I found myself obliged instantly to carry it down to my cellar.

ERRATA.

P. 15. l. 13.	*for*	it to	*read*	to it
p. 24. l. 20.	——	has	——	had
p. 60. l. 22.	——	inflammable	——	in inflammable
p. 84. l. 5.	——	experiments	——	experiment
p. 145. l. 16.	——	with	——	of
p. 153. l. 1.	——	that is	——	this air
p. 199. l. 17.	——	ingenious	——	ingenuous
p. 211. l. 23.	——	of	——	, if
p. 243. l. 27.	——	diminishing	——	diminished
p. 272. l. 21.	——	seem	——	seems
p. 301. l. 31.	——	——	——	one end
p. 303. l. 5.	——	——	——	the nitrous
p. 304. l. 21.	——	deslrium	——	delirium
p. 306. l. 2.	——	recet.	——	recent.
p. 308. l. 7.	——	per	——	Peruv.
p. 313. l. 27.	——	usual	——	useful
p. 300. to 314. passim	——	Diarrhæa	——	Diarrhœa
p. 316. l. 11.	——	remains	——	remainder
p. 524. l. 15.	——	it	——	iron.

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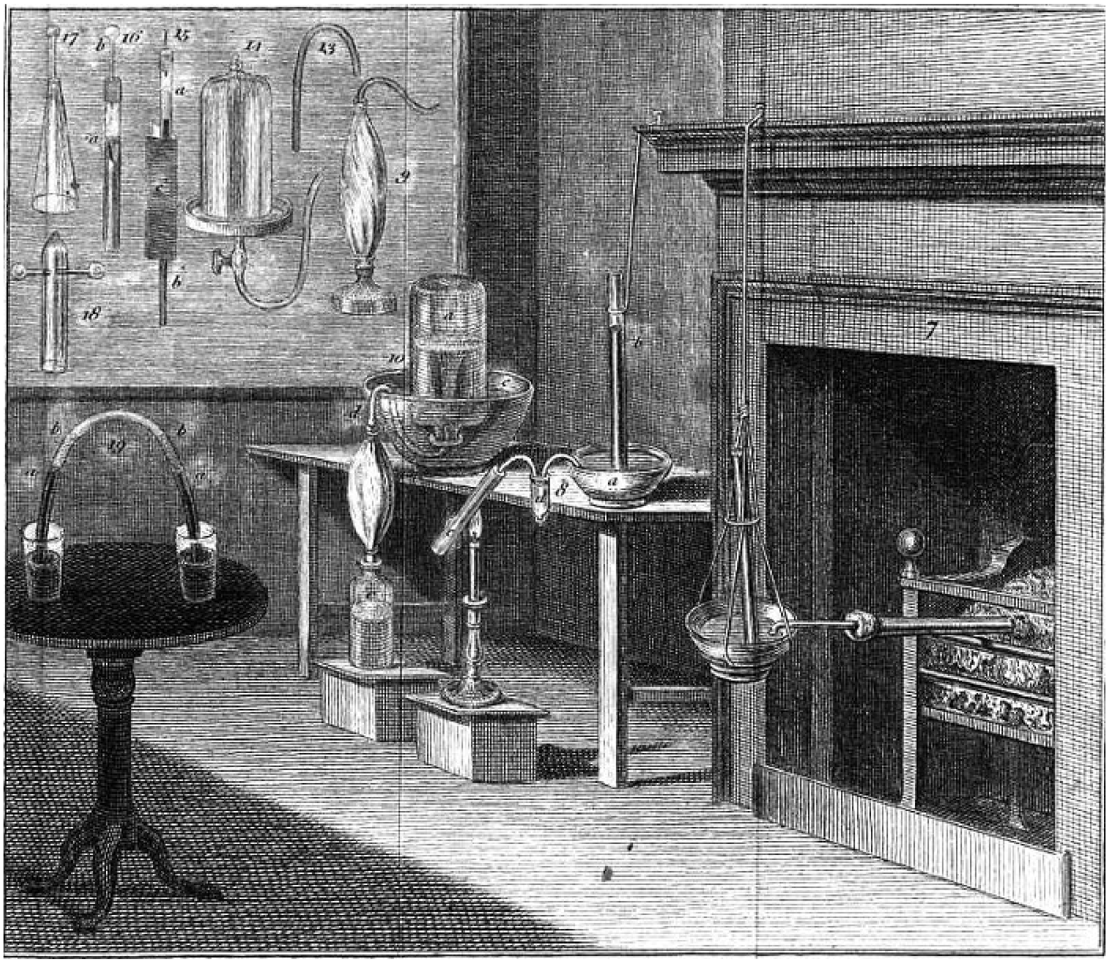

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