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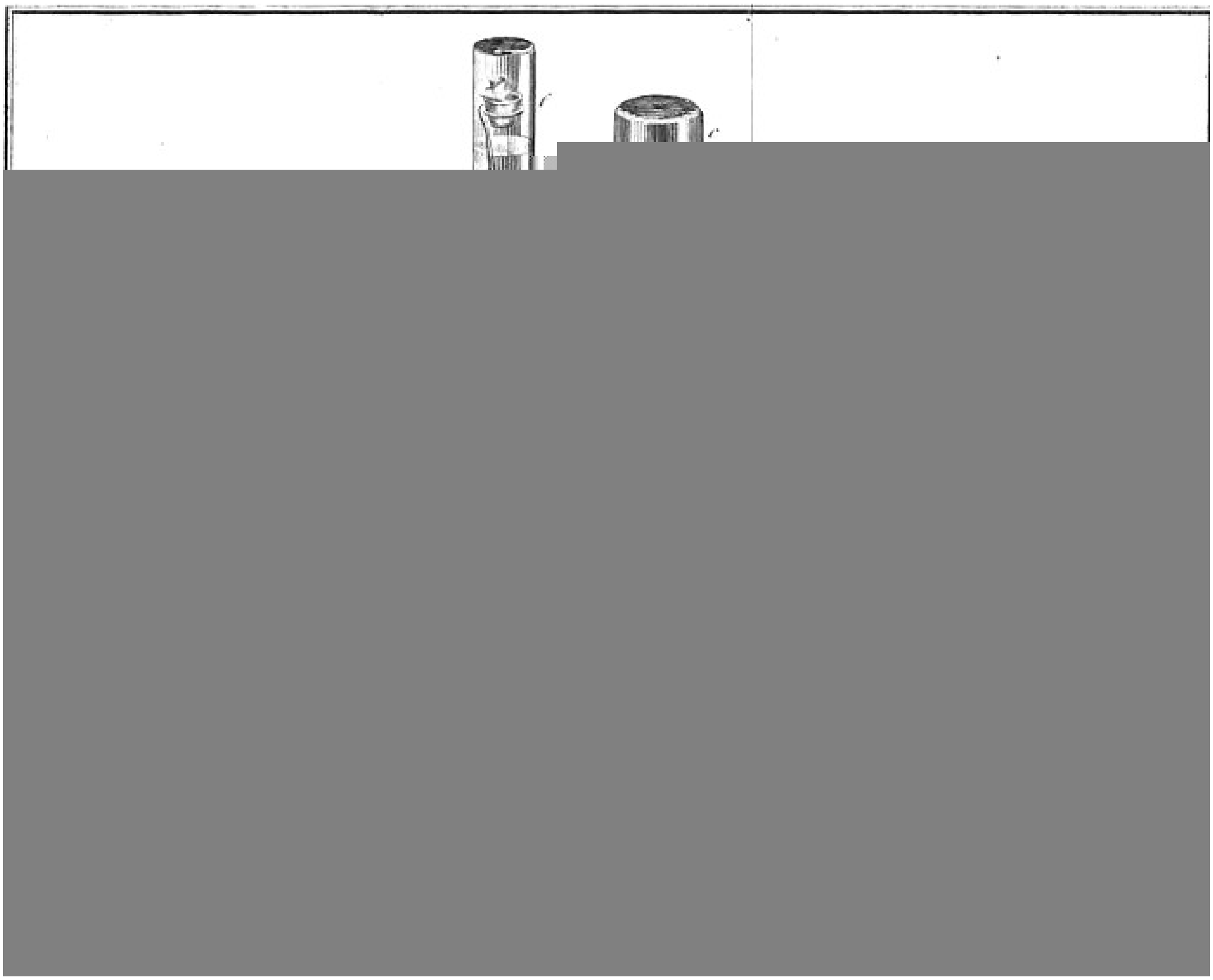

To face the Title.

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 anterposuerunt, 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.

