EXPERIMENTS

AND

OBSERVATIONS

ON DIFFERENT KINDS OF

AIR, &c.

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

LORD BACON IN INSTAURATIONE MAGNA.

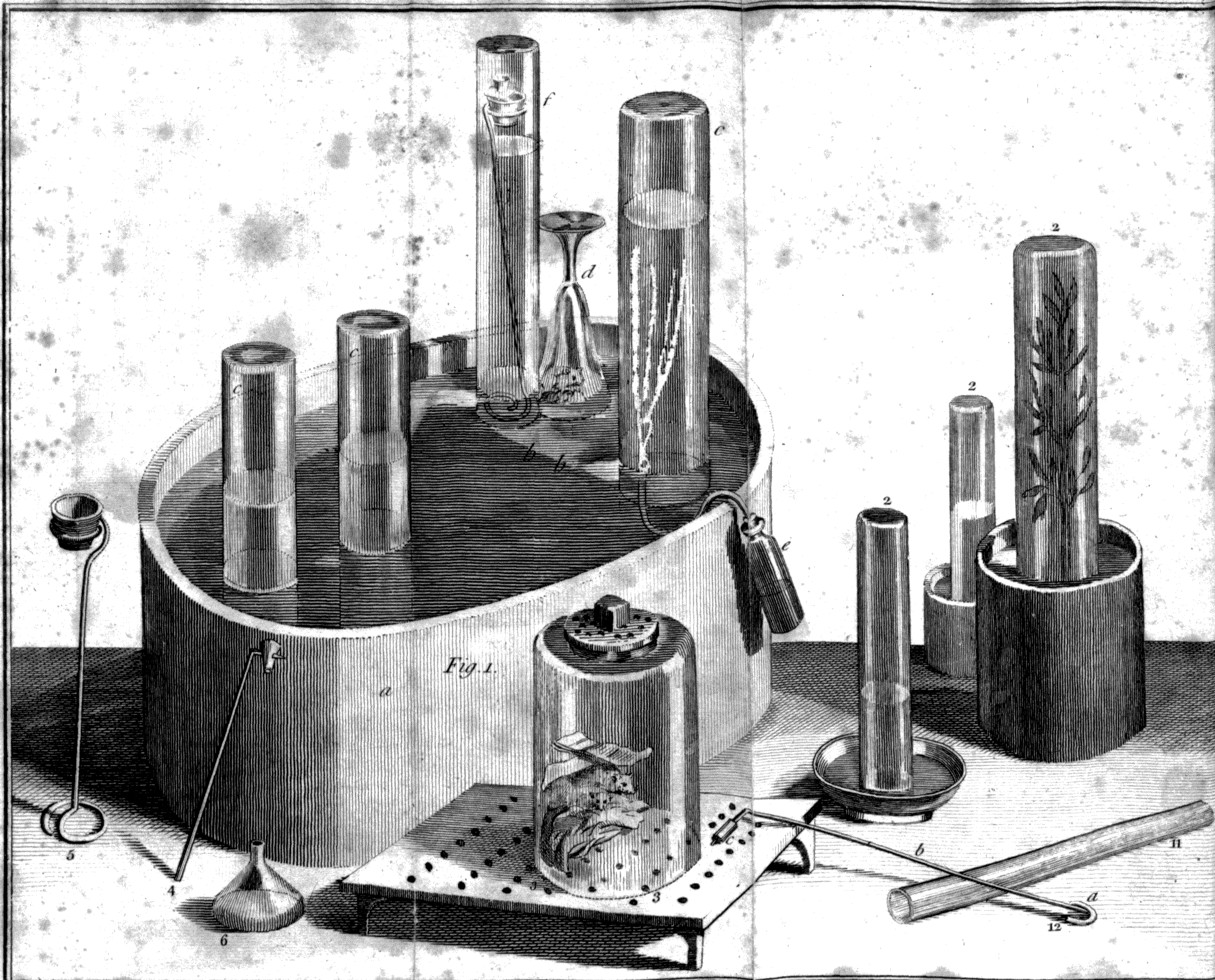
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Fig. 1.
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11 — 59

EXPERIMENTS

AND

OBSERVATIONS

ON DIFFERENT KINDS OF

A I R,

AND OTHER BRANCHES OF

NATURAL PHILOSOPHY,

CONNECTED WITH THE SUBJECT.

IN THREE VOLUMES;

Being the former Six Volumes abridged and methodized, with many Additions.

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

AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. ITAL. HARLEM. AUREL. MED. PARIS. CANTAB. AMERIC. ET PHILAD. SOCIUS.

VOL. I.

Fert animus caufas tantarum expromere rerum,
Immenfumque aperitur opus.

LUCAN.

Motto to the First of the Six Volumes.

BIRMINGHAM,

PRINTED BY THOMAS PEARSON;

AND SOLD BY J. JOHNSON, ST. PAUL'S CHURCH-YARD, LONDON.

MDCCXC.

TO

HIS ROYAL HIGHNESS

GEORGE PRINCE OF WALES,

SIR,

IN dedicating this work to your ROYAL HIGHNESS, I expreſs my own earneſt wiſh, and that of many others, that to your other excellent qualities your ROYAL HIGHNESS may add a diſpoſition to patronize a branch of ſcience, in the extenſion of which the natives of Great Britain have ever borne a diſtinguiſhed part, and which has for its object the benefit of all mankind.

It is by increaſing our knowledge of *nature*, and by this alone, that we acquire the great art of commanding it, of availing ourſelves of its powers, and applying them to our own purpoſes; true *ſcience* being the only foundation of all thofe *arts* of life, whether relating to peace or war, which diftinguifh *civilized* nations from thofe which we term *barbarous*; a diftinction not lefs confpicuous than that between fome nations of *men* and fome fpecies of *brutes*. And that branch of this great fcience to which the fubject of this work relates, viz. *chemiftry*, is perhaps of more various and extenfive ufe, than any other part of natural knowledge; and by the application that is now given to it, it is continually growing in relative magnitude and importance.

In the age of Newton chemiftry was but little cultivated; and its value not being generally known, it was not regularly taught in places of liberal education, in which *natural philofophy* was always more or lefs attended to; whereas at prefent every thing that is not denominated chemiftry is but a fmall part of a fyftem of *natural knowledge*. It is no lefs remarkable that the doctrine of *air*, of which little or nothing was known in the time of Newton, and which a few years

years ago was hardly mentioned in the writings of chemiſts, now makes a very conſiderable figure in the maſs of chemical knowledge, and throws the greateſt light on the moſt important proceſſes.

It is, therefore, earneſtly to be wiſhed, that this branch of natural ſcience ſhould be aſſiduouſly cultivated; and the patronage of Princes may be eminently uſeful to this end, by diffuſing a taſte for it among thoſe whoſe opulence will enable them to proſecute it to the moſt advantage.

It is true that we are indebted to the poverty of many perſons for ſome of the moſt ſimple and effectual modes of operating in chemiſtry; *neceſſity* having in this, as well as in many other caſes, been the happy *mother of invention.* But in ſome caſes it is well known that the moſt promiſing projects have become abortive for want of the means that were neceſſary to carry them into execution. For in this ſcience mere *obſervation* and *reflection* will not carry a man far. He will frequently have occafion to put the fubftances which he examines into various new fituations, and obferve the refult of circumftances, which, without *expence*, as well as *labour*, he can have no opportunity of knowing.

Hence it is that the greateft and happieft effects may be expected from the patronage of fcience by perfons of your ROYAL HIGHNESS's rank and expectations, whofe wifhes and inclinations are often alone fufficient to give a turn to the tafte and purfuits of the rich and great. And hitherto almoft every country in Europe can boaft of more perfons among their nobility, and men of fortune, who are devoted to fcientifical purfuits, than Great Britain.

It will perhaps be faid, that men of high rank and fortune in this country are occupied about the greater objects of *civil policy*, and attending to the interefts and liberties of the nation. But admitting this to be the cafe of *all*, which is evidently that of a ſmall number only, no one object wholly engages the attention of any man. All men have their *pleaſures* as well as their *buſineſs*; nor is it deſirable that any one object ſhould ſo much ingroſs any perſon, as that he ſhould give no degree of attention to any other; and no purſuit can have a juſter claim to the leiſure hours of men of rank and fortune than that of *natural ſcience*; ſince, independently of any views of *utility*, none can furniſh more rational *amuſement*.

Permit us, then, who are engaged in the quiet purſuits of philoſophy, to flatter ourſelves that they will have the additional recommendation of ſo effectual a patronage as that of your ROYAL HIGHNESS; and I am perſuaded that your ROYAL HIGHNESS does not need to be reminded, that the greateſt princes have been the protectors of ſcience and of letters, and that they have ever conſidered this patronage as reflecting luſtre on their crowns.

In

In ſome countries the ſciences ſeem to require the ſupport of princes, or of the community, by penſions and eſtabliſhments. In ours theſe aids are unneceſſary. Our *Royal Society*, which gives none but honorary rewards, is all that is wanted in the way of *eſtabliſhment*; and it has been, and is, eminently uſeful. In this country patronage is not wanted for thoſe who cultivate the ſciences, but rather for the ſciences themſelves; to give them their due value and conſideration, to apply the influence which the great poſſeſs over the minds and opinions of men, in directing their taſtes to uſeful purſuits, and thus to incite a ſufficient number of able inquirers to explore the hidden powers which the Deity has impreſſed on matter.

Conſidering your ROYAL HIGHNESS as deſtined to be the future ſovereign of this country, I cannot wiſh you greater glory or happineſs, than that you ſhould conſider it as conſiſting, not in the *extent*, but in the *flouriſh-*

flourishing state, of your dominions, to which *science*, *manufactures*, and *commerce* (each the true source of the other) will most eminently contribute; and that you should not be dazzled by the flattering, but often fatal, idea of extending what is called the *royal prerogative*; but rather study to give your subjects every power which they can exercise for their own advantage. And whatever flatterers may suggest, the people (each of them giving his whole attention to those things in which he is most interested) will always be able to do more for themselves than the most enlightened and best disposed princes can do for them.

As a person whose deliberate judgment has led him to dissent from the mode of religion by law established in this country, permit me, Sir, to express something more than a wish, that, as the future sovereign of Great Britain you will be the equal father of all your subjects; and that in your reign every man will meet with encouragement and favour in proportion to the services he renders

renders his country, and the credit he is to it.

There has of late years been a wonderful concurrence of circumſtances tending to expand the human mind, to ſhew the inconvenience attending all *eſtabliſhments*, civil or religious, formed in times of ignorance, and to urge the reformation of them. Let theſe be ſuffered to operate without obſtruction; and have the true magnanimity to let no impediment be thrown in the way of the efforts of the more enlightened part of the community to improve the ſtate of it in any reſpect.

A ſovereign conducting himſelf by theſe liberal maxims will rank among the few truly *great and good princes*, whoſe object has not been themſelves, and their perſonal glory and power, but the real good of their country; and not *that* only, or excluſively, but the benefit of all the human race. A character thus ſupported will be admired, and beloved, when that of other princes, generally,

generally, but falſely, called *great*, will be conſigned to what is worſe than oblivion, the deteſtation of all good men.

That your ROYAL HIGHNESS may prove a truly patriot king, an ornament to human nature, and a bleſſing to your country, and to mankind, is the ſincere wiſh, and prayer, of

Your ROYAL HIGHNESS'S,

Moſt obedient

And moſt humble ſervant,

J. PRIESTLEY.

BIRMINGHAM,
March 24, 1790.

THE

PREFACE*.

HAVING, at different times, publiſhed ſix volumes of obſervations and experiments relating chiefly to the ſubject of *air*, and they being at preſent ſo far out of print, that a complete ſet cannot be had new, it ſeemed more adviſable to *new model* the whole work, than *reprint* the former volumes.

In ſuch a multiplicity of obſervations, made at very different times, it could not be but that many muſt now be ſuperfluous; and there muſt alſo be a variety of imperfections, with which it is not worth while to trouble the reader. It will alſo be more agreeable to any perſon who is entering upon theſe inquiries, to get acquainted with what I have done in a better method than that in which the particulars happened to occur to myſelf, and eſpecially to

* Into this Preface I have introduced every thing that I thought worth preſerving in the prefaces to all the ſix volumes; and it is hoped that the importance of the obſervations it contains, will be a ſufficient apology for the length of it.

ſee

ſee all that has been diſcovered with reſpect to any ſubject of experiment, ſuch as any of the different kinds of air, &c. with as little mixture of other matter as poſſible.

Having had a view to ſuch readers, I have endeavoured in this new edition to digeſt the contents of all the ſix volumes, and alſo of thoſe papers which, ſince the publication of them, have been inſerted in the Philoſophical Tranſactions, into ſomething like a *ſyſtem*; ſome regard, at the ſame time, being had to the *order of time*, and of *diſcovery*, the better to enable the reader to enter into my views, and trace the actual progreſs of my thoughts in the ſeveral inveſtigations.

For the ſake of conciſeneſs, I have not, indeed, troubled the reader with every conjecture and hypotheſis which I formerly adopted; but I have not failed to mention the moſt conſiderable of them; not being aſhamed of the miſtakes I have made, and being willing to encourage young adventurers, by ſhewing them that, notwithſtanding the many errors to which even the moſt ſagacious, and the moſt cautious, are incident, their labours may be crowned with conſiderable ſucceſs.

No

No perſon, I am confident, will now wiſh that, in order to prevent ſuch miſtakes, I had deferred the publication of any of my volumes till I had more nearly completed the courſes of experiments, of which they contain an account; and I ſhall ſtill purſue the ſame method of *ſpeedy publication*, though the conſequence of it ſhould be the neceſſity, in ſome future time, of making another new modelled, and better purged edition of all my philoſophical writings.

To repeat what I ſaid in the preface of the very firſt volume of experiments on air; conſidering the attention which 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 high idea of their judgment and penetration; they are juſtly puniſhed for their ingratitude to the fountain of all knowledge, and for their want of a genuine love of ſcience and of mankind, in finding their boaſted diſcoveries anticipated, and the field of honeſt fame pre-occupied, by men, who, from a natural ardour of mind engage in philoſophical purſuits, and with an ingenuous ſimplicity immediately communicate to others whatever occurs to them in their inquiries.

As to myſelf, I find it abſolutely impoſſible to produce a work on this ſubject that ſhall be any thing like *complete*. Every publication I have frankly acknowledged to be very imperfect, and the preſent, I am as ready to acknowledge, is 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 had no idea before; ſo that we cannot ſolve one doubt without creating ſeveral new ones.

No philoſophical inveſtigation can be ſaid to be completed, which leaves any thing unknown that we are prompted by it to wiſh we could know relating to it. But ſuch is the neceſſary connection of all things in the ſyſtem of nature, that every diſcovery bring to our view many things of which we had

had no intimation before, the complete diſcovery of which we cannot help wiſhing for; and whenever theſe diſcoveries are completed, we may aſſure ourſelves they will farther increaſe this kind of diſſatisfaction.

The greater is the circle of light,the greater is the boundary of the darkneſs by which it is confined. But, notwithſtanding this, the more light we get, the more thankful we ought to be. For by this means we have the greater range for ſatisfactory contemplation. In time the bounds of light will be ſtill farther extended; and from the infinity of the divine nature, and the divine works, we may promiſe ourſelves an endleſs progreſs in our inveſtigation of them: a proſpect truly ſublime and glorious. The works of the greateſt and moſt ſucceſsful philoſophers are, on this account, open to our complaints of their being imperfect.

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

> So pleas'd at firſt the tow'ring Alps we try,
> Mount o'er the vales, and ſeem to tread the ſky.

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

ESSAY ON CRITICISM.

Newton, as he had very little knowledge of *air*, ſo he had few doubts concerning it. Had Dr. Hales, after his various and valuable inveſtigations, given a liſt of all his *deſiderata,* I am confident that he would not have thought of one in ten that had occurred to me at the time of my firſt 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.

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 object of his purſuits, he has no occaſion to diſtreſs himſelf about leſſer things. In the progreſs of of his inquiries he will generally be able to rectify his own miſtakes; or if little and envious minds 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.

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

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

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

I shall pass for an enthusiast with some, but I am perfectly easy under the imputation, because I am happy in those views which subject me to it; but considering the amazing improvements in natural knowledge which have been made within the last century, and the many ages, abounding with men who had no other object besides study, in which, however, nothing of this kind was done, there appears to me to be a very particular providence in the concur-

rence

rence of thoſe circumſtances which have produced ſo great a change; and I cannot help flattering myſelf that this will be inſtrumental in bringing about other changes in the ſtate of the world, of much more conſequence to the improvement and happineſs of it.

This rapid proceſs 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 X. 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.

This is not now a buſineſs of *air* only, as it was at the firſt; but appears to be of much greater mag-

 nitude

nitude and extent, ſo as to diffuſe light upon the moſt *general principles* of natural knowledge, and eſpecially thoſe about which *chemiſtry* is particularly converſant. And it will not now be thought very aſſuming to ſay, that, by working in a tub of water, or a baſon of quickſilver, we may perhaps diſcover principles of more extenſive influence than even that of *gravity* itſelf, the diſcovery of which, in its full extent, contributed ſo much to immortalize the name of Newton.

I would, however, caution my reader not to be too ſanguine in his expectations from the happy train which this branch of philoſophy ſeems to be in. Conſidering the unexampled rapidity with which diſcoveries have hitherto been made in it, the number of perſons in many and diſtant countries now engaged in theſe purſuits, and the emulation that is neceſſarily excited in ſuch circumſtances; and conſidering, at the ſame time, how nearly this ſubject is allied to the moſt general and comprehenſive laws of nature with which we are acquainted; ſome may be apt to imagine, that every year muſt produce diſcoveries equal to all that were made by a Newton or a Boyle; and I am far from ſaying that this may not be the caſe, or that it is very improbable.

But,

But, though I have little doubt, from the train that things are viſibly in, that philoſophical diſcoveries in general will go on with an accelerated progreſs (as indeed they have done ever ſince the revival of letters in Europe) it would be too raſh to infer, from the preſent flattering appearances, that any particular expedition into the undiſcovered regions of ſcience will be crowned with more diſtinguiſhed ſucceſs than another. Nothing is more common, in the hiſtory of all the branches of experimental philoſophy, than the moſt unexpected revolutions of good or bad ſucceſs. In general, indeed, when numbers of ingenious men apply themſelves to one ſubject, that has been *well opened*, the inveſtigation proceeds happily and equably. But, as in the hiſtory of *electricity*, and now in the diſcoveries relating to *air*, light has burſt out from the moſt unexpected quarters, in conſequence of which the greateſt maſters of ſcience have been obliged to recommence their ſtudies, from new and ſimpler elements; ſo it is alſo not uncommon for a branch of ſcience to receive a check, even in the moſt rapid and promiſing ſtate of its growth.

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

and fortune in other countries give to them. But with us this loss is made up by men of leisure, spirit, and ingenuity, in the middle ranks of life, which is a circumstance that promises better for the continuance of this progress in useful knowledge than any noble or royal patronage. With us, also, 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 to the display of the human faculties than the latter, as the *system of nature* is superior to any *political system* upon earth.

If extensive *usefulness* be the object, science has the same advantage over politics. The greatest success

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 formerly received from that excellent, and in my opinion, not too enthuſiaſtical philoſopher, father Beccaria, of Turin.

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

Scientifical purſuits have ſuch an advantage over moſt others, as ought more eſpecially to recommend them

them to perſons of rank and fortune. They never fail to furniſh materials for the moſt agreeable and active purſuits, and ſuch as are, at the ſame time, in the higheſt degree, uſeful and honourable, and are, by this means, capable of doing unſpeakably more for them than the largeſt fortunes can do without this reſource. Were perſons thus engaged, there would be leſs temptation to have recourſe to pleaſure and diſſipation, for the employment of their vacant time; and ſuch purſuits would be particularly valuable to thoſe who have no *talent* for politics, or any proper *call*, to occupy themſelves in public affairs. Beſides, the laſt is a path in which, from the nature of things, only a very few can walk; and the former, viz. a courſe of vicious pleaſure, it is much to be lamented that any human being ſhould tread.

Man is a being endued by his creator with excellent faculties, and not to have *ſerious objects of purſuit* is to debaſe and degrade himſelf. It is to rank himſelf with beings of a lower order, aiming at nothing that is much higher than the low pleaſures they are capable of; at the ſame time that, from the remains of nobler powers, of which he cannot wholly diveſt himſelf, he is incapable of that unallayed enjoyment of ſenſual pleaſures that brutes have.

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I am ſorry to have occaſion to obſerve, that natural ſcience is very little, if at all, the object of *education* in this country, in which many individuals have diſtinguiſhed themſelves ſo much by their application to it. And I would obſerve that, if we wiſh to lay a good foundation for a philoſophical taſte, and philoſophical purſuits, perſons ſhould be accuſtomed to the ſight of experiments, and proceſſes, in *early life*. They ſhould, more eſpecially, be early initiated in the theory and practice of *inveſtigation*, by which many of the old diſcoveries may be made to be really *their own*; on which account they will be much more valued by them. And, in a great variety of articles, very young perſons may be made ſo far acquainted with every thing neceſſary to be previouſly known, as to engage (which they will do with peculiar alacrity) in purſuits truly original.

At all events, however, the curioſity and ſurprize of young perſons ſhould be excited as ſoon as poſſible; nor ſhould it be much regarded whether they properly underſtand what they ſee, or not. It is enough, at the firſt, if ſtriking facts make an impreſſion on the mind, and be remembered. We are, at all ages, but too much in haſte to *underſtand*, as we think, the appearances that preſent themſelves to us. If we could content ourſelves with the bare knowledge of new *facts*, and suspend our judgment with respect to their *causes*, till, by their analogy, we were led to the discovery of more facts, of a similar nature, we should be in a much surer way to the attainment of real knowledge.

I do not pretend to be perfectly innocent in this respect myself; but I think I have as little to reproach myself with on this head as most of my brethren; and whenever I have drawn general conclusions too soon, I have been very ready to abandon them, as all my publications, and this work in particular, will evidence. I have also repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to consider new *facts* only as *discoveries*, and mere *deductions* from those facts, as of no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves.

I also cannot help expressing a wish that during the establishment of peace in Europe (and happily it is not in the power of any state to be always at war) we may see every obstruction to the progress of knowledge, which is equally friendly to all states, removed. Taxes on the importation of books, and other articles of literature, are so impolitic, as well as illiberal, that it is earnestly wished that something

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may be ſtipulated by contending powers for aboliſhing them. There are ſtateſmen whoſe minds are ſufficiently enlarged to ſee that philoſophy gives an ample equivalent for the exemption.

I might enlarge much more than I have done in this preface on the *dignity*, and *utility*, of experimental philoſophy; but ſhall only obſerve farther, that it is nothing but a ſuperior knowledge of the laws of nature, that gives Europeans the advantage they have over the Hottentots, or the loweſt of our ſpecies. Had theſe people never known Europeans, they could not have formed an idea of any mode of life ſuperior to their own, though it differs but little from that of the brutes. In like manner, ſcience advancing, as it does, with an accelerated progreſs, it may be taken for granted, that mankind ſome centuries hence will be as much ſuperior to us in knowledge, and improvements in the arts of life, as we now are to the Hottentots, though we cannot have any conception what that knowledge, or what thoſe improvements, will be. It is enough for us to ſee that nature is inexhauſtible, that it is a rich mine, in which we ſhall never dig in vain, and that it is open to infinitely more labourers than are now employed in exploring its contents, or in digging for them.

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Having been a pretty ſucceſsful adventurer in this great mine, my philoſophical friends in general wonder that I do not confine my attention to it. Their diſſatisfaction with me is ſo great, and I hear of it from ſo many quarters, that I think it right to take ſome opportunity (and a better than the preſent will hardly occur) to make an apology for my conduct, eſpecially to thoſe of my friends by whoſe aſſiſtance I am enabled to give my time to theſe liberal purſuits; being pleaſed to think that my attention to them will be of ſome advantage to ſcience and the public.

In the firſt place, I would obſerve, that I follow my own beſt judgment in devoting my time to what I really apprehend to be the moſt important purſuits, thoſe from which myſelf, and mankind at large, will finally derive the greateſt advantage; and I muſt be allowed to ſay, that the greater variety of objects to which it is evident that I have given attention, muſt qualify me to be a better judge in this caſe than thoſe who cenſure my conduct. Perſons who have only one object of purſuit, never fail to over-rate it, and of courſe to undervalue other things. I would farther obſerve, that the attention I have given to *theology* (which, by the way, is my original and proper province, and for which I may, therefore, be allowed to have a juſtifiable

juſtifiable predilection) does not engroſs ſo much of my time as ſome perſons may imagine. I am particularly complained of at preſent, as having thrown away ſo much time on the compoſition of my *Hiſtory of the Corruptions of Chriſtianity*, of the *Opinions concerning Chriſt*, and of the *Chriſtian Church in general*. But I can aſſure them, and the nature of the thing, if they conſider it, may ſatisfy them, that the time I muſt neceſſarily have beſtowed upon the experiments of which an account is contained in any one of ſix volumes, is much more than I have given to three or four of thoſe of which the other conſiſt, and to all the controverſial pieces that I have written in defence of them. In general, during the compoſition of thoſe works, the greateſt part of every day was ſpent in my laboratory, and the evenings and mornings only in reading or writing. Beſides, theſe different ſtudies ſo relieve one another, that I believe I do more in each of them, by applying to them alternately, than I ſhould do, if I gave my whole attention to one of them only.

But my principal defence reſts on the ſuperior dignity and importance of *theological ſtudies* to any other whatever, and with ſome obſervations of this kind I ſhall chuſe to conclude this long Preface.

Every rational being ought to diſtinguiſh, by the greater attention that he gives to them, thoſe objects which are of the greateſt importance to himſelf, and to mankind at large. And certainly, if there be any juſt rule for eſtimating the value of a problem, or query, that is propoſed to us, we muſt think it of infinitely more moment to diſcover whether there be a future, and eſpecially an endleſs, life after this, and how to ſecure a happy lot in that future life, than to make the beſt proviſion poſſible for ourſelves in this life, which is the ultimate object of all natural philoſophy. Studies, but remotely connected with that great object, muſt have a dignity and importance infinitely ſuperior to any other. A man muſt never have thought a moment on the ſubject, if he heſitate to give a decided preference in the caſe. To think or act otherwiſe, would be like a man buſying himſelf about farthings, who has large eſtates, or kingdoms, depending, and who ſhould neglect the latter in order to ſecure the former.

All that any philoſophical perſon can pretend to ſay in the caſe, muſt be, that the expectation of a future life is ſo manifeſtly chimerical, that it can never be worth a wiſe man's while to loſe a moment in thinking about it, or to employ his time in any ſtudy relating to it. This I know to be the opi-

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nion of many who will read this book, if not this preface. But in this I muſt take the liberty to differ from them, and for reaſons which I ſhall ſubmit to their ſerious conſideration.

Natural phenomena, I agree with them, are unfavourable to any expectation of a future life, and the doctrine of an *immaterial ſoul*, capable of ſubſiſting and acting when the body is in the grave (on which the doctrine of a future ſtate is generally founded) I am as fully perſuaded as they can be, is unauthorized by any natural appearances whatever. My expectation of a future life reſts on another foundation; and, improbable as I acknowledge the doctrine to be, according to the light of nature, it is nevertheleſs ſuch as I firmly believe, on the plaineſt of all evidence; the author of nature having given us an abſolute aſſurance of it, by perſons authorized to ſpeak in his name, and whoſe divine miſſion was proved by ſuch works as no other than the author of nature could have enabled them to perform.

That ſuch works have been performed, and for this important purpoſe, muſt, I apprehend, be true, if there be any truth in hiſtory. And there is no kind of evidence more eaſily ſubjected to a rigorous examination than that which is of the hiſtorical kind, the maxims of which we are every day converſant with.

Now it appears to me, that we muſt either admit the truth of the goſpel hiſtory, which contains an account of the doctrine, miracles, death, and reſurrection of Chriſt (on which the belief of a future life depends) or believe what is infinitely more incredible, viz. that ſeveral thouſand people, preſent at the tranſactions, and who had no motive to believe them without ſufficient evidence, but every motive to turn their eyes from them, or diſbelieve them if they could, ſhould yet, without ſuch evidence, have given the firmeſt aſſent to them, and have entertained ſo little doubt of the extraordinary facts, as to maintain their faith in them at the hazard of every thing dear to them in life, and even chearfully lay down their lives, rather than abandon their faith. Let philoſophers, as ſuch, account for this great *fact*, without admitting more real miracles, and thoſe of a more extraordinary kind, than the belief of chriſtianity requires of me, and I will relinquiſh my preſent faith, dear as it is to me, and join them in expoſing it.

As philoſophers, the queſtion between us is, whoſe faith, ſtrictly ſpeaking, is more agreeable to *preſent appearances*. Whatever we may think of an *author*

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of nature, and of his attention to it, we equally believe in *the uniformity of the laws of nature*, and that *man*, whose constitution is a part of the system of nature, was the same kind of being two thousand years ago that he is now; as much as that a horse of that age, or an oak tree of that age, had the same properties with the horses and oaks of the present. Consequently, whatever was possible with respect to *man* in any former period, is equally possible now.

But will any man, who gives a moment's attention to the subject, say that it is even *possible* that several thousand persons, in London or Paris, could be made to believe that any man in London or Paris, died and rose from the dead in their own life-time, that they should persist in this persuasion through life, without shewing any sign of insanity, that they should gain numerous proselytes to their opinion, though it subjected all who embraced it to all kinds of persecution, and even to death; and that the belief of it should establish itself against all opposition, without any person being able to detect the imposition?

Now I apprehend that this might take place more easily in London, or in Paris, at this day, than it could have done at Jerusalem in the time of our Saviour. Human nature could not have been the ſame thing then that we find it to be at preſent, if mankind could have been ſo impoſed upon. This I therefore think abſolutely incredible, and conſequently, as the leſs difficulty of the two, as believing a thing much leſs improbable, I admit the truth of the goſpel hiſtory, the admiſſion of which makes the ſubſequent account of the propagation of chriſtianity (which all hiſtory, and even the preſent ſtate of things, proves to be true) perfectly eaſy and natural. Admitting theſe leading facts, all the reſt follows of courſe, and all things came to be as they are without any farther miracle. But real miracles we muſt have ſomewhere, in order to account for the preſent ſtate of things; and if we muſt admit miracles, let them be ſuch as have a *great object*, and not ſuch as have no object at all, but only ſerve to puzzle and confound us.

The hiſtory of the Jews, and the books of the Old Teſtament, furniſh many *facts*, which no hypotheſis beſides that of the divine origin of their religion can explain. Let the philoſopher only admit as *a poſtulatum* that Jews are, and always were, *men*, conſtituted as other men are, and let him not deceive himſelf, by conſidering them as beings of another ſpecies. All I wiſh in this reſpect is, that perſons who pretend to the character of *philoſophers*, would be ſo throughout, and carry the ſame ſpirit into the ſtudy of hiſtory, and of human nature, that they do into their laboratories; firſt aſſuring themſelves, with reſpect to *facts*, and then explaining thoſe facts by reducing them to *general principles* (which, from the uniformity of nature, muſt be univerſally true) and then I ſhall have no doubt of their becoming as firm believers in chriſtianity as myſelf. They will find no other *hypotheſis*, that can explain ſuch appearances as they cannot deny to be real. Let philoſophers now ſay, whether there be reaſon in this, or not.

I therefore take the liberty, having been led to advance thus much, to addreſs my brother philoſophers on a ſubject equally intereſting to us as *philoſophers*, and as *men*. Do not diſregard a queſtion of infinite moment. Give it that degree of attention to which it is naturally intitled; and eſpecially do not ſo far abandon the ſerious character of *philoſophers*, as to *laugh* where you ought to *reaſon*. At leaſt, do this great ſubject, and yourſelves, the juſtice to conſider the *facts*, and endeavour to frame ſome *hypotheſis* by which to account for them; and do not decide in half an hour, on an inquiry which well deſerves the ſtudy of a great part of your lives.

If I have a ſtronger bias than many other perſons in favour of chriſtianity, it is that which philoſophy gives me. I view with rapture the glorious face of nature, and I admire its wonderful conſtitution, the laws of which are daily unfolding themſelves to our view. It is but little that the life of man permits us to ſee at preſent, and therefore I feel a moſt eager deſire to renew my acquaintance with it hereafter, and to reſume thoſe inquiries with which I am ſo much delighted now, and which muſt be interrupted by death.

Could I imagine that the knowledge of nature would ever be exhauſted, and that we were approaching to a termination of our enquiries, I could more contentedly ſhut my eyes on a ſcene in which nothing more was to be ſeen, or done. But to quit the ſtage at preſent (and I believe the aſpect of things will be exactly ſimilar in any future period of our exiſtence) without the hope of re-viſiting it, would fill me with the deepeſt regret. The general who, like Epaminondas, or Wolfe, dies in the arms of victory, dies with ſatisfaction; but not ſo he that is cut off in the beginning of a doubtful, though promiſing, engagement. Thus I feel on the idea of ceaſing to breathe, when I have but juſt begun to know what it is that I breathe.

M. Herſchell's late diſcoveries in, and beyond, the bounds of the ſolar ſyſtem, the great views that he has given us of the arrangement of the ſtars, their revolutions, and thoſe of the immenſe ſyſtems into which they are formed, are peculiarly calculated to inſpire an ardent deſire of ſeeing ſo great a ſcene a little more unfolded. Such diſcoveries as theſe, give us a higher idea of the value of our being, by raiſing our ideas of the *ſyſtem* of which we are apart, and, with this, an earneſt wiſh for the continuance of it.

Beſides, *civil ſociety* is but in its infancy, the world itſelf is but very imperfectly known to the civilized inhabitants of it, and we are but little acquainted with the real value of thoſe few of its productions of which we have ſome knowledge, and which we are only beginning to name, and to arrange. How muſt a *citizen of the world* wiſh to know the future progreſs of it?

To have no wiſh of this kind certainly argues a low, an ignoble, and I will ſay, an unphiloſophical mind. I conſider all ſuch perſons, how ſuperior ſoever they may be to myſelf in other reſpects, with pity and concern. They would have unſpeakably more ſatisfaction in their philoſophical purſuits, if they carried them on with the views of things that

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I have. It has been juſtly obſerved, that great views indicate, and indeed conſtitute, great minds. What elevation of mind, then, would the proſpects of the chriſtian, add to thoſe of the philoſopher!*

With men of reflection this apology for my conduct will, I doubt not, be admitted as ſatisfactory; and till I hear better reaſons than have yet been offered to me for changing my conduct, I ſhall continue to give my attention to my different purſuits, according to my own ideas of their reſpective importance; and my friends have no reaſon to fear that I ſhall neglect *philoſophy*. It has, perhaps, but too ſtrong charms for me. I ſhall endeavour, however, to keep it in its proper place, and not ſo

* If any of my philoſophical friends ſhould be induced, by what I have here urged, to look into my *theological writings*, I would take the liberty to recommend to them my *Letters to a Philoſophical Unbeliever*, the *Inſtitutes of natural and revealed Religion*, the *General Hiſtory of the Chriſtian Church, till the Fall of the Weſtern Empire*, and the *Hiſtory of the Corruptions of Chriſtianity*, eſpecially the *Concluſion*, Part I. relating to Mr. Gibbon, who has declined engaging in the diſcuſſion I there propoſed to him. If they wiſh to ſee more particularly in what manner chriſtianity came to be encumbered with the doctrine of *the trinity*, which has been the foundation of one of the greateſt objections to it, I would further refer them to my *Hiſtory of early Opinions concerning Chriſt*, where they will ſee it traced to its proper ſource in the Platonic philoſophy, and where it is proved that the primitive chriſtian church was unqueſtionably unitarian.

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much attach myſelf to the ſtudy of the laws which govern *this* world, as to loſe ſight of the ſubſerviency of this world, and of all things in it, to *another* and a better; in which I hope to reſume theſe pleaſing philoſophical purſuits, and to ſee, in a comprehenſive view, thoſe detached diſcoveries which we are now making here.

At preſent all our *ſyſtems* are in a remarkable manner unhinged by the diſcovery of a multiplicity of *facts*, to which it appears difficult, or impoſſible, to adjuſt them. We need not, however, give ourſelves much concern on this account. For when a ſufficient number of new facts ſhall be diſcovered (towards which even imperfect hypotheſes will contribute) a more *general theory* will ſoon preſent itſelf; and perhaps to the moſt incurious and leaſt ſagacious eye. Thus, when able navigators have, with great labour and judgment, ſteered towards an undiſcovered country, a common ſailor, placed at the maſt head, may happen to get the firſt ſight of the land. Let us not, however, contend about *merit*, but let us all be intent on forwarding the *common enterprize*, and equally enjoy any progreſs we may make towards ſucceeding in it; and above all, let us acknowledge the guidance of that Great Being, *who has put a ſpirit in man, and whoſe inſpiration giveth him underſtanding*.

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I have not, in this edition, given a *ſummary view of facts*, ſuch as I gave in the *fifth* of the preceding volumes, partly becauſe I found it would have made the laſt volume of a diſproportionate ſize, but chiefly becauſe the arrangement of the preſent work, and the *Index* to the whole, rendered it leſs neceſſary. Such a ſummary will be found in Mr. Keir's Chemical Dictionary, and in Elementary Treatiſes, comprizing what all experimenters on air have diſcovered.

As I wiſh to preſerve the memory of my patrons (though I hope to do in a more effectual manner than this) I would obſerve that the ſix original volumes were inſcribed to the following perſons; viz. the Marquis of Lanſdown, Sir George Savile, the late Earl of Stanhope, Sir John Pringle, Doctor Heberden, and William Conſtable, Eſq. of Burton Conſtable.

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THE

INTRODUCTION.

SECTION I.

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

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

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

Also Van Helmont, and other chymists who succeeded him, were acquainted with the property of some *vapours* to suffocate, and extinguish flame, and of others to be ignited; effects, indeed, which could not but have been known in all ages. But they had no idea that the substances (if, indeed, they knew that they were *substances*, and not merely *properties*, and *affections* of bodies which produced those effects) were capable of being separately exhibited in the form of a *permanently elastic vapour*, not condensable by cold, to which I give the name of *air*, any more than the thing that constitutes *smell*. In fact, they knew nothing at all of any air besides *common air*, and therefore they applied the term to no other substance whatever.

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, through 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.

Mr. Boyle was, I believe, the first who discovered that what we now call *fixed air*, and also *inflammable air*, are really *elastic fluids*, capable of being exhibited in a state unmixed with common air, a fact which nothing that was known before his time could have given him the least reason to expect; nor, in fact, did he make the discovery by any kind of reasoning *a priori*. It was the unexpected result of his experiments.

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

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

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

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

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 $1\frac{1}{2}$ heavier than common air, and the latter ten times lighter. He alſo ſhewed that water would imbibe more than its own bulk of fixed air.

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

Beſides theſe two kinds of factitious air, that which I call *nitrous air* obtruded itſelf upon Dr. Hales; but even he, as I obſerved, had no idea of there being more than *one kind of air*, loaded with different vapours; and was far from imagining that they differed from one another ſo very eſſentially as they are now known to do. And though Mr. Boyle, Dr. Hales, and others, could not but be acquainted with the effluvium of *ſpirit* of ſalt, and alſo of *volatile alkali*, they could have no idea that the ſubſtance which had thoſe powers was capable of being ſeparated from common air, and of being exhibited free from moiſture, in the form of a permanently elaſtic vapour, to appearance exactly like that which conſtitutes the common atmoſphere. Or if any perſon, till within theſe very few years, had ſuch a notion (of which, however, I do not believe that they have given the leaſt intimation) it muſt have been a mere *random conjecture*, and what nothing but actual experiment could have aſcertained.

Even Mr. Cavendiſh, whoſe experiments relating to air immediately preceded my own, appears not to have had ſo much as a ſuſpicion of this kind. For he relates an experiment of his, on the ſolution of copper in the marine acid, as inexplicable, except on the hypotheſis of there being a kind of *air that loſt its elaſticity by the contact of water*, which admits of the eaſieſt ſolution imaginable, on the ſuppoſition of the ſpirit of ſalt emitting a vapour, which though capable of being confined by quickſilver, and of being by that means exhibited in the form of air, was inſtantly *abſorbed* by water, which would thereupon become poſſeſſed of all the properties of common ſpirit of ſalt.

In

In fact, none of the chymists appear to have had the least idea of its being even possible to separate the acid or alkaline principles from the water with which they are always found combined; and therefore, though they did suppose them capable of farther *concentration*, they still considered a certain portion of water, as absolutely *essential* to them; and consequently all the experiments that have hitherto been made on the affinities of the acids, and alkalis, are, in fact, nothing more than the affinities of *compound substances*, consisting of the *acids* or *alkali*, and *water*.

The above-mentioned, 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 comprize 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.

Of the Use of Terms.

IN writing on the subject of *different kinds of air*, I found 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 were, *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 before him, had given that denomination; since it is originally part of some solid substance, and exists in an unelastic 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 have used the term *fixed air*, in the sense in which it is now commonly used, and have distinguished the other kinds by their properties, or some other periphrasis. I have been under

a ne-a neceſſity, however, of giving names to thoſe kinds of air, to which no names had been given by others, as *nitrous*, *acid*, *alkaline*, &c.

No perſon was ever more temperate, or more cautious, than I have been in the introduction of *new terms*, conſidering the number of *new facts* that I have diſcovered. It was with great heſitation, though compelled by neceſſity, that I did it at all; generally with the advice of my moſt judicious friends, and always adopting ſuch as were analogous to others in eſtabliſhed uſe. Thus when I found the terms *common or atmoſpherical air*, *fixed air*, and *inflammable air*, uſed by all philoſophers, and no perſon whatever had objected to them, it was certainly natural for me to continue to apply the term *air* to other *elaſtic tranſparent fluids*, not condenſable by cold, and to diſtinguiſh them by other appellations, drawn from the peculiar circumſtances of their production, as *nitrous air*, *acid air*, *alkaline air*, *phlogiſticated* and *dephlogiſticated air*, *&c.* uſing the term *air* as expreſſive of the mere *form* in which a ſubſtance is exhibited, without any conſideration of the elements of which it conſiſts. I therefore think the term *gas*, which many uſe, in this ſenſe, to be unneceſſary; the term *air*, as it had long been uſed by philoſophers, being ſufficient for the purpoſe.

They

They who chuſe to apply the term *air* to a *ſubſtance*, and not to a *form*, are certainly at full liberty ſo to do, if they pleaſe; and provided we underſtand one another, no inconvenience will reſult from our uſe of a different language. But then the ſame perſons ſhould be uniform in their objections and practice, and call nothing by the name of *air* that they do not believe to conſiſt of that one *elementary ſubſtance* to which they profeſs to appropriate the term. The language that I adopt, in this reſpect, implies no attachment to any hypotheſis whatever, and may ſtill be uſed though I ſhould change my opinion on that ſubject; which is certainly a very great advantage in philoſophical language. In adopting the terms *phlogiſticated* and *dephlogiſticated* air, I did not, I own, uſe the ſame judgment; but as by good fortune, they do not appear at all improper, I do not ſee any ſufficient reaſon to abandon them. The *azote* in the new nomenclature is not expreſſive of any thing peculiar to what I have called phlogiſticated air; and the term *vital*, does not ſufficiently diſtinguiſh dephlogiſticated from *common*, or *atmoſpherical* air.

Some perſons more particularly 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

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

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

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

SEC-

SECTION III.

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

RATHER than defcribe at large the manner in which every particular experiment that I fhall have occafion to recite was made, which would both be very tedious, and require an unneceffary multiplicity of drawings, I think it more advifeable to give, at one view, an account of all my apparatus and inftruments, or at leaft of every thing that can require a defcription, and of all the different operations and proceffes in which I employ them.

It will be feen that my apparatus for experiments on air is, in fact, nothing more than that of Dr. Hales, Dr. Brownrigg, and Mr. Cavendifh, diverfified, and made a little more fimple.

For experiments in which air will bear to be confined by water, I firft ufed an oblong trough made of earthern ware, as *a* Plate I. fig. 1. about eight inches deep, at one end of which I put thin flat ftones, about an inch, or half an inch, under the water, ufing more or fewer of them according to the quantity of water in the trough. I afterwards found it more convenient to ufe a larger wooden trough, of of the ſame form, with a ſhelf about an inch lower than the top, inſtead of the flat ſtones above-mentioned. But I now uſe a trough, two feet two inches long, one foot two inches wide, and nine inches deep, for common purpoſes, and others of different dimenſions for particular uſes. In making them the joints are fixed in freſh paint, which renders them perfectly water tight.

In one end of this trough are *ledges*, on which it can ſlide, ſo that I can take it out with pleaſure; I have alſo a *ſhelf* like Fig. 1. Plate III. except that it is not ſuſpended, as that is, by thin pieces of copper, bended into the form of hooks, which, however, anſwered very well. The ſhelf is about an inch and an half in thickneſs, for the convenience of excavating the under-ſide in the form of *funnels*, the orifices of which, about a quarter of an inch in diameter, appear on the upper ſide, as the form and ſize of the cavity below is expreſſed by the dots above. This was an ingenious contrivance of the Duc de Chaulnes.

Theſe funnels ſhould be made as capacious as poſſible; but care ſhould more eſpecially be taken, that no part of them be too flat, leſt any bubbles of air ſhould be retained, and not paſs into the veſſels placed to receive them.

When freſh air is generated, it is convenient to introduce the tube of the phial in which it is produced, quite under the ſhelf, into the hollow of the

the funnel. But when it happens that the ſweep of the tube is to ſhort for that purpoſe, I make uſe of a ſmall production of the upper part of the ſhelf, with a ſlit in it, under which the ſhorter tube may be brought; and the edge of the jar that receives the air, may be made to ſlide over the place at which the bubbles iſſue.

Fig. 2. Plate III. is a ſide view of a glaſs *funnel* ſupported by a wooden pillar, riſing from a baſe, to which a plate of lead is faſtened, in order to make it ſink, and keep its place in the water. At the top of the pillar is a piece of wood cut in front (but, for that reaſon, not viſible in this figure) in a concave form, for ſupporting a glaſs tube, that, reſting on the orifice of the funnel, may lean againſt it. Both this piece of wood, and alſo that which ſupports the funnel, are made to ſlide up and down, and are fixed by wedges at whatever height is found to be moſt convenient. This apparatus ſaves the trouble and inconvenience of keeping one's hand in the water for the ſake of holding the funnel, while the air is pouring through it.

Fig. 3. Pl. III. repreſents an apparatus that would not deſerve a copper-plate, but that there is often great convenience in little things. It exhibits a baſon of water, or quickſilver, ſo placed, in a frame of wood, as to contain ſeveral glaſs tubes, which may be ſupported with little trouble, and diſpoſed of without materially interfering with each other. In this

this manner I have often more than half a dozen in ufe at the fame time.

After ufing this *bafon* for quickfilver, which, on many accounts, is, in general, more convenient than any other form of a *refervoir*. I found I had had occafion to transfer air from one jar to another in quickfilver, in the fame manner as I had ufed to do in water; and then I found it abfolutely neceffary for this purpofe, to make ufe of an oblong *trough*, Pl. V. fig. 1. That which I have commonly ufed is made of wood, feven inches long, three wide, and three deep, made cylindrical at the bottom, in order to make the leaft quantity of of quickfilver neceffary. But I have an upright piece of wood at one end, contrived to fupport tall glafs veffels without danger of falling. It is only with fuch an apparatus as this, that given quantities of alkaline and acid airs can be mixed, as is defcribed in the courfe of the work.

The feveral kinds of air I ufually keep in *cylindrical jars*, as *c*, *c*, Pl. I. fig. 1, about ten inches long, and two and an half wide, being fuch as I have generally ufed for electrical batteries; but I have likewife veffels of very different forms and fizes, adapted to particular experiments.

When I want to remove veffels of air from the large trough, I place them in *pots* or *difhes*, of various fizes, to hold more or lefs water, according to the time that I have occafion to keep the air, as fig.

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

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

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

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

Mice muſt be kept in a pretty exact temperature, for either much heat or much cold kills them preſently. The place in which I have generally kept them, was 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.

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

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

There may appear, at first sight, some difficulty in opening the mouth of a phial, containing any substance, solid or liquid, to which water must not be admitted, in a jar of any kind of air, which is an operation that I have sometimes had recourse to; but this I easily effect by means of *a cork cut tapering,* and

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 in the trough of water, I can eaſily convey the phial through the water into the jar, which muſt either be held by an aſſiſtant, or be faſtened by ſtrings, with its mouth projecting over the ſhelf. When the phial is thus conveyed into the jar, the cork may eaſily be removed, and may alſo be put into it again at pleaſure, and conveyed the ſame way out again.

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

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

In order to expel air from ſolid ſubſtances by means of heat, I ſometimes put them into a *gun-barrel*, Pl. II. 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 by wires, in the manner deſcribed in the figure, or reſting on a ſolid ſupport: 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, ſuch as *a*, *a*, *a*, Pl. IV. full of quickſilver, with their mouths immerſed in the ſame, and then throwing the focus of a burning mirror upon them. For this purpoſe the phials ſhould be made with their bottoms round, and very thin, that they may not be liable to break with a pretty ſudden application of heat.

If I want to expel air from any liquid, I nearly fill a phial with it, and having a cork perforated, I put through it, and ſecure with cement, a glaſs tube

tube, bended in the manner represented at *e* Pl. I. 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 a bason of quicksilver. 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.

I would observe, with respect to this process, and every other in which vessels are to be filled with quicksilver, and then to be placed inverted in basons of the same, that no operation is easier (unless the mouth of the vessel be exceedingly wide) when the mouth of it is covered with soft leather, and, if necessary, tied on with a string, before it be turned upside down; and the leather may be drawn from under it when it is plunged in the quicksilver. If the mouths of the vessels be very narrow, it will be sufficient, and most convenient, to cover them with the end of one's finger.

But if the air, disengaged from any substance, will be attracted by mercury, as is the case with all those which contain the nitrous acid, this process cannot be used, and recourse must be had to the vacuum; and for this purpose it is necessary that the operator be provided with receivers made very thin, on purpose for these experiments. Such as are commonly uſed for other experiments are much too thick for this purpoſe, being very liable to break with the application of the heat produced by the burning lens. In this proceſs, care ſhould be taken to place the materials on glaſs, a piece of crucible, or ſome other ſubſtance that is known to yield no air by heat.

The figure, *b* Pl. IV. repreſents a common glaſs phial with a ground ſtopper, with many ſmall holes in it, which was a happy contrivance of my ingenious pupil and friend Mr. Benjamin Vaughan. It is of excellent uſe to convey any liquid, or even any kind of air, contained in it, through the water, into a jar ſtanding with its mouth inverted in it, without admitting any mixture of the common air, or even of the water; and yet the air generated within it has a ſufficient out-let. Theſe phials will be found uſeful in a great variety of experiments.

The figure *c*, repreſents a phial of the ſame form with *a*; but the neck is thicker, in order to be fitted with a ground ſtopper, perforated, and drawn out into a tube, to be uſed inſtead of the phial *e*, Pl. I. Till I hit upon this contrivance, which was executed for me by the direction of Mr. Parker, I had a great deal of trouble in perforating common corks, bending and fitting tubes to them; and, after all, the corks themſelves, or the cement, with which I generally found it convenient to cover the ends of the

the tubes, were apt to give way, and to be the occasion of very disagreeable accidents. Besides, if any hot acid was used, the vapour would corrode the cork, and an allowance was to be made for the effect of that circumstance on the air: whereas, with this apparatus, which is exceedingly convenient and elegant, the operator may be sure that nothing but glass is contiguous to the materials he works upon, as he can perfectly exclude every other foreign influence; and while it remains unbroken, it is never out of repair, or unfit for use.

For many purposes, however, the former method, with corks and tubes, will be found very sufficient, and much less expensive; especially with the *fluor acid*, which corrodes glass, and which will presently eat through one of these delicate phials. For this purpose, therefore, I would recommend the use of a common and very thick phial, especially as no great degree of heat, and never any sudden application of heat, is wanted.

The phial *c*, will be found sufficient for any purpose that does not require more heat than the flame of a candle held close to the bottom of it, can supply: but if there be occasion to place the phial in a sand-heat, and consequently if it must be put into a crucible placed on the fire, it will be necessary to have the tube, in which the ground stopper terminates, made as long as may be, as represented by *e*;

otherwiſe the veſſels that receive the air will be too near the fire. Nine or twelve inches, however, will be a ſufficient length for any purpoſe.

I have great reaſon to congratulate myſelf on this apparatus, having found it to be of moſt admirable uſe. For, in experiments with air, where the greateſt poſſible accuracy is required, *lutes* are by no means to be truſted, ſince a variety of vapours, coming into contact with them, are conſiderably affected; whereas theſe ſtoppers being ground air-tight, the operator may be perfectly at eaſe, both with reſpect to the quantity and the quality of his produce. To expreſs this proceſs as conciſely as poſſible, I generally allude to it, by ſaying that the phials have *ground ſtoppers and tubes.*

In experiments in which it is not worth while to be at the expence of theſe phials with ground ſtoppers and tubes, and yet where gun-barrels cannot be truſted to, on account of the materials corroding the iron, I have recourſe to a kind of long phial, or a tube made narrower at the open end, nine or twelve inches in length, and of an equal thickneſs throughout, repreſented Pl. IV. fig. *d.* When theſe phials are put into a crucible with ſand, the bottom may be made red-hot, while the top is ſo cool, that a common cork (into which a glaſs tube is inſerted) will not be affected by the heat. In fact, this veſſel is a kind of a gun-barrel made of glaſs, and is uſed exactly like the gun-barrel, except that it is not exposed to so great a degree of heat.

When the materials are put into this vessel, it must be filled up to the mouth with fine sand, that will give no air by the application of heat, and the cork must be thrust down close upon the sand. The air must be received as in plate 2. fig. 7. These glass vessels, however, will not bear a great degree of heat, and therefore by applying to Mr. Wedgwood (who is as great, and generous a friend of *science*, as he is distinguished by the wonderful improvements he has made on his own beautiful *art)* I got *earthen tubes* and *retorts*, which will bear any degree of heat, and being glazed, or not, as the occasion requires, I have found them of the most extensive use in my experiments.

When a person has a great many trials to make of the goodness of air, it is of no small importance to have contrivances by which he may save time. Having, particularly, had frequent occasion to measure the purity of air by means of nitrous air, in which it is sometimes necessary to put several measures of one kind to one measure of the other; and being wearied with taking all the measures separately, at length I hit upon the very useful expedient of having the measures ready made, consisting of vessels, the capacities of which had a known proportion to each other, as *f*, *f*, *f*, Pl. IV. each vessel holding twice as much as the size next less than it.

I found

I found it likewise convenient to have the veſſels in which the mixture of air is made, fig. *g*, marked in a manner correſponding to theſe phials, that the diminution of the air may be perceived at once, without the application of any meaſure. If one of theſe phials contain an ounce-meaſure, and the reſt be multiplies and ſubdiviſions of it, it will be ſtill more convenient.

There is a great variety of methods of mixing nitrous and common air, in order to aſcertain the purity of the latter. But the manner in which I have now long been accuſtomed to perform that operation is ſtill more ſimple, though it has nothing to boaſt of with reſpect to ingenuity. It is neceſſary to deſcribe it, becauſe it is referred to through the greater part of this work.

I firſt provide a phial, containing about an ounce of water, which I call *the air meaſure*. This I fill with air by having firſt filled it with water, and placed it over the opening of the funnel in my ſhelf; and when it is filled I ſlide it along the ſhelf, always obſerving that there be a little more air than I want. The phial being thus exactly filled with the air which I am about to examine, and care being taken that it be not warmed by holding in the hand, &c. I empty it into a jar about an inch and an half in diameter, and then introduce to it the ſame meaſure of nitrous air, and let them continue together about two minutes. I chuſe to have an overplus of nitrous air, that I may be ſure to have phlogiſton enough to ſaturate all the common air. If I find the diminution with theſe meaſures to be very conſiderable, I introduce another meaſure of nitrous air; but the pureſt dephlogiſticated air will not, I believe, require more than two equal meaſures of nitrous air.

Sometimes I leave the common and nitrous air in the jar all night, or a whole day; but always take care that, whatever kinds of air I be comparing together, they remain the ſame ſpace of time before I proceed to note the degree of diminution. If the two kinds of air be agitated on coming into contact with each other, the diminution will be much greater; and therefore this circumſtance ſhould always be expreſſed.

When the preceding part of the proceſs is over, I transfer the air into a glaſs tube, about two feet long, and one third of an inch wide, carefully graduated according to the air-meaſure, and divided into *tenths* and *hundred parts*; ſo that one of the latter will be about a ſixth or an eighth of an inch. Then immerſing the tube in a trough of water, ſo that the water in the inſide of the tube ſhall be on a level with the water on the outſide, I obſerve the ſpace occupied by them both, and expreſs the reſult in *meaſures*, and *decimal parts of a meaſure*, according to the graduation of the tube.

It is ſome trouble to graduate a tube in this manner; but when it is once done, the application of it is extremely easy. As it will seldom happen that a glass tube is of an equal diameter throughout, I generally fill that part of the tube which contains one measure, with quicksilver, and then weighing it, and dividing it into ten parts, put them in separately, in order to mark the primary divisions. This operation is performed very readily by having a glass tube drawn out to a fine orifice, in order to take up a small quantity of quicksilver at a time, as it may be wanted.

Measuring the purity of respirable air, I mix with it an equal quantity of nitrous air, or if it be highly dephlogisticated, two equal quantities of nitrous air, which is always particularly mentioned in the course of this work: after this I transfer the mixture into a graduated tube. Consequently a less number in the result is always an indication of greater purity. This number, in order to be as concise as possible, I have in this work termed *the measure of the test*, or the *standard of the air*. Thus, if when I mix two equal quantities of common air and nitrous air, they afterwards occupy the space of one measure, and two tenths of a measure, I say *the measures of the test were* 1. 2. or the standard of the air was 1. 2.

If the quantity of the air, the goodness of which I wanted to ascertain, was exceedingly small, so as to be contained in a part of a glass tube, out of which water will not run spontaneously, I formerly

merly had recourſe to the following method; I firſt meaſured with a pair of compaſſes the length of the column of air in the tube, the remaining part being filled with water, and laid it down upon a ſcale; and then, thruſting a wire of a proper thickneſs, into the tube, I contrived, by means of a thin plate of iron, bent to a ſharp angle, to draw it out again, when the whole of this little apparatus was introduced through the water into a jar of nitrous air; and the wire being drawn out, the air from the jar muſt ſupply its place. I then meaſured the length of this column of nitrous air which I had got into the tube, and laid 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 forced the two ſeparate columns of air into contact; and when they have been a ſufficient time together, I meaſured the length of the whole, and compared it with the length of both the columns taken before. But I now have tubes, made very ſmall for this purpoſe, and a longer tube, graduated in proportion, which I uſe as I do the larger veſſels when the quantity of air is ſufficient.

In experiments on thoſe kinds of air which are readily imbibed by water, I often make uſe of quickſilver, in the manner repreſented Pl. II. fig. 8, in which *a* is the baſon of quickſilver, *b* a glaſs veſſel con-

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* Pl. I. In order to invert the veſſel *b*, I firſt fill it with quickſilver, and then carefully cover the mouth of it with a piece of ſoft leather; after which it may be turned upſide down without any danger of admitting the air, and the leather may be withdrawn when it is plunged in the quickſilver.

In order to generate air by the ſolution of metals, or any proceſs of a ſimilar nature, I put the materials into a phial, prepared in the manner repreſented at *e* Pl. I. 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 Pl. II fig. 9, which conſiſts of a bladder, furniſhed at one end with

with a ſmall glaſs tube bended, and at the other with a cork, perforated ſo as juſt to admit the ſmall end of a funnel. When the common air is carefully preſſed out of this bladder, and the funnel is thruſt tightly into the cork, it may be filled with any kind of air as eaſily as a glaſs jar; and then a ſtring being tied above the cork in which the funnel is inſerted, and the orifice in the other cork cloſed, by preſſing the bladder againſt it, it may be carried to any place, and if the tube be carefully wiped, the air may be conveyed quite free from moiſture through a body of quickſilver, or any thing elſe. A little practice will make this very uſeful manœuvre perfectly eaſy and accurate. But I find it more convenient to have a ſmall *braſs cock*, to thruſt into the cork, through which the air is introduced into the bladder.

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* Pl. II. 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,

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 connexion with a phial *c* fig. 10, containing the same materials (as chalk, salt of tartar, or pearl ashes in diluted oil of vitriol, for the generation of fixed air) and taking care (lest, in the act of effervescence, any of the materials in the phial *c* should get into the vessel *a)* to place this phial on a stand lower than that on which the bason was placed, I press out the newly generated air, and make it ascend directly into the fluid. For this purpose, and that I may more conveniently shake the phial *c*, which is necessary in some processes, especially with chalk and oil of vitriol, I sometimes make use of a flexible leathern tube *d*, and sometimes only a glass tube. For if the bladder be of a sufficient length, it will give room for the agitation of the phial; or if not, it is easy to connect two bladders together by means of a perforated cork, to which they may both be fastened.

When I want to try whether any kind of air will admit a candle to burn in it, I make use of a cylindrical glass vessel, Pl. I. 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

that the candle is admitted. In this manner I have frequently extinguifhed a candle more than twenty times fucceffively, in a veffel of this kind, though it is impoffible to dip the candle into it without giving the external air an opportunity of mixing with the air in the infide more or lefs. The candle at the other end of the wire is very convenient for holding under a jar ftanding in water, in order to burn as long as the inclofed air can fupply it; for the moment that it is extinguifhed, it may be drawn through the water, before any fmoke can have mixed with the air.

In order to draw air out of a veffel which has its mouth immerfed in water, and thereby to raife the water to whatever height may be neceffary, it is very convenient to make ufe of a glafs *fyphon*, putting one of the legs up into the veffel, and drawing the air out at the other end by the mouth. If the air be of a noxious quality, it may be neceffary to have a fyringe faftened to the fyphon, the manner of which needs no explanation. I have not thought it fafe to depend upon a valve at the top of the veffel, which Dr. Hales fometimes made ufe of.

If, however, a very fmall hole be made at the top of a glafs veffel, it may be filled to any height by holding it under water, while the air is iffuing out at the hole, which may then be clofed with wax or cement.

If the generated air will neither be abſorded by water, nor diminiſh common air, it may be convenient to put part of the materials into a cup, ſupported by a ſtand, and the other part into a ſmall glaſs veſſel, placed on the edge of it, as at *f* Pl. I. fig. 1. Then having, by means of a ſyphon, drawn the air to a 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, 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 Pl. II. 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

kind of air with which I would fill the receiver, I only turn the cock, and I gain my purpose. In this method, however, unless the pump be very good, and several contrivances, too minute to be particularly described, be made use of, a good deal of common air will get into the receiver.

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 Pl. II. fig. 16; and having got the air I want into the tube, I place it inverted in a bason containing either quicksilver, or any other fluid substance by which I chuse to have the air confined. I then, by the help of the air-pump, drive out as much of the air as I think convenient, admitting the quicksilver, &c. to it, as at *a*, and putting a brass ball on the end of the wire, I take the sparks or shocks upon it, and thereby transmit them through the air to the liquor in the tube.

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

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

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

I have found it very convenient to have a number of *glaſs veſſels*, ſuch as repreſented Pl. V. fig. 2, for the purpoſe of making a quantity of air paſs through a body of water, or any kind of fluid, or any ſubſtance in the form of powder; the air entering by the tube which goes to the bottom of the veſſel, and being delivered by that which is inſerted only at the top. I alſo found it neceſſary to have theſe veſſels of various ſizes, the largeſt containing about a pint, and the ſmalleſt about half an ounce meaſure of water. The larger end of this veſſel I have generally cloſed with a cork, and cement; but I ſometimes found it neceſſary to have this part alſo of glaſs, with

with only two ſmall perforations, for the inſertion of glaſs tubes.

I have frequently had occaſion to make uſe of a great number of theſe veſſels at the ſame time, ſo diſpoſed, as that the ſame air might paſs through them all in ſucceſſion, in the manner repreſented, fig. 3.

In ſome caſes, however, I found it neceſſary to exclude all cement, and every kind of luting, from an apparatus of this kind; having had all the glaſs tubes fitted to their ſeveral holes by grinding. But this makes the apparatus very expenſive, and eſpecially the repairs of it.

Annexed to the laſt-mentioned apparatus, is a *long phial*, *a* fig. 3, with a tube fitted to it by grinding, and bent, ſo as to diſcharge the air, or vapour, iſſuing from it, downwards. This kind of phial I have generally uſed for my experiments with nitrous vapour. The phial is deep, in order to admit a ſudden and violent effervеſcence without the danger of the liquor being thrown over, and the tube ſhould be long enough, to go to the bottom of any veſſel in which the vapour is to be delivered.

In diſtilling ſpirit of nitre, I have generally made uſe of the apparatus repreſented Pl. V. fig. 4, which was invented by Mr. Woulfe, conſiſting of a retort *a*, an adopter, if neceſſary, *b*, and a receiver *c*, with two orifices; one *d*, for the diſcharge of the diſtilled acid, and the other *e*, to ſerve as an outlet for the ſuperabundant vapour; which, paſſing through the glaſs tube *f*, may impregnate the water in the baſon *g*.

Pl. III. fig. 4, repreſents a *cylindrical veſſel* made of tin, incloſing another of iron wire. In the outer veſſel a charcoal fire may be made, ſurrounding the inner cylinder, which, being open at the bottom, will admit the upper part of a glaſs jar, ſupported in whatever manner the operator may find moſt convenient. Thus a jar, with the air, &c. contained in it, may be heated as much as the glaſs will bear, without giving more heat than is neceſſary to the lower part of it. In this manner alſo, an equal degree of heat may be given to every ſide of the upper part of the glaſs.

Pl. III. fig. 5, explains the manner in which I make an electrical exploſion paſs through any ſubſtance in the form of vapour. It repreſents a glaſs ſyphon, in each leg of which is an iron wire, of ſuch a length, that there ſhall only be about half an inch between the heads of them. The ſyphon muſt be filled with mercury, and each of the legs inſerted in ſeparate baſons, alſo containing mercury. After this, the ſubſtance may be introduced into the ſyphon by means of a glaſs tube, and, being lighter, it will take its place in the bend of the ſyphon; which may then be placed near the opening of a ſmall furnace, or in the apparatus deſcribed fig. 3, when whatever

ever lodges in the upper part of the ſyphon will be converted into vapour, and the exploſion will be made in it by making the ſyphon part of an electrical circuit. Mercury itſelf may be converted into vapour in the ſame manner.

It may be worth while to give a ſhort account of the *earthen jar*, in which I made many of the experiments on the growth of plants in different kinds of air, recited in this volume; and a bare inſpection of Pl. VI. fig. 1, will be almoſt ſufficient for this purpoſe.

The jar was about eighteen inches in diameter at the top, and of the ſame depth. It was placed in an open expoſure in the garden, and ſticks were thruſt into the earth in a perpendicular poſition, quite round it; and to theſe ſticks glaſs jars, filled with water, with their mouths inverted in the water of the earthen jar, were faſtened by ſtrings. After I had introduced into one of theſe jars any particular kind of air, I afterwards drew through the water, and put into it, any plant, the top and leaves of which I wiſhed to expoſe to it; ſupporting the root or ſtalk at a proper height in the earthen jar, if I found that any ſuch ſupport was neceſſary. In ſome caſes it will be found that the top of the plant was in one jar, and the root or ſtalk in another; which it was not at all difficult to do.

Pl. VI. fig. 2, repreſents the inſtrument by which I endeavoured to aſcertain the conducting power of different kinds of air with reſpect to heat. It conſiſts of a glaſs bulb open at both ends, ſo that I could eaſily faſten a thermometer with its bulb in the center of it, where it would be ſurrounded by any kind of air, introduced into it after it had been previouſly filled with mercury. The manner in which the experiments were made is ſufficiently deſcribed in the account of them.

Pl. VII. fig. 1, is a view of the apparatus with which the principal experiments relating to the ſeeming converſion of water into air were made. It conſiſts of *an earthen veſſel*, the bulb of which, containing moiſtened clay, is fixed in the inſide of *a glaſs veſſel*, through which the heat of a burning lens may be thrown upon it; while the inſide has a communication with a baſon of water, or mercury, in which veſſels may be placed to receive the air that is forced through the body of the earthen veſſel; while the water, or mercury, in the *baſon* in which the glaſs veſſel ſtands, riſes within it, to ſupply the place of that air.

Pl. VII. fig. 2, ſhews the diſpoſition of the apparatus by which ſteam is tranſmitted through a red-hot tube, containing iron, &c. with *a worm tub* to collect the ſuperfluous water, &c. and a veſſel to receive the air

air that is produced. This veſſel is here drawn very ſmall, that it might not take up much room in the plate; but I have generally uſed a large trough for this purpoſe, and jars of conſiderable ſize to receive the air. Inſtead of the ſmall *furnace* to heat the water, &c. I now uſe one of *Mr. Argand's lamps*, which is, on ſeveral accounts, a very valuable addition to a chemical apparatus. Fig. 6, repreſents the method of receiving the air in this proceſs under a funnel, fixed in a trough of water, which may be uſed when large ballōons are filled, and when no account is taken of any water that is condenſed in the proceſs.

Fig. 4, repreſents a large glaſs balloon, in which inflammable air, iſſuing from the orifice of a ſmall tube, burns like a candle, while the water produced by the proceſs is collected in the inſide of it.

Fig. 5, repreſents a ſtrong cylindrical glaſs veſſel, in which inflammable and dephlogiſticated air may be fired. It is furniſhed with a wooden cap, firmly cemented to the open end of it, and cloſed with a ſcrew, and two iron wires are inſerted at the top of it, between which an electric ſpark can be taken.

ADVER-

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. Having sometimes used the* penny-weight, *it may be necessary to acquaint Foreigners, that* 24 *grains are a penny-weight, that* 20 *of such penny-weights make an ounce, and* 12 *ounces a pound.*

The same ounce Troy, is, by Apothecaries, divided into eight drams, each dram into three scruples, and the scruple into twenty grains.

BOOK I.

OBSERVATIONS AND EXPERIMENTS RELATING TO FIXED AIR.

PART I.

OF THE RELATION OF FIXED AIR TO WATER.

SECTION I.

Of the impregnation of water with fixed air.

IT was in conſequence of living for ſome time in the neighbourhood of a public brewery, a little after Midſummer in 1767, that I was induced to make experiments on fixed air, of which there is always a large body, ready formed, on 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

fore far from being perfectly pure, yet there is a conſtant freſh ſupply from the fermenting liquor, and it is pure enough for many purpoſes.

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

Making an agitation in this air, the ſurface of it (which ſtill continues to be exactly defined) is thrown into the form of waves, which 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.

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.

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

This floated upon the ſurface of the fixed air like other fumes, and continued as long.

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

One would naturally think, that having actually impregnated common water with fixed air, produced in a brewery, I ſhould immediately have ſet about doing the ſame thing with air let looſe from chalk, &c. by ſome of the ſtronger acids. But, eaſy as the practice proved to be, no method of doing

doing it at that time occurred to me. I ſtill continued to make my Pyrmont water in the manner above mentioned till I left that ſituation, which was about the end of the ſummer 1768; and from that time, being engaged in other ſimilar purſuits, I made no more of the Pyrmont water till the ſpring of the year 1772.

In the mean time I had acquainted all my friends with what I had done, and frequently expreſſed my wiſhes that perſons who had the care of large *diſtilleries* (where I was told that fermentation was much ſtronger than in common breweries) would contrive to have veſſels of water ſuſpended within the fixed air which they produced, with a farther contrivance for agitating the ſurface of the water; as I did not doubt but that, by this means, they might, with little or no expence, make great quantities of Pyrmont water; by which they might at the ſame time both ſerve the public, and benefit themſelves. For I never had the moſt diſtant thought of making any advantage of the ſcheme myſelf.

In all this time, viz. from 1767 to 1772, I never heard of any method of impregnating water with fixed air but that above mentioned. My thinking at all of reducing to practice any method of effecting this, by air diſlodged from chalk, and other calcareous ſubſtances, was occaſioned by my hearing of Dr. Irving's method of diſtilling ſea water for

for the uſe of the navy. For it occurred to me, that if ſeamen could be taught a method of impregnating that or any other water with fixed air, it might be farther uſeful to prevent, or to cure the ſea ſcurvy, going upon Dr. Macbride's idea of fixed air being an antiſeptic.

Mentioning this ſcheme to Sir George Saville, he introduced me to Lord Sandwich, then at the head of the admiralty, who procured an order for the college of phyſicians to examine it. As they were pleaſed to recommend the trial of it, I drew up an account of the method which I had then deviſed, in a ſmall pamphlet; the ſubſtance of which, as it is no longer publiſhed ſeparately, I inſert here.

Directions for impregnating water with fixed air.

If water be only in contact with fixed air, it will begin to imbibe it, but the mixture is greatly accelerated by agitation, which is continually bringing freſh particles of air and water into contact. All that is neceſſary, therefore, to make this proceſs expeditious and effectual, is firſt to procure a ſufficient quantity of this fixed air, and then to contrive a method by which the air and water may be ſtrongly agitated in the ſame veſſel, without any danger of admitting the common air to them; and this is eaſily done by firſt filling any veſſel with water, and introducing the

the fixed air to it, while it ſtands inverted in another veſſel of water.

Take therefore a glaſs veſſel, *a*, Pl. VIII. fig. 1. with a pretty narrow neck, but ſo formed, that it will ſtand upright with its mouth downwards (or it may be ſupported as in Pl. III. fig. 3) and having filled it with water, lay a ſlip of clean paper, or thin paſteboard upon it. Then, if they be preſſed cloſe together, the veſſel may be turned upſide down, without danger of admitting common air into it; and when it is thus inverted, it muſt be placed in another veſſel, in the form of a bowl or baſon, *b*, with a little water in it, ſo much as to permit the ſlip of paper or paſteboard to be withdrawn, and the end of the pipe *c* to be introduced.

This pipe muſt be flexible, and air-tight, for which purpoſe it is, I believe, beſt made of leather, ſewed with a waxed thread, in the manner uſed by ſhoe-makers. Into each end of this pipe a piece of a quill, or tube of tinned iron, ſhould be thruſt, to keep them open, while one of them is introduced into the veſſel of water, and the other into a cork, which muſt be perforated, and fitted to a veſſel *e*, two thirds of which ſhould be filled with chalk, or pounded marble, well covered with water.

Things being thus prepared, and the veſſel containing the chalk and water being detached from the veſſel of water, pour a little oil of vitriol upon the chalk and water, and put the cork into the bottle a little time after the effervescence has begun; and then introduce the end of the pipe into the mouth of the veſſel of water, as in the drawing, and, if neceſſary, agitate the chalk and water briſkly. This will preſently produce a conſiderable quantity of fixed air, which will force its way through the pipe, and aſcend into the veſſel of water, the water at the ſame time deſcending, and coming into the baſon.

When about one half of the water is forced out, let the operator lay his hand upon the uppermoſt part of the veſſel, and ſhake it as briſkly as he can, and in a few minutes the water will abſorb the air; and taking its place, will nearly fill the veſſel as at the firſt. Then ſhake the phial containing the chalk and water again, and force more air into the veſſel, till, upon the whole, about an equal bulk of air has been thrown into it. Alſo ſhake the water as before, till no more of the air can be imbibed. As ſoon as this is perceived to be the caſe, the water is ready for uſe; and if it be not uſed immediately, ſhould be put into a bottle as ſoon as poſſible, well corked, and cemented. It will keep, however, very well, if the bottle be only well corked, and kept with the mouth downwards.

In general, the whole proceſs does not take up more than about a quarter of an hour, the agitation not five minutes; and in nearly the ſame time might a veſſel of water, containing two or three

gallons,

gallons, or indeed any quantity that a perſon could well ſhake, be impregnated with fixed air, if the phial containing the chalk and oil of vitriol, be larger in the ſame proportion.

To give the water as much air as it can receive in this way, the proceſs may be repeated with the water thus impregnated. I generally chuſe to do it two or three times, but very little will be gained by repeating it oftener; ſince, after ſome time, as much fixed air will eſcape from that part of the ſurface of the water which is expoſed to the common air, as can be imbibed from within the veſſel.

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.

All calcareous ſubſtances contain fixed air, and any acids may be uſed in order to ſet it looſe from them; but pounded lime ſtone, or the ſawings of marble, and oil of vitriol are, both of them the cheapeſt, and, upon the whole, the beſt for the purpoſe.

 I ſhould

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 air.

If any perſon would chuſe to make this medicated water more nearly to reſemble genuine Pyrmont water, Sir John Pringle informs me, that from eight to ten drops of *Tinctura Martis cum ſpiritu ſalis* muſt be mixed with every pint of it. It is agreed, however, on all hands, that the peculiar virtues of Pyrmont, or any other mineral water which has the ſame briſk or acidulous taſte, depend not upon its being a chalybeate, but upon the fixed air which it contains.

But water impregnated with fixed air does of itſelf diſſolve iron, as the ingenious Mr. Lane has diſcovered; and iron filings put to this medicated water make a ſtrong and agreeable chalybeate, ſimilar to ſome other natural chalybeates, which hold the iron in ſolution by means of fixed air only, and not by means of any acid; and theſe chalybeates, I am informed, are generally the moſt agreeable to the ſtomach.

By this proceſs may fixed air be given to wine, beer, and almoſt any liquor whatever: and when beer is become flat or dead, it will be revived by this means; but the delicate agreeable flavour, or acidulous taſte communicated by the fixed air, and which is manifeſt in water, will hardly be perceived in wine, or other liquors which have much taſte of their own.

I would not interfere with the province of the phyſician, but I cannot intirely ſatisfy myſelf without taking this opportunity to ſuggeſt ſuch hints as have occurred to myſelf, or my friends, with reſpect to the *medicinal uſes* of water impregnated with fixed air, and alſo of fixed air in other applications.

In general, the diſeaſes in which water impregnated with fixed air will moſt probably be ſerviceable, are thoſe of a *putrid* nature, of which kind is the *ſea-ſcurvy*. It can hardly be doubted, alſo, but that this water muſt have all the medicinal virtues of Pyrmont water, and of other mineral waters ſimilar to it, whatever they be; eſpecially if a few iron filings be put to it, to render it a chalybeate, like genuine Pyrmont water. It is poſſible, however, that, in ſome caſes, it may be deſirable to have the *fixed air* of Pyrmont water, without the *iron* which it contains.

Having this opportunity, I ſhall alſo hint the application of fixed air in the form of *clyſters*, which occurred to me while I was attending to this ſubject, as what promiſes to be uſeful to correct putrefaction in the inteſtinal canal, and other parts of the ſyſtem to which it may, by this channel, be conveyed. It has been tried once by Mr. Hey, of Leeds, and the recovery of the patient from an alarming putrid fever, when the ſtools were become black, hot, and very fetid, was ſo circumſtanced, that it is not improbable but that it might be owing, in ſome meaſure, to thoſe clyſters. The application, however, appeared to be perfectly eaſy and ſafe. Alſo Dr. Warren, of Taunton, adminiſtered fixed air in the ſame manner, with the moſt happy effect.

I cannot help thinking that fixed air might be applied externally to good advantage in other caſes of a putrid nature, even when the whole ſyſtem was affected. There would be no difficulty in placing the body ſo, that the greateſt part of its ſurface ſhould be expoſed to this kind of air; and if a piece of putrid fleſh will become firm and ſweet in that ſituation, as Dr. Macbride found, ſome advantage, I ſhould think, might be expected from the ſame antiſeptic application, aſſiſted by the *vis vitæ*, operating internally, to counteract the ſame putrid tendency. Some Indians, I have been informed, bury their patients, labouring under putrid diſeaſes, up to the chin in freſh mould, which is alſo known to take off the fœtor from fleſh meat beginning to putrify. If this practice be of any

uſe,

uſe, may it not be owing to the fixed air imbibed by the pores of the ſkin in that ſituation?

Being no phyſician, I run no riſque by throwing out theſe random hints and conjectures. I ſhall think myſelf happy, if any of them ſhould be the means of making thoſe perſons, whom they immediately concern, attend more particularly to the ſubject.

There is another ingenious method of impregnating water with fixed air, contrived by Dr. Nooth, by means of three glaſs veſſels, as repreſented in Pl. IX.

In the loweſt veſſel, the chalk or marble, and the water acidulated with oil of vitriol, muſt be put, and into the middle veſſel the water to be impregnated. During the efferveſcence, the fixed air riſes into the middle veſſel, and reſts upon the ſurface of the water in it, while the water that is diſplaced by the air riſes through the bent tube into the uppermoſt veſſel, the common air going out through the channel in the ſtopper. When the bent tube is of a proper length, the proceſs requires no attention; and if the production of air be copious, the water will generally be ſufficiently impregnated in five or ſix hours. At leaſt, all the attention that needs be given to it is to raiſe the uppermoſt veſſel once or twice, to let out that part of the fixed air which is not readily abſorbed by water. If the operator chuſe to accelerate the proceſs, by agitating the water, he muſt ſeparate the two uppermoſt veſſels from the loweſt. For if he ſhould agitate them all together, he will occaſion too copious a production of air; and he will alſo be in danger of throwing the liquor contained in the loweſt veſſel into contact with the ſtopper which ſeparates it from the middle veſſel, by which means ſome of the oil of vitriol might get into the water.

SECTION II.

Of the State of Air in Water.

AFTER treating of the impregnation of water with fixed air, I ſhall recite the obſervations I have at different times made on the ſtate of air expelled from water by heat, eſpecially as in ſeveral caſes this is fixed air.

I have frequently found air expelled from water to be much better than common air; but I have not yet undertaken any regular courſe of experiments on the ſubject; ſuch as examining the ſame water at different times of the year, with different impregnations, different expoſures, &c. which I wiſh to have done; becauſe I think it poſſible, that ſomething worth know-

knowing relating to the properties of water, or of air in water, eſpecially reſpecting phlogiſton, and the general ſtate of the atmoſphere, may be diſcovered by this means. Such obſervations as I have occaſionally made I ſhall here put down.

Boiling generally expels more or leſs of fixed air from water. On the 5th of June, 1779, I found my pump water to yield air, one fifth of which was fixed air, and the meaſures of the teſt for the reſiduum were 1. 5.* The ſame pump water, which had been boiled ſome time before, gave air, one ſeventh of which was fixed air, and the meaſures of the teſt for the reſiduum were 1. 4. In general I believe a greater difference than this will be found in theſe two caſes. I do not know that water will attract fixed air from the atmoſphere, at leaſt in the proportion in which it is generally found in pump water, which is probably acquired from calcareous matters firſt held in ſolution, and then partially decompoſed in it.

Water diſtilled in a glaſs, which had been long expoſed to the open air, yielded air, of which little or none was fixed air, and with equal quantities of nitrous air, the meaſures of the teſt were 1.1.

A quantity of rain water taken from a large tub, which had long ſtood expoſed to the open air, yield-

* In the experiments mentioned in this book, the two kinds of air were not agitated when they were mixed.

ed

ed one ſixtieth of its bulk of air, of which no part was fixed air, and the meaſures of the teſt were 1.4. Perhaps the wood of the tub, or ſome other matter caſually falling into it, might have contaminated this air.

A quantity of river water, not very far from the ſpring, gave one fiftieth of its bulk of air of which the ſmalleſt part imaginable was fixed air, and the meaſures of the teſt were 1.05. This air was very pure; but the part of the river from which I took it was nearly ſtagnant, and very full of water plants.

Lime water is certain not to contain any fixed air. From a quantity of this water I expelled air ſo pure that the meaſures of the teſt were 1.0. The quantity of air was one fiftieth of its bulk. Upon the whole I am inclined to infer, from all the obſervations I have hitherto made, that this is about the ſtandard of air contained in water, which has no fixed air, and has been expoſed to no influences except thoſe of the common atmoſphere, in its uſual ſtate.

From a ſpring which was remarkable for its petrefying quality, I expected much fixed air, but I found none; and the air I extracted from it was a little worſe than common air. It is plain that, in this caſe, a boiling heat had not decompoſed the lime ſtone it contained.

I alſo

I alſo filled a phial with pump water and pounded lime ſtone, expoſed to the ſun from the 28th of May to the 3d of July, when it yielded air ſo pure, that with two equal quantities of nitrous air, the meaſures of the teſt were 1.04. I ſhould have ſuſpected ſome green vegetable matter in this water, but I could not perceive any. Perhaps ſome latent, or naſcent vegetation, might be the cauſe of this very pure air.

That water imbibes dephlogiſticated air from the atmoſphere, is evident from the following obſervation. I took ſome of the Briſtol water in which fiſhes had died, and which then yielded air thoroughly phlogiſticated; and having expoſed it to the ſun from the 28th of May to the 3d of July, I found it to yield a conſiderable quantity of air; and ſo pure that, with an equal quantity of nitrous air, the meaſures of the teſt were 0.76, and with two equal quantities of nitrous air the meaſures were 1.18.

Fixed air abounds ſo much in ſome mineral waters, that their peculiar virtues are certainly owing to this ingredient in their compoſition. This conſideration has led ſome perſons to aſcribe the virtues of other mineral waters to this principle, though they contain it in ſo very ſmall a proportion, as to make that opinion very improbable. Some, for inſtance, have thought that the virtues of the *Bath-*

water

water were owing, in a great meaſure, to the fixed air it contains; and living at no great diſtance from that celebrated ſpring, I thought I ſhould incur a juſt cenſure, if I did not endeavour to aſcertain what kind of air is contained in that water, and in what proportion. Accordingly, I made an excurſion as far as Bath, chiefly with that view, and made the following experiments, which, having no apparatus of my own along with me, I was enabled to perform by the friendly zeal and ingenuity of Mr. Painter; Dr. Guſthart, Dr. Falconer, and Dr. Watſon, favouring me with their preſence.

In order to aſcertain what proportion of air is contained in the water, in the ſtate in which it is drank, I filled a pint-phial with the water hot from the pump, and expelled the air from it, by boiling it about four hours, receiving the produce in quickſilver. This air was about one thirtieth of the bulk of the water, and about one half of it was fixed air, precipitating lime in lime-water, and being readily abſorbed by water. The reſiduum appeared, by the teſt of nitrous air, to be rather better than air in which a candle had burned out.

The quantity of fixed air that appears, by this experiment, to be contained in the Bath-waters is ſo very ſmall, that I think it very improbable that their virtues ſhould be at all owing to it. Few ſpring-waters, I believe, contain much leſs fixed air, and

and many I know, which have no medicinal virtue at all, contain more. The pump-water belonging to the houſe in which I lived at Calne, contains about one fourteenth of its bulk of fixed air; and my pump-water at Leeds, contained about one fiftieth of its bulk of air, of the very ſame compoſition as the air of the Bath-waters, viz. half of it fixed air, and half common air, a little phlogiſticated, ſo as to be in about the ſame ſtate as air in which a candle had burned out.

Beſides, the length of time which the Bath-waters, and indeed moſt other ſpring-waters, require to expel the air by means of heat, ſhews that the air expelled from them, was not contained in them in that ſtate in which it is contained in waters properly impregnated with fixed air, out of which it may always be expelled by the heat of boiling water in leſs than an hour. In fact, the fixed air is not united to the *water*, but to ſome *calcareous matter* in the water, out of which the air is expelled with much more difficulty. Accordingly, Dr. Falconer informs me, that there is a depoſit made by this water, after long boiling. If ſo, it may be preſumed, that theſe waters do not ſo properly contain fixed air, as a calcareous earth; which, though it contain fixed air, may not part with it in the ſtomach, unleſs it meet with ſome acid to decompoſe it.

Beſides

Befides the air contained in the Bath-water, there is a confiderable quantity of air continually bubbling up from almoft every part of the foil, through the water in the bath. To examine this, I took about a pint of that air, and found, upon examination, that only about one twentieth of its bulk was fixed air, precipitating lime in lime-water, and being readily abforbed by water. The reft extinguifhed a candle, and was fo far phlogifticated, that two meafures of it, and one of nitrous air, occupied the fpace of $2\frac{18}{20}$ of a meafure; that is, it was almoft perfectly noxious.

Being in Germany in the fummer of the year 1774, we happened to pafs by the famous fpring of Seltzer-water, near Schwallbach, and alfo another very hot fpring near the road from that place to Mentz. Through both thefe fprings there was a bubbling of air, exactly fimilar to that in the Bath-waters; but I had not time, or convenience, for making the fame experiments upon them, and therefore contented myfelf with finding that the air of both of them extinguifhed a candle.

PART

PART II.

OF THE SUBSTANCES WHICH YIELD FIXED AIR CHIEFLY BY HEAT.

SECTION I.

Of Air extracted from mineral Substances.

HAVING in an early period of my experiments, found that manganese, and other natural mineral substances, yield a very pure air by extreme heat; it occurred to me that subterraneous fires might maintain themselves by means of the air which they dislodged from such substances as they found in the bowels of the earth. This led me to try what *kind*, and what *quantity* of air, would be yielded by various mineral substances, in great heats, and it may not be useless to recite the experiments, as a knowledge of the results may be useful in other philosophical inquiries; and as many of them yielded fixed air, I shall insert the account of them in this place.

As the original object of my inquiry respected *volcanic fires*, I gave particular attention to the exa- mination

mination of volcanic ſubſtances, eſpecially with a view to aſcertain whether a ſubſtance which had been in a ſtate of *fuſion* will yield air by being heated again or not; in order to diſtinguiſh the products of volcanos from other ſtony matters. Though charcoal, which has been expoſed to the moſt intenſe heat, will imbibe air from the atmoſphere, and give it out on being heated a ſecond time, yet this is not a ſubſtance that can be *fuſed*; and as this does not appear to be the property of earthy ſubſtances, ſome dependence may perhaps be placed on this teſt. If ſo, *baſaltes* can hardly be claſſed among volcanic productions, becauſe they yield more air by heat than any known *lava* that I have met with.

But Mr. Keir has obſerved to me, that a ſubſtance from which air has been expelled by fuſion, may yield more air by being melted again in a greater degree of heat, ſo that this teſt is not deciſive.

As the reſults of the experiments that I made both with lavas and baſaltes were various, I ſhall briefly cite them.

Of lava from Iceland, four ounces and one fifth, heated in an earthen retort, gave twenty ounce meaſures of air, of which one half, towards the beginning of the proceſs, was fixed air, and the remainder of the ſtandard of 1.72, extinguiſhing a candle. In the interſtices of this lava, there was a browniſh ſand, which I could not ſeparate from it.

Of

Of lava from Vefuvius, five ounces and a half, yielded thirty ounce meafures of air, of which the firft portion had a flight appearance of fixed air, and the reft was phlogifticated, from the ftandard of 1.64, to 1.38, which came laft. The retort was broken by the fwelling of the mafs in cooling.

Another ounce of lava, of the confiftence of a hard ftone, yielded only three ounce and a half meafures of air, chiefly inflammable, which, I fuppofe, came from the gun-barrel in which this particular experiment was made.

From thefe experiments it feems probable, that genuine lavas do not give much air; but this will depend upon the degree of heat to which they have been fubjected in the fubterraneous fire.

It has been much difputed whether bafaltes be a volcanic production, or only a cryftalization of a mafs of matter in a fluid form. The following experiments incline me to the latter opinion.

Seven ounces of bafaltes from Scotland, heated in an earthen retort, yielded 104 ounce meafures of air, of which the firft portion had a flight appearance of fixed air, and was fo much phlogifticated as to extinguifh a candle, being fometimes of the ftandard of 1.68.

About two ounces of the *giants caufeway* in Ireland, yielded forty ounce meafures of air, the firft portion of which had a flight appearance of fixed air, and the reſt phlogiſticated, of the ſtandard of 1.65. It was reduced by fuſion to a hard black glaſs.

Of baſaltes from Scotland, five ounces one hundred and ſixty-two grains, yielded ſeventy-eight ounce meaſures of air, of which no part appeared to be fixed air; but was all phlogiſticated, ſometimes of the ſtandard of 1.7, and towards the laſt 1.41.

The neighbourhood of Birmingham abounds with a ſtone which, from its being chiefly got from a village called Rowley, near Dudley, is commonly called *Rowley-rag*. When it is broken, it very much reſembles baſaltes, though it is not found in the ſame regular form. Dr. WITHERING, of this place, has given a moſt excellent analyſis of this ſubſtance, which may be ſeen in a late volume of the Philoſophical Tranſactions. All that I did with reſpect to it was, to ſubject it to a ſtrong heat in an earthen retort; and from this mode of examination it ſhould ſeem to be of the ſame nature with the baſaltes, whatever that be.

Four ounces of the Rowley-rag yielded forty ounce meaſures of air, containing hardly any appearance of fixed air, but was phlogiſticated, of the ſtandard of 1.6, and 1.5; the laſt portion 1.31. It was reduced to a black glaſſy ſubſtance, which broke with a poliſh, exactly reſembling that which remained from the baſaltes.