[Pg v]

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. [Pg vi]

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. [Pg vii]

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. [Pg viii]

> 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. [Pg ix]

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. [Pg x]

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. [Pg xi]

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. [Pg xii]

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. [Pg xiii]

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. [Pg xiv]

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. [Pg xv]

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 [Pg xvi] 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 [Pg xvii] *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 [Pg xviii] 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. [Pg xix]

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, [Pg xx] 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 [Pg xxi] 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. [Pg xxii]

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. [Pg xxiii]

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 INTRODUCTION.

[Pg 1]

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. [Pg 2]

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. [Pg 3]

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 [Pg 4] 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. [Pg 5] 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. [Pg 6]

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 [Pg 7] 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. [Pg 8]

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 [Pg 9] 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 [Pg 10] 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 [Pg 11] 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 [Pg 12] 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 [Pg 13] 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. [Pg 14]

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 [Pg 15] 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 [Pg 16] 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 [Pg 17] 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 [Pg 18] 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 [Pg 19] 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 [Pg 20] 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 [Pg 21] 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 [Pg 22] 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. [Pg 23]

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. [Pg 24]

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. [Pg 25]

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 [Pg 26] 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. [Pg 27]

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. [Pg 28]

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 [Pg 29] 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. [Pg 30]

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 [Pg 31] 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 [Pg 32] 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. [Pg 33]

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 [Pg 34] 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. [Pg 35]

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 [Pg 36] 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. [Pg 37]

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.

[Pg 38] 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 [Pg 39] 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. [Pg 40]

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 [Pg 41] 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. [Pg 42]

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. [Pg 43]

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 [Pg 44] 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 [Pg 45] 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. [Pg 46]

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. [Pg 47]

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, [Pg 48] 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 [Pg 49] 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 [Pg 50] 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 [Pg 51] 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 [Pg 52] 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. [Pg 53]

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. [Pg 54]

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. [Pg 55]

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. [Pg 56]

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 [Pg 57] 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 [Pg 58] 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 [Pg 59] 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 [Pg 60] 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, [Pg 61] 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 [Pg 62] 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. [Pg 63]

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 [Pg 64] 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. [Pg 65]

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. [Pg 66]

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 [Pg 67] 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 [Pg 68] 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, [Pg 69] 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. [Pg 70]

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. [Pg 71]

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 [Pg 72] 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 [Pg 73] 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 [Pg 74] 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. [Pg 75]

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 [Pg 76] 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 [Pg 77] 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, [Pg 78] 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 [Pg 79] 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 [Pg 80] 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 [Pg 81] 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.

[Pg 82] 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 [Pg 83] 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 [Pg 84] 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, [Pg 85] 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 [Pg 86] 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 [Pg 87] 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 [Pg 88] 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 [Pg 89] 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 [Pg 90] 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 [Pg 91] 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 [Pg 92] 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 [Pg 93] 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 [Pg 94] 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 [Pg 95] 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 [Pg 96] 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 [Pg 97] 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, [Pg 98] 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. [Pg 99]

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. [Pg 100]

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. [Pg 101]

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. [Pg 102]

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, [Pg 103] 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 [Pg 104] 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. [Pg 105]

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 [Pg 106] 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. [Pg 107]

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 [Pg 108] 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 [Pg 109] 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. [Pg 110]

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 [Pg 111] 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 [Pg 112] 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 [Pg 113] 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 [Pg 114] 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 [Pg 115] 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 [Pg 116] 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; [Pg 117] 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. [Pg 118]

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 [Pg 119] 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. [Pg 120]

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. [Pg 121]

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. [Pg 122]

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. [Pg 123] [Pg 124]

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 [Pg 125] 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*, [Pg 126] 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 [Pg 127] 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. [Pg 128]

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. [Pg 129]

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 [Pg 130] 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 [Pg 131] 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 [Pg 132] 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 [Pg 133] 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 [Pg 134] 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 [Pg 135] 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 [Pg 136] 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; [Pg 137] 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. [Pg 138]

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 ma