The

The Derbyshire *toadstone*, in its appearance, very much resembles the Rowley-rag, excepting that it is full of white spots, consisting of a calcareous substance. Dr. WITHERING has analysed this, as well as the Rowley-rag, and both from his experiments and mine, they seem to be nearly a-kin to each other. Two ounces and 384 grains of this substance, from which the calcareous part had been dissolved by spirit of nitre, yielded sixty ounce measures of air, the first portion of which contained a little fixed air, perhaps from some unperceived remains of the calcareous matter. The rest was phlogisticated, of the standard of 1.7.

In another experiment, an ounce and a quarter of this substance, from which the calcareous part had been extracted first by oil of vitriol, and then by spirit of nitre, yielded 40 ounce measures of air, of the same quality of that in the former process. There remained from both of them a black glassy matter, which seemed to be very liquid when it was hot, as part of it had boiled up into the neck of the retort.

Granite, like basaltes, has been thought by some to be the product of volcanoes, and by others to be a crystalization from a liquid state. The latter is the opinion favoured (but for the reason given above not decisively proved) by these experiments. From about an ounce and an half of this subſtance I got twenty ounce meaſures of air, the firſt portion of which contained a little fixed air, but the reſt was phlogiſticated, from 1.7 to 1.28, which is nearly the ſtandard of common air; but the heat was very intenſe, as the ſubſtance was reduced to a glaſs.

Again, five ounces and 252 grains of a blue granite yielded ſeventy ounce meaſures of air, of the ſame quality with the preceding. It was alſo reduced to a firm uniform glaſſy ſubſtance, of a dark-brown colour. Upon the whole, therefore, it ſeems probable, that the origin of granite is ſimilar to that of baſaltes.

In Cornwall there is a ſubſtance called *elvain*, the natural hiſtory of which is very like that of granite, and the reſult of my experiments upon it ſhews that they are of the ſame nature. There is a black and white kind of elvain.

Of the black elvain one ounce and 288 grains yielded twenty-five ounce meaſures of air, the firſt portion of which contained a little fixed air, and the reſt was phlogiſticated, of the ſtandard of 1.54. It was melted into a browniſh black maſs.

Of the white elvain one ounce and 384 grains yielded thirty ounce meaſures of air, of the ſame quality with the preceding. It was converted into a very porous ſubſtance, exactly reſembling a pumice ſtone, but much harder.

The

The ſubſtance called *groan clay* is ſaid to be formed by the decompoſition of granite. Of this ſubſtance one ounce and ſeventy-two grains yielded thirty-two ounce meaſures of air, containing no ſenſible quantity of fixed air, but all phlogiſticated, of the ſtandard of 1.62 and 1.33. After the experiment this matter was eaſily ſhaken out of the retort, and was not ſenſibly changed in its appearance.

Such were the experiments that I made with ſubſtances that are, or are ſuppoſed to be, volcanic. Of thoſe which are certainly *not volcanic*, but which may come in the way of volcanic fires, I found thoſe into which the vitriolic acid enters to yield the greateſt quantity of pure air[*]; but by no means ſufficient to keep alive ſuch fires as we make on the ſurface of the earth.

From ſeven ounces of *gypſum*, which I kept in a ſtrong heat twelve hours, I got 230 ounce meaſures of air, the greateſt part of which would have extinguiſhed a candle; the moſt phlogiſticated being of the ſtandard of 1.8, but it was afterwards much purer; and at the laſt conſiderably dephlogiſticated; for with two equal meaſures of nitrous air, the teſt was 1.3. The air was very turbid as it was produced, and the pureſt of all came rapidly, at the end of the proceſs. It is poſſible that, with a ſtronger heat, more, and purer air might have been procured. The ſubſtance was reduced to *a hard maſs*, yellow next to the retort, but in the middle very white.

The *ſtones* which I found to furniſh the greateſt quantity of air, though not the pureſt, were thoſe of the *ſchiſtus* kind, which are found in great quantities in many mountainous countries; and after being ſubjected to a very great heat, have the neareſt reſemblance to the generality of lavas of any ſubſtance on which I have yet made the experiment.

From four ounces of a *blue ſlate* I got 320 ounce meaſures of air, a very ſmall portion of which was fixed air, and the greateſt part of the reſt (the whole, I believe, except about twenty ounce meaſures) ſo impure, that the ſtandard was generally 1.8. Towards the laſt it was 1.5, and the laſt of all 1.35; ſo that a candle would juſt have burned in it. The air was very turbid, and had a very ſtrong ſmell. The ſubſtance was perfectly vitrified, and quite black, exactly reſembling lava. It then weighed, as nearly as I could gueſs (for in the fuſion it had adhered cloſely to the retort) three ounces and 288 grains.

From eight ounces of another kind of ſchiſtus, I got ſeventy ounce meaſures of air, of the ſame quality

quality with that in the preceding experiment; and it was melted into a black mafs, harder than the former, fo as to make a ftill more perfect lava.

Oil of vitriol has been fuppofed to enter into the compofition of *clay*. From four ounces of it I got twenty ounce meafures of air, in every portion of which one-tenth was fixed air, and the reft of the ftandard of 1.72, 1.52, and at laft 1.44.

Putting oil of vitriol to this clay, it yielded much more air, and of a better quality. Two ounces of the clay, moiftened with this acid, gave 210 ounce meafures of air, exceedingly turbid, containing very little fixed air, and the reft of the ftandard of 1.5, 1.7, 1.58, in the order in which they are here put down; but the laft portion was 1.08, and was confiderably dephlogifticated.

A quantity of fine white *clay from the Apallachian mountains* gave air of the fame kind at the beginning of the procefs with common clay, but the retort being cracked, the experiment was interrupted.

Nothing in the form of a *ftone* yields fo much air as *lime ftone*, and this is by no means all fixed air, as I believe has generally been fuppofed. For a very great proportion of it is more or lefs phlogifticated, and the laft portions often tolerably pure, fo that a candle would nearly burn in it.

From four ounces of white *cryſtals of lime ſtone* I got 830 ounce meaſures of air, the firſt portion of which had only one-fourth of fixed air, but in the courſe of the experiment it varied, being once three-fourths, then one-half, and at the laſt one-third. The ſtandard of the reſiduum was never better than 1.56, nor worſe than 1.66.

From five ounces and a half of lime ſtone of an excellent kind, I got in all 1160 ounce meaſures of air. Of this one-tenth only was phlogiſticated, and the reſt fixed, but the laſt portion of all was half phlogiſticated.

From ſeven ounces of a tranſparent ſubſtance, found in a ſtone in the neighbourhood of Oxford, which is chiefly calcareous, I got 1280 ounce meaſures of air, of which about one-third of the the whole was fixed air. The ſtandard of the reſiduum was at firſt 1.55, and afterwards 1.44.

From ſix ounces of a *blue ſtone*, found in the neighbourhood of Stratford, I got 1030 ounce meaſures of air, of which, till near the end of the proceſs, about one half was fixed air, and at the laſt about one fourth. The ſtandard of the remainder was about 1.6.

From three ounces of *chalk* I got 630 ounce meaſures of air, of which at the firſt one fourth was fixed air, then almoſt two-thirds, then ſome-

thing

thing more than one half, and again a little more than a third. The ſtandard of the reſiduum was from 1.66 to 1.34.

The pureſt calcareous earth is *chalk*, and the moſt perfect chalk is that which is called *whiting*, which is therefore uſeful in many experiments, ſo that it is worth while to know what air it contains. From ſeven ounces of this ſubſtance, I got, in an earthen retort, 630 ounce meaſures of air, by which it was reduced to four ounces. Every portion of the air contained about one-third that was not fixed air, the ſtandard of which was 1.36, 1.38. Again, from ſix ounces of whiting, I got 440 ounce meaſures of air, about half of which was fixed air, and the remainder of the ſtandard of 1.4. The whiting was reduced to three ounces and 312 grains.

In order to try whether any peculiar kind of air might be procured from whiting ſaturated with acids, I moiſtened ſome, which had been well calcined, with water impregnated with vitriolic acid air; and then by heat expelled from it ninety ounce meaſures of air, the former part of which was more than three-fourths fixed air, and the reſiduum of the ſtandard of 1.5. The laſt portion had leſs fixed air in it, and the ſtandard of the reſiduum was 1.44. The ſubſtance was rendered black

black and hard, but in spirit of salt it became white and soft.

When *quick lime* is suffered to fall in the open air, it first attracts moisture, and then that moisture gives place to fixed air. From three ounces and a quarter of this fallen lime I got 375 ounce measures of air, of which about one-fifth was fixed air, and the standard of the residuum was 1.4.

Iron ores may be pretty well distinguished by the quality of the air that they yield by heat, as well as by their weight, and external appearance. The residuum of the air from other stony substances, after the fixed air is separated from it, I have always found to be phlogisticated, but that from iron ore is inflammable.

Three ounces and one-half of *white spathose iron ore* yielded 560 ounce measures of air, of which at the first one-third was fixed air, then only something more than one half, and again at the last a third. The standard of the residuum was about 1.7, and inflammable. The substance was reduced to one hard mass, and the bottom of the earthen retort was melted along with it.

I tried one iron ore that was of a *light* colour, and another of a *darker*. Six ounces of the lighter coloured ore yielded 750 ounce measures of air, of which at the first two-thirds were fixed air, then only a little more than a half, and at the laſt a fifth. The reſiduum of the middle portions of this air only was inflammable, that of the reſt phlogiſticated; the ſtandard of it about 1.7. It was reduced to a hard black ſlag full of cavities.

Four ounces of the dark coloured ore yielded 510 ounce meaſures of air, the quality of which varied very much, like that in the preceding experiment, and the air was not more ſtrongly inflammable.

There is, I believe, ſome iron in the ſubſtance that is called *black lead (molybdena)* and therefore I mention the experiment that I made with it in this place. From half an ounce of it I got twenty-five ounce meaſures of air, of the ſtandard of 1.6, and 1.42. I have no note of any part of its being fixed, or inflammable. It had loſt only eighteen grains in weight. From eight ounces and a half of another kind of black lead, I got ſixteen ounce meaſures of air, one fifteenth, or one twentieth of which was fixed air, and the reſt inflammable, burning with a blue flame.

I did not purſue theſe experiments on ores to any great extent; but having ſome *ſtream tin*, I found that 110 grains of it, gave twenty ounce meaſures of air, a ſmall portion of which was fixed air, and the remainder of the ſtandard of 1.44, and at laſt 1.34.

From

From two ounces and one fifth of *steatites*, I got forty three ounce meafures of air, which had the flighteft appearance of containing fixed air. The remainder was thoroughly phlogifticated, except that, at the laft, it was of the ftandard of 1.65. It came out of the retort a yellow mafs, but powdery, as it was put into it.

Two ounces of *terra ponderofa*, gave twenty-fix ounce meafures of air, without any mixture of fixed air, the ftandard of it 1.62, 1.42, and 1.29. The fubftance was concreted into one mafs, but was eafily broken by fhaking the retort, and then it did not appear to be changed in its external appearance.

Two ounces of *black wad* from Derbyfhire, yielded eighty ounce meafures of air, no part of which was fixed air, but all better than common air, the ftandard of it being 1.05. This circumftance may help to account for this fubftance taking fire, and burning as it does, when it is mixed with linfeed oil. For if by any means it is fo far heated, as to give out its pure air, this muft affift the combuftion; and the chemical attraction between the phlogifton in the oil, and the dephlogifticated matter in the wad may, without its affuming the form of air, be the caufe of the mafs becoming hot.

Seven ounces of *fluor* gave eight ounces of air, a fmall proportion of which was fixed air, and the reft reſt of the ſtandard of 1.45. It was melted into a hard maſs. Six ounces of white fluor yielded in all ten ounce meaſures of air, of which the ſlighteſt portion imaginable was fixed air, the reſt of the ſtandard of 1.34, and 1.3. In this experiment the bottom of the retort was quite diſſolved. N. B. There was no appearance of fluor acid in the water in which this air was received, and the melted maſs gave fluor acid air with oil of vitriol.

From four ounces of a kind of *ſand-ſtone*, I got ſeventy five ounce meaſures of air, a ſmall portion of which was fixed air, the ſtandard of the reſt, for the moſt part, 1.75, and at the laſt 1.35. When taken out of the retort, it weighed three ounces and three fourths. That part of it which was next to the bottom of the retort was whiter than the reſt, but very hard, adhering to it; and, what was pretty remarkable, the remainder had acquired juſt as firm a texture as it had before it was pounded for the purpoſe of the experiment.

Five ounces of a fine *white ſand-ſtone*, yielded about ten ounce meaſures of air, containing a little fixed air, and the reſt of the ſtandard of 1.6. This alſo was again reduced to a ſtone quite as compact as it had been before it was pounded.

Six ounces of another ſand-ſtone yielded 102 ounce meaſures of air, of which a very ſmall portion was fixed air, and the reſt of the ſtandard of

1.57, and 1.35. This alſo was reduced to a hard dark coloured ſtone, having ſeparated itſelf from the retort about a quarter of an inch, except at the bottom where it adhered to it.

From one ounce and 175 grains of *belemnite*, I got 320 ounce meaſures of air, of which at the firſt one ſixteenth was fixed air, the reſt of the ſtandard of 1.75, 1.55. All the air came while the heat was very moderate.

From four ounces of *cryſtals of quartz*, I got 25 ounce meaſures of air, a very ſmall portion of which was fixed air, the reſt being of the ſtandard of 1.8, and 1.44.

From ſeven ounces of a *granulated quartz*, I got about ten ounce meaſures of air, containing a little fixed air, and the reſt of the ſtandard of 1.42. It came out of the retort a looſe friable ſubſtance, weighing ſix ounces 290 grains. The retort was cracked, or more air would probably have been procured.

From one ounce and eighty four grains of *mica*, I got twelve ounce meaſures of air, of which no part was fixed air, but of the ſtandard of 1.4, and 1.35.

From 120 grains of *talc*, I got a quantity of air, but the retort being cracked at the beginning of the proceſs, I took no account of the quantity. Part of it was evidently fixed air, and the reſt of the ſtan-

dard

dard of 1.4, and a candle burned in it. The ſubſtance was reduced to a dark hard cinder, adhering to the retort.

From four ounces 355 grains of *cryſtalized glaſs*, in the form of a whitiſh ſtone, I got twelve ounce meaſures of air, which contained no fixed air, and of the ſtandard of 1.42, 1.36, and 1.31. Perhaps I uſed a greater degree of heat than the glaſs had been ſubjected to before. Otherwiſe this experiment might help to account for lava giving ſome quantity of air, though it had been in a ſtate of fuſion, having afterwards cryſtalized, like this glaſs.

The laſt experiment that I ſhall mention was made with *pit coal.* Three ounces of ſuch coal as we have at Birmingham, gave 700 ounce meaſures of air, of which I could not be ſure that any portion was fixed air. It was all inflammable, the firſt portion of it burning with a white lambent flame, and the laſt with a blue one.

To the ſubſtances from which I had endeavoured, at different times, to extract air by heat, it may be juſt worth while to mention *crude antimony*. From one ounce of it, in a glaſs veſſel, and with a red ſand heat, I got very little air, not more than its bulk. The laſt portion was in a great meaſure fixed air, and the reſiduum extinguiſhed a candle. The antimony on which this experiment was made, and which had been pounded, formed a concrete maſs

maſs when taken from the fire, being mixed with any of the acids.

A degree of heat ſufficient to bake *clay*, evidently expels fixed air from it. In order to aſcertain this fact, I filled a gun-barrel with tobacco-pipe clay, and, putting it into the fire, I received the air that came from it, in ſeveral portions; but the whole was not more than about five times the bulk of the clay. The firſt produce was inflammable; but afterwards the air was fixed, precipitating lime in lime-water, and being readily abſorbed by water. I never met with purer fixed air.

No *calx* of any metal on which I made the experiment yielded inflammable air, but all of them fixed air, and generally in great plenty. *Ruſt of iron* gave a great deal of air, two thirds of which was fixed air, and the reſt was not affected by nitrous air, and extinguiſhed a candle; ſo that the whole produce ſeemed to be fixed air, only with a larger reſiduum than uſual of that part which is not miſcible with water. At another time, however, I got from the ruſt of iron fixed air that was very pure, there being little of it that was not miſcible with water.

N. B. That part of the ruſt on which the focus of the lens fell, turned very black.

I obſerved that both the *grey calx of lead*, and *litharge*, yielded fixed air, and that a great quantity

quantity of fixed air is contained in *red lead*, and in other preparations of that metal.

I got a little air by means of the burning lens in quickſilver, from *cinnabar prepared with antimony*; but not enough to form a judgment of the quality of it. From common *vermillion* I got more air, viz. about forty times its own bulk, and it was all fixed air, being readily abſorbed by water. This ſubſtance, like the ruſt of iron, turned black in the focus of the lens.

SECTION II.

Air from ſaline Subſtances.

MOST ſaline ſubſtances, I believe, contain more or leſs fixed air; and it may be worth while to examine what *quantity* of it may be extracted from each of them, and alſo the quality of the reſiduum, which I find to differ conſiderably in different caſes. But this may depend, in a great meaſure, upon the ſtate of the water in which the experiments are made. A few obſervations that I have had occasion to make of this kind may be just worth noticing.

Both *vitriolated tartar*, and *Glauber salt*, which I have often occasion to make in the course of my experiments, I find contain fixed air. Dissolving a quantity of vitriolated tartar, which was formed in making spirit of nitre, and collecting the air that came from it, I found one twelfth of it to be fixed air and with an equal quantity of nitrous air, the measures of the test for the remainder were 1.3. At another time I filled the retort in which the salt was contained with boiled pump water, and then I found no fixed air in it; having, I suppose, been absorbed by the water, and the measures of the test for the remainder were 1.46. Again I dissolved a quantity of this salt in pump water, and then found one fourth of the whole to be fixed air; the pump water itself containing a good deal, and the measures for the residuum were 1.44.

From half an ounce of *vitriolated tartar*, in a gun-barrel, I got about an ounce-measure and a half of air, which was chiefly fixed air. The last produce diminished common air a little; but this I attribute to the gun-barrel not having been perfectly cleaned from the materials used in a former experiment.

I also dissolved a quantity of Glauber salt, which remained from the process for making spirit of salt,

and

and I found the residuum of the fixed air to be sensibly worse than common air.

The first experiment that I made upon *alum*, was with the sun-beams, in quicksilver; when I got from it a little air, which appeared to be fixed air, by extinguishing a candle, and by being readily absorbed by water. I repeated the experiment with the same result. The quantity of air extracted from a piece of alum, was about one third of its bulk; but I imagined that a little, though not much, more might have been extracted, by a longer continuance of the operation.

I observed, upon this occasion, that I could calcine only a given quantity of alum in a given quantity of air; and that when this was saturated, I could only keep the alum in a fluid state by heat. But it was easily calcined in vacuo; and as the receivers in which the calcination was made became very moist, it is pretty evident that this operation is performed by the mere expulsion of the water which enters into the composition of this salt; so that when the surrounding air can take no more water, that calcination can proceed no farther. I also observed, upon this occasion, that when I had calcined a quantity of alum in a given quantity of common air, the air was not diminished, or in the smallest degree injured, by the operation.

After this, I endeavoured to get air from *calcined alum*, with a burning lens; and I did get a little: but I made no other obſervation upon it, than that it was not diminiſhed by nitrous air. But when I put a quantity of calcined alum into a gun-barrel, I got from it a conſiderable quantity of air, part of which was fixed air, precipitating lime in lime-water, and the remainder did not differ from the reſiduum of fixed air, extinguiſhing a candle, and neither affecting common air, nor being affected by nitrous air.

N. B. The pure air from the alum, and the inflammable from the iron of the gun-barrel, would produce the fixed air.

In diſſolving alum, in order to get ſome earth of alum, I obſerved that air was diſcharged from it. This I collected, and found it to contain very little fixed air, and the meaſures of the teſt for the reſiduum were 1.12. At another time I had the ſame reſult, but the air was not quite ſo good, though purer than common air.

Precipitating a ſolution of alum with pot aſh, I caught the fixed air, which was diſcharged in great abundance; and examining the reſiduum, found it to be better than common air, in the proportion of 1.2 to 1.3; the diminution being in that proportion when mixed with equal quantities of nitrous air.

From

From one ounce of *calcined alum*, very white and clean, I got ſixty ounce meaſures of air, without any fixed air, or the leaſt imaginable, and ſo pure, that with two equal meaſures of nitrous air, the teſt was 1.4. Still the reſiduum had an acid taſte, ſo that with more heat, it is probable that more, and purer air, would have been produced.

The *metallic ſalts*, if they gave any air at all, gave fixed air, which I find to be contained in moſt ſaline ſubſtances. I ſhall recite a few experiments of this kind, without any particular regard to the order of them.

I could get no air whatever from *ſugar of lead*, or from *nitre of lead*. The former melted into a liquid ſubſtance; the latter changed from white to a dull grey colour, and broke into powder, with a crackling noiſe.

All the kinds of *copperas* gave fixed air. I firſt tried common *green copperas* in quickſilver. It diſſolved into a great quantity of water, but the air produced from it was not one twentieth of its bulk. Half of this air was readily abſorbed by water, and the remainder was too ſmall to be examined. I repeated the experiment on calcined copperas, both in a gun-barrel, and likewiſe in a tall glaſs veſſel filled with ſand; but the produce, in all the caſes, was fixed air. Half an ounce of calcined copperas yielded near a pint of air.

When I had extracted air from the calx of green copperas in a glass-vessel, I put the same materials into a gun-barrel; but still I extracted nothing from them besides fixed air, mixed with acid air, as appeared by the extremely small bubbles to which the large ones were presently reduced in passing through water.

When I made the experiment on *blue vitriol*, which consists of oil of vitriol and copper, in quicksilver, the result was the same as with the green copperas, except that much less water was produced.

White vitriol, which consists of oil of vitriol and zinc, gave ten times as much air as the other kinds. Half of it was absorbed by water, and a candle burned in the remainder.

Mercurial nitre gave a great quantity of air in quicksilver, and this was pure nitrous air; but possibly the nitrous acid being let loose from this substance, had produced the nitrous air by dissolving the quicksilver.

White lead yielded air in great plenty, by the heat of the burning lens, and it was all pure fixed air.

From four ounces of *white lead* I expelled, in an earthen retort, 240 ounce measures of air, before the retort was dissolved by it. Of the first produce there remained one third, not fixed air, of the standard of 1.36; and towards the last, the residuum was of the standard of 1.28, when with the common air it was 1.23.

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SECTION III.

Air from Subſtances of a vegetable Origin.

TARTAR is a ſubſtance concerning which there has been a great diverſity of opinions among chemiſts. On this account ſome of my chemical friends requeſted that I would examine what kind of air it yielded in different circumſtances. Accordingly, to ſatisfy them, and my own curioſity at the ſame time, and without any particular expectation (for I had formed no opinion whatever with reſpect to it) I began with putting a ſmall quantity of the cream of tartar into ſome oil of vitriol, contained in a phial with a ground ſtopper and tube (which is the method that I uſually employ to procure vitriolic acid air) and, with the flame of a candle, I made it boil.

The acid preſently became black, and the mixture yielded a great quantity of air, till it was quite viſcid; when, there being ſome danger of choaking the tube, I withdrew it. The air was at firſt half fixed air, making lime water turbid, and half inflammable, burning with a lambent blue flame; but towards the laſt two thirds of it was inflammable. I did not uſe more than a few penny-weights of the tartar, and the quantity of air exceeded two quarts, and much more might certainly have been procured. The next day the matter, which I had poured out of the phial, had the confiftency, colour, and fmell of treacle; except that there were fome fmall concretions in it. Some time after I took the refiduum above-mentioned, and putting it into a glafs veffel, I again extracted from it, in a fand heat, a large quantity of air, as much as before, and exactly of the fame kind. In the middle of the procefs, when the production of air was moft copious, it was very turbid; and when any of the bubbles burft in the open air, they were perceived to have a ftrong fmell of treacle.

After this I ceafed to make ufe of oil of vitriol, in order to try what air the tartar would yield of itfelf; and I prefently found that the acid had contributed nothing at all to the air that I had got from it. From an ounce of cream of tartar, in a glafs veffel, and a fand heat, I got 170 ounce meafures of air, the firft portions of which were almoft pure fixed air. The refiduum, however, was inflammable, and burned with a blue flame. At laft only about two thirds of the air was fixed air, and the reft inflammable. In the greateft part of the procefs, the air was very turbid; but it was fo in the recipient, and the part of the tube next to it, a confiderable time before it was turbid in the reft of the tube,

tube, or in the glaſs veſſel that contained the materials. Towards the end of the proceſs the empyreumatic oil came over, which was very offenſive, though, at firſt, the ſmell of the air had been rather pleaſant, reſembling that of burnt ſugar.

I repeated this experiment, and again got about 170 ounce meaſures of air from an ounce of cream of tartar, of which thirty eight ounce meaſures were inflammable, and the reſt fixed. It burned with a large white flame, but at laſt with a light blue one, owing, I ſuppoſe, to the mixture of fixed air in it.

That cream of tartar ſhould yield *fixed air* will not be thought extraordinary; but its yielding inflammable air, ſeems to ſhew that it had acquired a good deal of the conſiſtence of vegetable matter, or of pit-coal, ſince thoſe ſubſtances yield the ſame kind of air.

After this, neglecting the produce of air, I ſimply calcined a quantity of cream of tartar, in a red heat, in a glaſs veſſel filled up with ſand; and obſerved that it loſt about half its weight. Notwithſtanding its calcination in a red heat, this ſubſtance obſtinately retained a great deal of its fixed air, in which it reſembles chalk. For when I put this calcined cream of tartar into ſpirit of ſalt it yielded a conſiderable quantity of air, which I found to be fixed air, with a phlogiſticated reſiduum. It alſo, effervеſced in the ſame manner, and no doubt gave

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the ſame kind of air in oil of vitriol, and ſpirit of nitre. But even ſpirit of ſalt did not diſſolve the whole of it.

To obſerve the phenomena of this calcination more particularly, I made the proceſs in an open crucible, which I kept in a red heat a long time. But when there was no appearance of any farther change, and the ſubſtance was pretty hard, I took it from the fire, on which it preſently aſſumed a blackiſh, or dirty brown colour. Spirit of ſalt diſſolved this ſubſtance with as much rapidity, to all appearance, as it had done the mere black coal of tartar in the former experiment, and expelled as much air from it. It ſtill, however, did not diſſolve the whole: for a dirty powder remained undiſſolved.

I threw the focus of the lens upon a piece of fine *white ſugar*, in quickſilver. It was readily melted and converted into a brown ſubſtance, yielding about two thirds of its bulk of air, one third of which was readily abſorbed by water, and the remainder extinguiſhed a candle. I repeated the experiment with a browniſh powdered ſugar, with the ſame reſult, excepting that more air was generated from this than from the white ſugar, in proportion to their bulks.

From two ounces and three quarters of *wood aſhes* I got, in a very ſtrong heat, 430 ounce meaſures

ſures of air, of the firſt portion of which one tenth, of the ſecond one third, and of the third one half was fixed air. The reſiduum of the ſecond portion was of the ſtandard of 1.6, and that of the third 1.7. It extinguiſhed a candle; ſo that the air came properly from the *aſhes*, and not from any remaining particles of the charcoal mixed with them. After the proceſs, the aſhes weighed 839 grains. Being expoſed to the open air one day, they weighed 842 grains, and, perhaps with more heat than before, yielded fifty ounce meaſures of air, of which about an eighth was fixed air, and the ſtandard of the reſiduum was 1.38, and 1.41. A candle burned in it; ſo that it is evident ſome of the deplogiſticated part of the atmoſphere had been imbibed by theſe aſhes. They then weighed 789 grains and a half.

From three ounces of *pit-coal aſhes*, I got air, the ſtandard of which was 1.7, and extinguiſhed a candle. I took no note of the quantity of fixed air, and through an accident in the proceſs moſt of the air eſcaped.

It is well known that all fermented liquors, that are not quite flat or vapid, contain fixed air; and I had the curioſity to try, what proportion of this air is contained in different kinds of wine, and in wines in different ſtates. For this purpoſe, I took one of the phials with a ground-ſtopper and tube;

tube, containing an ounce-meaſure and a half, and filling it accurately with each ſpecies of wine, I plunged it into a veſſel of water, which was ſet on the fire to boil, receiving the air in quickſilver. The air that I got from all kinds of fermented liquors was pure fixed air; but, except champaigne and cyder, it was in much leſs quantity than I expected; the reſults being as follows.

The quantity of air contained in

Madeira, was -	$\frac{1}{100}$	of an ounce-meaſure.
Port of ſix years old	$\frac{1}{48}$	—————————
Hock of five years old	$\frac{1}{24}$	—————————
Barrelled Claret -	$\frac{1}{12}$	—————————
Tokay of ſixteen years	$\frac{1}{20}$	—————————
Champaigne of two years	2	—————————
Bottled Cyder of 12 years	$3\frac{1}{4}$	—————————

Some champaigne ſparkles much in conſequence of containing much air; but there is a kind of champaigne which does not ſparkle, and contains very little air. The difference, as I was informed, when I made enquiry concerning it, in that part of France where the wine is made, is owing to this; that when they wiſh to have the wine ſparkle, they check the fermentation as much as poſſible at the time that the wine is made; ſo that the fermentation going on gradually, the fixed air pro-

duced

duced by it is abſorbed by the liquor: whereas, when they do not chuſe to have it ſparkle, they let it ferment freely, like any other kind of wine.

In other caſes, therefore, where fermented liquors contain much air, as in moſt kinds of malt-liquor, cyder, and our Engliſh made-wines, I take it for granted, that the fermentation is either purpoſely checked, or that the liquor is of ſuch a nature, that the fermentation will neceſſarily continue a long time, after it is put into the caſk or bottle.

I once found that a quantity of port-wine contained its own bulk of fixed air; but I now imagine that the wine was not genuine, but muſt have been made chiefly of cyder. Perhaps this may not be a bad method of diſtinguiſhing genuine foreign wines from compoſitions made of cyder.

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SECTION IV.

Air from Animal Subſtances.

I Had obſerved, that animal ſubſtances, in putrefying, diſcharge air that is in part fixed, and part inflammable. Being willing to find the *proportion* of each of theſe kinds of air, in the different ſtages of the putrefactive proceſs, as well as the *whole produce* of both kinds, I took a piece of the lean muſcular part of mutton, weighing 102 grains, on the 13th of September, 1776, and put it into a jar filled with quickſilver, ſtanding inverted in a baſon of the ſame, and placed it near the fire, where the heat was variable, but at a medium of about 100 degrees of Fahrenheit.

On the 15th I took from the mutton half an ounce meaſure of air, two thirds of which was fixed air, making lime-water turbid, and the reſt was ſtrongly inflammable. On the 16th it had yielded one third of an ounce meaſure of air, of which the fixed air and the inflammable were exactly in the ſame proportion to one another as before; but the inflammable air at this time was all fired at one exploſion, and without that redneſs

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in the flame that I had perceived before. On the 19th I took from it about half an ounce meaſure of air, three fourths of which was fixed air, and the reſt inflammable.

After this I removed the mutton and quick-ſilver into the common temperature of the atmoſphere, where they continued to the 13th of January following, in all which time very little was added to the air that had come from it before its removal. I then, however, took from it half an ounce meaſure of air, and it was all pure fixed air, without the mixture of any thing inflammable in it. Then placing it near the fire as before, it preſently yielded another half ounce meaſure of air, which was alſo wholly fixed air.

Obſerving that it ſtood near twenty-four hours after this without producing any more air, though it was in the ſame degree of heat, I plunged the whole into a pan of water, and made it boil; by which means I got from it about one eighth of an ounce meaſure of air, the whole of which was fixed air; at leaſt the reſiduum was not larger than is uſual in pure fixed air; for it was too ſmall a quantity to make an experiment upon with the flame of a candle. After this I kept it in a boiling heat a conſiderable time, without getting from it any air at all. It appears therefore, that this piece of mutton yielded in all $2\frac{1}{2}\frac{1}{4}$ meaſures of air, of of which $2\frac{1}{20}\frac{5}{6}$ was fixed, and the reſt inflammable, and that all the inflammable part was exhauſted a conſiderable time before the fixed air.

On the 13th of March, 1780, I took two dead mice, of about equal ſize, and put them into two ſeparate cups, under different jars of common air, of very nearly equal capacities, one of them containing 155 ounces of water, ſtanding in quickſilver, and the other 160 ounces, ſtanding in water.

Leaving them in the country to the care of a perſon who ſupplied the veſſels in which they ſtood occaſionally with water or quickſilver, I went to London, and after my return, in the beginning of Auguſt, I found, by marking the veſſels, and meaſuring them afterwards, that the air in the veſſel which had ſtood in water was reduced to 140 ounce meaſures; and on the 28th of Auguſt it was reduced to 135, but after ſtanding a fortnight longer, it was not ſenſibly diminiſhed any farther. The air in the veſſel which had ſtood in quickſilver was not ſenſibly diminiſhed at all.

Admitting lime water to this veſſel, it preſently became turbid; but this being a ſlow diminution I removed the veſſel after ſome days to a trough of water, and then found that the air contained in it made lime water exceedingly turbid; and agitating this air in ſmall portions it was preſently reduced to 125 ounce meaſures; ſo that all the quantity diminiſhed

diminiſhed ſeems to have been fixed air, making lime water turbid, and being abſorbed by water in the very ſame manner.

The air in the veſſel which had ſtood in water, notwithſtanding the opportunity there was for the fixed air depoſited by it being readily abſorbed, made lime water very turbid; and by agitation in ſmall portions this air was reduced to 130 ounce meaſures. Upon the whole it appears, that the diminution in both of theſe caſes was nearly equal, viz. a little more than one fifth.

In theſe experiments the two mice were thoroughly putrefied, and indeed quite diſſolved, and no doubt had yielded all the air they were capable of yielding. But if the experiments on the putrefaction of mice in quickſilver recited above be compared with theſe, it will be found that the addition of fixed air, or air of any other kind, from the putrefied mice was quite inconſiderable, viz. an ounce meaſure and half of fixed air, and half an ounce meaſure of inflammable from each.

It is true that mice putrefying in *water* yield perhaps more fixed air than in this proportion; but here they putrefied in *air* only. And that a very inconſiderable quantity is produced in theſe circumſtances, is evident from there being little or no increaſe of the air when it is confined by *quickſilver*, which could not imbibe fixed air, if any had been difcharged from the putrefying mice. It will be found hereafter, that water is a neceffary ingredient in the conftitution of both fixed and inflammable air.

It might be queftioned, whether the fixed air contained in our aliments, can be conveyed by the courfe of circulation into the blood, and by that means impregnate the urine. I have found, however, that it may do it; having more than once expelled from a quantity of frefh-made urine, by means of heat, about one fifth of its bulk of pure fixed air, as appeared by its precipitating lime in lime water, and being almoft wholly abforbed by water; and yet a very good air-pump did not difcover that it contained any air at all.

It muft be obferved, however, that it required feveral hours to expel this air by heat; and after the procefs, there was a confiderable whitifh fediment at the bottom of the veffel. This was, probably, fome calcareous matter with which the fixed air had been united; and by this fixed air, the calcareous matter, which would otherwife have formed a ftone or gravel, may have been held in folution; and therefore, drinking water impregnated with fixed air, may, by impregnating the urine, enable it to diffolve calcareous matters better than it would otherwife have done, and may therefore be a means of preventing or diffolving the

the ſtone in the bladder, agreeable to the propoſal of my friend Dr. Percival.

From four ounces of dry *ox blood* I got 1200 ounce meaſures of air, and I conjectured that not leſs than 200 ounce meaſures eſcaped. It contained no fixed air. The firſt portion of it burned with a large lambent white flame, the middle portion fainter, and the laſt was hardly inflammable at all, but had a ſlight blue flame. What remained of the blood weighed 255 grains, and was a very good conductor of electricity, which is not uſually the caſe with the charcoal of animal ſubſtances.

PART III.

VARIOUS PROPERTIES OF FIXED AIR.

SECTION I.

The Effects of fixed Air on Animals and Vegetables.

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 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.

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While I was making experiments on the fixed air produced by the fermentation of beer, in a public brewery, which was a confiderable time before I attempted to procure it in any other manner, I had the curiofity, among other things, to try what effect it would have on the *vegetation of plants*, and the *colours* of fome delicate flowers; both which I could eafily fufpend within the region of fixed air over the fermenting vats. The refult of a few experiments, which I made in thefe circumftances, was as follows.

Fixed air is prefently fatal to vegetable life. At leaft fprigs of mint growing in water, and placed over the fermenting liquor, will often become quite dead in one day; nor do they recover when they are afterwards brought into the common air. I am told, however, that fome other plants are much more hardy in this refpect.

A red rofe, frefh gathered, loft its rednefs, 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 reft of it. Another red rofe turned perfectly white in this fituation: but various other flowers, of different colours, were very little affected. Thefe experiments were not then repeated, as I wifhed they might be done, in pure fixed air, extracted from chalk by means of oil of vitriol.

After this I found a contrary opinion to prevail, viz. that fixed air is so far from being *destructive* to vegetation, that it is the proper *pabulum* of vegetables; making them to flourish much more than they could do in other circumstances; and that, instead of discharging the colour of rose leaves, it is a means of preserving them, and all other most delicate flowers, in the greatest perfection. I therefore made the following experiments.

On the 5th of June, 1776, I put two sprigs of mint into two equal jars, filled to the same height with pure fixed air, extracted from chalk by oil of vitriol, the lower parts of each jar containing equal quantities of the same rain water; with this difference, that into one of the jars I conveyed a little oil, to prevent the too quick absorption of the fixed air by the water. Also in the same trough of water, in which stood the jar without oil, I placed another jar, filled to the same height with pure fixed air, without any plant. I presently observed that the water rose in all the jars exactly alike, except in that which had the oil on the surface of the water; and the next morning both the plants appeared to be quite dead, their stems and leaves having became almost black and

and flaccid. After two days, when there was evidently no probability of the plants recovering themselves, I took them out, and found the air to which they had been exposed not in the least changed, being just as much absorbed by water as other fixed air.

Thinking it possible, that though these plants died in a total change of atmosphere, they might, notwithstanding, have borne a *partial charge* of it, I took three other plants, and put one of them into a jar of air of which *two thirds*, another into one of which *one half*, and a third into one of which *one fourth* was fixed air. But, to all appearance, all these plants died as quickly as the two former had done, which I believe was, in fact, almost instantly: for the cessation of vegetable life must have considerably preceded such *visible effects* of it as the blackness and flaccid state of the leaves and stalks.

To close this set of experiments, I, in the last place, put only *one eighth* part of fixed air to two plants which had been growing some time very well in phials of water, over which I had placed jars full of common air only, in order to avoid wetting the plants, or doing them the least imaginable injury, in any respect. But, notwithstanding this, and though very little indeed of the fixed air could be supposed to remain a long time, of so very small a quantity, exposed to so very large a surface of water,

ter, in a few days the tips of the leaves, even to the tops of the plants, turned black: both of them ſoon ſhewed evident marks of decay: one of them died in about ten days, and the other did not ſurvive more than about three weeks. For in ſuch a languiſhing ſtate, it is not eaſy to ſay at what preciſe time a plant muſt be pronounced to be properly dead.

In the next place I tried the effect of water impregnated with fixed air on the *roots* of plants.

In one caſe, a ſprig of mint, in the impregnated water, grew better than a ſimilar plant in the ſame water not impregnated with fixed air; but another plant grew much worſe than its companion in common water. Beſides, though it ſhould appear that, for a time, a plant ſhould grow better in this kind of water, it may, perhaps, be attributed to the effects of *ſtimulus* only, which is not peculiar to fixed air, but might reſult from the action of any other acid. And when I put a little common ſalt, or even a little ſpirit of nitre into the water in which the plants were growing, I imagined that, for ſome time, it rather promoted their growth. Alſo, though, in general, plants die almoſt immediately in water impregnated with nitrous air, yet in one caſe of this kind, when the ſuperfluous nitrous air was carefully let out under water, ſo that no part of it was decompoſed in contact with the water, the plant grew in it remarkably well.

The

The few obſervations that I have made on the growth of plants in water impregnated with fixed air, but which I do not pretend to be ſufficient to decide the queſtion, were the following. On the 20th of Auguſt 1776, I gathered two ſlips of mint, and likewiſe two ſmall plants of the ſame kind, with roots; of which I put one of each kind into an eight-ounce phial of rain-water, and the others into other ſimilar phials, filled with the ſame water impregnated with fixed air; putting at the mouth of each of them a little ſoft clay, to prevent the too eaſy eſcape of fixed air from thoſe that contained it, and to put the others, as nearly as poſſible, into the ſame circumſtances.

For ſome time all theſe plants appeared to flouriſh equally well; but after a week it was evident that the ſlip of mint in the impregnated water, grew better than its companion. On the 4th of September the plant in the ſimple water was in a dying condition, and the other began ſenſibly to languiſh, and was dead, I think, about a week after the other.

The two plants with roots grew very well; but that in the ſimple water much better than the other; and more of the water had been exhaled from the phial, the reverſe of which had been the caſe with the ſlips. On the 24th of September the plant in the fixed air was abſolutely dead; but the other in

ſimple

ſimple water was very flouriſhing on the 28th, when I put an end to the experiment. Examining the phials of impregnated water, I found that neither of them had intirely loſt its fixed air. That in which the ſprig of mint had grown, ſtill contained one ſixth of its bulk of fixed air, and the water in which the plant with its root had grown, which was a much longer time, retained, however, one twelfth of it.

To try the effect of different *ſtimuli* on the roots of plants, I firſt put into phials containing an ounce meaſure and a half of common water, ſmall quantities of common ſalt, from one grain to twelve, and more. In all thoſe which contained more than twelve grains, the plants died immediately, but in that phial the plant lived a few days; and the reſt died, in their order, to that which contained three grains of ſalt, which ſeemed to grow as well as the plant in ſimple water. And it was remarkable that not only this plant, but alſo thoſe which had died ſeemed to flouriſh more *at the firſt*, than thoſe which grew in ſimple water: and that which had three grains of ſalt, and alſo that which had one grain, continued to live after the plant in ſimple water was dead in the ſame room. This was in my laboratory, a place unfavourable, indeed, to any vegetation, but equally ſo to all.

Sprigs

Sprigs of mint in one and a half ounce phials, containing one, and even two drops of the ſtrongeſt nitrous acid flouriſhed very well, better, ſeemingly, than thoſe in mere water; but in water containing more of this acid, they died inſtantly.

I am far from pretending that theſe few experiments on the vegetation of plants in water impregnated with fixed air, are deciſive; but I think they ſhew that a very great number of experiments, and thoſe uniform in their reſult, are neceſſary to determine this queſtion. When ſome plants grow better, and ſome worſe, it makes it probable that the difference in the growth depends upon ſome other circumſtance than the water in which they grow.

While I was attending to the compariſon of the growth of plants in dephlogiſticated and common air, I at the ſame time made a few farther experiments on the growth of plants with their leaves expoſed to fixed air, though I was pretty well ſatisfied, from the experiments recited above, that this kind of air is undoubtedly injurious to plants growing in it. I wiſhed alſo, once more, to try the effect of inflammable air, with reſpect to vegetation.

Accordingly, in the month of April 1777, I introduced a ſprig of mint into a phial of air, one third fixed and the reſt common; and having only once ſupplied it with freſh fixed air (when the bulk of of the former was abſorbed by the water) I obſerved, that on the 3d of May following, there were black ſpecks on ſeveral of its leaves, and in the courſe of a week it was almoſt wholly black, and evidently dead. It had not grown at all.

At the ſame time I put another ſimilar plant into a jar of half freſh made inflammable air and half common air, but it died preſently. I found, however, by ſubſequent trials, that plants would bear a greater proportion of inflammable than they would of fixed air; ſo that from the circumſtance of plants merely *living* in a proportion of fixed air, it cannot be inferred that it is *of itſelf*, at all favourable to their growth.

The few experiments that I had an opportunity of making before, left me altogether undecided with reſpect to the effect of water impregnated with fixed air on the *roots* of plants. But the many experiments that I have made ſince, in 1777, and 1778, have not left a ſhadow of doubt on my mind, that ſuch water is hurtful, and finally fatal to the plants growing in it, at leaſt to ſprigs of mint; for I did not make the trial with any other plants.

On the 28th of May I placed, in a green houſe, and not in my laboratory, as in the experiments mentioned before, three ſprigs of mint, with their roots in phials of water impregnated with fixed air, and three other plants of the ſame kind

with

with their roots in the ſame water unimpregnated. After a week I changed the impregnated water, on account of the mouths of the phials being left open, leſt the plant ſhould have been injured by putting any thing about them, to prevent the eſcape of the air from the water.

During two or three days at the firſt, the plants in the impregnated water were more vigorous than the others; but on the 8th of June following, they all looked much worſe than thoſe in the common water. Alſo thoſe in the common water had long white filaments ſhooting from their roots, whereas thoſe in the impregnated water had none of them. On the 18th of June, the plants in the impregnated water were all quite dead, their leaves having all fallen off one after another, beginning at the bottom. Examining one of the phials, I found that it contained between one fifth and one ſixth of its bulk of fixed air.

I repeated theſe experiments ſeveral times in the courſe of that ſummer, generally uſing many more plants than in theſe laſt mentioned, but the reſult was the ſame in them all. However, as it generally happened, on what account I cannot tell, that the plants in the unimpregnated water died, though later than the others, I deferred the laſt and deciſive trial till the year following, after which I had no doubt remaining on the ſubject.

On

On the 4th of May, 1778, I put seven sprigs of mint into pump water impregnated with fixed air, and ten or twelve in the same water unimpregnated, the phials being similar, and I placed them all in a summer house, in the same exposure. I renewed the impregnated water every week, till the 23d of June, when all the plants in the water impregnated with fixed air were dead, the roots being black and rotten; while the other plants were in as flourishing a state as possible, and continued to flourish long after, till I discharged the experiment.

On this occasion I did not observe that the plants in the impregnated water were at any time more flourishing than the others, not even at the beginning; and after a fortnight the difference in appearance, to the disadvantage of those in the impregnated water, was very visible. Those which grew in the common water threw out many white filaments from their roots, many of them so long as quite to fill the phial, twisting themselves in all directions, and exhibiting a very beautiful appearance; whereas there was nothing of this kind in any of the phials of impregnated water. On the contrary, the roots became presently black, and at length rotted quite away.

One of these I had overlooked, and had neglected to change the water; and this plant threw

out

out a few white filaments; but, on renewing the impregnated water, they presently became black and perished.

It was remarkable also, that two of the plants in the impregnated water threw out thick knots of those white filaments in the necks of the phials, just above the surface of the water, but not one of them within the water itself, or ever entered the water. Also, when I took one of these plants, the roots of which were quite perished, out of the impregnated water, and put it into a phial of common water, it threw out new white roots above the place that was decayed, and afterwards grew very well.

Mr. Hey, of Leeds, passing through Calne, where I then resided, happened to see these plants in the last stage of the process, and thought that no experiment could be more satisfactory.

SECTION II.

Of the Change made in fixed Air by the electric Spark.

I Obſerved in a very early period of my experiments, that by taking the electric ſpark in fixed air, a part of it is converted into air that is not abſorbed by water. I have ſince repeated this experiment with more care; and though I have never been able to make the whole of any proportion of fixed air immiſcible with water by this means, yet I have always ſo far changed it, that the reſiduum was more conſiderable than before, but in different proportions.

I took the electric ſpark about two hours in a ſmall quantity of fixed air confined in a glaſs-tube by mercury. Before the experiment, one thirtieth of the air was unabſorbed by water, but afterwards one fourth. The glaſs tube, in which this experiment was made became very black in the inſide; and as this change is made in mercury by the addition of phlogiſton, it looks as if ſome of the phlogiſton, which had made a part of the fixed air, had, by this proceſs, been ſeparated from it; and leaving a greater proportion of dephlogiſticated air

air in the remainder, would neceſſarily make it leſs miſcible with water. The blackneſs on the inſide of the tubes, in which the electric ſpark is taken through vitriolic acid air or common air, I before diſcovered to be mercury ſuperſaturated with phlogiſton.

The next time that I repeated this experiment, I attended to the quality of the reſiduum before and after the proceſs; and the reſult was ſuch as ſeems to confirm the above-mentioned conjecture. I took the electric ſpark an hour and ten minutes in little more than half an ounce meaſure of fixed air, after which one fifth of the whole was unabſorbed by water, and the ſtandard of the reſiduum was 0.9. Of the original fixed air about one thirtieth was unabſorbed by water, and the ſtandard of the reſiduum was 1.0. In this experiment I alſo obſerved that the quantity of the air in which I made the experiment was increaſed about a twentieth part, which I do not pretend to explain.

Again, I took the electric ſpark an hour in half an ounce meaſure of fixed air, after which there remained as much reſiduum unabſorbed by water as had remained in about five times the quantity of the ſame fixed air in which no ſpark had been taken. This reſiduum was alſo much purer than that of the original fixed air, the ſtandard of it being 0.8, whereas that of the original fixed air had been, as before, 1.0. I repeated the experiment, and found the residuum still greater, but of the same pure quality; and in this case I observed a good deal of the black matter adhering to the inside of the tube.

In the following experiment I observed a farther change in this substance. In a small tube, containing about one fifteenth of an ounce measure of fixed air, I took the electric spark about an hour; after which there was a good deal of the black matter clouding all the inside of the tube; but the lower part of it was covered with something of a yellow colour, like sulphur. In this case the residuum not absorbed by water was between one fourth and one fifth of the whole, and less pure than the former residuums. Had not the dephlogisticated air in the fixed air passed into the mercury, tending to make it a precipitate *per se?* Was not this the cause of the residuum being less pure than before? And does not this experiment also prove, that phlogisticated air may be composed of the same materials with fixed air, viz. dephlogisticated air and phlogiston?

Again, I took the electric spark three hours in a small quantity of fixed air, and observed that it was first increased, and then diminished about one eighth of the whole; the inside of the tube being very black, and below the mercury very yellow, about

about the ſpace of a quarter of an inch quite round the tube. But that ſpace, or at leaſt part of it, had been above the mercury at the beginning of the proceſs. There remained one third of the air unabſorbed by water, and ſo impure, that the ſtandard of it was 1.8.

To vary the experiment, I took the electric ſpark in a quantity of fixed air confined by water, impregnated with fixed air. The quantity was much increaſed by the air extricated from the water, and after the proceſs by far the greater part of it was incapable of being abſorbed by lime water. In the courſe of this experiment, I obſerved that water impregnated with fixed air is by no means ſo good a conductor of electricity as water impregnated with any of the mineral acids.

Again I took the electric ſpark in fixed air, confined by a little common water, and obſerved that the blackneſs mentioned above extended more than a quarter of an inch below the ſurface of the mercury, in the ſame manner as the yellow colour had done before. In this caſe alſo, the reſiduum was purer than that of the original fixed air.

Again I took the electric ſpark half an hour in ſeven tenths of an ounce meaſure of fixed air, after which one tenth of it was immiſcible with water, and the reſiduum was evidently better than the natural reſiduum of the ſame fixed air. The ſtandard of that had been 1.0, and of the other about 0.85.

I took the electric ſpark three hours in about three fourths of an ounce meaſure of fixed air, after which it was increaſed in bulk one eighteenth. Water being admitted to it, there remained one ſixth unabſorbed. Being examined, the ſtandard was found to be as before, a little better than the reſiduum of the ſame fixed air*.

Being deſirous of aſcertaining whether this change in the conſtitution of the fixed air was owing to the *light*, or the *heat* produced by the electric ſpark, or to ſomething peculiar to electricity. I firſt threw a ſtrong light by means of a burning lens, on ſome pounded glaſs, confined in fixed air, for ſome hours. But though the reſiduum was by this means a little increaſed, yet being of the ſame quality with the common air, I ſuſpected that it was the air which was neceſſarily introduced through the quickſilver along with the pounded glaſs. There was no change in the dimenſions of the air after the experiment.

I repeated the proceſs with fine glaſs-houſe ſand, which had been previouſly expoſed to a ſtrong

* The addition of air in theſe experiments, Mr. Monge found to be *inflammable*, which muſt have come from the calcination of the mercury, and not, as he ſuppoſes, from the decompoſition of the *water* diffuſed through the fixed air. Mem. De l'Academie des Sciences for 1786, p. 430.

heat. But though the reſiduum was increaſed, the experiment was not, upon the whole, more ſatisfactory than the former. I alſo heated bits of crucibles in the ſame manner, and found the reſiduum larger than before, in the proportion of 10 to 6.6; but the quality of it was worſe. To what this ſhould be owing, I cannot tell.

I once more repeated the experiment with bits of crucibles, and the reſult was certainly favourable to the hypotheſis of a real change being made in the quality of the air by *heat*, but I do not pretend to ſay that it was deciſively ſo. After the proceſs with fifty ſix meaſures of the air there was a reſiduum of three meaſures; whereas before the experiment, the ſame quantity of the fixed air had left a reſiduum of only two meaſures. And that the additional meaſure was not the common air, introduced into the veſſel by adhering to the bits of crucibles, was evident from the quality of the reſiduum, which was the very ſame, viz. of the ſtandard of 1.1. I alſo aſſured myſelf that there was no fallacy of this kind in the experiment, by introducing the very ſame bits of crucibles into another equal quantity of fixed air. For I did not find that any ſenſible quantity of common air had been carried into the veſſel along with them.

However, by heating *iron* in fixed air, there can be no doubt but that a ſenſible quantity of it is converted into phlogiſticated air; which agrees with the experiments that I formerly made by putting pots of iron filings and brimſtone into fixed air. The experiments that I made of this kind were the following, in which it will be obſerved, that, though in ſome of them, there was an increaſe of the quantity of air after the proceſs, yet that it was by no means equal to the quantity that remained, unabſorbed by the water; and therefore, there muſt have been a farther addition made of this kind of air in the proceſs.

After heating turnings of malleable iron in a quantity of fixed air for ſome time, I examined a part of it, and found that about one tenth of the whole was immiſcible with water. Having reſumed the proceſs with the remainder, I found a reſiduum of one fourth of the whole. There ſeemed to be a ſmall addition to the quantity of air after the firſt part of the proceſs, but I could not perceive that there was any after the ſecond. I reſumed the proceſs a third time, but did not find that I had made more than one fourth of the whole immiſcible with water. At another time I heated the ſame kind of iron in fixed air, till of three ounce meaſures and three quarters of air there was a reſiduum of 0.8 of a meaſure, which was ſlightly inflammable, burning with a blue flame; and in this caſe there was no ſenſible addition to the quantity of air at all. Laſtly, I heated iron in three ounce meaſures of fixed air till there was an addition of 0.4 of a meaſure to the

quantity

quantity of it; but there was a residuum of one measure and a half not absorbed by water, which burned with a slightly explosive blue flame.

SECTION III.

Miscellaneous Observations on the Properties of fixed Air.

1. *The Acidity of fixed Air.*

FIXED air itself may be said to be of the nature of an acid, though of a week 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. Mr. Bewley proved in the most decisive manner, the acidity of fixed air, in the Appendix to the second of my former volumes of Experiments, p. 382.

2. *Fixed Air expelled from Water by boiling.*

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

3. *The freezing of Water impregnated with fixed Air.*

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

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

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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.

4. *Fixed Air, how affected by Iron Filings and Sulphur.*

Having obſerved a remarkable change in nitrous air, by a mixture of iron filings and ſulphur, I wiſhed to know whether any alteration would be made in the conſtitution of fixed air, by the ſame means. 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,

on, in all respects, as if the air in the inside had been common air.

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

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

5. *Iron in fixed Air.*

Though fixed air incorporated with water dissolves iron, fixed air without water has no such power,

power, as I observed before. I imagined that, if it could have dissolved iron, the phlogiston would have united with the air, and have made it immiscible with water; but after being confined in a phial full of nails from the 15th of December to the 4th of October following, neither the iron nor the air appeared to have been affected by their mutual contact.

6. *Fixed Air changed by Incorporation with Water.*

Mr. Cavendish observed that a certain portion of fixed air is no more liable to be absorbed by water than common air. This, he states at about one sixtieth part of the whole. I had the curiosity to try, whether, if I saturated a quantity of water with fixed air, and expelled it again by heat, that very air which had actually been in the water, would not be wholly imbibed by fresh water; and whether I could not, by this means, get a purer kind of fixed air than that which is immediately procured by means of chalk and oil of vitriol. This experiment I made twice, with all the care that I could apply, and found, in both the cases, that even the fixed air which had been in the water, contained as large a portion of that which would not be imbibed by water again, as the air which had been immediately dislodged from chalk by oil of vitriol.

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In order to be more ſure of this fact, I was more eſpecially careful, the ſecond time that I made the experiment, to uſe every precaution that I could think of, in order to prevent any error in the concluſion. For this purpoſe, I took rain-water, and boiled it about two hours, in order to get it perfectly free from air; and I began to impregnate it with fixed air a long time before it was cold, and therefore before it could have imbibed any common air; and, in order to expel the air from it, I put it into a phial, which I plunged in a veſſel of water ſet on the fire to boil, taking care that both the phial containing the impregnated water, and the glaſs-tube through which the air was to be tranſmitted, were completely filled with the water, and no viſible particle of common air lodged on the ſurface of it. I alſo received the expelled air in water, which contained very little air of any kind, leſt the very ſmall degree of agitation which I made uſe of, in order to make the water re-imbibe the air, ſhould diſengage any air from it. Alſo, that leſs agitation, and leſs time, might be ſufficient, I chiefly made uſe of lime-water for this purpoſe. But notwithſtanding all theſe precautions, I found a very conſiderable reſiduum of air, not leſs than Mr. Cavendiſh had ſtated, that water would not imbibe.

At a time when this reſiduum of fixed air hardly gave the leaſt ſenſible whiteneſs to lime-water, I examined

examined the ſtate of it, and found, by the teſt of nitrous air, that it was very little worſe than common air; two meaſures of this air, and one of nitrous air, occupying the ſpace of two meaſures only.

7. *Fixed Air expoſed to Heat.*

I expoſed fixed air, as well as all the other kinds of air, to a continued heat, and in this caſe I made uſe of a green glaſs tube. I kept it in hot ſand a whole day, ſo hot that one end of the tube was much dilated, but had not burſt. Opening it under water, one half of the tube was inſtantly filled, and the remainder was the pureſt fixed air. I did not perceive any thing depoſited on the glaſs, as in the caſe of the marine and vitriolic acid air.

8. *A Source of Deception from fixed Air, contained in Water.*

I obſerved that, in one produce of air from a ſolution of biſmuth in the nitrous acid, I found a ſmall quantity of *fixed air*, but that when I repeated the experiment, I could not find any appearance of the kind. I afterwards made an obſervation that will probably explain this diverſity of appearances, and which alſo ſhews that, unleſs care be taken

taken that the water in which the experiments are made contain little or no fixed air, miſtakes of this kind will certainly be made. For I found at one time that, if any kind of air was made to paſs through a quantity of water containing much fixed air, it would attract a portion of it, and would not eaſily part with it afterwards. At another time, however (I think it was in colder weather) I found that air conveyed through the ſame kind of water (which was from a pump) did not attract any fixed air. But I have not had leiſure to examine the circumſtances that might occaſion this difference. Of both the *facts* I am very certain.

9. *Of fixed Air in acetous Fermentation.*

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

During

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

The ſmell of this air was exceedingly pungent, but different from that of the putrid effluvium.

10. *Fixed Air from putrefying animal Subſtances.*

When I made my experiments on air affected by *putrefaction*, I obſerved that the water in which the mice were ſuffered to putrefy, muſt have tranſmitted ſome *volatile effluvium* from the putrefying ſubſtances, into the ſurrounding air. This I ſuppoſed muſt be phlogiſton, which putrefying ſubſtances certainly do emit, loaded with that matter which affects the noſtrils with the ſenſe of ſmell, concerning which I know nothing. But beſides this, I have found that, by this means, water becomes thoroughly impregnated with *fixed air*, diſcharged, no doubt, from the putrefying ſubſtance. That

That this water might have got *ſome* fixed air I ſuſpected; but to find that it had got ſo very much, I own ſurprized me.

Having put two dead mice into a quantity of water, and examining the proceſs after a month, I found that the water was ſtrongly impregnated with a putrid effluvium, which was very offenſive, and that ſome air, unabſorbed by the water, lodged in the top of the phial; it having been filledwith water, and inverted in a baſon of the ſame. With the impregnated water I filled a phial with a ground ſtopper and tube, and making it boil, I expelled from it about its bulk of air, which, when examined, was found to be all pure fixed air. N. B. The water was very turbid, and during the proceſs it depoſited a white matter, reſembling a ſoft mucilage, with ſome ſmall ſpecks of black in it.

PART IV.

OF THE CONSTITUENT PRINCIPLES OF FIXED AIR.

SECTION I.

Fixed Air contains Water.

THAT water is an effential ingredient in the conftitution of fixed air, as well as probably of all kinds of air, is demonftrated by my experiments on *terra penderofa aerata*; and thefe may ferve to explain fome of the following more early obfervations on getting fo little air from *chalk*.

Heat, I obferved, fometimes is able to expel but very little air from chalk. I kept a very fmall quantity of chalk in the focus of a burning lens, twelve inches in diameter, and twenty inches focal diftance, more than half an hour, when the fun was near its greateft altitude, on the 23d of July; but notwithftanding this long expofure to fo intenfe a degree of heat, it feemed to give as much fixed air when thrown into a veffel of water, acidulated with oil of vitriol, as an equal quantity of chalk which had not been expofed to any heat at all. Of this, however, I only judged by the viſible effervеſcence, and did not make any attempt to meaſure the produce of air, in order to aſcertain the effect of theſe different circumſtances with accuracy. I have alſo kept chalk more than a quarter of an hour in the ſtrongeſt heat of a ſmith's forge, in a crucible, without making any ſenſible alteration in it.

When I put a quantity of chalk into a tall glaſs-veſſel, and kept it in as ſtrong a ſand-heat as it would bear, without melting, I extracted from it only about its own bulk of air; and this was fixed air.

Terra ponderoſa aerata (a ſubſtance of which Dr. WITHERING has given us an excellent analyſis) gives no fixed air by mere heat. But I find, that when ſteam is ſent over it, in a red heat, in an earthen tube, fixed air is produced with the greateſt rapidity, and in the ſame quantity as when it is diſſolved in ſpirit of ſalt: and, making the experiment with the greateſt care, I find, that fixed air conſiſts of about half its weight of water.

From two ounces of the terra ponderoſa I got, by means of ſteam, 190 ounce meaſures of fixed air, ſo pure that at firſt 150 ounce meaſures of it were reduced by agitation in water to three and a half, and of the laſt produce, 30 ounce meaſures were reduced to one. Examining the reſiduum of the firſt portion by means of nitrous air, I found it to be of the ſtandard of 1.5.

After

After this, attending to the *water* expended in the procefs, I found that I procured 330 ounce meafures of fixed air with the lofs of 160 grains of water. According to this, as the air weighed 294 grains, the water in the fixed air muft have been 80 parts of 147 of the whole.

In another experiment, having previoufly found that three ounces of the terra ponderofa yielded about 250 ounce meafures of fixed air, I attended only to the lofs of water in procuring it, and I found it to be about one fifth of an ounce, in two fucceffive trials. The quantity of fixed air would weigh 225 grains, and the water expended about 100 grains; fo that, in this experiment alfo, the fixed air muft have contained about one half of its weight of water.

That water enters into the compofition of fixed air, and adds confiderably to its weigh, is farther probable from the folution of terra ponderofa in fpirit of falt. Becaufe when the folution is evaporated to drynefs, and the refiduum expofed to a red heat, the weight of the air, and of this refiduum, exceeds that of the fubftance from which it was procured; and it is probable, that a red heat would expel any marine acid adhering to it.

Forty eight grains of terra ponderofa diffolved in fpirit of falt, and then evaporated to drynefs, and expoſed to a red heat, loſt four grains, and yielded eight ounce meaſures of fixed air, which would weigh 7.2 grains; conſequently, three ſevenths of the weight of the air was ſomething that had been gained in the proceſs, and therefore probably water.

The near coincidence of the reſults of theſe different experiments is remarkable, and makes it almoſt certain, that no marine acid is retained in the terra ponderoſa that has been diſſolved in it, after expoſure to a red heat; that the generation of the fixed air carries off part of the water in the menſtruum, and that this part of the weight is about one half of the whole.

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SECTION II.

Fixed Air may be procured by Means of nitrous Acid.

THAT *nitrous acid*, and *fixed air*, conſiſt of the ſame elements, differently combined, will be demonſtrated by my experiments on the ſubject of nitrous acid, and this may throw ſome light on the following more early experiments.

When heat can expel no more fixed air from charcoal, it ſhould ſeem that ſpirit of nitre (if this acid itſelf be not converted into fixed air) can extract more from it. For when I diſſolved, in ſpirit of nitre, ſome pieces of charcoal, which had been made with the ſtrongeſt heat of a ſmith's fire, long continued, ſo that no more air could be expelled from them by that means; part of it was evidently fixed air, as appeared by its precipitating lime in lime-water.

One of the moſt deciſive experiments of this kind, was made with ſpirit of wine, which nobody, I believe, ſuſpects to contain any fixed air. For though it makes lime-water turbid, Dr. Black has juſtly obſerved, that this is produced by its union with the *water*, in conſequence of which the lime is precipitated in a cauſtic ſtate. A doubt, however, might be made, whether the turbid appearance made by the air which I produced from the ſpirit of wine, was really the effect of fixed air. I endeavoured, therefore, and with ſucceſs, to purſue the experiment farther, and I did it in the following manner:

From a mixture of ſpirit of wine and ſpirit of nitre, diluted with water, I produced a very conſiderable quantity of air, the greateſt part of which, being received in a large body of lime-water, was readily abſorbed, making the water very turbid. Waiting about a quarter of an hour, till the precipitated matter ſubſided, I poured the water from it, and putting a very ſmall quantity of the precipitate into ſome water out of which the air had been well boiled, I poured a little diluted oil of vitriol upon it, in a phial with a ground ſtopper and tube, and found it to yield air in great plenty; and this air, being admitted to lime-water, appeared to be, in all reſpects, genuine and pure fixed air.

By this experiment it appears, that the ſubſtance formed by the union of this air and the lime was really chalk, or lime-ſtone, yielding genuine fixed air with acids, exactly as other calcareous ſubſtances do. The air was firſt generated from ſpirit of nitre, and ſome other principle contained in the ſpirit of wine: it was then incorporated with lime, and

and after that dislodged from the lime by the vitriolic acid, and made to appear in the form of air again.

Fixed air was also generated in the solution of iron in spirit of nitre.

Having dissolved a quantity of iron in spirit of nitre, diluted with an equal quantity of water, before the effervescence was over I removed the vessel in which it was contained (which was a tall glass phial) into a sand-heat, and received the air, which was transmitted through a glass tube, luted with a mixture of sand and clay, in phials containing rain water. The quantity of air produced in these circumstances was very considerable, and part of it was unquestionably fixed air, and what is remarkably to the purpose of the experiment, the proportion of fixed air kept increasing as the process advanced, till at the last it was more than one third of the whole. All the rest of the air was nitrous, and after the process some of the iron was found undissolved.

SECTION III.

Fixed Air may be formed by Means of ſomething imbibed from the Atmoſphere.

IT is evident, that the ſolutions of ſome of the metals in the nitrous acid, which do not immediately yield any fixed air, will do ſo after they have been expoſed to the common atmoſphere. This appears in the following experiment:

Upon 108 grains of quickſilver I poured the ſame weight of ſtrong ſpirit of nitre, hanging balanced in a pair of ſcales; when I obſerved that the mixture loſt weight by the eſcape of air, till it was reduced to 123 grains. After this it gained weight, till it was conſiderably more than the original quantity, but how much this additional weight was I neglected to take any account of. The mixture was made on the 25th of September, and when it had ſtood in an open and ſhallow veſſel till the 12th of January following, I diſtilled the whole of it to dryneſs, in a glaſs phial; when I found that one ſeventh of the air produced from it was fixed air, and the reſt dephlogiſticated. I took in all, ſeven ounce meaſures, but loſt a good deal

deal that efcaped by the luting, and by the veffel breaking before the procefs was over.

I then put the fame quantity of quickfilver and fpirit of nitre into a clean phial, and diftilling it to drynefs *immediately*, without giving it any opportunity of communicating with the external air (but not beginning the diftillation till the folution was completed) I received in all thirty two ounce meafures of air, of which the firft fourteen were pure nitrous air, and the remainder pure dephlogifticated air, without the leaft mixture of fixed air in either of them.

Wood-afhes have alfo the property of imbibing one of the elements of fixed air from the common atmofphere, but they require confiderable time to do this, in any very fenfible degree; for when they had been well burned, I have not found that they yielded any air that I could collect after being expofed to the open air a day or two; but that they do become faturated with fixed air in a courfe of time, is evident from the following experiment.

From about three quarters of an ounce meafure of wood-afhes, from which I had, about three months before, expelled as much air as I poffibly could, by the greateft heat of a common fire, urged with a pair of bellows, in a gun-barrel of about half an inch diameter, I got by the fame procefs fifteen

fifteen ounce meaſures of air, eleven of which were completely abſorbed by water, and the remainder burned with a lambent blue flame. The phlogiſton requiſite for this appearance might come either from the gun-barrel, or from ſome imperceptible bits of charcoal contained in the aſhes.

From twice the quantity of wood-aſhes, which had been burned about the ſame time with the others, in a much wider gun-barrel, I got about twice the quantity of air, the greateſt part of which, as in the former experiment, was fixed air, and the remainder burned with a lambent blue flame.

Having taken this air in ſeveral portions, I obſerved that the firſt contained a much greater proportion of fixed air than the laſt, though what there was of it ſeemed to be equally inflammable.

A more deciſive experiment relating to the *generation of fixed air* than that which is mentioned above with *wood-aſhes,* is one that I made with the *aſhes of pit-coal.* Pit-coal itſelf, diſtilled in a glaſs veſſel, yields no fixed air, but only inflammable air, which, being fired in a wide-mouthed jar, burns with a bright lambent flame, without exploſion. But the aſhes of the ſame pit-coal yielded much air, of which one half was fixed, and the reſt inflammable. When I had expelled all the air that I could from a quantity of theſe aſhes, I mixed ſpirit of nitre with them, and they immediately

yielded

yielded as much air as before; and of this one half was fixed, and the rest nitrous. Mixing more spirit of nitre with the same ashes again, the produce was the same as before.

To be more fully satisfied with respect to the above-mentioned experiment with wood-ashes, and also the quantity of fixed air imbibed by them in a given time, I kept the same ashes, and extracted air from them at certain intervals. I also did the same thing with several other substances of a similar nature, and the results were as follows.

On the 18th of April, 1778, I extracted all the air I could from half an ounce of wood ashes, and got about eighty ounce measures, half fixed air, and half inflammable throughout; and on the 25th of the same month I repeated the process on the same ashes, in a gun-barrel, and got from them twenty ounce measures of air, the greatest part of which was fixed air, and the rest inflammable. The ashes were become almost black after the experiment.

June the 2d, I extracted, by heat, in a gun-barrel, from wood ashes from which air had often been extracted before, in the same manner, and the last time on the 9th of May preceding, all the air that they would yield. It was twenty one ounce measures; the first portions of which were half fixed air, and afterwards one third; the remainder

mainder in both cafes being inflammable, probably from the iron. A good deal of moifture diftilled from thefe afhes, though they feemed to be perfectly dry. After the procefs, they weighed eighteen penny-weights, and, judging from their colour, not much more than two thirds of them had been affected by the heat.

On the 23d of October following, the fame wood afhes weighed nineteen penny-weights twelve grains, and I got from them, in a gun-barrel, about thirty ounce meafures of air, of which more than twenty five ounce meafures was pure fixed air, the remainder inflammable, burning with a blue flame. They had not all been equally affected by the heat. After the procefs, they weighed eighteen penny-weights fix grains. That they had attracted fixed air is evident, efpecially from the laft procefs, in which the greateft part of it was very pure.

On the 18th of April, 1778, I got, from an ounce of *pit-coal afhes*, in a gun-barrel, nineteen ounce meafures of air, of which at firft two thirds, and at the laft one third was fixed air, and the reft inflammable. On the 24th of the fame month, I extracted from the fame pit-coal afhes (which, as well as the wood afhes in the preceding experiment, had been expofed to the open air in a difh, fo as to lay about half an inch thick) 110 ounce meafures measures of air; but with more heat than before. Of the first part of this air one third was fixed air, but of the last hardly any, the remainder being inflammable, burning with a blue flame; but so faintly, that probably the greatest part of it was phlogisticated air.

Heating the same ashes over again, in a shallow iron vessel, and letting them cool, I got from them, by the same process, fifteen ounce measures of air, one third of which was fixed air, and the rest inflammable.

Common pit-coal, I have observed, yields no fixed air, though the *ashes* do; but I have found that one species of pit-coal, called *Bovey coal*, yields fixed air in the first instance, which seems to indicate that there is something of a vegetable nature in that coal. From half an ounce of this coal I got, in a gun-barrel, about an hundred ounce measures of air, three fourths of which was fixed air throughout, and the remainder inflammable; the first part of it burning with a bright white flame, like inflammable air from common pit-coal, the last part exploding like inflammable air from metals, only more faintly. Part of this air had probably come from the gun-barrel.

Bone ashes, I found, had not the same property of drawing fixed air from the atmosphere that

that the ashes of vegetable and minerable substances have; but that the addition of spirit of nitre gives them that property.

SECTION IV.

Of the Generation of fixed Air from the vitriolic Acid.

I Had an evident proof of the generation of fixed air from the *vitriolic acid* united with spirit of wine, or with ether, which is produced from them both; so that these two acids, viz. the vitriolic and nitrous, agree in being capable of forming both dephlogisticated and fixed air.

After going through the process for making ether, from concentrated oil of vitriol and rectified spirit of wine, I had the curiosity to push the process as far as it would go, in order to examine whether any kind of air would be yielded in any stage of it. I therefore continued the distillation till the whole residuum was converted into a black mass, full of gross matter; and taking as much of the black lumps as filled about one fifth of an ounce

ounce meaſure, I put them into a tall glaſs veſſel, and diſtilled them to dryneſs in a red hot ſand heat.

The firſt air that came over was the common air a little phlogiſticated, then the vapour of the watery part, and after that a large quantity of air, at firſt clear, but towards the middle of the proceſs very turbid and white, but clear again at the laſt. I received in all about a pint and a half, in four portions, each of which contained about four fifths of fixed air, and the reſt inflammable, burning with a blue flame; but the proportion of fixed air was ſomething greater in the middle portions than either in the firſt or the laſt. I thought it poſſible that the cork, with which, as well as with clay and ſand, the glaſs tube was joined to the glaſs veſſel that contained the materials, might ſupply the inflammable air in part, as I perceived it was corroded and become black. It may be worth while to repeat this proceſs in a glaſs retort.

Having gone over this proceſs with ſpirit of wine, I recollected the black matter that was produced when I got vitriolic acid air from vitriolic acid and ether; and therefore determined to repeat that proceſs and carry it farther; to ſee whether I ſhould, in any part of it, get fixed air, as in the preceding experiment with the ſpirit of wine.

I therefore put one eighth part of vitriolic ether to a quantity of freſh diſtilled oil of vitriol, and in a glafs phial with a ground-ftopper and tube, and with the heat of a candle, I got from it a great quantity of air, part of which was vitriolic acid air, which was abforbed by the water. But I obferved, as the procefs advanced, the part that was not readily abforbed by water kept increafing, till at length the greater part of the produce was of this kind; and in the middle of the procefs it was very turbid. Examining this air it appeared to be fixed air, making lime water turbid, and being readily abforbed by water; but there was a refiduum of phlogifticated air, about one fixth of the whole.

I then put the remaining materials, which were about an ounce meafure, into a glafs veffel; and with a fand heat I collected much more air than before, about two pints in all, the firft part of which was the pureft fixed air I had ever feen, having the fmalleft refiduum. The laft portion had more refiduum, and this burned with a lambent blue flame. But this inflammable matter might poffibly come from the cork with which the veffel was clofed, as before; though I think it not fo probable. At laft the procefs was interrupted by an accident; but I concluded, from feveral circumftances, efpecially from the time that elapfed before the vapour ceafed to iffue from the orifice of the veffel (which continued buried in the

hot

hot ſand) that more than twice the quantity of air might have been collected. The air had been very cloudy before the laſt portion, which contained the reſiduum of inflammable air.

From this experiment, eſpecially that with the ether, in the glaſs phial and ground ſtopper, I think it is pretty evident, that fixed air is a *factitious ſubſtance*, and that the vitriolic, as well as the nitrous acid, may be converted into it.

SECTION V.

Of the Compoſition of fixed Air from dephlogiſticated Air, and Phlogiſton, by the Generation of it from heating together Subſtances containing each of them.

I Have ſeveral times given it as my opinion, that fixed air is a *factitious ſubſtance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that concluſion; but that it was compoſed of dephlogiſticated air and phlogiſton, though maintained by my friend Mr. Kirwan, I was far from being ſatisfied with, till I was forced to conſent to his proof of it from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on falts. But I have lately had two direct proofs of it by experiment.

The firft experiment which feemed to prove that fixed air may be compofed of dephlogifticated air and phlogifton, was made with *charcoal* and red precipitate, the charcoal being made with fo great a degree of heat, that no fixed air could be expelled from it, not even when it was wholly difperfed by the heat of the fun in vacuo. This experiment is certainly, however, not fo conclufive as the former; becaufe, fince dry wood and imperfectly made charcoal yield fixed air, it may be faid that all the *elements* of this kind of air were contained in the moft perfect charcoal. And though this fubftance alone will not, even with the affiftance of water, give fixed air, it might be faid, that this might be effected by its treatment with other fubftances, without their imparting any thing to it; efpecially as the inflammable air which is procured from charcoal by means of water appears to contain fixed air, when decompofed with dephlogifticated air. I think, however, that I have proved that this fixed air is really a compofition of phlogifton contained in charcoal, and of the dephlogifticated air with which it was inflamed, the charcoal contributing nothing to it befide its phlogifton.

gifton. In this place I fhall only recite the facts concerning the production of great quantities of fixed air from perfect charcoal and red precipitate.

In order to expel all fixed air, I made a quantity of perfect charcoal from dry oak; and while it was hot I pounded it, and immediately mixing four meafures of it with one of red precipitate, and putting them into an earthen retort, I prefently got, in no greater a degree of heat than was neceffary to revive the mercury, a large quantity of air, half of which was fixed air. Afterwards the proportion of fixed air was lefs, and towards the conclufion of the experiment there came no fixed air at all. This refiduum was a little better at the firft than at the laft, when it was of the ftandard of 1.5.

As this air contained a greater portion of phlogifticated air than the common air of the atmofphere, and no fpirit of nitre, or any thing that could yield fpirit of nitre, was concerned in the experiment, it fhould feem that phlogifticated air may be compofed of phlogifton and deplogifticated air; though this compofition, according to the very capital difcovery of Mr. Cavendifh, may be reduced to fpirit of nitre, or rather become one element in the compofition of that acid.

In another experiment I hit upon a better proportion of the charcoal and red precipitate for making pure fixed air. For mixing one ounce of red precipitate (which all chemiſts, I believe, are agreed to be the ſame thing with *precipitate per ſe)* and one ounce of perfect charcoal, freſh from the retort in which it was made; and putting them into a coated glaſs veſſel, I procured from the mixture, by heat, about thirty ounce meaſures of air, the whole of which was the pureſt fixed air, leaving only about one fortieth part not abſorbed by water, and this not inflammable, but of the ſtandard of 1.7, or almoſt perfectly phlogiſticated.

This experiment made me recollect thoſe which I had formerly made with charcoal heated in nitrous acid, in which I had always procured a quantity of fixed air. I therefore repeated the experiment with ſome of the ſame charcoal which I had uſed in the preceding experiment, on the goodneſs of which I could depend; and I found that, when it was heated in the acid, in a glaſs phial with a ground ſtopper, it gave air, one fifth of which was fixed air. At another time I got air in this proceſs, one half of which was fixed air. To the formation of this air, I preſume, that the phlogiſton from the charcoal and the dephlogiſticated air, which is known to be produced by heating nitrous acid, muſt have contributed.

Being then apprized of the objection that might be made to the uſe of *charcoal,* as, notwithſtanding the

the great heat with which it was made, containing at leaſt the elements of fixed air, I made uſe of *iron*, to which no ſuch objection could be made; and mixing an ounce of the red precipitate with an ounce of iron filings, and then heating them in a coated glaſs retort, I got twenty ounce meaſures of air, of which only one ſeventh remained unabſorbed by water. The reſiduum was of the ſtandard of 1.52, but ſlightly inflammable.

Again, from half an ounce of red precipitate, and half an ounce of iron filings, I got twenty ſix ounce meaſures of air, of which the firſt part was pretty pure fixed air; but afterwards one tenth of it remained unabſorbed by water. Then, increaſing the proportion of iron, I mixed one ounce of red precipitate with two ounces of iron filings, and got about forty ounce meaſures of air, of the firſt portions of which only one twentieth was unabſorbed by water, though towards the concluſion of the proceſs this reſiduum was greater. In this proceſs I got, in the whole, thirty ſix ounce meaſures of pure fixed air, completely abſorbed by water, beſides what was abſorbed both in the firſt reception of the air (which was in veſſels containing water) and afterwards in transferring this air into thoſe veſſels in which the quantity of it was noted, the whole of which I ſuppoſe might be about four ounce meaſures more. Examining the firſt reſiduum of this procefs by nitrous air, the ftandard of it was 1.6, and afterwards 1.7 *.

Having heard that it was objected to this experiment, that iron contains a quantity of *plumbago*, and that the fixed air which I procured might come from that ingredient in it (though the quantity was certainly much too great to be accounted for in that way) I made ufe of other metals, to which no fuch objection could be made, viz. *brafs* and *zinc*, and with the fame refult.

With two ounces of brafs duft I mixed one ounce of red precipitate, and in a coated glafs retort I got from it a quantity of air, two thirds of which was fixed air. The ftandard of the refiduum was 0.6; fo that there had been too great a proportion of the

* It appeared, in fome of thefe experiments, that three ounce meafures of dephlogifticated air go into the compofition of two ounce meafures of fixed air. For one ounce of this red precipitate gave fixty ounce meafures of dephlogifticated air; and when mixed with two ounces of iron filings, it gave about forty ounce meafures of fixed air that were actually abforbed by water, befides a refiduum that was inflammable. I had the fame proportion when I ufed half an ounce of each of the materials. But when I ufed one ounce of each, I got only twenty ounce meafures of fixed air, including the refiduum. At other times I had different proportions with different quantities of iron filings and charcoal.

It muft be obferved, however, that part of the fixed air is always imbibed by the water in which it is firft received. Otherwife, in this experiment, the fixed air would have weighed no more than the dephlogifticated air in the compofition of it, fo that nothing would be left for the inflammable air.

red

red precipitate. But fixed air was produced in a quantity abundantly ſufficient for my purpoſe.

In a coated glaſs retort, I put a mixture of one ounce of red precipitate and one ounce of filings of *zinc*, and got ſome air, part of which was clearly fixed air; but the retort very ſoon cracking, put an end to the experiment, and I did not think it neceſſary to repeat it. I imagine, however, that it will only be at the beginning of this proceſs that much fixed air can be procured, unleſs more precaution be uſed in conducting it. For the neck of the retort breaking quite off, there iſſued from it a ſtrong flame, which evidently aroſe from the burning of the zinc in the dephlogiſticated air from the precipitate.

As turbeth mineral gives dephlogiſticated air, as well as red precipitate, I mixed this ſubſtance with iron filings, and had a ſimilar reſult, when I heated them together in an earthen retort. One ounce of the turbeth mineral with two ounces of iron filings, yielded about ſixteen ounce meaſures of air, of which about one third was fixed air, and the reſt of the ſtandard of 1.5.

Another experiment which ſeems to prove the formation of fixed air from phlogiſtion and dephlogiſticated air, is the expulſion of it from that *black powder* which is formed by the union of lead and mercury. This powder, I have obſerved, can only be made in pure air, which is no doubt abſorbed by the metals; and this being again expelled by heat, together with the phlogiſton which had belonged to the lead, is that, I preſume, which forms the fixed air that is found in this proceſs.

When I began to make obſervations on this black powder, I mentioned my having expelled ſome fixed air from it. This was from ſuch powder as I had found ready made; and therefore, not knowing with certainty what the compoſition of it was, I diſſolved one ounce of lead in pure mercury, and then expelled it again in the form of this black powder, which, when the running mercury was pretty carefully preſſed out of it, weighed about twelve ounces. Then expoſing it to heat, in a coated glaſs retort, I got from it about twenty ounce meaſures of air, making allowance for the quantity of fixed air, which, as I ſuppoſed, might have been abſorbed by the water, in receiving and transferring the air before any account was taken of the quantity of it. Of this air about one thirtieth part only was not abſorbed by water. The reſiduum I did not examine. I muſt however obſerve, that in general, beſides the fixed air, I obtained a conſiderable quantity of the pureſt dephlogiſticated air, from this black powder.

In making the black powder that was uſed in the preceding experiment, I occaſionally changed the air

air in the phial, in which I ſhook the mercury, by blowing into it, ſometimes with a pair of bellows, and ſometimes with my mouth; and as it was ſuggeſted that this might have ſupplied the fixed air which I afterwards found in the black powder, I diſſolved two ounces of lead in mercury, and got the black powder without blowing into the veſſel at all, only changing the air ſo much oftner as was then neceſſary. From ſix ounces of the black powder thus carefully prepared, I expelled four ounce meaſures and a half of air, of which one and a half was pure fixed air. This was ſufficient to ſatisfy me that *ſome* fixed air is certainly procured in this proceſs. The reſiduum of this fixed air was of the ſtandard of 1.7, or 1.8. I did not at this time get from this powder all the air it would have yielded.

Being now ſatisfied that there was no occaſion to prepare this black powder with the precaution mentioned above, I repeated the experiment with ten ounces of it prepared in the readier method which I had uſed before, with a view to examine the reſiduum of the air, when the fixed air ſhould be ſeparated from it. The produce of air was in all about twenty three ounce meaſures, which I received in four portions of five ounce meaſures each, and another containing the remainder. All theſe portions I examined ſeparately, obſerving the proportion

portion of residuum in each of them, and the quality, as measured by my usual standard, and the result was as follows. Of the first portion there remained one fourth, of the standard of 1.6; of the second one third, of the standard of 1.44; of the third one half, of the standard of 0.8; and of the fourth three fourths, of the same quality with the preceding.

In the last portion the residuum was one half of the whole, and that I found to be so pure, that, mixing it with two equal quantities of nitrous air, the standard of it was 0.63; so that the quality of these residuums was continually purer, till at the last it was pretty highly dephlogisticated.

It may be inferred from both these courses of experiments, that fixed air consists not of inflammable air (which I suppose necessarily contains water) but of pure phlogiston, and dephlogisticated air. In the experiments with the red precipitate and iron, no water at all is concerned, unless either the iron itself contain some, or the mercury, or dephlogisticated air: since when the red precipitate is decomposed by itself, nothing is produced besides mercury and dephlogisticated air, without any water. The experiment with the black powder will equally authorize the same conclusion, as neither the lead, the mercury, nor the pure air that combines with them, has been supposed to contain any water. It must however

ever be obſerved, that the greateſt part of dephlogiſticated air is water.

While I am upon the ſubject of this black powder, I ſhall obſerve, that it occurred to me to mix with it more matter containing phlogiſton, in order to ſee what change that would make in the reſiduum of the produce of air.

From four ounces of the black powder mixed with two ounces of iron filings, heated in an earthen retort, I expelled fifty four ounce meaſures of air, of which not more than four ounce meaſures were fixed air, and the reſiduum, examined at different times, was of the ſtandards of 1.3, and 1.44; but the greater part of it was of 1.52, ſo that there was a conſiderable production of inflammable air from the iron. In this experiment, I raiſed the heat very gradually, till I had got one third of the produce of air. This I did from an idea that this moderate heat might increaſe the quantity of the fixed air, but it did not appear to make any difference in this reſpect.

Then varying the proportion of the ingredients, I mixed twenty ounces of the black powder with only one ounce of iron filings, and receiving the air in three portions, obſerved as follows. The firſt portion, which contained ſix ounce meaſures, had a reſiduum of 3.5, of the ſtandard of 1.6. The ſecond, which was one ounce meaſure, had a reſiduum of 0.12. of the ſtandard of 1.7; and the third portion, which was only one ounce meaſure, had a reſiduum of 0.12, of the ſtandard of 1.7. Whether this was the whole of the produce of air, I do not recollect.

In order to try more fully the effect of different degrees of heat, I repeated the proceſs with the black powder, only determining to ſuſpend the proceſs in the middle of the produce of air. Accordingly I heated two ounces of the black powder in a porcelain veſſel; when I obſerved that ſome portions of the produce contained about one half fixed air, and that this proportion kept growing leſs and leſs, till the produce conſiſted of nothing but the pureſt dephlogiſticated air, the ſtandard of it being, with two equal meaſures of nitrous air, 0.2. I then let the veſſel cool, and obſerved that, on reſuming the experiment, the air came with the ſame purity to the laſt.

Examining the reſiduum in the retort, I found half an ounce of red powder, the colour of which could hardly be diſtinguiſhed from that of percipitate *per ſe*. So that, no doubt, the mercury had been converted into it, and this very pure air was probably that which came from the precipitate as it was reviving. In this way, therefore, it would be eaſy to make this precipitate in large quantities, could a method be found of ſeparating it from the

red

red lead, with which it is, in this proceſs, neceſſarily mixed.

In the preceding experiment it will have been obſerved, that, at firſt, the reſiduum was conſiderably phlogiſticated, but at the laſt remarkably pure. An accident in a ſubſequent experiment I once thought had diſcovered the cauſe of this difference. In the middle of one of the proceſſes, in which I was uſing the black powder only, heating it in a glaſs veſſel, a quantity of water was drawn up through the tube that communicated with the recipient, and got into the veſſel that contained the black powder; and in all the remainder of that proceſs, the reſiduum of the air was no better than about the ſtandard of 1.7. Water came over along with this air to the very laſt, though the bottom of the veſſel was red hot. When the proceſs was over, the matter taken out of the veſſel was ſtill moiſt, and of a dark grey colour.

On this I made a paſte of the powder with water, and drying it a little, immediately repeated the experiment with it; but I found no ſenſible difference between the ſubſtance in this ſtate, and that which had not been wetted. Four ounces of it yielded 120 ounce meaſures of air, of which about twelve were pure fixed air, completely abſorbed by water, and the reſt highly dephlogiſticated. However, in one proceſs of this kind, from two ounces and a half of this powder, which had been moiſtened

moiſtened and dried again, I got ſeventy ounce meaſures of air, of which only a very ſmall part was fixed air, and the reſiduum was by no means pure dephlogiſticated air. For with two equal meaſures of nitrous air, the ſtandard was 1.2 and 1.3. At other times alſo I have had much leſs fixed air from this black powder when it had not been wetted, than in ſeveral of the inſtances above-mentioned; and I have not as yet been able to diſcover the circumſtance on which the production of it in a greater or leſs quantity depends.

In the preceding proceſſes with this black powder, I always got from it more or leſs of fixed air. But thinking to produce more of it by heating this ſubſtance with a burning lens in dephlogiſticated air, I was ſurpriſed to find, that I only increaſed the quantity of dephlogiſticated air in the veſſel, and produced no fixed air at all. Whence this remarkable difference could ariſe, I do not pretend to ſay. It will be ſeen, that, in this proceſs with inflammable air, I found it to be a matter of indifference whether I uſed this black powder or the red precipitate; both of them equally imbibing inflammable air, without producing either water or fixed air.

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SECTION VI.

Of the Generation of fixed Air by heating Subſtances containing Phlogiſton in dephlogiſticated Air.

ANOTHER deciſive proof of the generation of fixed air from phlogiſton and dephlogiſticated air, is the conſtant production of it when iron is melted in dephlogiſticated air over mercury, by means of a burning lens. This experiment being a very pleaſing one, I repeated it very often; and as it is on too ſmall a ſcale to admit of great exactneſs, I ſhall mention the reſults of ſeveral of them, obſerving, in the firſt place, that no water is produced in this proceſs.

In ſix ounce meaſures and a half of dephlogiſticated air, I melted turnings of malleable iron till there remained only an ounce meaſure and one third, and of this twenty ſeven thirtieths of an ounce meaſure was fixed air. In ſix ounce meaſures of dephlogiſticated air, of the ſtandard of 0.2, I melted iron till it was reduced to two thirds of an ounce meaſure, of which one half was fixed air, and the remainder completely phlogiſticated. Again, I melted

melted iron in ſeven ounce meaſures and a half of dephlogiſticated air of the ſame purity with that in the laſt experiment, when it was reduced to an ounce meaſure and one third, and of this four fifths was fixed air, and the remainder phlogiſticated. In this caſe I carefully weighed the finery cinder that was formed in the proceſs, and found it to be nine grains; ſo that the iron that had been melted (being about two thirds of this weight) had been about ſix grains. I repeated the experiment with the ſame reſult.

When the dephlogiſticated air is more impure, the quantity of fixed air will always be leſs in proportion. Thus having melted iron in ſeven ounce meaſures of dephlogiſticated air of the ſtandard of 0.65, it was reduced to 1.6 ounce meaſures, and of this only one third of an ounce meaſure was fixed air.

Pruſſian blue is generally ſaid to be a calx of iron ſuperſaturated with phlogiſton, though of late it has been ſaid by ſome, that it has acquired ſomething that is of the nature of an acid. From my experiments upon it with a burning lens in dephlogiſticated air, I ſhould infer that the former hypotheſis is true, except that the ſubſtance contains ſome fixed air, which is no doubt an acid. For much of the dephlogiſticated air diſappears, juſt as in the preceding ſimilar proceſs with iron.

I threw

I threw the focus of the burning lens upon fifty three grains of Prussian blue, in a vessel of dephlogisticated air of the standard of 0.53, till all the colour was discharged. Being then weighed, it was twenty two grains. In this process seven ounce measures and a quarter of fixed air had been produced, and what remained of the air was of the standard of 0.94. Heating the brown powder to which the Prussian blue was reduced in this experiment in inflammable air, it imbibed eight ounce measures and a half of it, and became of a black colour; but it was neither attracted by the magnet, nor was it soluble in oil of vitriol and water, as I had expected it would have been.

Again I heated Prussian blue in dephlogisticated air of the standard of 0.2, without producing any sensible increase of its bulk, when I found three ounce measures of it to be fixed air, and the residuum tolerably pure, for, with two measures of nitrous air, the standard of it was 1.35. The substance had lost eleven grains, the greatest part of which was evidently water.

To determine what quantity of fixed air Prussian blue would yield by mere heat, I put half an ounce of it into an earthen tube, and got from it fifty six ounce measures of air, of which sixteen ounce measures were fixed air, in the proportion of one third in the first portion, and one fourth in the

 last.

laſt. The remainder was inflammable. There remained 140 grains of a black powder, with a very little of it, probably the ſurface, brown.

Comparing theſe experiments, it will appear that the fixed air procured by means of Pruſſian blue and dephlogiſticated air, muſt have been formed by phlogiſton from the Pruſſian blue and the dephlogiſticated air in the veſſel. For if 240 grains of this ſubſtance yield ſixteen ounce meaſures of fixed air, ten grains of it, which is more than was uſed in this experiment, would have yielded 0.6 ounce meaſures. Nor is it poſſible to account for the diſappearing of ſo much dephlogiſticated air, but upon the ſuppoſition of its being employed in forming this fixed air.

In all the experiments with iron it cannot be doubted but that the greater part of the dephlogiſticated air (viz. the water in it) incorporates with the iron, converting it into a *ſcale*, or *finery cinder*, being the very ſame ſubſtance with that which is produced by tranſmitting ſteam over iron when it is red hot; but at the ſame time ſome phlogiſton muſt be expelled from the iron, and unite with the dephlogiſticated air in the veſſel, in order to form the fixed air that is found in it; as in other caſes it unites with *water*, and makes inflammable air.

Perhaps as deciſive a proof as any of the real production of fixed air from phlogiſton and dephlogiſticated

gisticated air, may be drawn from the experiments in which I always found a quantity of it when I burned sulphur in dephlogisticated air. In one of those experiments to which I gave more particular attention, six ounce measures and an half of dephlogisticated air were reduced to about two ounce measures, and one fifth of this was fixed air. Much vitriolic acid air had been produced in this process: For, before I admitted any water to it, the six ounce measures and a half were only reduced to six. When both the vitriolic acid and the fixed air were absorbed by water, the remainder was very pure dephlogisticated air, the standard of it being 0.3.

I had always concluded that no fixed air could be produced by the decomposition of inflammable air, which had been procured by means of the mineral acids, because I had not been able to do it with that which I had got by means of the vitriolic acid; but I learned from Mr. Metherie, that this is peculiar to the vitriolic acid, the remains of which, diffused through the inflammable air procured by it, he conjectures, may decompose the fixed air actually produced in the process. See his Treatise, p. 110. For, as I have hinted before, when the inflammable air is produced from iron, by means of spirit of salt, there is a very perceivable quantity of fixed air, when it is united with dephlogisticated air. When I decompofed thefe two kinds of air in equal quantities, they were reduced to about 0.5 of a meafure, and of this not more than about one fortieth part was fixed air. This experiment, ought, however, to be added to the other proofs of fixed air being produced by the union of the dephlogifticated air and phlogifton.

The laft inftance of the generation of fixed air from phlogifton and dephlogifticated air, which I fhall mention in this fection, is of a much more ftriking nature than any that I have yet recited. Having made what I call *charcoal of copper*, by making vapour of fpirit of wine pafs over copper when it was red hot, I took a piece of it, and, with no very particular view, heated it in different kinds of air. Among others, I did this in common air, and not obferving any increafe or decreafe of the quantity of air, concluded, but too haftily, that no change was made in it. For when I repeated the experiment in dephlogifticated air, the charcoal burned very intenfely; and when a part of it was confumed (which, like common charcoal in the fame procefs, was done without leaving any fenfible refiduum) I found that no heat which I could apply afterwards had any farther effect on what was left of the charcoal. Concluding, therefore, that fome change muft be made in the quality of the air, I examined it, and found about nine

tenths

tenths of it to be the pureſt fixed air, and the reſiduum was ſuch as would have been made by ſeparating the abſolutely pure part of the dephlogiſticated air, and leaving all the impurities in what remained.

Having aſcertained this fact, I repeated the experiment, weighing the piece of charcoal very carefully before and after the proceſs, and then found that, by the loſs of one grain of the charcoal, I reduced four ounce meaſures of dephlogiſticated air till one ninth only remained unabſorbed by water; and again, with the loſs of one grain and an half of the charcoal, I reduced ſix meaſures and an half of dephlogiſticated air till five ounce meaſures and a half were pure fixed air.

In this proceſs there was a diminution of the bulk of the air after the experiment, as might be expected from the change of the air into one of a heavier kind by means of a ſubſtance, or principle, that could not add much to the weight of it; but I did not accurately meaſure this. In one of the experiments 4.3 ounce meaſures of dephlogiſticated air were diminiſhed, I obſerved, about one thirtieth part of the whole. But being in a pretty wide veſſel, ſuch a meaſure cannot be accurate enough for computation. In this caſe, when the fixed air was ſeparated by water, there was a reſiduum of 0.75 of a meaſure of the ſtandard of

1.0; whereas the dephlogiſticated air before the experiment had been of the ſtandard of 0.2.

That dephlogiſticated air actually enters into the compoſition of fixed air in this experiment, is evident from the weight of the fixed air, which far exceeds that of the charcoal, which is diſperſed in the proceſs. For in this laſt experiment the weight of the fixed air produced was 4.95 grains. Conſequently, ſuppoſing the charcoal to be wholly phlogiſton, as it is very nearly ſo, fixed air may be ſaid to conſiſt of 3.45 parts of dephlogiſticated air, and 1.5 phlogiſton. So that the dephlogiſticated air is more than three times the proportion of the phlogiſton in it.

I muſt not conclude this ſection without obſerving that, I never failed to produce fixed air, by heating iron in vitriolic acid air. I repeated the experiment many times, and always had this very remarkable reſult. In this caſe the acidifying principle, which is the chief ingredient in dephlogiſticated air, muſt have been ſupplied by the acid in the air.

In one of the experiments, four ounce meaſures of the vitriolic acid air were reduced to 0.65 of an ounce meaſure; and of this three parts and one half of the whole was fixed air, abſorbed by lime water, and the remainder was ſlightly inflammable. In another experiment I could not perceive any thing inflam-

inflammable in the residuum. It appeared to be only phlogisticated air. But these residuums are always small, so that it is not easy to distinguish weakly inflammable air from that which is phlogisticated.

SECTION VII.

Of the Production of fixed Air by heating Substances containing dephlogisticated Air in inflammable Air.

AS fixed is always produced when iron, or any other substance containing phlogiston is heated in dephlogisticated air; so when precipitate *per se*, or any other substance containing dephlogisticated air is heated in inflammable air, fixed air never fails to be procured.

In ten ounce measures of inflammable air from malleable iron I revived *red precipitate* till there remained only 1.1 ounce measure of air, and of this 0.07 ounce measures was fixed air, being completely absorbed by water. The weight of this air would be 0.063 gr. But, since 960 grains of iron will yield 1054 ounce meafures of inflammable air, the iron employed in procuring all the inflammable air that was ufed in this experiment, viz. 8.9 ounce meafures (without allowing for any that went to the revivification of the mercury) would be 8.1 grains; and fince M. Bergman fuppofes, that 100 grains of iron contains 0.12 grains of plumbago, the quantity of it in this iron would only be 0.01008 gr. which is not quite a fixth part of the weight of the fixed air.

With fome *precipitate per fe*, fent me by M. Berthollet, I revived mercury till eight ounce meafures and a half of inflammable air was reduced to two ounce meafures and a half, and of this 0.04 oz. m. at leaft was fixed air. This is not quite fo much in proportion as in the preceding experiment, but abundantly more than the weight of the plumbago.

In eight ounce meafures of inflammable air I revived *minium* (which I found to have exactly the fame effect in this procefs as red precipitate, or precipitate *per fe*) till it was reduced to 1.2 ounce meafures; and of this 0.028 oz. m. was fixed air, which would exceed the weight of the plumbago more than three times. In reviving lead from mafficot (which I prepared by expelling the pure air from minium) I had no fixed air in the refiduum.

In

In ſeven ounce meaſures of inflammable air from tin by ſpirit of ſalt, I revived red precipitate till it was reduced to 1.1 ounce meaſure; and in this the fixed air was ſomething more than in proportion to that in the laſt experiment.

I do not know that any objection can be made to the inflammable air from *tin*, as this metal has not been proved to contain plumbago. I wiſhed, however, to repeat this experiment with inflammable air from *ſulphur*. But though, when ſteam is ſent over melted ſulphur, a ſmall quantity of inflammable air is procured; yet, as ſulphur cannot part with much phlogiſton, except in proportion as it imbibes pure air, to form oil of vitriol, I could not in this manner eaſily procure enough for my purpoſe.

In order to ſupply the ſulphur with pure air, I mixed with it a quantity of *turbith mineral*; but this made it yield vitriolic acid air, though in great abundance, there not being, I imagine, *water* enough to form inflammable air: for when iron is diſſolved in concentrated acid of vitriol, vitriolic acid air is produced; but in diluted vitriolic acid, the produce is inflammable air. With a view to ſupply theſe materials with water, I ſent ſteam over them; but it did not combine with the air, which was ſtill only vitriolic acid air.

Since,

Since, however, vitriolic acid air unqueſtionably contains the ſame principle which forms the inflammability of inflammable air, this experiment proves that ſulphur is not that ſimple ſubſtance which the antiphlogiſtians ſuppoſe it to be; but that it contains phlogiſton. Had it been nothing more than a ſubſtance which had a ſtrong affinity to pure air, it would have united with the pure air from the turbith mineral, and have made vitriolic acid; but no vitriolic acid air would have been produced.

That vitriolic acid air contains the ſame inflammable principle with inflammable air is evident from the quantity of vitriolic acid air which I produced by reviving copper from blue vitriol in inflammable air. Mr. Kirwan alſo produced this air from ſulphur and red precipitate. See his Treatiſe on Phlogiſton, p. 29.

When I uſed a ſmall quantity of ſulphur in proportion to the turbith mineral, the firſt produce was vitriolic acid air, and afterwards dephlogiſticated air, from the turbith mineral alone, the effect of the ſulphur having been exhauſted.

According to the antiphlogiſtic theory, *phoſphorus*, as well as ſulphur, is a ſimple ſubſtance; and when it is ignited imbibes pure air, and thereby becomes the phoſphoric acid, without parting with any thing. But I find, that after the accenſion of it it in dephlogisticated air, there is a considerable quantity of fixed air in the residuum; and this fixed air could only be formed by the union of the dephlogisticated air in the vessel with the phlogiston contained in the phosphorus. Mr. Kirwan had a similar result from phosphorus confined in atmospheric air. As it is not pretended, that there is any plumbago in phosphorus, this experiment is not liable to the objection that has been made to those in which inflammable air from iron was made use of.

Comparing this experiment with that in which iron is ignited in dephlogisticated air, and those in which nitrous acid is produced by the accension of dephlogisticated and inflammable air, this general conclusion may be drawn, viz. that when either inflammable or dephlogisticated air is extracted from any substance in contact with the other kind of air, so that one of them is made to unite with the other in what may be called its *nascent state*, the result will be *fixed* air; but that if both of them be completely formed before their union, the result will be *nitrous acid.*

It has been said, that the fixed air produced in both these experiments may come from the *plumbago* in the iron from which the inflammable air is obtained. But since we ascertain the quantity of plumbago contained in iron by what remains after its solution in acids, it is in the highest degree improbable, that whatever plumbago there may be in iron, any part of it should enter into the inflammable air procured from it. Besides, according to the antiphlogistic hypothesis, all inflammable air comes from water only.

In the course of these experiments I discovered more completely than before the source of my former mistake, in supposing that fixed air was a necessary part of the produce of red lead, and also of manganese. Both these substances, I find, give of themselves only dephlogisticated air, and that of the purest kind; and all the fixed air they yielded in my former experiments must have come from the gun-barrel I then made use of, which would yield inflammable air, which, with dephlogisticated air, forms fixed air. For though the dephlogisticated air from red lead was so pure that, mixed with two measures of nitrous air, the three measures were reduced to five hundredth parts of a measure, and the substance gave no fixed air at all when it was heated in an earthen tube or retort; yet by mixing iron filings with it, or with manganese, as I had formerly done with red precipitate, I got more or less fixed air at pleasure, and sometime no dephlogisticated air at all.

I cannot conclude these observations without taking notice, how very valuable an instrument in philo-

philoſophy is a good burning lens. This muſt have been perceived in many of my former experiments, but more eſpecially in theſe. By no other means can heat be given to ſubſtances *in vacuo*, or in any other kind of air beſides atmoſpherical; and without ſome method of doing this, no ſuch experiments as theſe can poſſibly be made. I therefore congratulate all the lovers of ſcience on the ſucceſsful attempt of Mr. Parker to execute ſo capital an inſtrument as he has done of this kind. Such ſpirited and generous exertions reflect honour on himſelf, and on our country. It is only to be wiſhed, that we could have lenſes of a ſmaller ſize (viz. from twelve to eighteen inches diameter) made tolerably cheap, ſo that they might be in more common uſe. All the preceding experiments were made with one of twelve inches in diameter.

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SECTION VIII.

Of Air acting through a Bladder.

AS it decisively follows from my experiments on the action of different kinds of air through a bladder, that fixed air consists of dephlogisticated air and phlogiston, I shall introduce them in this place.

One of my former experiments which I was least able to account for, was the diminution of nitrous air in a bladder swimming at liberty in a trough of water; the consequence of which had always been, that in a few days the nitrous air was diminished about one fourth, and this was phlogisticated air. All the progress that I had then made in the investigation of this curious fact, was finding that it depended, as I then thought, upon the bladder being kept alternately dry and moist; because when the bladder was kept covered with water, it remained full, and the air within it was not changed. This was also the case when the bladder was kept dry. But I did not consider that when the bladder was kept under water, there was

no

no *air* in contact with it; and I did not then suspect that this change in the air depended on the action of the nitrous air upon the external common air through a moist bladder; though I had found that coagulated blood has a power of acting upon air, and is of course liable to be acted upon by air, through any bladder.

At length, suspecting that this *might* be the case, I made the following experiment. Taking a bladder which contained twenty ounce measures of nitrous air, and tying it very tight, I introduced it into a glass jar, which contained forty ounce measures of common air; because, in that proportion, they would be able, if they had any mutual action, to saturate one another. Wishing at the same time, to observe the changes that might gradually take place in each of the kinds of air, I examined them both at different periods.

The process was begun on the 18th of May, and on the 21st I found that there were only thirty four ounce measures of the common air, and eleven of the nitrous, the bladder being quite sound; so that it was sufficiently evident, that the two kinds of air, had affected each other through the substance of the bladder. On the 25th of the same month there were thirty one ounce measures and a half of the common air, and four and a half of the nitrous; and examining the state of both of them,

them, I found the ſtandard of the common air to be 1.8, which was a ſtate very near that of extreme phlogiſtication; and that of the nitrous 1.7. That is, equal meaſures of this and of common air, occupied the ſpace of 1.7 meaſures, which ſhews that it had almoſt loſt its power of affecting common air, or to expreſs myſelf perhaps more correctly, there was but a ſmall proportion of nitrous air in it.

On the 8th of June I examined them for the laſt time, after having obſerved no farther change for ſome days in the quantity of the common air (as indicated by marks which I had made on the outſide of the jar) and I found only twenty eight ounce meaſures of the common air, of the ſame quality as when I had examined it before, viz. of the ſtandard of 1.8, and only three ounce meaſures of the nitrous air, and it did not affect common air at all. Neither of them contained any portion of fixed air, and both of them extinguiſhed a candle.

Nothing now remained to my complete ſatisfaction, with reſpect to my former obſervation of the diminution of nitrous air, contained in a bladder. But I farther wiſhed to ſatisfy myſelf with reſpect to the action of *inflammable air*, on either common or dephlogiſticated air, in the ſame circumſtances. Nitrous air affects pure air by ſimple contact,

contact, without ignition; whereas, inflammable air, I had observed, has very little effect upon pure air when they are simply mixed together. I was, therefore, surprized to find that inflammable air has a very considerable action upon dephlogisticated air through a bladder, without any assistance from heat; and moreover, that the union of these two kinds of air, thus produced, forms fixed air. The experiments which I made for this purpose, were as follows.

Into a jar containing 123 ounce measures of dephlogisticated air, I introduced a bladder, containing twenty three ounce measures of inflammable air; and after a few days, I observed that the bladder in which it was contained was become a little flaccid. After about three weeks, I examined both the kinds of air, and found that the bladder contained only two ounce measures, and that this was no longer inflammable, but extinguished a candle, though it had in it a mixture of pure air. The air within the jar then contained one twentieth of its bulk of fixed air. The dephlogisticated air was diminished seven ounce measures; and from being of the standard of 0.5, with two equal measures of nitrous air, it was now become of 1.4. The bladder had a slight smell of putrefaction, but it was perfectly air tight.

It is obſervable, that in this experiment part of the dephlogiſticated air had paſſed unchanged into the bladder of inflammable air, whereas the inflammable air which had paſſed through the bladder into the dephlogiſticated air, had united to it, and formed fixed air. The tranſmiſſion of the dephlogiſticated air through the bladder was much more remarkable in the following experiment.

Having introduced a bladder filled with inflammable air into a large jar of dephlogiſticated air, the bladder, after two days only, had in it a great mixture of dephlogiſticated air, and was as much diſtended as when it was firſt put into the jar. A quantity of it exploded exactly like a mixture of one third dephlogiſticated, and two thirds inflammable air. The bladder was perfectly ſound and ſweet, and the dephlogiſticated air was not ſenſibly altered.

Again, having introduced a bladder containing ten ounce meaſures of inflammable air into a jar containing one hundred ounce meaſures of dephlogiſticated air, of the ſtandard of 0.3, I found, about a month afterwards, that the air in the jar was diminiſhed to ninety ounce meaſures, and the inflammable air to five ounce meaſures and an half. The quality of the air in the bladder and of that in the jar was very nearly the ſame, though the bladder was perfectly ſound and ſweet. The air in the

the bladder, with equal meaſures of nitrous air, was of the ſtandard of 0.76, and that in the jar of 0.74. Both of them alſo contained a ſmall portion of fixed air. In this caſe, therefore, both the kinds of air had not only been tranſmitted through the bladder, but ſome decompoſition had alſo taken place within it, as well as within the jar.

In another experiment of this kind, both the bladder of inflammable air, and the jar of dephlogiſticated air, after ſome time, contained each of them a portion of fixed air, and likewiſe both the kinds of air unaffected by each other. For both of them exploded when they were examined ſeparately.

It ſeems to follow from theſe experiments, that fixed air is really *formed* when inflammable air of charcoal, &c. is exploded together with dephlogiſticated air; and alſo that the greatneſs of the heat prevents its formation, when inflammable air from metals is uſed. For though, in the exploſions with the electric ſpark, no fixed air was produced from the decompoſition of the pureſt inflammable air, it was evidently ſo with the ſame kind of inflammable air in theſe experiments with a bladder in which no heat is uſed.

The formation of fixed air from phlogiſton and dephlogiſticated air, is more evident from the great quantity of it which is found when an animal ſubſtance putrefies in dephlogiſticated air, compared with the ſmall quantity that is procured by its putrefying in inflammable air.

After the preceding experiments on the conſequence of having one kind of air in the bladder, and the other in the jar in which it was confined, I filled the bladder with the ſame air that was in the jar, and let them remain till they became putrid and burſt. The jar and the bladder of dephlogiſticated air contained together one hundred ounce meaſures of the ſtandard of .95, but after the proceſs and waſhing the air in water there were only 37.5 ounce meaſures, which was phlogiſticated.

At another time ninety ounce meaſures of dephlogiſticated air of the ſtandard of 0.16, were reduced to forty ſeven ounce meaſures of the ſtandard of 0.6; whereas a jar of inflammable air of the ſame ſize, and treated in the ſame manner, contained, after the proceſs, not more than one thirtieth of its bulk of fixed air. In this it was obſervable, that the bladder and the air were moſt abominably offenſive, whereas the bladder which had been in dephlogiſticated air was hardly offenſive at all.

It will appear by computation, that in both theſe caſes of the formation of fixed air, by the bladders putrefying in dephlogiſticated air, phlogiſticated gifticated air was produced, *fix* ounce meafures being generated in the former cafe, and *five* in the latter; and though all fixed air contains a part not abforbed by water, and this is always more or lefs phlogifticated, this was much more than in that proportion, the phlogifticated air being in the former cafe one fixth of the whole, and in the latter nearly one half. For in the former cafe the phlogifticated air before the procefs was 31.7 ounce meafures, and after it 37, and in the latter it was 4.86 ounce meafures before, and 9.4 after.

BOOK II.

EXPERIMENTS AND OBSERVATIONS RELATING TO INFLAMMABLE AIR.

PART I.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PRODUCTION OF INFLAMMABLE AIR,

SECTION I.

Of inflammable Air from Metals, by Means of Acids, &c.

THE metals from which this ſpecies of air has been procured are *iron*, *zinc*, and *tin*. I found it in *copper*, and *lead* by ſpirit of ſalt, as may be ſeen in the account of the diſcovery of marine and air. I have alſo procured it in various other ways; and have lately found that *regulus of antimony* diſſolved in marine acid, with the application

cation of heat, yielded a ſmall quantity of air, which was weakly inflammable. *Biſmuth* and *nickel* were diſſolved in marine acid with the help of a conſiderable degree of heat, but little or no air was got from either of them. If there was any more than the common air which had lodged within the phial containing the mixture, I could not perceive that it was inflammable: but theſe metals treated in this manner yielded a ſtrong ſmell of *liver of ſulphur*.

It is ſomething remarkable, that all the acids that produce any air by the ſolution of metals give inflammable air, except ſpirit of nitre only, which forms a different kind of union with the inflammable principle; making *nitrous air*, more or leſs modified. Beſides oil of vitriol and ſpirit of ſalt, I have obſerved that the *vegetable acid* alſo produces inflammable air, by the ſolution of metals, though in a much leſs quantity. Perhaps the proportion of the ſtrength of the acids may be aſcertained by this means. The concentrated vinegar which I made uſe of in my experiments on the vegetable acid air, diſſolved zinc almoſt as rapidly as ſpirit of ſalt, and produced inflammable air; and *radical vinegar*, which is unqueſtionably a pure vegetable acid, had the ſame effect when applied both to zinc and iron.

In order to measure the strength of this acid, I put as much radical vinegar as occupied the space of fifty two grains of water upon a quantity of filings of zinc diluted with water, and found that it yielded one fourth of an ounce measure of inflammable air, without heat; and two ounce measures more with heat; and a little more might have been procured, if care had been taken that no part of the liquor had boiled over. What proportion this produce of inflammable air bears to a similar produce from spirit of salt may be found by comparing this observation with some that are mentioned relating to marine acid air.

In my first experiments on fixed air, I found that, when a mixture of iron filings and brimstone, moistened with water, was made to ferment in it, a part of it was made immiscible with water, that is, that there was in it a greater residuum of phlogisticated air than usual, which I supposed to come from the phlogiston set loose in this process; though I could not find that phlogiston in any other process produced that effect. At that time it could not but occur to me, that, possibly, this mixture itself might generate air, in which case the fact I have been reciting would not prove that there had been any alteration in the constitution of the fixed air; since there would have been a real *addition* to it, of another kind of air from the mixture. To

try

try this, I then made this mixture to ferment under water, and found that no air whatever was produced from it.

I have ſince tried the ſame thing in the beſt *vacuum* that I could make with Mr. Smeaton's air pump; when, though the fermentation went on as uſual, yet when water was admitted to it afterwards, no air was found in the receiver. I alſo made this fermentation when the materials were buried in quickſilver, and in theſe circumſtances alſo no air was produced in the temperature of the atmoſphere.

I mention theſe circumſtances, becauſe I have found that when this fermentation is made in quickſilver, and *in a warm place*, a true inflammable air is generated. The experiment was made in as accurate a manner as I could contrive, and in the courſe of it, it will be ſeen that probably a quantity of vitriolic acid air was alſo generated, and abſorbed again by the water that was mixed with the iron and brimſtone, and which is neceſſary to enable them to act upon each other.

Having filled a ſmall phial with a mixture of iron filings and brimſtone moiſtened with water, I plunged it in a veſſel filled with quickſilver, ſtanding inverted in a baſon of the ſame, and placed the whole apparatus near the fire. In about half an hour the fermentation began, and ſo much air

iſſued

iſſued from the mixture, as occupied the ſpace of four times the bulk of the materials. In a few minutes the quantity of air diminiſhed, being probably vitriolic acid air, and having been abſorbed by the water; but there remained about one fourth of the bulk of the mixture that was permanent air, not imbibed by water; and this was inflammable.

Since zinc, as well as iron, yields inflammable air with oil of vitriol, I ſuſpected that poſſibly it might be affected as iron is by the oil of vitriol ſet looſe from the ſulphur in this proceſs, and I found that when I ſubſtituted filings of zinc for the filings of iron, in the circumſtances abovementioned, they anſwered equally well. In this experiment a quantity of air was produced equal to the bulk of the materials, all ſtrongly inflammable.

Having once put a pot of iron filings and brimſtone into a jar of nitrous air (the firſt effect of which is to reduce it to one fourth of its bulk, and leave it in the ſtate of phlogiſticated air) and having ſome time after this found the air much increaſed in quantity, and ſtrongly inflammable, I had ſome doubt whether the inflammable matter came from ſome farther change in the nitrous air, or from an exhalation of proper inflammable air from the iron and brimſtone. My doubt aroſe from

from my never having found that this paſte of iron filings and brimſtone, whether kept in water, or in vacuo, had yielded air at any time, except in a conſiderable degree of heat. In conſequence, however, of repeated experiments, I am now ſatisfied, that the inflammable air came from this mixture. For though ſome pots of it have not yielded inflammable air, they have all, with *long keeping*, even in the temperature of the atmoſphere, yielded either phlogiſticated or inflammable air; the latter generally when the compoſition was freſh made, and the former when it was old.

Theſe experiments have alſo led me to the obſervation, that, in this and many other caſes of the diminution of common air by phlogiſtic proceſſes, a true inflammable air is firſt produced, and in its *naſcent ſtate*, as it may be called, is immediately decompoſed, previous to the phlogiſtication of the common air. The very ſame ſubſtances which, in water or quickſilver, yield inflammable air, only phlogiſticate common air: ſo that I am almoſt ready to conclude univerſally, that air is never phlogiſticated, but by materials which, in certain circumſtances, would yield inflammable air; though when inflammable air is previouſly produced, and then mixed with common air, it will not be decompoſed in the temperature of the atmoſphere, except in a very ſmall degree. Theſe two kinds

of

of air will, therefore, continue mixed without much affecting each other, except in a red heat, by which the inflammable air is fired. It is then well known to ceafe to be inflammable air, the phlogifton being feparated from it, and uniting with the dephlogifticated air in the common air; when nothing is left but the phlogifticated part of the common air, which is about three fourths of the whole. I have fince obferved that *nitrous acid* is formed in thefe circumftances.

The experiments which led to thefe conclufions, and which I fhall now proceed to recite, may ferve as a caution to myfelf and others, not to be too hafty in drawing general conclufions; fince what may appear to be the *fame materials*, and the *fame preparation* of them, may have different refults, in confequence of there having been fome circumftance, refpecting either the materials or the procefs, that was unnoticed, but which was the fecret caufe of the unexpected refults.

That nitrous air might be changed into inflammable air, was not extremely improbable *a priori*; fince I had found that it contained nearly as much phlogifton as inflammable air, bulk for bulk; and fince it is, by feveral proceffes, convertible into what has the appearance of a fpecies of inflammable air. Befides, in this very cafe, the fame compofition of iron filings and brimftone, which I now find

find generally yields inflammable air in the temperature of the atmosphere, does not do so at all times.

Thinking that if the iron filings and brimstone had really yielded the inflammable air which I found in the vessel of nitrous air, it would do the same in common air, I confined a large pot of this mixture in a very small quantity of common air in the beginning of February, 1779. But though on the 19th of May following it was increased in bulk, it was all mere phlogisticated air, and had nothing inflammable in it. Even the air that was entangled within the cavities of this pot of iron filings and brimstone, and which I catched by breaking it under water, was not inflammable. It is possible, however, as I observed before, that this phlogisticated air might have been inflammable air in its origin, or *nascent state*, and have become phlogisticated air afterwards. At another time I put a pot of this mixture under water, as I had done formerly, and now also observed, that though it fermented very well, and turned black, yet it did not yield a particle of air in about a fortnight: and in experiments of this kind few persons, I believe, would look for any farther change beyond that time.

Soon after, however, I found that a pot of this mixture, fresh made, and kept under water three weeks, had yielded about its bulk of air; and this

this was ſtrongly inflammable. But at the ſame time another mixture of this kind, kept in the ſame circumſtances, yielded only phlogiſticated air; and yet I did not knowingly make any difference in the compoſition, always mixing equal bulks of the two ingredients.

As the phlogiſton which conſtituted the inflammable air in the experiments that occaſioned theſe muſt probably have come from the iron, and not from the ſulphur; eſpecially ſince iron alone is capable of making a very remarkable change in nitrous air, I confined a quantity of this air, in a veſſel full of iron nails, from the beginning of February to the 18th of May; but after this long interval it was only phlogiſticated air, and not in the leaſt inflammable.

Having found, however, that this mixture of iron filings and brimſtone was capable of producing inflammable air in water, I made a trial of it in quickſilver, and found it to have the ſame effect. For confining a quantity of this mixture in quickſilver from the 13th to the 30th of June, in the temperature of the atmoſphere, it had yielded, in this time, its own bulk of air, ſtrongly inflammable.

I found afterwards, in a proper number of trials, that in a ſufficient ſpace of time, this mixture increaſed all the kinds of air into which I introduced

duced it, by the addition of a quantity of inflammable air, more or less, according to circumstances, known or unknown. But when the experiment was made in common air, it first diminished it about one fourth, as I have often noted; and some time after that I perceived an addition made to the bulk of the air, and examining it, found it at first to be slightly inflammable, but afterwards more strongly so. This experiment shews that, in the first instance, the inflammable air yielded by iron filings and brimstone must have been decomposed by uniting with the dephlogisticated air in the common air.

It appeared upon one occasion, recited above, that one pot of this mixture, *fresh made*, produced inflammable air, at the same time that a pot of an *old* mixture of this kind yielded only phlogisticated air. But at what time these mixtures will cease to give inflammable air, and begin to yield phlogisticated air, I cannot determine. For I find that on the 23d of June a pot of iron filings and brimstone, which must have been mixed about a year before, confined in a small quantity of common air, had made an addition to it of three ounce measures on the 26th of July; and this air was inflammable. At the same time I found that another quantity, which had been mixed the 1st of July, had yielded inflammable air, in about the ſame proportion, according to the time. Alſo ſome old iron filings and brimſtone, which had been taken out of the pot, and mixed with water the 3d of July, had yielded about one tenth of its bulk of air on the 2d of Auguſt, ſtrongly inflammable.

That future experimenters may form ſome idea of the quantity of inflammable air that they may generally expect from ſuch mixtures as I have uſually made of iron filings and brimſtone, uſing equal bulks of each, and therefore be leſs apt to deceive themſelves in the reſults, I ſhall recite the iſſue of ſome that I made with this and other mixtures, and which I was obliged to put an end to when I removed my habitation on the 21ſt of July, 1780.

A gallipot, containing an ounce meaſure and half of this mixture, having been confined, in a ſmall quantity of common air in the beginning of July, 1779, had at the time above-mentioned produced fourteen ounce meaſures of air, ſtrongly inflammable; but the production was much more rapid at the firſt than afterwards. The mixture was very hard.

Another gallipot of the ſame ſize, put into a veſſel of water, without any air, on the 23d of June, 1779, had three ounce meaſures of inflammable air taken from it on the 26th of July following,

lowing, and at this time there were eleven ounce meaſures, ſtrongly inflammable. The mixture was very ſoft.

Another equal quantity had yielded ſtrong inflammable air from the 24th of June to the 15th of July, 1779, and had from that time yielded about three ounce meaſures of air, but ſlightly inflammable. The mixture was very ſoft.

There is the ſame uncertainty attending experiments made with *liver of ſulphur*, which alſo exhales phlogiſton, and produces the ſame effect both on common air and nitrous air, as iron filings and brimſtone. On the 19th of May, 1779, I found a quantity of nitrous air, in which ſome liver of ſulphur had been confined from the 12th of December preceding, and which was conſiderably increaſed in bulk, to be ſtrongly inflammable; and yet another quantity of this ſubſtance, and freſh made, was confined in quickſilver ſeveral months without producing any air at all.

I have procured inflammable air, in a conſiderable quantity, by diſſolving iron filings in a ſolution of galls; and very probably the ſame would be produced by means of any other aſtringent ſubſtance. Indeed moſt things that really *decompoſe* the metal, and do not unite with the *whole maſs* of it, will, I imagine, ſet looſe the phlogiſton it contains, in the form of inflammable air; though, in ſeveral of the caſes, the phlogiſton might join ſome of the principles in the menſtruum, and contribute to compoſe a different ſubſtance.

I was led to this obſervation of the production of inflammable air by the ſolution of galls, in conſequence of being informed by Mr. Delaval, that ink might be made by putting iron to the ſolution of galls; for that the acid in the vitriol, which is commonly uſed for the purpoſe of making ink, is an unneceſſary, and frequently an inconvenient ingredient.

Having mixed a quantity of pounded galls, iron filings, and water, I firſt obſerved, that, after a day or two, the whole maſs was very much ſwelled, and that it was full of bubbles of air, which at the ſurface were very large. Suſpecting, from the ſmell, and other circumſtances, that the air contained in them was inflammable, I burſt ſeveral of them near the flame of a candle, and found that they all made ſmall exploſions, ſo that I could have no doubt concerning the quality of the air.

I then mixed three ounces of pounded galls with water and iron filings, the quantity of which I did not note; and covering them with a large jar full of water, found that, in about a week, they had produced ſix ounce meaſures of air, which was ſtrongly inflammable, exactly like that which is produced from iron by the acids. In the ſame

3 manner

manner I procured a quantity of this inflammable air by putting the above-mentioned mixture into a phial with a ground ſtopper and tube. But this proceſs is too ſlow for any uſe.

SECTION II.

Of inflammable Air from Oil.

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

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

It being evident that the *ether* occasioned the difference that was observable in these two cases, I next proceeded to take the electric spark in a quantity of ether only, without any air whatever; and observed that every spark produced a small bubble; and though, while the sparks were taken in the ether itself, the generation of air was slow, yet

yet when ſo much air was collected, that the ſparks were obliged to paſs through it, in order to come to the ether and the quickſilver on which it reſted, the increaſe was exceedingly rapid; ſo that, making the experiment in ſmall tubes, as fig. 16, Pl. I. the quickſilver ſoon receded beyond the ſtriking diſtance. This air, by paſſing through water, was diminiſhed to about one third, and was inflammable.

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

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

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

By the ſame proceſs I got inflammable air from the *volatile ſpirit of ſal ammoniac*; and as I have obſerved before, the alkaline air which is expelled from the ſpirit of ſal ammoniac is inflammable.

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

In all theſe caſes it is probable that the electric ſpark only gave the ſubſtances the degree of *heat* that was neceſſary to give the phlogiſton, and the water they contained the form of permanent inflammable air; for this was done much more effectually by a direct application of heat in the experiments recited in the next ſection.

Inflammable air will ſometimes iſſue ſpontaneouſly from oil of turpentine. I once opened a pint phial, half filled with this kind of oil, and the cork being very tight, there ruſhed out of it a great quantity of air; when applying the flame of a candle to the mouth of the phial, I found the remainder to be ſtrongly inflammable. The oil was then quite full of air bubbles, and by the heat of boiling

boiling water I expelled from a quantity of it an equal bulk of air, all ſtrongly inflammable, like that which is obtained from metals. It was eight or ten hours in giving this air. When I could perceive the colour of the flame, I found it to be blue.

I then took a quantity of the ſame kind of oil, which had been kept in another phial, but I found the air incumbent upon it, within the phial, to be only common air; but making it boil in a retort, I expelled from it twice its bulk of air, all ſtrongly inflammable. I could not diſtinguiſh the colour of its flame.

When I had thus expelled all the air which a quantity of this oil of turpentine ſeemed to contain, I agitated it very ſtrongly, and frequently, in the courſe of two days, in order to make it imbibe more air, that I might expel it again; but I did not find that it had imbibed more than a very ſmall quantity, and this, when it came out again, was only common air ſlightly phlogiſticated. The firſt boiling had made it brown, and very viſcid.

SECTION III.

Of the Production of inflammable Air from different Substances, by means of Heat and Water.

IT is probable, that every fubftance which contains *phlogifton* may be made to yield inflammable air. But for this purpofe they require different modes of treatment, according to their refpective natures. If the fubftances be *fluid*, heat applied to them directly makes no change in their conftitution; but when they are made to pafs, in the form of *vapour*, through tubes previoufly made red hot, in which they are neceffarily expofed to a red heat themfelves, they are readily decompofed; and the quantity of inflammable air that was yielded by fome of them, in this mode of treatment, appeared to me rather extraordinary.

I began thefe experiments with *fpirit of wine*, having an apparatus proper to receive any *water*, or other fluid, that might be formed, or condenfed, in the procefs, Pl. VII. fig. 2. From two ounce meafures of fpirit of wine, which was made to pafs in vapour, through a red hot earthen tube, I got about 1900 ounce meafures of air, which was all inflammable, without

without any mixture of fixed air in it, and which burned with a lambent blue flame. Thirty ounce meaſures of this air weighed eight grains leſs than an equal quantity of common air. In this proceſs I collected 0.35 of an ounce meaſure of water.

In this experiment the air would have weighed - - - 633 grains
the watery reſiduum 168

801

and the ſpirit of wine would have weighed 821; ſo that the produce was pretty nearly what might have been expected from the materials, the nature of the proceſs conſidered.

I then proceeded to ſubject to the ſame proceſs a quantity of *vitriolic ether*; and making an ounce meaſure of it paſs through the hot earthen tube, almoſt filled with pieces of broken retorts, or crucibles (in order to make a greater quantity of red hot ſurface) I collected one tenth of an ounce meaſure of water, and 740 ounce meaſures of air, all inflammable, without any mixture of fixed air. It burned with a large lambent white flame, like that of wood in a common fire, and would not explode with any mixture of dephlogiſticated air. Twenty-nine ounce meaſures of this air weighed five grains leſs than an equal bulk of common air.

In the next place, I made ſome vapour of *ſpirit of turpentine* paſs through the hot earthen tube, and procured

procured from it a quantity of inflammable air, that was very turbid, like black ſmoke. But the black matter contained in it was ſoon depoſited on the ſurface of the water in which it was received. This alſo contained no fixed air, and burned with a lambent flame, but much leſs luminous than that in the preceding experiment. The ſmell of this air was ſo exceedingly offenſive, that, the apparatus being a little deranged, I diſcontinued the proceſs before I had aſcertained the quantity of air, and without collecting any water, which I ſuppoſe would have been given. Thirty ounce meaſures of this air weighed eight grains leſs than an equal quantity of common air.

I did not repeat this experiment with *olive oil*, being apprehenſive that the proceſs would be even more offenſive than that with the ſpirit of turpentine, and nothing material depending upon it. But, upon another occaſion, I mixed an ounce of olive oil with 874 grains of *calcined whiting*; and ſubjecting it to a red heat in an earthen retort, I got from it near 300 ounce meaſures of air, and ſhould probably have got much more, if there had been more whiting in proportion to the oil. The firſt portion of this air burned with a large white flame; and the laſt with a ſlight lambent blue one, exactly reſembling the varieties in the proceſs for extracting air from wood; ſo that there can be no doubt,

doubt, but that it is the *oil* in the wood that gives the air. That excellent philoſopher, Mr. Volta, was the firſt who hit upon this method, or a ſimilar one, of getting inflammable air from *oil*; and he has given a large account of its peculiar properties.

From other experiments that I made, it appears, that *water* is eſſential to the formation of inflammable air. In all the liquid ſubſtances mentioned above, the water that enters into their compoſition is ſufficient for the purpoſe; and ſpirit of wine, and ether, appear to contain more water than is neceſſary. But when the ſubſtances are *dry*, and water does not enter as a neceſſary ingredient into their compoſition, water muſt be introduced into the proceſs. This is the caſe with all the *metals*, and it is no leſs ſo with *ſulphur*, *arſenic*, and probably other ſubſtances of a ſimilar nature, which mere heat only ſublimes.

Tranſmitting ſteam over a quantity of *ſulphur*, which was melting in a hot earthen tube, I procured from it a quantity of inflammable air, without any fixed air; and by analyſis it appeared to be of the ſame quality with that which is procured from iron by oil of vitriol. This proceſs is rather troubleſome, on account of the ſulphur ſubliming, and filling up the tubes through which the air is conveyed.

I then

I then repeated the ſame proceſs with *arſenic*, and from this ſubſtance I procured air in great plenty. One ſeventh of it was fixed air, but the reſt ſtrongly inflammable, and the ſmell of it could not be diſtinguiſhed from that of phoſphorus. Twenty ounce meaſures of this air weighed four grains and a half leſs than an equal quantity of common air. This experiment was no leſs troubleſome than the preceding, on account of the arſenic ſubliming, and choking up the tubes.

Having found a very heavy kind of inflammable air by heating the *ſcales of iron* mixed with *charcoal*, I made the following experiment in order to aſcertain the quantity of air that might be procured from a given quantity of theſe materials. Mixing two ounces of the *ſcales*, or *finery cinder* (which I found to be the ſame thing) with one ounce of perfect charcoal, I got from it, in an earthen retort, 580 ounce meaſures of air, one tenth of the firſt part of which was fixed air; but afterwards it was all inflammable. The ſubſtances were pretty firmly concreted together, and weighed 1044 grains; ſo that the loſs of weight was 396 grains, which muſt have been very nearly the weight of the air procured. Forty ounce meaſures of this air, freed from all fixed air, weighed two grains more than an equal quantity of common air.

Beſides

Besides the *water*, which seems to be essential to the constitution of inflammable air, this species of air readily imbibes more water, which adds greatly to its specific gravity. This seems at least to be indicated by the following experiment. Filling a dry bladder with inflammable air, received immediately from the vessel containing the iron and diluted oil of vitriol, from which it was generated, I found that thirty ounce measures of it weighed more than seventeen grains less than an equal bulk of common air; but when I weighed that inflammable air of the same kind which had been confined by water in the same bladder, it was only fourteen grains less than an equal quantity of common air. This I repeated several times with the same result. This air, therefore, could only be about three times lighter than common air; whereas the other was more than ten times lighter.

Having frequently examined the specific gravity of inflammable air which has been long confined by water, by weighing it in a bladder, and then pressing out the air, and weighing it when empty (which has the same effect as weighing it full of common air) I have seldom found such air more than five times lighter than an equal portion of common air; so that the specific gravity of this air is soon doubled by being kept in these circumstances.

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SECTION IV.

Of Air produced by Substances putrefying in Water.

THE experiments recited in this and the following section were entered upon chiefly to discover the *principle of nutrition* in vegetable and animal substances; and they seem to lead us to suppose, that this principle is phlogiston, or the principle of inflammability, in such a state as to be capable of becoming, by putrefaction, a true inflammable air, but not generally such as to burn with explosions, but rather with a blue and lambent flame, mixed with a certain proportion of fixed air.

In the putrefactive process the phlogiston is merely evolved, and not again combined with any thing, except what may be necessary to its assuming the form of inflammable air; but in nutrition it is immediately held in solution by the gastric juice, and in the chyle formed by it. But if any part of the aliment pass the stomach, and the first intestines, without having all its phlogiston incorporated with the chyle, that principle remains in the excrement, where it is often set loose in in the form of inflammable air, the ſame form that it would have taken if it had gone through the ſimple putrefactive proceſs. The phlogiſton of the aliment, thus entering into the circulation with the chyle, after anſwering purpoſes in the animal œconomy which are yet very imperfectly known to us, is thrown out again by means of the blood in the lungs, and communicated to the air, which is phlogiſticated by it.

All alimentary ſubſtances not only contain phlogiſton, but I believe are capable of yielding a proper inflammable air by putrefaction. But in the following experiments on ſuch vegetables as are generally uſed for food, *roots* ſeem to yield it in a greater abundance than other parts of plants; but there are ſome remarkable differences among them in this reſpect. For though potatoes are exceedingly favourable to the growth of that green vegetable ſubſtance, which yields pure air ſo copiouſly, owing probably to the phlogiſton they contain, *onions*, perhaps equally nutritive with potatoes, are exceedingly unfriendly to that plant; but then they yield inflammable air in an aſtoniſhing quantity, when they are left to putrefy in water. This I rather ſuſpect is a proof, that onions contain more phlogiſton, and are the more nutritive ſubſtance, of the two.

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On the 28th of June I expoſed to the ſun eighteen penny-weights of onions, in a jar of an hundred ounces of river water, inverted in a baſon of the ſame. They preſently began to yield air, but without ever becoming green; and on the 15th of July the quantity was fifteen ounce meaſures, a ſmall part of which was fixed air, and the reſt ſtrongly inflammable. The water was white and turbid, and the air had a ſtrong ſmell of onions.

About the ſame time I obſerved that it made no difference, with reſpect to the quality of this air, whether the onions were placed in the light or in the dark, the principle of vegetation not being concerned in this caſe. And though I obſerved the following differences in the quantities of air produced in the ſun and in the ſhade, they were not uniform, and therefore muſt have depended upon ſome unknown accidental circumſtances.

On the 17th of July I put two onions, each weighing an ounce and a quarter, in the ſun, and two others of the ſame ſize, in a ſimilar jar in the dark. On the 23d I examined them, and had twenty four ounce meaſures of air in the ſhade, and only twelve from thoſe in the ſun; but the latter was more ſtrongly inflammable than the former, which burned with more of a lambent flame, though both exploded in ſome meaſure, ſo as as to be ſomething more inflammable than air from marſhes.

Having kept a quantity of this air, from the time above-mentioned to the 20th of July, 1780, I found it then ſtrongly inflammable, little inferior to the inflammable air from metals. Perhaps the fixed air, which had been mixed with it before, was now completely expelled from it. It appears, however, that this kind of inflammable air has an inflammability of as permanent a nature as any whatever. The air from marſhes alſo, which, with Sig. Volta, I doubt not comes from putrefying vegetable ſubſtances, I have alſo found to be equally permanent.

On the 1ſt of Auguſt I took two halves of the ſame onion (which was an old one, and beginning to ſprout) each half weighing ſeventeen pennyweights twelve grains, and I placed one of them in the ſun, and the other in the ſhade, both in ſimilar receivers. On the 24th of the ſame month, that in the ſun had given an ounce meaſure and three quarters of air, of which one fifth was fixed air, and the reſt inflammable. From that in the dark I took two ounce meaſures and a quarter of air, one third of which was fixed, and the reſt inflammable. From theſe experiments I was ready to conclude, that onions (and therefore, probably, other vegetable ſubſtances) would always give more air in the dark than in the light; but the following experiments ſhewed that this is by no means the caſe always.

The 30th of July I placed in the ſun, in a veſſel containing fifty ounces of water, a part of a freſh gathered onion, weighing nine penny-weights, and alſo another part of the ſame onion, and of the ſame weight, in a veſſel of the ſame ſize in the dark. On the 24th of Auguſt that in the ſun had yielded three ounce meaſures of air, all inflammable, and that in the dark had produced as nearly as poſſible the ſame quantity, and as inflammable, when the fixed air that was mixed with it was waſhed out of it. The fixed air which had been extricated in the ſun had been diſſipated by means of the free acceſs of freſh air.

Upon a former occaſion I got only fixed air from onions confined by quickſilver; but then they wanted moiſture, or were not kept till they were properly putrid. For I have ſince got inflammable air, as well as fixed air, from onions kept in quickſilver, from the 2d of September, 1779, to the 31ſt of March, 1780. The onions weighed twelve penny-weights twenty grains, and the air was half an ounce meaſure, three fourths of which was fixed air, and the reſt inflammable. It appears from this, as well as from many other obſervations which I ſhall have occaſion to mention hereafter, that neither

neither fixed air, inflammable air, or nitrous air, can be produced without a considerable quantity of water, part of which we may therefore, with great probability, infer enters into the composition of these kinds of air; though when they *are* formed, we may not know any method of discovering, and re-producing that water.

Both *carrots* and *parsnips* yield great quantities of inflammable air, and equally in the sun or in the shade. I was at one time much amused with observing the inflammable air issuing from one of the carrots in the sun. It came sometimes in a constant stream, or in large successive bubbles, from one particular place, neither at the centre, nor near the outside of the carrot, but in the place where the air holes are the largest.

To ascertain the quantity of air produced from a given weight of these two roots, I placed as much of a parsnip as, by expelling water from a cylindrical vessel, I found to occupy the space of two ounce measures and a quarter of water, in the sun; and the next day I took from it four ounce measures of air, all fixed air, the residuum extinguishing a candle. This was on the 29th of July, and on the 31st of the same month I took from it four ounce measures more, of which two thirds of an ounce measure was inflammable. On the 2d of August I again took from it four ounce measures, one fourth of which was inflammable, exploding with a blue flame. Laſtly, on the 24th of Auguſt, perceiving that no more air would be produced, I took from it one third of an ounce meaſure; one third of which was fixed air, and the reſt not inflammable, but phlogiſticated.

From carrots occupying the ſpace of an ounce meaſure and a half of water, expoſed to the ſun in rain water, from the 26th to the 31ſt of July, I took ten ounce meaſures of air, of which an ounce meaſure and half was ſtrongly inflammable, exploding with a red flame; and on the 4th of Auguſt I took from them near four ounce meaſures of air, of which more than one half was inflammable. The water, which had a large ſurface, had probably abſorbed much of the fixed air. This, however, was all the air that theſe carrots would yield.

An equal weight of carrots, expoſed the ſame time in the dark, yielded nearly the ſame quantity of air, but only a ſmall proportion of it was inflammable. This, however, I do not attribute to the darkneſs, but to ſome other unknown circumſtance.

A ſliced *turnip* freſh gathered, weighing near three ounces, expoſed in the ſun in rain water, yielded twelve ounce meaſures of air, one third of which was fixed air, and the reſt ſtrongly inflammable.

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On the 30th of July two ounces of turnip, fresh gathered, were placed in the dark, in a vessel containing seventy ounce measures of water; and on the 24th of August I took from it an ounce measure and a quarter of air, of which one ounce measure was phlogisticated, not inflammable. The water was exceedingly offensive. This phlogisticated air had been, I doubt not, inflammable in its origin, and in much greater quantity. When a turnip was sliced very thin, and the quantity of water large, I shall observe, that dephlogisticated air is produced.

Fruits, I found by no means favourable to the production of pure air. Like the preceding roots, they putrefied, and yielded inflammable air, mixed with fixed air. From *peaches*, both in the sun and in the shade, I got air, three fourths of which was fixed air, and the rest inflammable; but on this occasion the quantity of air produced in the sun was twice as much as that produced in the shade; though the quantity of water in which they were exposed was the same, and the peaches themselves were, as far as I could perceive, of the same size, and in the same state.

I placed two Morella cherries, one in the sun, and the other in the shade, in equal vessels of water. From that in the sun I got one third of an ounce meaſure of air, and from that in the ſhade one fifth of an ounce meaſure, both inflammable. I had the ſame reſult with apricots.

Having found the capacity of theſe nutritive ſubſtances to yield inflammable air, I next tried whether they would part with any of it in *boiling*. But I found that none of them did, but only in *putrefying* afterwards; ſo that this mode of preparation (and the ſame I doubt not would be found to be the caſe with roaſting, &c.) does not deprive any of theſe aliments of any part of their nutritive power.

From nineteen penny-weights eighteen grains of *onions* I expelled, by boiling in river water, half an ounce meaſure of air, of which one third was not abſorbed by water, and extinguiſhed a candle.

From one ounce fifteen penny-weights of *lettuce* I got three quarters of an ounce meaſure of air, of which half an ounce meaſure was phlogiſticated air.

From one ounce ſixteen penny-weights twelve grains of *carrots* I got three quarters of an ounce meaſure of air, of which about one ounce meaſure was phlogiſticated air.

Theſe differences are inconſiderable, and ſome of the air, no doubt, came from the water in which

which these substances were boiled. Afterwards the potatoes and carrots, putrefying in water, yielded each more than two ounce measures of air, one half of which was fixed air, and the rest inflammable. The onions yielded only about half an ounce measure of air, but it was of the same kind, and the lettuce gave only a tenth of of an ounce measure, in which nothing could be perceived to be inflammable. But I did not begin to collect this air till a day or two after the process of boiling, when I perceived some of the substances to be in a state of yielding air.

SECTION V.

Of Air produced by various Subſtances putrefying in Quickſilver.

IN ſome of the firſt of my experiments I amuſed myſelf with putting different vegetable and animal ſubſtances into tall glaſs veſſels, previouſly filled with mercury, and the following were among the reſults which I then noted.

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 one ſeventh 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.

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

By means of theſe experiments, and thoſe in the preceding ſection, it may be poſſible to determine the nutritive powers of different vegetable and animal ſubſtances, and alſo other problems in philoſophy; though too much muſt not be expected from them.

It might have been imagined, that by this means we ſhould be able to aſcertain the quantity of air that any maſs of putreſcent matter would thoroughly phlogiſticate. For any given quantity of inflammable air will completely phlogiſticate twice its bulk of common air. But it will be found that a putrefying mouſe will phlogiſticate much more than that proportion of air. There muſt, therefore, be much more phlogiſton in a mouſe than forms

forms the inflammable air which comes from it. Perhaps, therefore, that phlogiſton which contributes to animal nutrition, may alſo be more than that which enters into the compoſition of the inflammable air that comes from the putrefying ſubſtance. This is a ſubject that requires and deſerves much farther inveſtigation. I only recite the following as *leading experiments*, to the ſolution of greater problems. They are, indeed, upon too ſmall a ſcale to be of much uſe even for this purpoſe; except to ſhew that the ſame kind of ſubſtance, which in a large quantity yields inflammable air, in a ſmall quantity may yield phlogiſticated air.

A ſmall *fiſh*, weighing forty four grains, being confined in quickſilver from the 21ſt of May to the 24th of Auguſt, gave ſomething more than half an ounce meaſure of air, two thirds of which was fixed air, and the remainder extinguiſhed a candle, but was not ſenſibly inflammable.

From two pennyweights of well boiled *beef* I got a very ſmall quantity of air, the bulk of which was fixed air, and the reſt not inflammable. At another time, from one pennyweight and nineteen grains of *raw beef*, I got 0.22 of an ounce meaſure of air, nine tenths of which was fixed air, and the reſt extinguiſhed a candle.

From fifty-three grains of *raw lamb*, I got 0.17 of an ounce meaſure of air, the bulk of which was fixed

fixed air, and the reſt not ſenſibly inflammable: but from two pennyweights and two grains of well *roaſted lamb*, I got three quarters of an ounce meaſure of air, half of which was fixed air, and the reſt highly inflammable; and ſome time after I took from the ſame ſubſtance half an ounce meaſure of air more, of which three fourths was fixed air, and the reſt inflammable.

From thirteen pennyweights and four grains of the *tendon* of a roaſted neck of veal, I got an ounce meaſure and half of air, of which half was fixed air, and the reſt phlogiſticated. Afterwards I took from it one ounce meaſure and three quarters of pure fixed air, with the ſmalleſt reſiduum poſſible. In the former experiment alſo, as well as on a former occaſion, I found that the inflammable air was extricated firſt, and a long time before all the fixed air was exhauſted.

Having had occaſion to make many experiments with putrefying *mice*, and having more in proſpect, I was particularly deſirous to aſcertain the quantity and quality of the air produced by a mouſe of the middle ſize putreſying in quickſilver, and I found as follows. A mouſe weighing ſix pennyweights and three grains, confined by quickſilver, which had putrefied from the 8th of April, had yielded on the 24th of July one ounce meaſure and three quarters of air, of which one fourth was weakly inflammable, and the reſt fixed air. This I found, by other experiments, was nearly as much as a mouſe would yield in theſe circumſtances.

Having left another mouſe to putrefy in quickſilver, I took the air produced from it at different times, in order to ſatisfy myſelf more fully with reſpect to the proportion that the fixed and inflammable air bore to each other, from the beginning to the end of the proceſs. The mouſe weighed five pennyweights and ten grains, and it was put into an inverted veſſel of quickſilver on the 13th of June. On the 26th of that month, I took from it near an ounce meaſure of air, three fourths of which was fixed air, and the reſt inflammable, burning with a very blue flame. On the 16th of Auguſt I took from it an ounce meaſure and a quarter of air, of which four fifths was fixed air, and the reſt, if it was inflammable at all, was ſo in the ſlighteſt degree imaginable; and laſtly, on the 3d of April following, I took from it a ſmall quantity of air, perhaps one tenth of an ounce meaſure, the whole of which was, as far as I could judge, all fixed air.

When a mouſe is left to putrefy in this manner, there comes from it a great quantity of diſſolved blood, or ſome other thin reddiſh liquor. This I carefully ſeparated from what was *ſolid* in the mouſe, and

and found that this continued to give air, when the liquor gave little or none; ſo that perhaps it may be ſomething *ſolid* in all bodies that contributes to the formation of permanent air. By long ſtanding, however, I did get a little air from this red liquor, and it was almoſt all fixed air. It was, perhaps, combined with it, at its ſeparation from the mouſe.

The experiments on ſome of the different *parts* and *ſecretions* of animal bodies were made on the ſame ſmall ſcale with moſt of the preceding, and therefore they can only have the ſame imperfect uſe.

From ſeven pennyweights of the medullary part of a ſheep's *brain* raw, I got four and a half ounce meaſures of air, of which one fifth part of an ounce meaſure was inflammable, and the reſt fixed air. I alſo found by ſimilar experiments, that the cortical part of the ſame brain gave ſomewhat leſs air than the medullary part; but the proportion of the inflammable to the fixed air was the ſame. No certain inference, however, can be drawn from experiments on ſo ſmall a ſcale as theſe.

Two pennyweights of *mutton gravy* yielded 0.02 of an ounce meaſure of air, the greateſt part of which was fixed air, and the remainder ſeemingly inflammable.

Two pennyweights of the *craſſamentum* of ſheep's blood gave only a ſmall bubble of air, too ſmall to be examined. The *ſerum* alſo yielded ſome air, the bulk

bulk of which was fixed air, and the reſt phlogiſticated.

An ounce meaſure of *milk* yielded near half an ounce meaſure of air, almoſt pure fixed air, a ſmall remainder being phlogiſticated.

An ounce meaſure and an half of the *bile* of a ſheep yielded half an ounce meaſure of air, almoſt all fixed air, the ſmall reſiduum being phlogiſticated.

I ſhould not have made theſe experiments on ſo very ſmall a ſcale, but that I expected a greater quantity of air from all the ſubſtances, and becauſe leſs quickſilver was wanted for the purpoſe; ſo that I could have more proceſſes going on at the ſame time. Had the ſame ſubſtances putrefied in *water*, they would have yielded many times more air, water appearing to be an eſſential ingredient in the conſtitution of inflammable air.

PART

PART II.

OF THE PROPERTIES OF INFLAMMABLE AIR.

SECTION I.

Various Experiments to change and decompose inflammable Air.

1. *Inflammable Air diminished by Charcoal.*

IN purſuance of the Abbé Fontana's experiment on the abſorption of air by charcoal, I dipped pieces of hot charcoal into a phial of inflammable air, and immediately inverted it in quickſilver. When one third of the whole quantity was imbibed, I found that both the remainder, and that which was again expelled from the charcoal, by plunging it in water, was inflammable; the former not to be diſtinguiſhed from what it had been, but the latter a little leſs inflammable.

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2. *Of Putrefaction in inflammable Air.*

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

3. *Plants growing in inflammable Air.*

I have made plants grow for ſeveral months in inflammable air made from zinc, and alſo from oak; but, though they 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.

4. *Water impregnated with inflammable Air.*

Neither does inflammable air undergo any change by impregnation with water, in which respect, it agrees with what I have observed of nitrous air. For having impregnated a quantity of rain water (out of which all its air had been carefully extracted by the air pump) with inflammable air, of which it imbibed about one thirteenth of its bulk; about a month afterwards, by making it boil in a phial, I expelled from it about the same quantity of air, and found it to be as strongly inflammable as it had ever been. After this process there was a deposit from the water of a filmy kind of matter, probably the earth of the metal that had been employed in producing the inflammable air. In both these respects inflammable air resembles nitrous air.

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

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

From this experiment I concluded that, by continuing the ſame proceſs, I ſhould deprive inflam-

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mable 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 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.

This change in the inflammable air proceeded, I doubt not, from its communication with the external air through the water; ſo that I ſhould not expect the ſame change from the agitation of it in cloſe veſſels. Phlogiſticated air is meliorated by agitation in open veſſels, but not in *cloſe* ones.

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.

5. *Inflammable Air agitated in Oil of Turpentine.*

The effect of agitating inflammable air in oil of turpentine, and also in spirit of wine, is not a little remarkable. They seem to bring it at last to the same state to which it is brought by agitation in *water*, only that, whereas it is *diminished* by the process in water, it is *increased* in these processes. Both these substances, however, as well as water, seem to deprive this air of part of its phlogiston. The facts, as I observed them, were as follows.

Having agitated a quantity of inflammable air in oil of turpentine, I presently observed an increase of its quantity, and I continued the process till it had increased one half. Agitation in spirit of wine produced the same effect, but more time was requisite for it. Allowing it to continue in these circumstances all night, I found that one half of the additional quantity of air had disappeared: but

but by repeating the agitation about a quarter of an hour, it was again increaſed as much as before. I then examined it, and found that it was not in the leaſt abſorbed by water, did not affect lime water, was but very ſlightly inflammable, and was diminiſhed by nitrous air almoſt as much as common air; which is in all reſpects the very ſtate to which agitation in water would have brought it, except that in water it would have been conſiderably diminiſhed, inſtead of being increaſed..

I agitated another quantity of inflammable air in oil of turpentine made pretty *warm*, but the effect was the very ſame as when it was cold. In this caſe, however, though I hardly ever diſcontinued the agitation, after I had begun it, when it had gained an increaſe of about one fourth of its bulk, it loſt it again, and was reduced to its original dimenſions. I then examined it, and found it to burn with a lambent blue flame. I own myſelf to be intirely at a loſs to account for the increaſe and decreaſe of the quantity of air in theſe experiments.

6. *Animals dying in inflammable Air.*

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 occafioning prefent death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become lefs noxious; but this did not appear to be the cafe; for I killed a great number of mice in a fmall quantity of this air, which I kept feveral months for this purpofe, without its being at all fenfibly mended; the laft, as well as the firft moufe, dying the moment it was put into it.

7. *Inflammable Air changed by keeping in Water.*

Inflammable air is not thought to be mifcible with water, and when kept many months, feems, in general, to be as inflammable as ever, Indeed, when it is extracted from vegetable or animal fubftances, a part of it will be imbibed by the water in which it ftands; but it may be prefumed, that in this cafe, there was a mixture of fixed air extracted from the fubftance along with it. I have indifputable evidence, however, that inflammable air, ftanding long in water, has actually loft all its inflammability, and even come to extinguifh flame much more than that air in which candles have burned out. After this change it appears to be greatly diminifhed in quantity, and it ftill continues to kill animals the moment they are put into it.

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This very remarkable fact first occurred to my observation on the 25th of May, 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately set by a common quart bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly mistaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was set by. That which had been produced from zinc was not altered, and filled the bottle as at first.

I think that, in all, I have had four instances of inflammable air losing its inflammability, while it stood in water. It is very possible, however, that there might be some impregnation in this water, of which I was not aware, since other persons, I find, have not found any change in inflammable air by keeping it in pure water.

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

8. *The electric Spark in inflammable Air.*

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

9. *The Smell of inflammable Air.*

Inflammable air, when it is made by a quick proceſs, has a very ſtrong and offenſive ſmell, from

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

If a quantity of inflammable air be contained in a glaſs veſſel ſtanding in water, and have been generated very faſt, it will ſmell even through the water, and this water will alſo ſoon become covered with a thin film, aſſuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red ochre, or the earth of iron, as I have found by collecting a conſiderable quantity of it; and if it have

have been generated from zinc; it is a whitiſh ſubſtance, which I ſuppoſe to be the calx of the metal. It likewiſe ſettles to the bottom of the veſſel, and when the water is ſtirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this ſcum for a conſiderable time after the air is removed from it. This I have often obſerved with reſpect to iron.

SECTION II.

Inflammable Air decompoſed by Heat, in Tubes of Flint Glaſs.

THIS kind of air remains unchanged when it is expoſed to heat in a tall jar of flint glaſs, in which it had free liberty to expand. I made this experiment at the ſame time with the ſimilar one that I ſhall have occaſion to mention on nitrous air. This air, as well as the nitrous, recovered its former dimenſions when it was cold, and appeared to be unchanged in its quality.

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A very ſingular decompoſition of inflammable air I obſerved in conſequence of expoſing a great variety of ſubſtances to the influence of a ſand heat, which I kept up for ſeveral months. Among other things, I buried in this hot ſand glaſs tubes hermetically ſealed, and previouſly filled with all the different kinds of air. I filled them in the following manner.

Having provided myſelf with glaſs tubes about four feet long, and about one third or one half of an inch in diameter, and of ſuch a thickneſs as that I could eaſily melt them with the flame of a couple of candles and a common blow pipe, I firſt ſealed the tubes at one end, then filled them with quickſilver, and placed them inverted in a baſon of the ſame. After this, either transferring the air in a bladder, from the jars in which they had been ſtanding in water, or generating the air a-freſh, if it was of a kind not to bear the contact of water, I filled the tubes completely with the kinds of air on which I wiſhed to make the experiment, diſplacing the quickſilver. This being done, I inclined the tube, and applying the flame of my candles with ſome care (holding the blow pipe in my mouth only, and keeping firm hold of the tube on each ſide of the place to which I was applying the heat) I melted the glaſs, and took off what lengths of it I pleaſed; and every

every piece was, of courſe, hermetically ſealed. Theſe pieces I marked with a file, keeping an account of the meaning of the marks, that when I took them out of the ſand, I might preſently know with what kind of air they had been filled.

When I was performing this part of the proceſs with inflammable air in flint glaſs tubes, I obſerved that the places to which I applied the heat were generally tinged black; but I gave little attention to this circumſtance, thinking it might be ſomething accidental; and without any particular expectation, I buried theſe tubes in the ſand, together with the others. This was on the 25th of September, 1777.

On the 20th of January following, I examined theſe tubes, together with every thing elſe that had been expoſed to the ſame heat. The tube containing the inflammable air was ten inches long, and by ſome accident was broken; but it was jet black throughout. At this I was very much ſurprized, but I did not then ſuſpect that it was at all owing to the inflammable air with which it had been filled; thinking it might have been occaſioned by ſome phlogiſtic matter in the ſand, or in ſome of the veſſels that had burſt in its neighbourhood.

Reflecting, however, on this odd circumſtance, and thinking, from the uniformity of the tinge,

that,

that, *possibly*, it might have been occasioned by the inflammable air, I filled another small glass tube with the same air; and, sealing it hermetically, buried it deep in sand, contained in an iron pot, which I set on the fire, and made very hot, nearly red; and taking it out the next day, I found the tube quite black, except a small part on one side of that end which had been uppermost, about two inches higher than the other, and which, consequently, had not been exposed to so great a degree of heat.

Being now fully satisfied that the blackness of the tube was *certainly* occasioned by heating the inflammable air within it, in circumstances in which it could not expand, I proceeded to examine the state of the air, and frequently found it to be inflammable; but, in general, the quantity was too small to make a satisfactory experiment.

Putting two glass tubes, about four inches in length, and a quarter of an inch in diameter, into a sand furnace, I kept them in it two days; when I took them out, and observed that the tube which I had placed at the bottom of the sand, in the greatest degree of heat, was nearly melted, and perfectly *blue*, like indigo; while the other tube, which had not been exposed to so great a degree of heat, was of a beautiful jet black throughout.

At one time I had a suspicion that this blackness communicated to the glass was something precipi-

tated

tated from the iron, by the ſolution of which the inflammable air had been made; but I was ſoon convinced of the contrary, by finding that the effect was the very ſame when the inflammable air was made from *zinc*.

I ſoon found that there was no occaſion for ſo long a proceſs to produce this effect, at leaſt upon the glaſs. For it begun to be diſcoloured the moment it was red hot, or rather when it became ſoft; as was evident by holding one of the tubes in an open fire, or in the flame of a candle. For wherever the heat was applied, the blackneſs immediately took place, without affecting any other part of the tube.

When I examined this black tinge narrowly, I found that it did not penetrate the glaſs, but formed a delicate ſuperficial tinge, leaving the glaſs as perfectly poliſhed as before the proceſs. But the blackneſs was indelible; at leaſt, it could not be ſcraped off without tearing the ſurface of the glaſs, and it made no change in it with reſpect to electricity. For the tube thus blackened was as perfect a non-conductor as ever.

The blue colour of the glaſs that was moſt heated, Mr. Delaval informed me, was owing to ſomething of *iron* in the compoſition of the glaſs. That it alſo depended upon the *degree of heat*, I aſcertained by placing one of theſe tubes in a vertical poſition in the

I ſand

ſand heat. For the lower end of the tube, which was moſt heated, had acquired a deep blue colour, and it paſſed into the black at the upper end of the tube without any intermediate colour. There was alſo no other colour higher than the black; ſo that the firſt tinge that the glaſs receives is a perfect black. Yet viewing the firſt tinge that it receives by the light of a candle placed beyond it, it ſeemed to have a ſhade of *red*.

As I was ſenſible that the blackneſs was owing to the precipitation of *phlogiſton* from the inflammable air, I thought it poſſible that ſome ſubſtance which had a near affinity with phlogiſton might diſcharge it; and trying *minium*, it ſucceeded immediately. Having filled one of theſe black tubes with this metallic calx, the moment I made it red hot, the blackneſs intirely diſappeared, and left the tube as tranſparent as ever it had been.

In the firſt experiment of this kind I uſed minium, out of which all its air had been expelled by heat, and which is of a yellow colour. In this proceſs it became whiter, and adhered a little to the glaſs. When I ſcraped it off, I could not be quite ſure that any part of it was become real *lead*; but it evidently approached towards a metallic ſtate, by being of a more compact texture than before.

In this ſtate of the experiments I communicated the reſult of my obſervations to my friend Mr. Bewly, who ſuggeſted to me, that, probably, it was the

the *lead* in the glaſs tubes that had attracted the phlogiſton; and I preſently found this to be the caſe. For when I had filled a *green glaſs* tube with the inflammable air, and ſealed it hermetically, as I had done the flint glaſs tubes, I expoſed it to a melting heat, which is greater than that which flint glaſs will bear, without producing any change of colour in it. What remained of the air in the tube, that did not eſcape when part of it was melted, was ſtill ſtrongly inflammable.

It appears, therefore, from this experiment, that the calx of lead, in the form of glaſs, has a ſtronger affinity with phlogiſton than any thing in the compoſition of inflammable air, in a degree of heat capable of melting glaſs. Or, if there be no proper conſtituent part of inflammable air beſides phlogiſton, the attraction of the calx is ſo great, as to reduce the phlogiſton from an elaſtic and uncombined ſtate to a fixed and combined one.

Having, by means of theſe glaſs tubes, effected a complete decompoſition of inflammable air, the phlogiſton in it having united with the glaſs of the lead; I thought that, if there had been any *acid* in its compoſition, it would then be diſengaged, and be found in the tube. In order to find whether there was any acid in it, or not, I poured into one of theſe tubes a ſmall quantity of water made blue with the juice of turnſole; but it came out as blue as it went in.

SEC.

SECTION III.

Of sulphurated inflammable Air.

THERE is no kind of air which admits such a variety of modifications as the inflammable; nor shall we think this extraordinary, when we consider that phlogiston, which is the distinguishing ingredient in it, enters into a greater variety of combinations with *solid substances* than perhaps any other principle in nature, and is the cause of a greater variety of properties in them. Spirit of wine, oil, sulphur, charcoal, and metals, are substances as different from each other, both in their external appearance, their degrees of consistence, and other chemical properties, as any things in nature, and yet the principal ingredient in them all is the same phlogiston, as may be proved by the actual transferring of it from any one to any other of them. Inflammable air likewise extracted from each of these substances, as also that from putrid vegetables, and by other processes, of which an account has been given in the preceding sections, are all remarkably different, and appear to be so, as we shall presently see, when they are decomposed. I shall now give an account of another ſpecies of this kind of air, which I term *ſulphurated*, from the ſtrong ſmell that it has of ſulphur, or rather liver of ſulphur, and its being loaded with a greater quantity of matter; which, though at the firſt black, yet on expoſure to the air preſently aſſumes a yellowiſh colour. I ſhall recite the experiments in which I obſerved this peculiar ſpecies of air, in the order in which I made them, noting the other appearances that accompanied them, though they have not any immediate relation to the *air* of which I am treating.

When I was engaged in that courſe of experiments in which ſteam, and the vapour of various fluid ſubſtances, was brought into contact with ſolid ſubſtances red hot, I treated *mangane*ſ*e* in this manner, and eſpecially a quantity with which Mr. Woulfe had formerly furniſhed me, which was not in powder, but in a large maſs, juſt as it is dug out of the earth. A few ounces of this I put into an earthen tube, open at both ends. But cloſing one of them with a cork, while the middle part of the tube was red hot, and the other orifice was furniſhed with an apparatus proper for collecting the air that might be expelled from it, I received forty ounce meaſures of air, of which one ſixth was fixed air, and the reſt of the ſtandard of 1.7, lambently inflammable. No more air coming in this diſpoſition of the apparatus, I opened

I opened the other end of the tube, and with a proper contrivance for the purpoſe, ſent through it a quantity of ſteam; in which circumſtances air was produced more copiouſly than before. Of this I received about fifty ounce meaſures, obſerving that one ſeventh of it was fixed air, and the reſt of the ſtandard of 1.8, not lambently, but exploſively, inflammable. The laſt portions of this air were very turbid, and the ſmell of the air, and eſpecially that of the laſt portion, was very ſulphureous, and, I obſerved, tinged the water of a very dark colour, by depoſiting in it a quantity of blackiſh matter. However, the air itſelf became preſently tranſparent, and had no other appearance than that of any other kind of air, when I left in my trough a jar filled with it.

Having been intent on ſome other experiments, I was ſurpriſed to find, on looking on the jar about ten minutes afterwards, that it was quite black, ſo that I could ſee nothing in the inſide of it. In order to obſerve how it came to be ſo, I afterwards filled another jar with this kind of air, and obſerved that when the water was well ſubſided, black ſpecks began to appear in different places, and, extending themſelves in all directions, at length joined each other, till the whole jar was perfectly black, and the glaſs quite opake. When this was done, I transferred the air to another clean jar, and it ſoon produced the ſame effect upon this, though it never became ſo black as the jar in which it had been firſt received. It alſo frequently happened that only the lower part of the jar would become black, as if this matter, with which it was loaded, had kept ſubſiding, though inviſibly, in the maſs of air, and occupied the lower regions of it only, leaving the upper part entirely free from it. When the veſſels thus tinged black were expoſed to the open air, that colour preſently diſappeared, and a yellow or brown incruſtation was left upon it.

Thinking, from this circumſtance, that this black coating conſiſted of ſome volatile phlogiſtic matter, I placed the jars which had this black tinge with their mouths inverted in veſſels of water, in order to obſerve the effect which the change of colour might have on the common air contained in them. In theſe circumſtances the black tinge preſently went off, and was ſucceeded by the yellow colour, but without producing any ſenſible change in the air. In ſome caſes, however, I thought that it was injured; but it was by no means ſo much ſo as I had expected. After depoſiting this black matter, the air ſtill retained its ſulphureous ſmell, and as far as I can judge, will never entirely leave it.

It is by no means the univerſal property of manganeſe to yield this ſulphurated inflammable air, but muſt have been owing to ſomething peculiar to this ſpecimen, and perhaps to ſomething accidentally mixed with it. For when I repeated the experiment with other manganeſe, which I had from a glaſs houſe, in which it is uſed, I had no ſuch appearance. From four ounces of this manganeſe, treated as the preceding, I got without ſteam, 256 ounce meaſures of air, of which about one tenth was fixed air. Then ſending ſteam over it, I got more air, but in no great quantity, about ten ounce meaſures in an hour; though probably much more might have been procured, if the proceſs had been continued. This was dephlogiſticated; for, mixed with two equal meaſures of nitrous air, the ſtandard was 0.28, which ſhews that it was exceedingly pure. But one tenth of this was fixed air, as in the former portion, which agrees with the experiments I formerly made with this ſubſtance when I found that heat alone would expel from it a quantity of very pure air.

The next time that I got this ſulphurated inflammable air, was as unexpected as the preceding; and this experiment was the firſt thing that gave me any inſight into the nature of it. Having occaſion to make a large quantity of inflammable air, inſtead of freſh turnings of iron, I happened to take ſome, parts of which had been heated by a burning lens in vitriolic acid air, in which, as I have obſerved, it melts with great readineſs, and gathers into balls. When this iron was diſſolved in diluted oil of vitriol, though there were only a few pieces in the quantity that I uſed which had been melted in this manner, the water in which the air was received was very black, and depoſited more ſediment than in the experiment with the manganeſe. The jars alſo which contained it were preſently as black as ink, but became yellow when expoſed to the open air. This inflammable air had alſo the ſame offenſive ſulphureous ſmell; ſo that there could be no doubt of its being the ſame kind of air which I had got from Mr. Woulfe's manganeſe. There was in it, however, a mixture of vitriolic acid air, as I perceived when I burned a large quantity of it in a glaſs balloon, in order to collect the *water* that might be produced in this proceſs. All the inſide of the balloon was filled with a denſe white cloud, all the time that the air was burning in it, and the water produced was very ſenſibly acid. In reality, the ſame effects were produced as if ſulphur had been burned in the veſſel.

As I had no doubt, but that the iron which had been melted in vitriolic acid air was the ſame as what is called *ſulphurated iron*, or iron with which ſulphur is incorporated, I now completely aſcertained it by making a quantity of ſulphurated iron, dipping it when red hot into melted ſulphur. This iron, treated as the other had been, yielded exactly ſuch air as I have been deſcribing, ſo that I could have no doubt with reſpect to the real origin of it.

When I decompoſed this air, by firing it with an equal quantity of dephlogiſticated air, the diminution of bulk was the ſame as when I uſed the common inflammable air, ſo that it did not appear to contain either more or leſs phlogiſton; but there was a ſmall quantity of *fixed air* produced, which is never the caſe with inflammable air procured with oil of vitriol, though it is ſometimes when it is procured from iron with ſpirit of ſalt.

When the ſulphurated inflammable air is received in veſſels containing mercury, there is very little black matter depoſited from it; but it appears when it is transferred into veſſels containing water.

Though jars thinly coated with this black matter become yellow when expoſed to the open air, this is not the caſe with that which is collected from the water in which the air has been confined. For when the water is evaporated from it, it adheres to the evaporating veſſel in the form of a perfectly black incruſtation. This ſubſtance, though it does not burn blue on a hot iron, yet shews evident signs of containing sulphur. For when the nitrous acid has taken from it its superfluous phlogiston, it has both the colour and the smell of sulphur.

SECTION IV.

Metals, and other Substances containing Phlogiston, formed by imbibing inflammable Air.

THERE are few subjects, perhaps none, that have occasioned more perplexity to chemists, than that of *phlogiston*, or, as it is sometimes called, *the principle of inflammability*. It was the great discovery of Stahl, that this principle, whatever it be, is transferable from one substance to another, how different soever in their other properties, such as sulphur, wood, and all the metals, and therefore is the same thing in them all. But what has given an air of mystery to this subject, has been that it was imagined, that this principle, or substance, could not be exhibited except in combination with other substances, and could not be made

made to assume separately either a fluid or solid form. It was also asserted by some, that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they chose to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemists, Mr. Lavoisier among others, that the whole doctrine of phlogiston is founded on mistake, and that in all cases in which it was thought that bodies parted with the principle of phlogiston, they in fact lost nothing; but on the contrary acquired something; and in most cases an addition of some kind of air; that a *metal*, for instance, was not a combination of two things, viz. an *earth* and *phlogiston*, but was probably a simple substance in its metallic state; and that the calx is produced not by the loss of phlogiston, or of any thing else, but by the acquisition of air.

The arguments in favour of this opinion, especially those which are drawn from the experiments that Mr. Lavoisier made on mercury, are so specious, that I own I was myself much inclined to adopt it. My friend Mr. Kirwan, indeed, always held that phlogiston was the same thing with inflammable air. I did not, however, accede

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to it till I thought I had difcovered it by direct experiments, made with general and indeterminate views, in order to afcertain fomething concerning a fubject which had given myfelf and others fo much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red hot in flint glafs tubes, gave them a black tinge, and was in a great meafure abforbed, which I difcovered to be owing to the calx of lead in the glafs, attracting phlogifton from the inflammable air.

I found, however, great difficulty in repeating thefe experiments; and the quantity of inflammable air operated upon in them, is neceffarily fo fmall, that the refult is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glafs, furrounded with inflammable air, or rather on the calx of lead alone, in the fame circumftances, would be a much eafier experiment, and might bring me nearer to my object; and on making the experiment it immediately anfwered far beyond my expectation.

For this purpofe, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient ftand, intro-

introduced it into a large receiver, filled with inflammable air, confined by water. As ſoon as the minium was dry, by means of the heat thrown upon it, I obſerved that it became black, and then ran in the form of perfect lead, at the ſame time that the air diminiſhed at a great rate, the water aſcending within the receiver. I viewed this proceſs with the moſt eager and pleaſing expectation of the reſult, having at that time no fixed opinion on the ſubject; and therefore I could not tell, except by actual trial, whether the air was decompoſing in the proceſs, ſo that ſome other kind of air would be left, or whether it would be abſorbed *in toto*. The former I thought the more probable, as, if there was any ſuch thing as phlogiſton, inflammable air, I imagined, conſiſted of it, and ſomething elſe. However, I was then ſatisfied that it would be in my power to determine, in a very ſatisfactory manner, whether the phlogiſton in inflammable air had any *baſe* or not, and if it had, what that baſe was. For ſeeing the metal to be actually revived, and that in a conſiderable quantity, at the ſame time that the air was diminiſhed, I could not doubt, but that the calx was actually imbibing ſomething from the air; and from its effects in making the calx into metal, it could be no other than that to which

which chemifts had unanimoufly given the name of *phlogifton.*

Before this firft experiment was concluded, I perceived, that if the phlogifton in inflammable air had any bafe, it muft be very inconfiderable: for the procefs went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be diftinguifhed from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was, therefore, pretty well fatisfied that this inflammable air could not contain any thing befides phlogifton; for at that time I reduced about forty five ounce meafures of the air to five.

In order to afcertain a fact of fuch importance with the greateft care, I afterwards carefully expelled from a quantity of minium all the phlogifton, and every thing elfe that could have affumed the form of air, by giving it a red heat when mixed with fpirit of nitre; and immediately ufing it in the manner mentioned above, I reduced a hundred and one ounce meafures of inflammable air to *two*. To judge of its degree of inflammability, I prefented the flame of a fmall candle to the mouth of a phial filled with it, and obferved, that it made thirteen feparate explofions, though weak ones (ftopping the mouth of the

the phial with my finger after each explosion) when fresh made inflammable air, in the same circumstances, made only fourteen explosions, though stronger ones.

After this experiment I could not hesitate to conclude*, that this inflammable air went totally, and without decomposition, into the lead which I formed at that time; and if the necessary circumstances of the experiment be considered, it will be thought extraordinary that, even admitting this, the result should be so decisively clear in favour of it: for, in the first place, the greatest care must be used to expel all air from the minium, and it must be used before it can have attracted any from the atmosphere; and in the next place, the water also (a considerable quantity of which must be used, and which will also be heated in the process) should be made as free from air as possible. In these circumstances, had I found the small residuum, of two ounce measures from a hundred and one, to have been phlogisticated or fixed air, I should not have been disappointed; and it would not have prevented my conclud-

* In this conclusion, I overlooked one obvious consideration, viz. that water, or any thing soluble in water, might be the basis of inflammable air. All that could be absolutely inferred from the experiment was, that this basis could not be any thing that was capable of subsisting in the form of *air*. It will be seen, that I afterwards made the experiment with the air confined by mercury.

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ing that *phlogiſton* was the ſame thing with *inflammable air*, contained in a combined ſtate in metals, juſt as fixed air is contained in chalk and other calcareous ſubſtances; both being equally capable of being expelled again in the form of air.

Afterwards, uſing a calx of lead, which had been prepared in the ſame manner with the former, but which had remained for ſome weeks expoſed to the air, I found, that when by uſing it I had reduced 150 ounce meaſures of inflammable air to ten, this reſiduum was phlogiſticated air. But examining this calx ſeparately, I found that it gave, by heat in a glaſs veſſel, a conſiderable quantity of phlogiſticated air.

I muſt obſerve, that the minium ſhould not be reduced to a perfectly compact *glaſs of lead*; for then it will be too refractory to be eaſily revived by this proceſs. Making uſe of ſome of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inſide of the receiver: an experiment which I ſhall repeat and reconſider. I muſt alſo obſerve, that the lead which I procured in the above mentioned proceſs was not to be diſtinguiſhed from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made uſe of inflammable air from wood, I found, that though I was able to reduce minium with

with it, it was effected with more time and difficulty. Forty ounce meaſures of this kind of inflammable air I reduced to twenty five; after which I found that the heat of the lens produced only *glaſs of lead*, and no *metal*. The air was ſtill, however, inflammable; and there was a ſmall mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have ſome reaſon to think, conſiſts of an intimate union of fixed air with that which is of the *exploſive* kind extracted from metals. The reſult of thoſe experiments which I made with that kind of inflammable air which is collected in the proceſs for making phoſphorus, and which burns with a lambent yellow flame, was ſimilar to thoſe which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable reſult with inflammable air, I immediately tried all the other kinds of air in the ſame manner; but in none of them did I procure any thing from the minium beſides glaſs of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogiſticated air, marine acid air, fluor acid air, as alſo in common and dephlogiſticated air, I got no *metal* at all. In vitriolic acid air there was but a ſmall quantity of lead produced, and I have obſerved that this kind of air imparts a certain portion of phlogiſton to common air (or rather imbibes

bibes a part of the dephlogiſticated air from it) rendering the remainder in ſome meaſure phlogiſticated, though by no means in ſo great a degree as nitrous air.

Though nitrous air and phlogiſticated air certainly contain phlogiſton, they appear by theſe experiments to hold it too obſtinately to part with it to minium in this proceſs, notwithſtanding nitrous air quits it ſo readily to reſpirable air. I would obſerve, that there were ſome peculiar appearances in the experiments I made to revive the calx of lead in theſe kinds of air in which the attempt did not ſucceed; but I muſt repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead ſeems to be formed from the minium as readily as in inflammable air; and indeed I thought rather more ſo; and this is a remarkable confirmation and illuſtration of thoſe experiments, in which, by taking the electric ſpark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times ſince I firſt publiſhed the account of it, and always with the ſame reſult.

This experiment alſo throws ſome light upon thoſe in which, by expoſing iron to nitrous air,

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I produced a ſtrong ſmell of volatile alkali; an experiment which I have alſo frequently repeated with the ſame reſult. The reviving of lead in alkaline air may alſo help us to conceive how all *acids* ſhould have an affinity both to *phlogiſton* and to *alkalies*, which have hitherto appeared to be things ſo very different from each other; ſince, from theſe experiments, it is probable that one of them is ſome modification of the other, or a combination of ſomething elſe with the other. To trace the connexion between the alkaline and inflammable principles, is a curious ſubject; and from theſe hints it may, perhaps, not be very difficult to proſecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound, and inflammable air, or phlogiſton, the more ſimple ſubſtance of the two.

From five ounce meaſures and a half of alkaline air I got, by means of litharge, ſeventeen grains of lead, beſides ſome that was diſſolved in the mercury, by which the air was confined. There remained two ounce meaſures and a half, which appeared to be phlogiſticated air, and to have no fixed air in it. At another time, in eight ounce meaſures of alkaline air I got fifteen grains of lead, beſides what was diſſolved in the mercury, which ſeemed to be a good deal in proportion to it. There remained in this procefs three ounce meafures and a half of phlogifticated air, without any mixture of fixed air in it.

Having thus produced *lead* in inflammable air, I proceeded in my attempts to revive other metals from their calces by the fame means; and I fucceeded very well with tin, bifmuth, and filver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arfenic, zinc, or the metal of manganefe.

I was defirous alfo of afcertaining by this means the *quantity* of phlogifton that enters into the compofition of the feveral metals; but in this I found more difficulty than I had expected; and this arofe chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not eafy to revive the whole of any quantity of calx completely.

After many trials, I think I may venture to fay, that an ounce of *lead* abforbs a hundred ounce meafures of inflammable air, or perhaps fomething more; for in one refult it feemed to have imbibed in the proportion of 108 ounce meafures.

An ounce of *tin* abforbs inflammable air in the proportion of 377 ounce meafures to the ounce.

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An ounce of copper from verditer abſorbed 403 ounce meaſures; from a ſolution of blue vitriol, precipitated by ſalt of tartar, and afterwards made red hot with ſpirit of nitre, 640; but from blue vitriol itſelf 909 ounce meaſures. In this caſe, however, much of the inflammable air went to the formation of the vitriolic acid air, the ſmell of which was very perceivable in the courſe of the experiment. The copper that I made in this way was brittle, and therefore ſeemed not to be perfectly metallized; but being fuſed with borax, it became perfect copper, and, as I think, without any loſs of weight.

Biſmuth abſorbed inflammable air in the proportion of 185 ounce meaſures to the ounce. The calx I uſed was a precipitate from the ſolution of this metal in ſpirit of nitre.

Iron I got from a precipitate of a ſolution of green vitriol by ſalt of tartar, moiſtened with ſpirit of nitre, and expoſed to a red heat. This calx abſorbed in the proportion of 890 ounce meaſures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiſton, ſhould be produced in this manner, ſince inflammable air may be expelled from perfect iron in this very process*.

Silver I evidently revived from a solution of it in spirit of nitre precipitated by salt of tartar, and also from *luna cornea*. A quantity of this last substance absorbed twenty three ounce measures of inflammable air; but I could not get any calx of silver free from small grains of the perfect metal, which was easily discovered by a magnifier, and therefore I could not ascertain the quantity of inflammable air absorbed by it.

Small grains of regulus of *cobalt* I produced from zaffre, and inflammable air was absorbed; but I did not estimate the quantity.

A quantity of *manganese* absorbed seven ounce measures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpose; and mixing with it some calcined borax, I repeated the experiment, when there was again an evident absorption of air, and in the course of that experiment, I once thought that I did perceive a small globule of metal.

Zinc and *arsenic* were only sublimed in this process. The same was the case with the glass of

* I have since found that inflammable air cannot be expelled from iron by heat, without some moisture, which therefore seems necessary to its constitution.

antimony;

antimony; but the experiment was attended with this peculiar circumſtance, that when the glaſs was melted in inflammable air, it formed itſelf into needle-like cryſtals, arranged in a very curious manner; and I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a ſufficient proof of its *containing* what has been called phlogiſton; and its being abſorbed by them *in toto*, without decompoſition, is a proof that, excluſive of water, it is nothing beſides *phlogiſton in the form of air*, unleſs there ſhould be ſomething ſolid depoſited from it at the ſame time that the proper phlogiſtic part of it is abſorbed. With reſpect to this, I can only ſay that, in the courſe of the experiments, I did not perceive any thing of the kind: for though in ſome of the proceſſes there was a black ſmoke produced, in others I could perceive nothing but part of the calx ſubliming, and clouding the glaſs. On this account, however, I could not pretend to aſcertain the weight of the inflammable air in the calx, ſo as to prove that it had acquired an addition of weight by being metalized, which I often attempted. But were it poſſible to procure a perfect calx, no part of which ſhould be ſublimed and diſperſed, by the heat neceſſary to be made uſe of in the proceſs, I ſhould not doubt but that the quantity of inflammable air imbibed by it would ſufficiently add to its weight.

Beſides the formation of metals from their calces, I had other proofs, and of a nature ſufficiently curious, of inflammable air containing phlogiſton. Thus, by means of it, I was able to make *phoſphorus*, *nitrous air*, *liver of ſulphur*, and *ſulphur* itſelf, in all of which phlogiſton is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glaſſy matter which is made from calcined bones by oil of vitriol in inflammable air, ſome of it was abſorbed, and all the inſide of the receiver was covered with an orange coloured ſubſtance, which had a ſtrong ſmell of phoſphorus. I then wanted ſun-ſhine to continue the experiment; but I was ſatisfied that there was ſufficient proof of phoſphorus being actually formed in this manner. With alkaline air I ſucceeded much better.

In two ounce meaſures and a half of this air, I produced, from the glaſſy matter mentioned above, two grains of phoſphorus in one maſs, the veſſel being only filled with white fumes during the proceſs. One fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the proceſs for making phoſphorus.

That nitrous air contains phlogiston is sufficiently evident, if there be any such thing as phlogiston: and I have farther proved, that it contains very nearly as much phlogiston, in proportion to its bulk, as inflammable air itself. I had now, however, the farther satisfaction to be able to make nitrous air from its two constituent principles, viz. nitrous vapour and inflammable air. The most easy process for this purpose is, to throw a stream of nitrous vapour into a large phial previously filled with inflammable air. In this manner nitrous air is instantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid solution of bismuth in spirit of nitre, which at the same time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the same thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I first produced by uniting nitrous vapour to minium (in consequence of which, from being a red and powdery substance, it becomes white, compact, and brittle) I placed it upon a stand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two thirds of the whole, and during this time lead was revived from the calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all strongly nitrous: and, from the circumstances in which it was produced, it must have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver. In order to ascertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the space of 1.32 measures. Fresh nitrous air made in the usual way, and mixed with common air in the same proportion, occupied the space of 1.26. This difference arose not from any impurity in the nitrous air, but from the mixture of the dephlogisticated air, which is also expelled from this calx by heat.

Liver of sulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Lastly, to produce *sulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen vessel, and evaporated it to dryness in a receiver filled with inflammable air; in consequence of which the inside of the receiver acquired a whitish incrustation, which when warmed had a strong smell of sulphur; and repeating

repeating the proceſs in the ſame receiver, I was able, this ſecond time, to ſcrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar ſmell, of ſulphur.

PART

PART III.

OF THE CONSTITUTION OF INFLAMMABLE AIR.

SECTION I.

Experiments which prove that Water is a necessary ingredient in inflammable Air.

AT first I had no suspicion that water was any part of inflammable air, and it may be worth while to recite the experiments which led to that conclusion. Having put a quantity of *iron-filings*, carefully sorted with a magnet, into one of the glass-vessels, fig. *a*, Pl. iv. I filled the rest of the vessel with quicksilver; and placing it inverted in a bason of quicksilver, I threw the focus of the lens upon the iron-filings, and presently air was produced; which, being examined, appeared to be inflammable, though not

not very ſtrongly ſo. It reſembled inflammable air that had been waſhed in water till its inflammability was nearly gone. I alſo could not diſtinguiſh the colour of the flame, when I made the exploſion in the uſual manner, by the approach of a candle. After the operation, the iron from which the air had been extracted, had an exceedingly ſtrong ſmell, exactly like that of very ſtrong inflammable air procured from metals by acids.

In the ſame manner I got air from the *filings of watch-ſprings* which are made of the beſt of ſteel; and it was not to be diſtinguiſhed from the inflammable air of the laſt experiment. Theſe filings, as well as thoſe of iron, I had carefully ſorted with a magnet, ſo that I believe there was no foreign matter mixed with them.

N. B. The ſpot on which the focus of the lens was thrown, was much blacker than any other part of the filings; and during the application of the heat, a quantity of the filings would ſometimes be diſperſed, as by an exploſion below the ſurface of them; owing, I ſuppoſe, to the ſudden generation of air from ſome of the filings that lay under the reſt, but where the heat could reach them.

Having thus got air from *iron*, I proceeded to make ſimilar experiments on other metals. But as all the other metals have more or leſs affinity with quickſilver, I was obliged to have recourſe to a

vacuum;

vacuum. But being poſſeſſed of Mr. Smeaton's air-pump, I could depend upon the vacuum being very exact; ſo that very little common air could be mixed with the air produced. That the filings of the different metals might be perfectly unmixed, I procured new files, quite clean, and uſed one ſide of each for each of the metals.

With this apparatus, I threw the focus of my lens upon filings of *zinc*, and preſently got from them air which was very ſtrongly inflammable. Zinc is ſaid to contain more phlogiſton than the other metals, and the difference between the inflammable air from zinc, and that which I got from iron, was very ſtriking.

From *braſs-duſt* I got inflammable air in conſiderable plenty, and alſo from tin; but this laſt was very ſlightly inflammable. I could not have perceived it to be ſo at all but by dipping a lighted candle into a veſſel full of it; whereas, in other caſes, I made the trials by preſenting the flame of a candle to the narrow mouth of a phial filled with the air. That braſs ſhould yield inflammable air, I attribute to the zinc, by the addition of which, copper is converted into braſs.

Thus all the metals that yield inflammable air, when diſſolved in acids, gave inflammable air alſo by heat only. With other metals I had no ſucceſs.

Regulus of *Antimony*, heated *in vacuo*, ſmoked very much,

much, and blackened all the inſide of my receiver; but the air that I got from it was very little indeed, and extinguiſhed a candle.

From *biſmuth*, and *nickel*, I got hardly any air at all; but in theſe experiments the heat was not advantageouſly applied, and the biſmuth ſoon melted into large lumps, on which my lens had no power.

I got no air from *lead* or *copper*. By throwing the focus of the lens upon the former, the receiver was filled with fumes; but the heat was by no means ſufficient for the experiment with copper.

It is generally ſaid, that charcoal is indeſtructible, except by a red heat in contact with air. But I found that it is perfectly deſtructible, or decompoſed, *in vacuo*, or as will appear hereafter, by means of *water* which it attracts when red hot from the moiſture in the receiver. For in theſe circumſtances, and by the heat of a burning lens it is almoſt wholly converted into inflammable air; ſo that nothing remains beſides an exceedingly ſmall quantity of white aſhes, which are ſeldom viſible, except when, in very ſmall particles, they happen to croſs the ſunbeam, as they fly about within the receiver. It would be impoſſible to collect or weigh them; but, according to appearance, the aſhes thus produced from many pounds of wood, could not be ſuppoſed to weigh a grain. The great weight of aſhes produced by burning wood in the open air, ariſes from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without the least particle of fixed air in it. But, in order to this, the charcoal must be perfectly well made, or with such a heat as would expel all the fixed air which the wood contains; and it must be continued till it yield inflammable air only, which, in an earthen retort, is soon produced.

Wood, or charcoal, is even perfectly destructible, that is, resolvable into inflammable air, in a good earthen retort, and a fire that would about melt iron. In these circumstances, after all the fixed air had come over, I have several times continued the process during a whole day, in all which time inflammable air has been produced equably, and without any appearance of a termination. Nor did I wonder at this, after seeing it wholly vanish into inflammable air *in vacuo*. A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce measures of inflammable air in twelve minutes.

That water in great quantities is sometimes produced from burning inflammable and dephlogisticated air seemed to be evident from the experiments of Mr. Cavendish and Mr. Lavoisier. I have also frequently collected considerable quantities of water in this way, though never quite so much as the weight of the two kinds of air decomposed. My apparatus,

apparatus for this purpoſe was the following. Into the mouth of a large glaſs balloon (*a*) Fig. 4. Pl. vii. I introduced a tube from the orifice of which there continually iſſued inflammable air, from a veſſel containing iron and oil of vitriol. This being lighted, continued to burn like a candle. Preſently after the lighting of it, the inſide of the balloon always became cloudy, and the moiſture ſoon gathered in drops, and ſettled in the lower part of the balloon. To catch what might iſſue in the form of vapour, in the current of air through the balloon, I placed the glaſs tube (*b*) in which I always found ſome water condenſed. It is very poſſible, however, that in both theſe modes of experimenting, the water may be converted into a kind of vapour, which is very different from *ſteam*, and capable of being conveyed a great way through air, or even water, without condenſation, along with the air with which it is mixed; and on this account it may not be poſſible, in either of theſe modes of experimenting, to collect *all* the water which the two kinds of air will yield. The nature of this kind of vapour into which water may be changed, and which is not readily condenſed by cold, is very little underſtood, but well deſerves the particular attention of philoſophers*. Even mercury will evapo-

* Mr. Sauſſure has made ſome valuable obſervations on this ſubject.

rate, ſo as to loſe weight, in a degree of heat below that of boiling water.

That the water collected in the balloon came from the decompoſition of the air, and not from the freſh air circulated through it, was evident from placing balls of hot iron in the place of the flame, and finding that, though the balloon was as much heated by them as by the flame of the burning of the inflammable air, and conſequently there muſt have been the ſame current of the external air through it, no moiſture was found in the balloon.

When, in this manner, I burned inflammable air from pure iron, the water I collected was as far as I could perceive free from acid, and the inſide of the balloon was quite clear, but when I uſed *ſulphorated iron*, there was a denſe white cloud that filled the inſide of the balloon. There was alſo a ſtrong ſmell of vitriolic acid air, and the water collected was ſenſibly acid to the taſte.

Afterward, ſeeing much water produced in ſome experiments in which inflammable air was decompoſed, I was particularly led to reflect on the relation which they bore to each other, and eſpecially Mr. Cavendiſh's ideas on the ſubject. He had told me that notwithſtanding my former experiments, from which I had concluded that inflammable air was pure phlogiſton, he was perſuaded that *water* was eſſential to the production of it, and even entered into

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it as a conſtituent principle. At that time I did not perceive the force of the arguments which he ſtated to me, eſpecially as, in the experiments with charcoal, I totally diſperſed any quantity of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no ſuſpicion that the wet leather on which my receiver ſtood could have any influence in the caſe, while the piece of charcoal was ſubject to the intenſe heat of the lens, and placed ſeveral inches above the leather: I had alſo procured inflammable air from charcoal in a glazed earthen retort two whole days ſucceſſively, in which it had given inflammable air without intermiſſion. Alſo iron filings in a gun-barrel, and a gun-barrel itſelf, had always given inflammable air whenever I tried the experiment.

Theſe circumſtances, however, deceived me, and perhaps would have deceived any other perſon; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intenſely hot. They will find, and attract it, in the midſt of the hotteſt fire, and through any pores that may be left open in a retort; and iron filings are ſeldom ſo dry as not to have moiſture enough adhering to them, capable of enabling them to give a conſiderable quantity of inflammable air. But my attention being now fully awake to the ſubject, I preſently found that the circumſtances above-mentioned had actually miſled me; I mean with reſpect to the *concluſion* which I drew from the experiments, and not with reſpect to the experiments themſelves; every one of which, I doubt not, will be found to anſwer, whenever they are tried by perſons of ſufficient ſkill and properly attentive to all the circumſtances.

Being thus apprized of the influence of unperceived moiſture in the production of inflammable air, and willing to aſcertain it to my perfect ſatisfaction, I began with filling a gun-barrel with iron filings in their common ſtate, without taking any particular precaution to dry them, and I found that they gave air as they had been uſed to do, and continued to do ſo many hours. I even got ten ounce meaſures of inflammable air from two ounces of iron filings in a coated glaſs retort. At length, however, the production of inflammable air from the gun-barrel ceaſed; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully ſatisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiſton.

I then repeated the experiment with the charcoal, making the receiver the ſtand on which I placed the charcoal, and the charcoal itſelf, as dry and as hot as poſſible, and uſing cement inſtead of a wet leather to exclude the air. In theſe circumſtances I was not

able,

able, with the advantage of a good ſun, and an excellent burning lens, to decompoſe quite ſo much as two grains of the piece of charcoal, which gave me ten ounce meaſures of inflammable air; and this I imagine, was effected by means of ſo much moiſture as was depoſited from the air in its ſtate of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as neceſſary as to that from iron.

As inflammable air was produced in ſome experiments, in which I endeavoured to change the nature of water, by making it red hot in a gun-barrel, the orifice of which was welded up, it may not be improper juſt to mention them in this place, as they ſhew the uſe of water in procuring this kind of air. They will likewiſe ſerve to ſhew the expanſive force of water in that ſtate. The experiments were made in March 1783.

Putting ſixteen grains of water into a gun-barrel, containing four ounce meaſures and a half, I got it welded up; and making it red hot, it burſt in the middle after a few minutes. I aſcertained the quantity of water, by putting it into a ſmall glaſs tube, which I ſealed hermetically, and put within the gun-barrel.

I then put ſix grains of water into the thicker half of a muſket barrel, and three grains and a half into a thinner barrel. Theſe did not burſt when they were red hot, and being pierced under water, inflammable air ruſhed out. I repeated theſe experiments, and always had the ſame reſult; inflammable air being procured, when the gun-barrels were opened under water; and if the water was in ſufficient quantity, part of it at leaſt (for I could not meaſure it with exactneſs) was found in the barrel.

If inflammable air always contains water, water ſhould be found whenever this kind of air is decompoſed; yet in heating *red precipitate* in inflammable air, I at one time found little or no water. But having uſed more precautions, I have ſince found it in ſufficient quantity in this proceſs, even though the inflammable air was previouſly well dried with fixed ammoniac. In this experiment I diſcontinued the proceſs after three ounce meaſures of air were abſorbed, leaving room in the veſſel, that the moiſture might be more eaſily collected. With this precaution, and warming the veſſel, I collected between an half and three-fourths of a grain of water.

This experiment may be thought to be favourable to the hypotheſis of water being compoſed of fixed and inflammable air; as all water was carefully excluded, and yet a ſufficient quantity was found in the proceſs. But beſides taking into the account the water that is neceſſary to conſtitute the inflammable air, why may not *red precipitate*, in its drieſt

drieſt ſtate, be ſuppoſed to contain water, as well as the ſcales of iron, which will bear any degree of heat without parting with it. Red precipitate is made by a liquid proceſs, and therefore the water, that may enter into its compoſition as a calx, may quit it when it becomes a metal.

Having found that water is an eſſential ingredient in the conſtitution of inflammable air, at leaſt as produced from iron, it ſtill remained to be determined whether, when a calx is revived, and the metal formed, the pure phlogiſton only entered the calx, or, together with it, that *water* which was neceſſary to its form of inflammable air.

In order to aſcertain this, I frequently revived dry calces of lead in dry inflammable air, and examined the appearances of moiſture afterwards. But notwithſtanding all the attention that I gave to the proceſs, I could not be abſolutely certain, whether more moiſture was left in the veſſel than might have exiſted *extraneouſly* in the inflammable air, or whether, when the phlogiſton was abſorbed, it left behind it any water that had been eſſential to it, as inflammable air. Appearances were ſuch as ſometimes inclined me to think that every thing which conſtitutes inflammable air goes into a calx, in order to form the metal; ſo that if this, though a compound thing, be called *phlogiſton*, it will ſtill be true that phlogiſton and inflammable air are the ſame thing; but, on the whole, I rather think that the water which was essential to the constitution of inflammable air was left behind.

That water, however, may exist in bodies in a *combined state*, without appearing to be water, we know in many cases; but it is in nothing more evident than in the *scales of iron*, than which no substance can have less the appearance of containing water.

But not to give a mere *opinion*, I shall recite the particulars of a few *experiments*, which I made with the view above-mentioned. In six ounce measures and a half of inflammable air from iron, I revived lead till it was reduced to one ounce measure and a half, care having been taken to make every thing as dry as possible. Some moisture, however, did appear, perhaps more than half a grain; but as this air had been confined by water, it was no more than might have been contained in it as an extraneous substance. It ought also to be considered, that it must be exceedingly difficult to expel all moisture by mere heat from such a powdery substance as the yellow calx of lead, without reviving the metal. All chemists well know how firmly moisture adheres to many substances, with which it does not properly *unite*, and how much heat is necessary to separate them.

Again, in six ounce measures and a half of inflammable air from iron, I revived lead till there remained 0,9 of a measure, and there was hardly any more moisture than I had reason to think might have

have been in the veſſel, independently of what was contained in the inflammable air; and in order to enable myſelf to judge of this, I melted an equal quantity of the ſame minium, under a dry glaſs veſſel with common air, when a little moiſture appeared on the inſide of the glaſs, about as much, I thought (for I could only judge by my eye) as when I had revived the lead from that minium in inflammable air. The quantity of lead revived was only ſixteen grains, but a good deal of the minium had been made black in the proceſs.

Laſtly, I expoſed ſome calx of lead to the heat of the lens in inflammable air, received immediately from the veſſel in which it was generated from iron and oil of vitriol, becauſe this contains leſs water than that which has been received in water and confined by it; and when ſix or ſeven ounce meaſures of the air were abſorbed, I could not ſuppoſe, from the appearance, that the water could be more than a quarter of a grain. However, when I repeated the experiment once more, I thought there might be about half a grain of water, which is more than I can well account for, without ſuppoſing that the water which was neceſſary to the conſtitution of inflammable air, and which I ſuppoſe to be about half its weight, was left behind when the pure phlogiſton revived the calx. This, therefore, is the

 opinion

opinion to which I am inclined; ſo that I do not think that any water enters into the conſtitution of any of the metals.

SECTION II.

Inflammable Air from Charcoal and Iron, &c. by Means of Steam.*

EVER ſince the diſcovery of the diminution of reſpirable air in thoſe proceſſes which are generally called *phlogiſtic*, it has been a great object with philoſophers to find what becomes of the air which diſappears in them.

Mr. Cavendiſh was of opinion, that when *air* is decompoſed, *water* only is produced; and Mr. Watt concluded from ſome experiments, of which I gave an account to the Royal Society, and alſo

* This ſection (which was an article in the Philoſophical Tranſactions, Vol. 75, p. 279) might have been introduced into Part I. which treats of the production of inflammable air; but as it likewiſe proves the compoſition of it from *water* and *phlogiſton*, it will, upon the whole, ſtand better in this connexion.

from

from ſome obſervations of his own, that water conſiſts of dephlogiſticated and inflammable air, in which Mr. Cavendiſh and M. Lavoiſier concur with him; but Mr. Lavoiſier is well known to maintain, that there is no ſuch thing as what has been called *phlogiſton*; affirming inflammable air to be nothing elſe but one of the elements or conſtituent parts of water.

Such were the hypotheſes to which I had a view, when I began the following courſe of experiments, which I hope will be an admonition to myſelf, as well as to others, to adhere as rigorouſly as poſſible to *actual obſervations*, and to be extremely careful not to overlook any circumſtance that may poſſibly contribute to any particular reſult. I ſhall have occaſion to notice my own miſtakes with reſpect to *concluſions*, though all the *facts* were ſtrictly as I have repreſented them. But whilſt philoſophers are faithful narrators of what they obſerve, no perſon can juſtly complain of being miſled by them; for to *reaſon* from the facts with which they are ſupplied, is no more the province of the perſon who diſcovers them, than of him to whom they are diſcovered.

I had tranſmitted the vapour of ſeveral fluid ſubſtances through red hot *earthen tubes*, and thereby procured different kinds of air. M. Lavoiſier adopted the ſame proceſs, but uſed an *iron tube*;

and by means of that circumſtance made a very valuable diſcovery which had eſcaped me. I had indeed, on one occaſion made uſe of an iron tube, and tranſmitted ſteam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myſelf with obſerving that *water*, after being made red hot, was ſtill water, there being no change in its ſenſible properties. Being now farther inſtructed by the experiment of M. Lavoiſier, I was determined to repeat the proceſs with all the attention I could give to it; but I ſhould not have done this with ſo much advantage, if I had not had the aſſiſtance of Mr. Watt, who always thought that M. Lavoiſier's experiments by no means favoured the concluſion that he drew from them. As to myſelf, I was a long time of opinion that his concluſion was juſt, and that the inflammable air was really furniſhed by the water being decompoſed in the proceſs. But though I continued to be of this opinion for ſome time, the frequent repetition of the experiments, with the light which Mr. Watt's obſervations threw upon them, ſatisfied me at length that the inflammable air came from the charcoal, or the iron.

I ſhall firſt relate the reſult of the experiments that was made with *charcoal*, and then thoſe with iron and other ſubſtances, in contact with which

(when

(when they were in a ſtate of fuſion, or at leaſt red hot) I made ſteam, or the vapour of other liquid ſubſtances, to paſs. I ſhall only obſerve that, previous to this, I began to make the experiments with coated glaſs tubes, which I found to anſwer very well during the proceſs, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. Lavoiſier diſcovered, ſteam had no effect; and at laſt I made uſe of earthen tubes, with which Mr. Wedgwood, that moſt generous promoter of ſcience, liberally ſupplied me for the purpoſe; and theſe, glazed on the outſide only, I find far preferable to copper.

The diſpoſition of the apparatus, with which theſe experiments were made, was as follows. The water was made to boil in a glaſs retort, which communicated with the copper or earthen tube that contained the charcoal or iron, &c. and which, being placed in an horizontal poſition, was ſurrounded with hot coals. The end of this tube oppoſite to the retort communicated with the pipe of a common *worm tub*, ſuch as is generally uſed in diſtillations, by means of which all the ſuperfluous ſteam was condenſed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was tranſmitted into a trough of water,

water, where proper veſſels were placed to receive it, and aſcertain the quantity of it; after which I could examine the quality of it at leiſure*.

In the experiment with *charcoal*, I found unexpected difficulties, and conſiderable variations in the reſult; the proportion between the *charcoal* and *water* expended, and alſo between each of them and the *air* produced, not being ſo nearly the ſame as I imagined they would have been. Alſo the quantity of fixed air that was mixed with the inflammable air varied very much. This laſt circumſtance, however; ſome of my experiments may ſerve to explain. Whenever I had no more water than was ſufficient for the production of the air, there was never any ſenſible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the caſe when I produced the air by means of a burning lens in an exhauſted receiver, and alſo in an earthen retort with the application of an intenſe heat. I therefore preſume, that when the ſteam tranſmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwiſe have been. The extremes that I have obſerved in the proportion of the fixed to the in-

* The diſpoſition of this apparatus may be ſeen Pl. VII. fig. 2.

flammable

flammable air have been from one twelfth to one fifth of the whole. As I generally produced this air, the latter was the usual proportion; and this was exclusive of the fixed air that was intimately combined with the inflammable air, and which could not be separated from it except by decomposition with dephlogisticated air; and this combined fixed air I sometimes found to be one third of the whole mass, though at other times not quite so much.

To ascertain this, I mixed one measure of this inflammable air from charcoal (after the uncombined fixed air had been separated from it by lime water) with one measure of dephlogisticated air, and then fired them by the electric spark. After this I always found that the air which remained made lime water very turbid, and the proportion in which it was now diminished, by washing in lime water, shewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this process, is evident from there being no fixed air found after the explosion of dephlogisticated air and inflammable air from iron*.

* When I wrote this paper, I imagined that the *fixed* air, which was found on the decomposition of this inflammable air with dephlogisticated air, had been contained in the inflammable air. But it will appear, that it must have been formed by the union

Notwithſtanding the above-mentioned variations, the loſs of weight in the charcoal was always much exceeded by the weight of the water expended; which was generally more than double that of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever depoſited from it.

The experiment which, upon the whole, gave me the moſt ſatisfaction, and the particulars of which I ſhall therefore recite, was the following. Expending ninety four grains of perfect charcoal (by which I mean charcoal made with a very ſtrong heat, ſo as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce meaſures of air, one fifth of which was fixed air, and of the inflammable part nearly one third more appeared to be fixed air by decompoſition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have aſcertained the quantity of it) conſiſting of fixed and inflammable air together, I found ſome variations in its ſpecific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of fourteen

union of phlogiſton (or inflammable air) and dephlogiſticated air, made by the exploſion; though it is remarkable that no fixed air is formed when the inflammable air from iron is uſed.

grains

grains to forty ounce meaſures is pretty near the truth, when the proportion of fixed air is about one fifth of the whole. With reſpect to the weight of the inflammable air after the fixed air was ſeparated from it, I found no great difference, and think it may be eſtimated at eight grains to thirty ounce meaſures.

Upon theſe principles, the whole weight of the 840 ounce meaſures of air will be 294 grains

that of the charcoal will be	94
that of the water - -	240
	334

which, conſidering the nature of the experiment, will perhaps be thought to be tolerably near to that of the air.

If the air be analyzed, the 840 ounce meaſures will be found to contain

	168 of uncombined fixed air	=151 grains.
and	672 impure inflammable	=179
ſo that the whole	840 will weigh - -	330

It may, however, be ſafely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itſelf may be very well ſuppoſed to have ſupplied.

There is, therefore, no reaſon for deſerting the old eſtabliſhed hypotheſis of *phlogiſton* on account of theſe experiments, ſince the fact is by no means inconſiſtent with it. The pure inflammable air, with the water neceſſarily contained in it, would weigh no more than about thirty grains, while the loſs of weight in the charcoal was ninety four grains. But to this muſt be added the phlogiſton contained in 392 ounce meaſures of fixed air, which, according to Mr. Kirwan's proportion, will be nearly ſixty five grains, and this and the thirty grains will be ninety five grains.

The baſis to this fixed air, as well as to the inflammable, muſt have been furniſhed by the *water*; and I afterwards found that water is about one half of the weight of fixed air.

Before I conclude my account of the experiments with charcoal, I would obſerve, that there is another on which I place ſome dependence, in which, with the loſs of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce meaſures of air, of which the laſt portion (for I did not examine the reſt) contained one ſixth part of uncombined fixed air. This was made in an earthen tube glazed on the outſide.

The experiments with *iron* were more ſatisfactory than thoſe with charcoal, being ſubject to leſs variation; and they by no means require us to ſuppoſe that the inflammable air comes from the *water*, but only from the *iron*, as the quantity of water

water expended, deducting the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this process is between one third and one half more than can be procured from iron by a solution in acids, the reason may be, that much phlogiston is retained in the solutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would farther observe, that the produce of air, and also the addition of weight gained by the iron, are much more easily ascertained in these experiments than the quantity of water expended in them, on account of the great length of the vessels used in the process, and the different quantities that may perhaps be retained in the worm of the tub; though I did not fail to use all the precautions that I could think of, to guard against any variation on these accounts.

Of the many experiments, which I made with *iron*, I shall content myself with reciting the following results. With the addition of 267 grains to a quantity of iron, and the loss of 336 grains of water, I procured 840 ounce measures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the consumption of 254 grains of water, I got 420 ounce meaſures of air *.

The inflammable air produced in this manner is of the lighteſt kind, and free from that very *offenſive ſmell* which is generally occaſioned by the rapid ſolution of metals in oil of vitriol, and it is extricated in as little time in this way as it is poſſible to do it by any mode of ſolution. On this account it occurred to me, that it muſt be by much the cheapeſt method that has yet been uſed of filling *balloons* with the lighteſt inflammable air. For this purpoſe it will be proper to make uſe of caſt iron cylinders of a conſiderable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itſelf will contribute to the production of air, and therefore may

* If the perfect accuracy of the former of theſe experiments may be depended on (and it may always be preſumed, that thoſe in which *little water* is expended are preferable to thoſe in which *more* is conſumed) the water that neceſſarily enters into this kind of inflammable air is about equal in weight to the *phlogiſton* that is in it.

The water expended was 336 grains, and the iron gained 267 grains. Suppoſing it to have loſt phlogiſton equal to half the weight of the inflammable air, viz. 840 ounce meaſures = 25 grains (the whole weight of that air being 50 grains) the water that really entered into the iron muſt be eſtimated at 292 grains (which is 267 + 25). This deducted from 336, leaves a remainder of 34, which is not much more than 25, or half the weight of the inflammable air.

become

become unfit for the purpose in time; yet, for any thing that I know to the contrary, the same tube may serve for a very great number of processes, and perhaps the change made in the inside surface may protect it from any farther action of the water, if the tube be of sufficient thickness; but this can only be determined by experiment.

Having recommended this process as the cheapest and the most convenient for filling balloons, especially when tubes of cast iron should be made use of; I was willing to make a trial of one, in order to form some judgment how long they would last for the purpose. I therefore procured one of an inch and a quarter in diameter, and not more than a quarter of an inch in the thickness of the metal; and making the middle part of it red hot, sent steam through it; and from the result of four or five processes with the same tube, I have little doubt, but that, if they were made of the thickness of half an inch in the metal, and care was taken to coat them on the outside with clay and sand, the same tube might probably serve twenty times. That the reader may form some judgment as well as myself, I shall mention the result of my observations.

I heated the tube four or five different times, and in each process transmitted as much water through it as would have been more than sufficient to decompoſe all the iron that it could have contained. At firſt four ounce meaſures of water procured 180 ounce meaſures of inflammable air, and then ſix ounce meaſures procured only 160 ounce meaſures. I then examined the tube, and found that when both the inſide and outſide were well ſcraped with a ſharp inſtrument, it had loſt twenty five grains in weight. The ſcales from the outſide weighed 282 grains, while all that I could get from the inſide weighed only thirty ſix grains. Conſequently the tube had gained in weight 283 grains. After this I heated it again, and tranſmitted through it ſix more ounces of water, which yielded only ſixty ounce meaſures of air.

From theſe experiments it may be inferred, that the tube would ſoon have ceaſed to give any air; the inſide being changed to ſome depth by the action of the ſteam, and yet it was not much diſpoſed to exfoliate. In time it would, no doubt, have become brittle, and might be in danger of breaking, from its diſpoſition to *bend* in the courſe of the proceſs. This bending was very conſiderable; but did not ſeem to ariſe from any tendency in the iron to *melt*. Perhaps by turning it in cooling, this bending, and conſequently the danger of cracking after much uſe, might be prevented; or this property of bending might in a great meaſure

ceaſe,

ceaſe, when the metallic ſtate of the tube was deſtroyed; and yet with care might continue a firm and compact tube, and as fit for this proceſs as at the firſt. If this ſhould be the caſe (which experience alone can determine) it is not to ſay how long a tube of this kind might laſt. It would then be a kind of *earthen tube*, of the moſt perfect nature, completely air tight, without being ſubject to ruſt or decay.

Upon the whole, ſhould the fondneſs for balloons be reſumed, I ſee no reaſon why far the greateſt part of the expence attending the filling of them might not be ſaved by means of this proceſs. A complete apparatus for it would not coſt half ſo much as the filling of a ſingle balloon, that would carry a man, in the common way, and would ſerve at leaſt a conſiderable number of times, with the expence of a very few pounds each time; as there would be hardly any thing to pay for beſides *fire* and *attendance*, for a few hours. For ſuch *iron* as would beſt anſwer for this purpoſe might, in moſt places, be had for a mere trifle. One apparatus, conveniently fixed, might ſerve for a whole town or neighbourhood,

Some eſtimate of what may be expected from this method of procuring inflammable air may be formed from the following obſervations. About twelve inches in length of a copper tube, three fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpofe than *iron filings*, as they do not lie fo clofe, but admit the fteam to pafs through their interftices) when it was heated, and a fufficient quantity of fteam paffed through it, yielded thirty ounce meafures of air in fifty feconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the fame manner, gave two hundred ounce meafures in one minute and twenty five feconds; fo that this larger tube gave air in proportion to its folid contents compared with the fmaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates fo readily to fome diftance, the rate of giving air will always be in a greater proportion than that of the fimple diameter of the tube.

The following experiment was made with a view to afcertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when diffolved in acids, will yield about 800 ounce meafures of air; but treated in this manner it yielded 1054 ounce meafures, and then the iron had gained 329 grains in weight, which is little fhort of one third of the weight of the iron.

Considering how little this inflammable air weighs, viz. the whole 1054 ounce measures not more than sixty three grains, and the difficulty of ascertaining the loss of water to so small a quantity as this, it is not possible to determine, from a process of this kind, how much water enters into the composition of the inflammable air of metals. It would be more easy to determine this circumstance with respect to the inflammable air of charcoal, especially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce measures of inflammable air, which, in the proportion of thirty ounce measures to eight grains, will weigh 3.3 grains; so that water in the composition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be some difficulty with respect to the fixed air intimately combined with this kind of inflammable air.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the same result when I made use of small nails of *cast iron*, except that these were firmly fastened together after the experiment, the surfaces of them being crystallized, and the crystals mixed with each other, so that it was with great difficulty that they could be got out of the tube after the experiment; and in general the solid parts of the nails were broken before they were ſeparated from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means ſo firmly.

Caſt iron annealed (by being kept red-hot in charcoal) is remarkably different from the caſt iron which has not undergone that operation; eſpecially in its being, to an extraordinary degree, more ſoluble in acids. With the turnings of annealed caſt iron I made the following experiment. From 960 grains of this iron, and with the loſs of 480 grains of water, I got 870 ounce meaſures of inflammable air, and tranſmitting ſteam through them a ſecond time, I got 150 ounce meaſures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were eaſily broken and got out of the tube, whereas it had required a long time, and a ſharp ſteel inſtrument, to clear the tube of the caſt-iron nails.

Having made another experiment with *iron*, with as much attention as I could give to it, I ſhall in the firſt place mention *that*. From two ounces of iron turnings (which is caſt iron annealed) I got in the firſt inſtance 985 ounce meaſures of air, with the loſs of 528 grains of water; and the iron, I found, had gained 292 grains in weight. Then making four ounces of water of this reſiduum paſs over the ſame iron, it gained four grains more, and all

all the air that I procured was 998 ounce meaſures. So that (as extreme accuracy is not to be obtained in proceſſes of this kind) it may be ſaid that two ounces of this kind of iron will, in this way, yield 1000 ounce meaſures of air; whereas by ſolution in vitriolic acid, it would have yielded about 800. Conſequently more air by two fifths may be procured by this new mode of treatment, which alone ſhould recommend it to thoſe who fill balloons.

Having procured water from the ſcales of iron (by heating them in inflammable air) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I ſhould be able to produce the ſame effect by heating it with charcoal in a retort; and I had likewiſe no doubt but that I ſhould be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of theſe conjectures I was right; but with reſpect to the latter, I was totally miſtaken.

Having made the ſcales of iron, and alſo the powder of charcoal very hot, previous to the experiment, ſo that I was ſatisfied that no air could be extracted from either of them ſeparately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in

which

which I could give it a very ſtrong heat; and connected with it proper veſſels to condenſe and collect the water which I expected to receive in the courſe of the proceſs. But, to my great ſurpriſe, not one particle of *moiſture* came over, but a prodigious quantity of *air*, and the rapidity of its production aſtoniſhed me; ſo that I had no doubt but that the weight of the air would have been equal to the loſs of weight both in the ſcales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air; and the inflammable air, which remained when the fixed air was ſeparated from it, was of a very remarkable kind, being quite as heavy as common air. The reaſon of this was ſufficiently apparent when it was decompoſed by means of dephlogiſticated air; for the greateſt part of it was fixed air.

The theory of this proceſs I imagine to be, that the phlogiſton from the charcoal reviving the iron, the water with which it had been ſaturated, being now ſet looſe, affected the hot charcoal as it would have done if it had been applied to it in the form of *ſteam* as in the preceding experiments; and therefore the air produced in theſe two different modes have a near reſemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the caſes I found theſe proportions ſubject to variations. In

one

one process with charcoal and scales of iron, the first produce contained one fifth of uncombined fixed air, the middle part one tenth, and the last none at all. But in all these cases the proportion of combined fixed air varied very little.

Why *air* and not *water* should be produced in this case, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to understand. There is, indeed, an obvious difference in the circumstances of the two experiments; as in that with charcoal the phlogiston is found in a combined state; whereas in that of inflammable air, it is loose, or only united to water; and perhaps future experiments may discover the operation of this circumstance*.

* This experiment seems to be decisive against the hypothesis of Mr. Lavoisier, and others, who say that the inflammable air procured by means of iron and charcoal, comes from the water, and who think that by this means they can exclude phlogiston. For, according to them, neither the scales of iron, nor the charcoal, contain phlogiston, or any thing from which inflammable air can be made, but are merely substances capable of imbibing pure air, and thereby setting at liberty the inflammable air contained in the water; supposing the scales of iron to have been only iron saturated with dephlogisticated air. But had this been the case, there was nothing in either of the materials made use of in this experiment from which the inflammable air could possibly come, there being no *water* contained in either of them. But supposing the reality of phlogiston, and its constituting a part of metals, of charcoal, and of inflammable air, the experiment is very intelligible.

After

After having tranſmitted ſteam in contact with *charcoal* and *iron* in a copper tube, I propoſed to do the ſame with other ſubſtances containing phlogiſton, and I began with *bones*, which were burned black, and had been ſubjected to an intenſe heat, covered with ſand, in an earthen retort. From three ounces of bones thus prepared, and treated as I had done the charcoal, I got 840 ounce meaſures of air, with the loſs of 288 grains of water. The bones were by this means made perfectly white, and had loſt 110 grains of their weight. As the air ceaſed to come a conſiderable time before all the water had been tranſmitted through the tube containing them, I concluded that the air was formed from the phlogiſton contained in the bones, and ſo much water as was neceſſary to give it the form of air.

This air differs conſiderably from any other kind of inflammable air, being in ſeveral reſpects a medium between that from charcoal and that from iron. It contains about one fourth of its bulk of uncombined fixed air, but not quite one tenth intimately combined with the remainder. The water that came over was blue, and pretty ſtrongly alkaline, which muſt have been occaſioned by the volatile alkali not having been intirely expelled from the bones in the former proceſs, and its having in part diſſolved the copper of the tube in which the experiment was made.

I ſub-

I subjected to the same process a variety of substances that are said not to contain phlogiston, but I was never able to procure inflammable air by means of them; which strengthens the hypothesis of the principal element in the constitution of this air having been derived from the substance supposed to contain phlogiston, and therefore that phlogiston is a real substance, capable of assuming the form of air by means of water and heat.

SECTION III.

Of the Action of Steam on various Substances in a red Heat.

HAVING procured inflammable air by sending steam over red hot *iron*, I afterwards extended the same process to other substances, and as most of those contained phlogiston, and yielded inflammable air, I shall recite them in this place.

Having been able to decompose iron by converting it into scales, I also found that in this way, I could readily procure *flowers of zinc*, as well as inflammable air from that metal. The flowers came over

over in a very attenuated ſtate, the air being loaded with them. It might be poſſible, however, to contrive an apparatus to collect them.

Braſs being made with a mixture of zinc and copper, and *zinc* being ſo eaſily decompoſed by ſteam, whenever a copper tube is ordered for the purpoſe of theſe experiments, particular care ſhould be taken that there be no mixture of braſs in it, though it is difficult to have copper caſt ſmooth and ſolid without a mixture of either braſs or tin, which is not much better. Even pure copper tubes become brittle, and at length crack in theſe experiments.

Having at one time been perſuaded to have a little braſs mixed with the copper in one of theſe tubes, I conſented that the ſmalleſt quantity that could be ſuppoſed to be neceſſary to make the tube compact, ſhould be put into it. But notwithſtanding this, and though the tube had a quarter of an inch thickneſs of metal, it fell to pieces in the very firſt experiment, in which I ſent the ſteam of no more than two or three ounces of water through it. Inflammable air was produced very copiouſly, and the flowers of zinc were mixed with it; ſo readily did the ſteam ſeparate the zinc from the copper, though the heat was only ſufficient to make the tube red hot, and was far from melting it.

Lead

Lead would probably be far leſs affected than copper in theſe experiments; but then it will not bear a red heat without melting. I made the ſteam of about four ounces of water paſs over four ounces of melted lead, in an earthen tube, with hardly any ſenſible effect. The loſs of water was not more than 0.2 of an ounce meaſure.

After uſing *charcoal* in the experiments recited in the preceding ſection, I went through one proceſs with *coak*, or the cinder of pit coal, and found that from 174 grains of coak, and with the loſs of 528 grains of water, I got 1700 ounce meaſures of air, of which one fifth was fixed air, and thirty ounce meaſures of it weighed ten grains leſs than an equal bulk of common air. The analyſis of this air will be found in the ſection appropriated to that ſubject.

On *iron ore* this proceſs had no effect. The ſame was the caſe with the trial I made of *quick lime*, and ſuch would probably be the caſe with dephlogiſticated earths in general.

With *mangane*ſ*e*, however, the reſult was different. Having made the ſteam of four ounces of water paſs over 828 grains of this ſubſtance, which had been expoſed to a ſtrong heat in an earthen retort ſome time before, I got thirty five ounce meaſures of air, almoſt the whole of which was pure fixed air, with a reſiduum a little better than com-

mon

mon air. The manganese had lost 132 grains, and from being *black*, was become very *brown*. Again, I transmitted the steam of eight ounces of water over two ounces and a half of manganese, and got about 100 ounce measures of pure fixed air, with a residuum a little phlogisticated. The manganese had lost 112 grains.

SECTION IV.

Whether inflammable or nitrous Air contain more Phlogiston.

IT is well known that both nitrous and inflammable air contain phlogiston, but in very *different states*, because their specific gravities, and other properties, are most remarkably different. Many schemes have occurred to me to ascertain the proportion of phlogiston that each of them contains; and at length I thought of attempting the solution of this problem, by burning inflammable air in a given quantity of common air. For though inflammable air will not part with its phlogiston to common air when *cold*, it will, like other combustible

tible ſubſtances, when heated to a certain degree. It is then decompoſed, and the phlogiſton that entered into its compoſition phlogiſticates the air in which it is burned; and the degree of phlogiſtication may be meaſured by the teſt of nitrous air. I, therefore, proceeded as follows.

In an eight ounce phial, containing many nails, and a quantity of water with oil of vitriol, I produced inflammable air; and making it burn with a ſmall flame, at the orifice of a glaſs tube through which the air was tranſmitted (being cemented into the cork of the phial) I covered the flame with a receiver that contained twenty-one ounce meaſures of air, ſtanding in water. After ſix minutes, the flame went out; when, immediately catching the air that was produced in the next ſix minutes, and alſo in the ſix minutes following, I concluded that ſeven ounce meaſures had been produced, and decompoſed, during the ſix minutes in which it had continued to burn.

Then examining the air in which it had burned, I found it ſo far phlogiſticated, that equal meaſures of it and of nitrous air occupied the ſpace of 1.65 meaſures; and common air mixed with one third as much nitrous air, being again mixed in equal proportions with the ſame freſh nitrous air, occupied the ſpace of 1.68 meaſures. It appeared, therefore, that the twenty one ounce meaſures of air, having received the phlogiſton of one third as much inflammable air, *viz.* ſeven ounce meaſures, was about as much phlogiſticated as it would have been with a mixture of the ſame proportion of nitrous air. Conſequently, equal meaſures of nitrous and inflammable air contain about equal quantities of phlogiſton.

Of this curious problem, however, I have obtained a more accurate ſolution from the mode of experimenting introduced by that excellent philoſopher Mr. Volta; who fires inflammable air in common air, by the electric ſpark, and conſequently can determine the exact proportion of the inflammable air decompoſed in a given quantity of common air. The reſult of this proceſs agreeing with that of the former, leaves little doubt with reſpect to the concluſion I have drawn from them.

Having prepared a ſtrong glaſs tube, in one end of which I had cemented a piece of wire, I filled it with water, and introduced into it another piece of wire, ſo as to come within about half an inch of the former wire, that an electric exploſion might eaſily paſs between them.

Into this tube, thus prepared, I transferred, in the firſt place, one meaſure of inflammable air, and three of common air; and then, by means of an electric exploſion between the wires, in the central place of the air, I fired all the inflammable air,

which

which would then be decomposed, and, of course, part with its phlogiston to the common air with which it was mixed. After the explosion, I accordingly found it to be completely phlogisticated. This also would have been the consequence of mixing the same proportion of nitrous air with the common air. But to determine the problem with accuracy, it was necessary to use such a proportion of inflammable as would only phlogisticate the common air in part.

I therefore mixed one measure of inflammable air with *three* measures of common air, and after the explosion found it to be so far phlogisticated, that one measure of this and one of nitrous air occupied the space of 1.8 measures; and this I also found, by the same test, to be exactly the state to which a mixture of one measure of the same nitrous air brought three measures of the same common air.

In order to obtain a farther confirmation of my conclusion, I mixed one measure of inflammable air with *four* measures of common air; and after the explosion I also found, by the test of nitrous air, that it was phlogisticated exactly as much as by the mixture of an equal quantity of nitrous air. And repeating the experiment with the same proportion of inflammable and common air, I found that after the explosion the air was diminished, without mixing with nitrous air, just as much as one meaſure of nitrous air diminiſhed four meaſures of common air, *viz.* from 7.4 to 5.2 meaſures.

Having ſince this given more attention to theſe experiments, I have ſeen reaſon to conclude that inflammable air from iron and water, contains more phlogiſton than nitrous air, in the proportion of *ten* to *nine*. For nine meaſures of inflammable air will diminiſh dephlogiſticated air as much as ten of nitrous air.

SECTION V.

The Analyſis of different Kinds of inflammable Air.

BEFORE I proceed to the analyſis of the different kinds of inflammable air, which is the ſubject of this ſection, I ſhall obſerve, that the pureſt we can procure (which is that from metals by ſolution in the mineral acids, or rather that by means of ſteam from red-hot iron) ſeems to conſiſt of phlogiſton and water, and that neither *acid* nor *alkali* is a neceſſary ingredient in it; though, when it is produced by means of either of them, a ſmall portion of either may be retained in it, as an extraneous ſubſtance. That this, however, is the caſe, has been very clearly ſhewn by Mr. Senebier, though I think that the production of inflammable air by means of iron and ſteam only, without either acid or alkali, ſufficiently proves that his hypotheſis of inflammable air neceſſarily acquiring ſome ſaline baſis, cannot be well founded.

It was, indeed, my own firſt opinion, that inflammable air conſiſts of acid and phlogiſton. Afterwards I adopted the opinion of Mr. Kirwan, viz. that it is pure phlogiſton in the form of air, but at preſent I am fully ſatisfied with the opinion of Mr. Cavendiſh, that *water* is an eſſential ingredient in the conſtitution of this kind of air.

That no acid is neceſſarily contained, or at leaſt in any ſenſible quantity, either in inflammable air, though produced by means of acids, or in the dephlogiſticated air of the atmoſphere, ſeemed to be evident from the following experiment, which I made with the greateſt care. Taking a baſon which contained a ſmall quantity of water tinged blue with the juice of turnſole, I placed in it a bent tube of glaſs, which came from a veſſel containing iron and diluted oil of vitriol; and lighting the current of inflammable air, as it iſſued from this tube, ſo that it burned exactly like a candle, I placed over it an inverted glafs jar; fo that the mouth of it was plunged in the liquor. Under this jar the inflammable air burned as long as it could, and when it was extinguifhed, for want of more pure air, I fuffered the liquor to rife as high as it could within the jar, that it might imbibe whatever fhould be depofited from the decompofition of either of the two kinds of air. I then took off the jar, changed the air in it, and lighting the ftream of inflammable air, replaced the jar as before. This I did till I had decompofed a very great quantity of the two kinds of air, without perceiving the leaft change in the colour of the liquor, which muft, I thought, have been the cafe if any acid had entered as a neceffary conftituent part into either of the two kinds of air. I alfo found no acid whatever in the water which was procured by keeping a ftream of inflammable air conftantly burning in a large glafs balloon, through which the air could circulate, fo that the flame did not go out.

With refpect to inflammable air itfelf, I have before obferved, that when fufficient care is taken to free it from any acid vapour that may be accidentally contained in it, it is not in the fmalleft degree affected by a mixture of alkaline air. On the whole, therefore, I have at prefent no doubt but that pure inflammable air, though it certainly contains *water*, does not neceffarily contain any acid. Yet an acid

vapour

vapour may be eafily diffufed through it, and may perhaps in many cafes be obftinately retained by it, as no kind of air feems to be capable of fo great a variety of impregnations as inflammable air is.

That there are different kinds of inflammable air, has been obferved by moft perfons who have made any experiments on air. That which has been moft commonly obferved is, that fome of them burn with what may be called a *lambent flame*, fometimes blue, fometimes yellow, and fometimes white, like the flame from wood or coals in a common fire; whereas another kind always burns with an *explofion*, making more or lefs of a report, when a lighted candle is dipped into a jar filled with it. Of the latter kind is that which is extracted from metals by means of acids, &c. and of the former kind is that which is expelled from wood, coal, and other fubftances by heat. It has alfo been obferved, that thefe kinds of inflammable air have different fpecific gravities, the pureft kind, or that which is extracted from iron, &c. being about ten times lighter than common air, but fome of the other kinds not more than twice as light.

The caufe of this difference I once thought I had difcovered to be the heavier kinds of inflammable air containing a proportion of *fixed* air, fo intimately combined with them, fo as not to be diſcoverable by lime water, while the lighteſt kind contained no fixed air at all. This hypotheſis I formed from decompoſing them with common or dephlogiſticated air, by the electric exploſion. For, after the experiment with the heavier kinds of inflammable air, I always found a quantity of fixed air in the reſiduum, but none at all after the experiment with the lighteſt kind.

In order to decompoſe any kind of inflammable air, I generally mix it with an equal quantity of dephlogiſticated air, and then confine them in a ſtrong glaſs veſſel, previouſly filled either with water or mercury, and I make an electric ſpark in ſome part of the mixture, by means of wires inſerted through the ſides of the veſſel, and nearly meeting within it. By this proceſs, I imagined, that I was able to aſcertain two things relating to the conſtitution of different kinds of inflammable air, viz. the quantity of *combined fixed air* (as I then thought it to be) and likewiſe the relative quantity of phlogiſton contained in each of them. The former appeared by waſhing the air with lime water after the exploſion, and obſerving how much of them was abſorbed, and the latter by examining the reſiduum with the teſt of nitrous air, and obſerving the purity of it. In moſt of theſe experiments I made uſe of dephlogiſticated air, in preference to common air, becauſe I could

not

not make ſome kinds of inflammable air to explode at all with common air. Otherwiſe I ſhould have preferred this, as being the moſt nearly of the ſame quality. However, I always noted the degree of purity of the dephlogiſticated air that I made uſe of before I began any of theſe analyſes.

Finding, however, that in ſome caſes more fixed air was found after the exploſion of the two kinds of air, than could poſſibly have been *contained* in the inflammable air, on account of the weight of it, I was ſatisfied that there muſt have been a real *generation* of it, by an union of the inflammable and dephlogiſticated air. It is remarkable, however, that ſome kinds of inflammable air ſhould ſo readily unite with dephlogiſticated air, ſo as to make a conſiderable quantity of fixed air; and that others, treated in the ſame manner, ſhould not do this at all; and alſo that thoſe which do it ſhould be the heavier kinds of inflammable air. This is a new and curious ſubject of inveſtigation.

The pureſt and lighteſt inflammable air is that which is extracted from iron, and other metals, by a ſolution in the acids, or by means of ſteam. One meaſure of this kind of air, and one of dephlogiſticated (ſuch as that, when mixed with two equal quantities of nitrous air, there remained 0.72 of a meaſure) exploded together in the manner deſcribed above, were reduced to 0.6 of a meaſure,

ſure, no fixed air was found in the reſiduum, and when examined with an equal quantity of nitrous air, was reduced to 0.87 of a meaſure.

With the ſame dephlogiſticated air I examined inflammable air that had been got from a mixture of finery cinder and charcoal, and found, that after the exploſion, the two meaſures were reduced only to 1.85, but that by waſhing in the lime water, they were reduced to 1.2. Conſequently 0.65 of a meaſure of fixed air had been generated in the proceſs. When this was ſeparated from it, and the remainder examined by nitrous air, it appeared to be of the ſtandard of 0.9; ſo that the dephlogiſticated air had been more injured by this than by an equal quantity of the common inflammable air, though the difference in this reſpect was not conſiderable.

In another proceſs with this kind of inflammable air, the diminution after the exploſion was to 1.55, and that after the waſhing in lime water to 0.65 of a meaſure; ſo that there had been a generation of 0.9 of a meaſure of fixed air. In another experiment the firſt diminution was to 1.6, and the ſecond 0.66, ſo that 0.94 of a meaſure of fixed air had been produced. And laſtly, in another proceſs, the firſt diminution was to 1.6, and the ſecond to 0.6 of a meaſure; ſo that there was a generation of one complete meaſure of fixed air,

and

and this was a clear proof that it could not have been *contained* in a combined ſtate, as I at firſt imagined, in the inflammable air; ſince then it muſt have been much heavier than I had ever found it to be; for, though I found the ſpecific gravity of it to be ſomething different at different times (and the preceding experiments were made with the air of different proceſſes) I had never found that forty ounce meaſures of this air was more than two grains heavier than an equal bulk of common air.

This, indeed, is a remarkable circumſtance with reſpect to a ſpecies of inflammable air, as it does not appear by the teſt of lime water to contain any fixed air; but it ought to have weighed more than one half heavier than common air, to have actually contained in combination all the fixed air that I found after its exploſion with the dephlogiſticated air. Indeed, if any quantity of inflammable air, of about the ſame ſpecific gravity with common air (which is the caſe with that ſpecies of it which I am now conſidering) yield ſo much as ſeven tenths of its bulk of fixed air, in conſequence of its exploſion with dephlogiſticated air, it is a proof that at leaſt part of that fixed air was generated in the proceſs, becauſe ſeven tenths of ſuch fixed air would weigh more than the whole meaſure of the inflammable air.

Inflammable air from *ſpirit of wine* (made by tranſmitting it *in vapour* through a red hot earthen tube) being analized in the manner above-mentioned, one meaſure of it, and one of the ſame dephlogiſticated air that was uſed in the former experiment, were reduced to one meaſure, and by waſhing in lime water to 0.6 of a meaſure; ſo that four tenths of its bulk of fixed air had been generated in the proceſs. The ſtandard of the reſiduum was 1.7; ſo that the dephlogiſticated air had been injured much more than in either of the former proceſſes, and conſequently it muſt have contained more phlogiſton.

I found conſiderable variations in the experiments with this, as well as with ſome other kinds of inflammable air. For, in another proceſs, in which the earthen tube had been filled with bits of crucibles (in order to expoſe more red ſurface to the vapour of the ſpirit of wine) the firſt diminution was to 1.6, the ſecond to 1.4; and the ſtandard of the reſiduum was 1.84. In another proceſs with this kind of air, the firſt diminution was to 1.2, and the ſecond to 0.9.

Having procured a quantity of inflammable air, by tranſmitting ſteam over red hot *platina*, I analized it in the ſame manner, and found that the two meaſures were reduced by the exploſion to 0.72. It contained no fixed air, and the reſiduum was of the ſtandard of 0.9.

Inflam-

Inflammable air, procured by making ſteam paſs over melted brimſtone, being examined in the ſame manner, the firſt diminution was to 0.6, and no fixed air was found in it. In this reſpect it ſeems to have been the ſame thing with inflammable air from iron, but the ſtandard of the reſiduum was 0.95; ſo that it ſeems to have contained more phlogiſton. But as the quantity of this air was not great, it probably contained a mixture of other air.

Inflammable air procured in the ſame manner from melted *arſenic*, appeared to be very different from that which was extracted from brimſtone. For the two meaſures were reduced by the exploſion to 1.15, and by waſhing with lime water, to 0.95; ſo that one fifth of its bulk of fixed air had been generated. The ſtandard of the reſiduum was 0.82.

At the ſame time I examined ſome inflammable air, that had been made by heating bits of crucibles in *alkaline air*, and found that the two meaſures were reduced by the exploſion to 0.96 of a meaſure, that the reſiduum contained no fixed air, and was of the ſtandard of 0.8.

The inflammable air that is made from *æther*, by tranſmitting the vapour of it through a red hot earthen tube, very much reſembles that which is got from ſpirit of wine. The two meaſures were reduced by the exploſion to 1.36, and by waſhing in water to 1.2; ſo that 0.16 of a meaſure of fixed air had been generated, and the reſiduum was of the ſtandard of 1.9.

Inflam-

Inflammable air procured by tranſmitting ſteam over red hot *charcoal of metals*, in the ſame manner as it is got from other charcoal, produces a conſiderable quantity of fixed air. For when the experiment was made with this air, the firſt dimunition was to 1.12, and the ſecond to 0.8; ſo that 0.32 of a meaſure of fixed air was generated, and the ſtandard of the reſiduum was 1.9. This analyſis was of the firſt portion that came in the proceſs. The ſecond was ſomething different. For with this, the firſt dimunition was to 1.0, and the ſecond to 0.75, the reſiduum being the ſame as before, viz. 1.9. Thirty ounce meaſures of this air weighed eight grains leſs than an equal bulk of common air.

Analizing the inflammable air from *coak*, or the charcoal of pitcoal produced by ſteam, the firſt diminution was to 1.15, and the ſecond to 0.95; ſo that one fifth of its bulk of fixed air was generated. The ſtandard of the reſiduum was 1.9. But I muſt obſerve, that the dephlogiſticated air with which this experiment was made, was ſo impure as hardly to deſerve the name. For two meaſures of nitrous air and one of this, occupied the ſpace of two meaſures. But this circumſtance may not affect the quantity of fixed air generated in the proceſs. Thirty ounce meaſures of this air weighed ten grains leſs than an equal bulk of common air.

Analizing the inflammable air that was produced in the ſame manner from *ſpirit of turpentine*, the firſt diminu-

diminution was to 1.7, and the ſecond to 1.6; ſo that only one tenth of fixed air was produced. The reſiduum was of the ſtandard of 1.9. Thirty ounce meaſures of this air weighed eight grains leſs than an equal bulk of common air.

When the vapour of ſpirit of wine was made to paſs over melted metals, inflammable air was produced from it juſt as if no metals had been concerned; but when I examined the air that was procured in this manner, it did not appear to be quite the ſame with that which came from pure ſpirit of wine. Analyzing the air that was produced in a proceſs with *copper*, the firſt diminution was to 1.7, the ſecond to 1.56, and the ſtandard of the reſiduum was 1.78. Thirty ounce meaſures of this air weighed ſeven grains leſs than an equal bulk of common air.

In another proceſs with inflammable air, procured in this manner, the firſt diminution was to 1.55, the ſecond to 1.48, and the reſiduum was of the ſtandard of 1.86. Thirty ounce meaſures of this air weighed eight grains and a half leſs than an equal bulk of common air. This air, it is obſervable, produced much leſs fixed air than the other, and it was alſo ſpecifically lighter than it.

When this proceſs was made with the air procured by tranſmitting vapour of ſpirit of wine over melted *ſilver*, the firſt diminution was to 1.9, the ſecond

ſecond to 1.78, and the ſtandard of the reſiduum was 1.9. Thirty ounce meaſures of this air weighed eight grains leſs than an equal bulk of common air.

In the analyſis of the air procured by this proceſs from *lead*, the firſt diminution was to 1.78, the ſecond to 1.6, and the ſtandard of the reſiduum was 1.78.

I examined, at the ſame time, inflammable air procured from *bones*, and alſo from *charcoal*, viz. by tranſmitting ſteam over them when they were red hot in earthen tubes, after all air had been previouſly expelled from them by heat. With the former, the firſt diminution was to 0.67, and the ſecond to 0.58; ſo that the fixed air produced was extremely inconſiderable, viz. only 0.09 of an ounce meaſure. The ſtandard of the reſiduum was 1.47. In the experiment with the air from *charcoal*, the firſt diminution was to 1.5, and the ſecond to 0.74; ſo that the fixed air was 0.76, and the ſtandard of the reſiduum was 1.7. From this experiment it may be inferred, as mentioned before, that inflammable air from *bones*, is a kind of medium between that from *metals*, and that from *charcoal*. In another proceſs with air from charcoal, the firſt diminution was to 0.82, and the ſecond to 0.63, and the ſtandard of the reſiduum was 1.37.

I made

I made the following experiment to aſcertain how much phlogiſton is contained in inflammable air from charcoal. In five ounce meaſures of this kind of air, I revived lead from maſſicot till it was reduced to three fourths of an ounce meaſure, when the lead revived weighed ten grains, and there remained one ounce meaſure of fixed air. But the minium itſelf yielded a little fixed air.

It is obſervable, that when wood is heated in an earthen retort, the firſt air that comes over is conſiderably different from that which comes in the middle, or at the end of the proceſs. Indeed the properties of it are continually changing during the whole proceſs. The firſt portion burns with a lambent white flame, like that from burning wood in an open fire; afterwards the flame is blue, and towards the end of the proceſs it is conſiderably exploſive, almoſt like air from iron. Alſo the air that comes over firſt is very turbid, owing perhaps, to oily, and other matters, that are rendered volatile by heat.

Having procured air from dry beech wood, I examined, in the method deſcribed above, the firſt portion of it, and alſo one of the middle ones. The former I found to contain four tenths and a half of its bulk of uncombined fixed air, the ſecond portion only two tenths. Afterwards it is well known that air procured in this manner ceaſes to have any uncombined fixed air in it.

When I examined the firſt portion of air, after the uncombined fixed air had been ſeparated from it, the firſt diminution was to 1.36, and the ſecond to 0.9; ſo that 0.46 of a meaſure of fixed air was generated, and the ſtandard of the reſiduum was 1.9. When the ſecond portion was examined, after the uncombined fixed air was likewiſe ſeparated from it, the firſt diminution was to 1.66, and the ſecond to 1.46; ſo that the fixed air generated in the proceſs, was 0.2 of a meaſure, that is, leſs than in the former experiment, and in nearly the ſame proportion as the uncombined fixed air had been. The ſtandard of the reſiduum in this laſt caſe was 1.15. At the ſame time, repeating the experiment with the ſame dephlogiſticated air and inflammable air from iron, the diminution after the exploſion was to 0.55, and the ſtandard of the reſiduum was 1.48, which is the uſual reſult of the decompoſition of inflammable and dephlogiſticated air, when both of them are as pure as they are generally procured.

There was a great quantity of fixed air produced by the decompoſition of ſome inflammable air extracted from ſome rich *mould* in a gun barrel, which Mr. Young was ſo obliging as to ſend me.

It

It burned with a lambent blue flame, and had a peculiarly offenfive fmell, the fame, as he obferves to me, that is yielded by air procured from putrid vegetables. Of this air one twentieth part is uncombined fixed air. When this was feparated from it, and the remainder decompofed with dephlogifticated air, the firft diminution was to 1.4, and the fecond to 0.67; fo that there was a generation of 0.73 of a meafure of fixed air. The refiduum was of the ftandard of 0.6.

The inflammable air that is procured from *caft iron* has a peculiarly offenfive fmell. On this account I had imagined that it might contain more phlogifton than common inflammable air, fo as to abforb more dephlogifticated air than the other. But this did not appear to be the fact. For when I mixed one meafure of each of the kinds of inflammable air with four meafures of common air, the diminution after the explofion, was the very fame with both, viz. to 1.56.

Though I think it to be unqueftionable, from the preceding experiments, that part at leaft of the fixed air which is found on the decompofition of lambent inflammable air is *generated* in the procefs; yet, in another experiment that I made, it fhould feem that fixed air, or the elements, as we may fay, of fixed air, may enter into the compofition of inflammable air, and actually remain there, without being difcoverable by lime water.

I took a quantity of *flaked lime*, which had been long kept clofe corked in a bottle, and found that when it was heated in an earthen retort, it gave air, of which one fifth was for the moft part fixed air; but in the gun barrel the fame lime yielded no fixed air at all, but a great quantity of pure inflammable air, of the explofive kind, like that which is got from iron alone with water. That the water in flaked lime will enable the iron of the gun barrel to yield inflammable air cannot be queftioned, but then what became of the fixed air which the fame lime would have yielded in an earthen retort?

This experiment appearing rather extraordinary, I repeated it with all the attention I could give to it, and had the following refult. I heated three ounces of flacked lime (but which had been fome time expofed to the open air) in an earthen tube, and got from it fourteen ounce meafures of air, of which only two meafures and a half remained unabforbed by water, all the reft being fixed air. This refiduum was flightly inflammable, but not perfectly phlogifticated. For, examining it with the teft of nitrous air, the ftandard of it was 1.6.

Imme-

Immediately after this I heated another three ounces of the ſame ſlaked lime in a gun barrel, and got from it about twenty ounce meaſures of air, of which no part was fixed air, but all inflammable. I expected, however, to have found fixed air on the decompoſition of this inflammable air with dephlogiſticated air; but after this proceſs it appeared to be exactly ſuch inflammable air as is procured from metals by the mineral acids, or rather by ſteam. For the diminution of the two kinds of air was the ſame, and though there was ſome appearance of fixed air in the reſiduum, it was not ſo much as is found after the decompoſition of the inflammable air that is procured by means of ſpirit of ſalt. In this caſe, therefore, there are no leſs than eleven ounce meaſures and a half of fixed air abſolutely unaccounted for, unleſs it be ſuppoſed that it was reſolved into its conſtituent principles, phlogiſton or dephlogiſticated air, and that the latter was decompoſed as it was produced. This, therefore, I think muſt have been the caſe.

Thinking that the two kinds of air might incorporate when one of them was generated within the other, I filled a gun barrel previouſly full of mercury with fixed air, and put the cloſed part of it into a hot fire. Inflammable air was accordingly produced, but when the fixed air was ſeparated from it, it exploded juſt like inflammable air from iron only.

I made an experiment ſomething ſimilar to this, by heating iron turnings in five ounce meaſures of fixed air, when the quantity of it was increaſed about one ounce meaſure, and there remained one ounce meaſure and three fourths unabſorbed by water. This was inflammable, and burned with a lambent blue flame, not like inflammable air from iron. It ſhould ſeem, therefore, that in this experiment, three fourths of an ounce meaſure of inflammable air had been formed by the union of the fixed air with the phlogiſton from the iron. This experiment I repeated with the ſame reſult, and I farther obſerved, that though the inflammable air procured in this manner did not appear, by the teſt of lime water, to contain any fixed air, yet when it was decompoſed, by being fired together with an equal quantity of dephlogiſticated air, fixed air was found in the reſiduum. For the firſt diminution was to 1.45, and one third of this reſiduum was fixed air. From this fact it ſhould ſeem that, though in ſome caſes, fixed air muſt be *generated* by the decompoſition of inflammable and dephlogiſticated air, yet that inflammble air, when thus produced in contact with fixed air, may *combine* with it, ſo as to be properly *contained* in it,

and

and in ſuch a manner, as that it cannot be diſcovered by lime water.

I alſo obſerved, after Mr. Metherie, that though no fixed air be found on the decompoſition of dephlogiſticated air with inflammable air procured by means of oil of vitriol; a ſmall quantity is produced when the inflammable air procured by means of ſpirit of ſalt. I did not find, however, more than a fortieth part of the reſiduum to be fixed air, when I decompoſed equal quantities of the two kinds of air.

BOOK III.

EXPERIMENTS AND OBSERVATIONS RELATING TO NITROUS AIR.

PART I.

OF THE SOURCE OF NITROUS AIR.

SECTION I.

Of nitrous Air from Metals.

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;

ſorbed; but I never expected to have the ſatisfaction of ſeeing this remarkable appearance, ſuppoſing it to be peculiar to that particular mineral. Happening to mention this ſubject to the Hon. Mr. Cavendiſh, when I was in London, in the ſpring of the year 1772, he ſaid that he did not imagine but that other kinds of pyrites, or the metals, might anſwer as well, and that probably the red appearance of the mixture depended upon the ſpirit of nitre only. This encouraged me to attend to the ſubject; and having no pyrites, I began with the ſolution of the different metals in ſpirit of nitre, and catching the air which was generated in the ſolution, I preſently found what I wanted, and a good deal more.

Beginning with the ſolution of braſs, on the 4th of June 1772, I firſt found this remarkable ſpecies of air, only one effect of which was caſually obſerved by Dr. Hales; and he gave ſo little attention to it, and it has been ſo much unnoticed ſince his time, that, as far as I know, no name has been given to it. I therefore found myſelf, contrary to my firſt reſolution, under an abſolute neceſſity of giving a name to this kind of air myſelf. When I firſt began to ſpeak and write of it to my friends, I happened to diſtinguiſh it by the name of *nitrous air*, becauſe I had procured it by means of ſpirit of nitre only.

I have

I have found that this kind of air is readily procured from iron, copper, brafs, tin, filver, quickfilver, bifmuth, and nickel, by the nitrous acid only, and from gold and the regulus of antimony by *aqua regia*. The circumftances attending the folution of each of thefe metals are various, but hardly worth mentioning, in treating of the properties of the *air* which they yield; which, from what metal foever it is extracted, has, as far as I have been able to obferve, the very fame properties.

Nitrous air is procured from all the proper metals by fpirit of nitre, except lead, and from all the femi-metals that I have tried, except zinc. For this purpofe I have ufed bifmuth and nickel, with fpirit of nitre only, and regulus of antimony and platina, with *aqua regia*.

I did not endeavour to afcertain the exact quantity of nitrous air produced from given quantities of all the metals which yield it; but the few obfervations which I firft made for this purpofe I fhall recite in this place:

dwt.	*gr.*		
6	0	of filver yielded	$17\frac{1}{2}$ ounce meafures.
5	19	of quickfilver	$4\frac{1}{2}$
1	$2\frac{1}{2}$	of copper	$14\frac{1}{2}$
2	0	of brafs	21
0	20	of iron	16
1	5	of bifmuth	6
0	12	of nickel	4

Having,

Having, at another time, diſſolved ſilver, copper, and iron, in equal quantities of ſpirit of nitre diluted with water, the quantities of nitrous air produced from them were in the following proportion; from iron 8, from copper 6¼, from ſilver 6. In about the ſame proportion alſo it was neceſſary to mix water with the ſpirit of nitre in each caſe, in order to make it diſſolve theſe metals with equal rapidity, ſilver requiring the leaſt water, and iron the moſt.

That iron contains more phlogiſton than copper, is probable from the much greater quantity of nitrous air that it yields. At one time I found that two penny-weights of iron diſſolved in ſpirit of nitre, diluted with rain-water, yielded forty five ounce meaſures of nitrous air, when the ſame quantity of pure copper, treated in the ſame manner, yielded only ſixteen ounce meaſures.

I have obſerved that I got little or no air by diſſolving *lead* in ſpirit of nitre. I afterwards, however, made another attempt of this kind, and with a little better ſucceſs. I poured ſmoking ſpirit of nitre into a phial with a ground-ſtopper and tube, containing one ounce meaſure and a half, filled with ſmall leaden ſhot, ſo as to leave no common air at all, either in the phial or in the tube; and I placed it ſo as to receive the air that might come from it in water.

After waiting an hour, in which little or no air was produced, I applied the flame of a candle, though not very near to it, and in these circumstances I got about an ounce measure of air; but upon some water rushing into the phial, while the candle was withdrawn, air was produced very plentifully. I collected, in all, about a quarter of a pint, and might probably have got much more; but that the salt formed by the solution of the lead had so nearly closed up the tube, that I thought proper to discontinue the process. The air, both of the first and of the last produce, was of the same quality, and so far nitrous, that two measures of common air, and one of this, occupied the space of two measures only; excepting that the very first and very last produce, mixed with common air, took up a little more room than that which I got in the middle of the process. When the air was produced very fast, it was exceedingly turbid, as if it had been filled with a white powder.

I have generally found it most convenient to get nitrous air from *copper*, on account of the pretty equable solution of that metal in the nitrous acid. If *iron* be made use of, the process is much more difficult, the increase of heat, and other circumstances, making a very great difference in the rapidity of the solution; so that very often, when the effervescence is very moderate at the beginning, it will be so violent after a short time, that the greatest part of the acid will be thrown out of the phial, and

and consequently the effect of it will be lost. This difficulty, however, only attends the pouring of a diluted spirit of nitre upon a quantity of nails, or other small pieces of iron, in order to effect a complete saturation of the acid that is made use of at one time, which I have found to be the most convenient upon the whole. If thicker pieces of iron be put to the acid, by which means the quantity of surface exposed to its action is not considerable, the produce of air may be made pretty regular; but I have not, upon the whole, found this method so convenient as the other.

Having sometimes, however, procured nitrous air from iron by this process, I have noted some circumstances attending this solution, which, because they are a little remarkable, I shall here recite. When I put a thick piece of iron into a quantity of very strong spirit of nitre, it was not at all affected by it: but by the application of a boiling heat it yielded nitrous air, about ten times the bulk of the acid. When a quantity of water was poured upon the spirit of nitre and iron, it became of a beautiful green or blue colour, and no motion was perceived in it for about a minute, when it burst out all at once into the most violent effervescence imaginable, and a prodigious quantity of nitrous air was instantly produced. It will be evident from subsequent experiments, that a certain proportion of *water* is neceſſary to the conſtitution of nitrous air, and therefore the diluted acid is more proper for this purpoſe.

Mr. Delaval was ſo obliging as to inform me that all *aſtringent vegetables*, as *galls*, the *peruvian bark*, and *green tea*, diſſolve with peculiar rapidity in the nitrous acid, in a manner not unlike the ſolution of ſeveral of the metals in the ſame acid; and that a great quantity of air is generated in the proceſs. I immediately made the experiment with *galls*, and was really ſurprized at the effect. The ſolution was, indeed, aſtoniſhingly rapid; but the quantity of air produced by it was not, ſeemingly, greater than would have been yielded by the ſame bulk of any other vegetable ſubſtance, diſſolved in the ſame acid, with more heat. The air was alſo of the ſame quality with that which is yielded by moſt vegetable ſubſtances. In this caſe, more than half of it was fixed air, making lime water turbid, and the reſiduum was ſo far nitrous, that two meaſures of common air and one of this, occupied the ſpace of two meaſures and a half.

SECTION II.

Of nitrous Air from Vapour of Spirit of Nitre and Water.

I WAS no ſooner in poſſeſſion of *nitrous vapour*, than I ſaw opened to me an entire new field for experiments, by means of a rapid ſolution of biſmuth in ſpirit of nitre, of which a fuller account will be given under the article of *nitrous acid.*

Three methods preſently occurred to me of applying this nitrous vapour, in order to form combinations with other ſubſtances, by which means only its proper nature, and peculiar powers, could be diſcovered. One was, to put the ſubſtance into a clean phial, and then to throw a ſtream of the vapour upon it. Another was, firſt to fill the phial with the vapour, by which method the quantity of it might be, in ſome meaſure, aſcertained, and then to introduce the ſubſtance to it at the mouth of the phial. Laſtly, if the ſubſtance was fluid, I could plunge the tube, through which the vapour was tranſmitted, as deep as I pleaſed into it, and thereby diffuſe the vapour through the whole body of it. The ſecond of theſe methods was the firſt I had

recourſe

recourse to, though soon afterwards I applied the first, and not long after that the third. And as I could not well produce this acid vapour at all without generating enough to fill a great number of phials, I generally placed six, eight, or ten of them in a row, filling them with the vapour one after another, and sometimes supplying them all several times in the course of one process.

The first experiment that I made with water, was to pour a small quantity of it into a phial filled with this vapour; when, shaking it about, it became, as would easily be supposed, genuine spirit of nitre; but it was weak and colourless.

After this, I threw a stream of the vapour upon a small quantity of distilled water, in a large phial, shaking it now and then, to promote the absorption of the vapour; when I observed that the water presently became warm, then began to sparkle very much, air issuing from all parts of it very copiously; and after this it assumed a light blue colour; in which stage of the process, it was, I doubt not, the very same thing that Mr. Woulfe had found by impregnating water with the superabundant nitrous vapour, in his method of distilling spirit of nitre. But, whereas he says his blue liquor continued blue, I found that mine presently lost its colour on being exposed to the open air, emitting a copious red fume.

Finding

Finding that, in this manner of impregnating water, I soon gained the point of saturation, by as much of the vapour escaping as I could readily throw into it, I contrived to impregnate the water more effectually, in the following manner. I got a vessel, *b*, fig. 2, Pl. V. in the form of a phial with a ground stopper, and two holes in the bottom; which, however, was to be placed uppermost when it was used. To one of these holes was fitted, by grinding, a glass syphon, one end of which was fitted in the same manner to the long phial in which the solution of the metal for the production of the vapour was made, while the other end of it went to the bottom of the vessel above mentioned, and which contained the water; so that whatever vapour was brought into the vessel by it, must necessarily pass through the whole body of the water; and to the other hole in this vessel there was fitted, by grinding also, the end of a bent tube, which conveyed the superfluous air, or vapour, into a common recipient. But sometimes I had several of these vessels connected together, as represented, fig. 3, so that the air and vapour discharged from the first of them must necessarily pass through the water in the next, and that which was discharged in this must pass through the water in the following, &c.

Making the experiment in this more accurate manner, ſo that the water had an opportunity of becoming thoroughly impregnated, I made the following obſervations. The water, after becoming warm, began, as before, to ſparkle, and emit air; after which it became *blue*, ſtill continuing to give air in much greater plenty than before. After this the water became *green*, about which time the emiſſion of air ceaſed; and laſtly, after the green colour had deepened very much, ſo as to appear almoſt black, when viewed in the ſame direction with the light that fell upon it, a *yellowiſh tinge* was perceived to be diffuſed through the green colour; and this was the laſt ſtate to which I could bring the water by this impregnation.

I alſo obſerved that, about the time that the water in the firſt of theſe veſſels became blue, that in the next began to ſparkle; and when the water in the firſt turned green, which was probably effected in no other way than by the mixture of the *yellow* (which diſtinctly appeared afterwards) with the preceding blue, the water in the next veſſel became blue, and that in the following to ſparkle, &c.

One of the moſt extraordinary circumſtances in this whole proceſs, is the production of *air* from the water in the two firſt ſtages of it, viz. while it is tranſparent, and while it is blue, before it becomes

comes green. At first I concluded that this was phlogisticated air; this kind of air having been the produce of a similar process for the impregnation of *oil* with the nitrous vapour. But having filled a phial with this water, at the time that it was discharging air most copiously, and having placed it inverted in a bason of the same, I presently got a considerable quantity of it, and found it to be all pure nitrous air, possessed of the peculiar properties of that kind of air in as great a degree as any whatever, and that it contained no portion of fixed air.

The quantity of nitrous air produced in this manner is very extraordinary. When I filled a phial with the water in the state of emitting air, and inverted it in a bason of water, it presently almost filled it, expelling the water. But when I filled a phial with a ground stopper and tube with the water, and caught all the air that came from it, with and without heat, I got at one time more than ten times the bulk of the water, all pure nitrous air.

This will appear the more extraordinary, if it be considered, that water cannot be made to imbibe more than about one tenth of its bulk of nitrous air. The production of it in this case, therefore, is quite *another thing*, and must have a different *cause*; though, had the quantity of it been small, it might have been imagined, that the nitrous air from the bismuth having impregnated the water, as, in some degree, it necessarily must, this nitrous air might have come from that solution.

So great is this discharge of nitrous air, that if the impregnated water be left to itself, it will continue to emit air for a day or two; so that it is not improbable, but that it may, from first to last, yield fifteen or twenty times its bulk. On this account, if this water be confined in thin phials, it will endanger the breaking of them; and the ground stoppers of strong phials have often been thrown out by it with great violence.

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SECTION III.

Of the increased Produce of nitrous Air by previously converting the Acid into Vapour.

HAVING obferved the remarkable production of nitrous air from water impregnated with nitrous vapour, in the following experiment I more accurately compared the quantity of nitrous air produced by pouring the impregnated water upon copper, with the quantity produced by an equal quantity of fpirit of nitre and copper, without impregnating water with the vapour that the acid would have yielded.

Having diffolved a quantity of bifmuth in a given quantity of fpirit of nitre, and having made the vapour which was raifed by the folution pafs through a quantity of water, I poured this water on fome clippings of copper, in a phial with a ground ftopper and tube, and found that it yielded one fixteenth more nitrous air than the fame quantity of nitrous acid diluted with water, and applied to the copper in the fame manner, would yield; the heat of a candle being applied in both thefe cafes till no more air could be procured. No allowance alfo was made for a confiderable quantity of red vapour which was loft in decanting the water, or for that which remained in the large phial in which the folution was made, or for the acid that was united with the bifmuth in the folution. The air yielded by the impregnated water and copper was thirteen ounce meafures, and the folution of the bifmuth ufed in this experiment being diluted with water, and then poured upon the copper, yielded fix ounce meafures and a half, which alone is more than half as much as the original quantity of the acid yielded. Upon the whole, therefore, fpirit of nitre, ufed in this manner, may be made to yield, by means of copper, one half more nitrous air than can be procured by it when applied in the ufual way.

For my greater fatisfaction, I alfo repeated an experiment fimilar to the former, with water impregnated with nitrous vapour, in the procefs for making dephlogifticated air from fpirit of nitre and red lead, and the refult was as follows. Having put fix penny weights of ftrong fpirit of nitre upon a quantity of red lead, and heating the mixture in a gun barrel, I made all the air, together with the redundant acid, pafs through a quantity of water; and found that the water, poured upon copper, would have yielded fourteen ounce meafures of nitrous air, (a part of the water having produced nitrous air in that proportion to the whole) but the fame quantity of of the acid, even with the aſſiſtance of heat, yielded only about eleven ounce meaſures and a half.

I alſo mixed with three ounces of red lead as much ſpirit of nitre as occupied the ſpace of eight penny-weights of water, when the produce was forty ounce meaſures of air, of which about five ounce meaſures was fixed air. The water through which it had paſſed in the veſſel No. 2, Pl. V. after making all proper allowances, and uſing a variety of precautions in applying it to the copper, too minute to be mentioned here, I judged to produce in all twenty four ounce meaſures of nitrous air, which I found to be clearly more than the original quantity of this acid would have yielded.

The above-mentioned experiments were made before I had much ſuſpicion of the great difference in the produce of nitrous air occaſioned by the application of *heat*, which is ſometimes very conſiderable, and by no means in the ſame proportion in all caſes; ſome kinds of the acid yielding almoſt the whole produce without external heat, and other kinds hardly more than one half. I therefore thought it neceſſary to go over this proceſs once more with a view to this circumſtance, and the reſult was ſtill the ſame as before, the water through which the generated air had paſſed producing more nitrous air than the whole quantity of the acid employed in the experiment would have done.

The quantity of acid which I uſed at this time occupied the ſpace of four pennyweights of water, and when applied to copper I could not, with any application of heat, make it yield more than twelve ounce meaſures and a half. But when the ſame quantity of this acid had been mixed with red lead, which was afterwards put into a gun barrel, and had been made to yield all the air that could be extracted from it, one ſeventh part of the water through which the air had paſſed produced two ounce meaſures of nitrous air; ſo that the whole quantity would have been fourteen ounce meaſures; and this was after the water had been decanted firſt from the veſſel repreſented fig. 2, into another phial, and ſome time afterwards, from that into the ſmall phial containing the copper. And it ſhould be conſidered, that after this proceſs (if it be continued till the water begin to emit air, a circumſtance of which an account will be given hereafter) it is ſo exceedingly volatile, that it is not poſſible to pour the water from one veſſel to another without the diſcharge of very copious red fumes, in which a good deal of the acid muſt be loſt. There muſt alſo be ſome loſs of that nitrous air which is emitted by the water itſelf; and I doubt not that the

the increaſe in the produce of nitrous air in theſe experiments is from this ſource, viz. that which is ſupplied from the water, in conſequence of the impregnation with nitrous vapour. Whereas when the acid is much dephlogiſticated, a great part of it becomes combined with the menſtruum, and therefore has no effect in producing nitrous air.

I hardly remember any thing, in the whole courſe of my experimenting, that appeared more extraordinary than this. It ſeemed as if there was an increaſe, inſtead of any loſs of acid, after part of it muſt have been employed in forming the air, and part alſo had been neceſſarily loſt in the courſe of the experiment.

I conſulted ſeveral of my chemical friends upon this ſubject; but they were all of them as much at a loſs to account for the fact as myſelf.

That the giving of nitrous air depends upon phlogiſton, is evident from the phenomena which attend the ſolution of iron in phlogiſticated and dephlogiſticated acids. Pouring a ſmall quantity of phlogiſticated nitrous acid into a large quantity of water, which had iron wire in it, it preſently became of a dark colour; but this was ſoon precipitated, and the ſolution aſſumed a lighter colour. I then poured off the ſolution, which was of a ſlight brown colour, and pouring into it more phlogiſticated

cated nitrous acid, it immediately became of a very dark colour, and emitted air copioufly.

On examination it appeared to be ftrong nitrous air. After the emiffion of this air, the dark colour difappeared. Thefe phenomena, therefore, exactly refembled thofe of a folution of green vitriol, which affumes a dark colour by imbibing nitrous air, and becomes clear again by the expulfion of it. The dark green fpirit of nitre had the fame effect as the brown phlogifticated acid, but the dephlogifticated nitrous acid had no fuch effect.

It is eafy to make a pretty ftrong folution of iron in dephlogifticated nitrous acid that fhall be green and give no air, if it be kept very cold during the procefs. But if phlogifticated nitrous acid be poured into the folution, it prefently becomes very black, and emits air. This blacknefs will fometimes, if the nitrous acid be very volatile, go off almoft immediately; but in all cafes it will do fo *in time*, and leave the liquor like water, or with a flight tinge of yellow; owing probably to part of the ochre having imbibed pure air, and thereby tending to become red.

Nitrous air alfo admitted to a green folution of iron in nitrous acid immediately turns it black, juft as it does a folution of green vitriol.

Phlo-

Phlogiſticated nitrous acid dropped into a ſolution of green vitriol alſo makes it black. The green ſolution of iron in ſpirit of nitre, yields very little air by heat, and this is not nitrous air. When charcoal was put into it, and heated, it alſo gave little or no air.

SECTION IV.

Of the Production of nitrous Air by Means of phlogiſticated nitrous Acid.

THE only method that I have uſed to meaſure the ſtrength of different kinds of nitrous acid, has been to find the quantity of nitrous air that a given quantity of the acid would yield, when diluted with equal quantities of water, from the ſame quantity of copper. It is neceſſary that theſe circumſtances be pretty rigorouſly attended to; for otherwiſe conſiderable miſtakes will be made. For in different circumſtances the produce of air from equal quantities of the ſame acid will be conſiderably different. I ſhall here ſubjoin a few of my obſervations of this kind, that the reader may be apprized of them, and alſo of the importance of attending to other differences of a ſimilar nature.

In

In a ſmall phial, and with a briſk effervescence, the quantity of four penny-weights of water of a ſtrong ſpirit of nitre produced ſixteen ounce meaſures of nitrous air; whereas, in a large phial, diluted with more water, and conſequently with a leſs effervescence, the ſame quantity of the ſame acid yielded only fourteen ounce meaſures of air. Alſo the quantity of copper (which was ſuch cuttings as the braziers commonly make) in the ſmall phial was about half as much as that in the large one. With the ſame quantity of ſpirit of nitre in the large phial, and with the application of the heat of a candle, I got fifteen ounce meaſures of air. At another time the ſame quantity of the acid without heat has not yielded much more than twelve ounce meaſures.

I have frequently obſerved that, unleſs the quantity of acid was ſufficient to produce a *briſk effervescence*, the produce of air has been greatly deficient, the briſkneſs of the effervescence occaſioning a conſiderable *heat*, which is always favourable to the ſolution of metals. But the application of equal degrees of heat will not make the produce of air equal, unleſs other circumſtances be attended to. Whenever I have compared the ſtrength of acids in this manner I have ſcrupulouſly attended, as far as I could, to all theſe circumſtances.

Having

Having procured nitrous acid in the ſeveral ſtates above-mentioned, viz. the original pale coloured acid, that out of which the colour had been expelled by heat, that which had been diſtilled again from freſh nitre, and that which had been phlogiſticated by heat in cloſe veſſels, I tried the ſtrength of them all by the ſolution of copper, meaſuring the quantity of nitrous air that equal bulks of them (all other circumſtances being the ſame) produced, and obſerved that a quantity of each occupying the ſpace of two pennyweights eighteen grains of water yielded as follows, viz.

	Ounce Meaſures.
The original pale coloured acid,	14
The colourleſs, - - -	11
That rediſtilled from nitre,	11
That coloured by heat, - -	11

This highly phlogiſticated acid hiſſed very much when mixed with water. The produce of air was more or leſs accelerated during the courſe of the ſolution in all of them, but moſt of all when I uſed the pale coloured acid. I muſt obſerve that, in making this colourleſs acid, I uſed more heat than was neceſſary, and therefore weakened it too much, though it is certainly impoſſible to expel the colouring phlogiſton without expelling, at the ſame time, the acid to which it is attached. It is ſomething remarkable, that the phlogiſton, *in this par-*

particular state, should attach itself wholly to one part of the acid only, though mixed with the rest of the acid, combined also with phlogiston, but in a different state. These experiments, however, sufficiently demonstrate this to be the case.

It is something remarkable, that though a great quantity of nitrous air is produced by the solution of copper in a diluted nitrous acid, no air at all is procured by a solution of the same metal in the strong acid. There is not even any appearance of air being formed, and afterwards absorbed by the acid, as in the similar solution of mercury.

Having saturated a quantity of strong spirit of nitre with copper, of which it dissolves but a small quantity, I distilled it in a green glass retort. The first part of the acid that came over was orange coloured, from being of a deep green; but the last was quite transparent and weak. No air, that I could perceive, was produced, but a tubulated receiver being made use of, a small quantity could not be discovered. There was not water enough to form nitrous air.

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SECTION V.

Of Air from Gunpowder.

BEING desirous of knowing what kind of air was produced by the explosion of gunpowder, I, for that purpose, mixed equal quantities of sulphur and salt-petre, both finely pounded, and put them into a tall glass vessel. The production of air was very rapid and copious, and so highly nitrous, that two measures of common air, and one of this, occupied the space of two measures and a quarter. Since the produce of air from spirit of nitre and charcoal is the very same with this, viz. nitrous air, it cannot be doubted but that nitrous air is also produced in the explosion of gunpowder, which is composed of those ingredients; the spirit of nitre not being destroyed, or so far decomposed as that its acid nature is lost, but only entering into the composition of this species of air.

Having got nitrous air from a mixture of salt-petre and sulphur, and also from spirit of nitre and charcoal, I concluded that nitrous air must be produced in the firing of gunpowder; and it favours this supposition, that when I fired gunpowder in common air, the air was in part phlogisticated by it. It is possible, however, that when the heat is applied very *suddenly*, the proper *earth* of the charcoal, and also that of the nitre itself may, in part, unite with the nitrous acid, and thereby compose a better kind of air than was produced in those experiments, in which the process was slow, so that the spirit of nitre had an opportunity of saturating itself with the *phlogiston* of the substances mixed with it, without touching the pure *earth*, and therefore the produce was nitrous air only.

I have been led to entertain this suspicion in consequence of being invited by Mr. Woulfe to examine the air that is produced in making *clyssus of nitre*, both with sulphur and with charcoal; when, in both the cases, I own, the air that was produced appeared to be considerably better than, from the materials, and the manner of making the experiments, I should have imagined it could have been. There was, indeed, a considerable quantity of common air in every thing belonging to the apparatus, which was not constructed with any view to the produce of air; but the process was continued so long, and the quantity of air produced was so great, that I do not, in my own mind, make much allowance for that circumstance.

It appeared that the air produced from the clyssus made with *sulphur*, contained one twelfth of fixed air,

air, making lime water turbid, and the remainder was phlogiſticated air, neither affecting common air, nor being affected by nitrous air, and extinguiſhing a candle. And the air that was produced in the proceſs with *charcoal*, contained no more than one twentieth of fixed air, and the remainder, though it extinguiſhed a candle, was ſo little phlogiſticated, that two meaſures of it and one of nitrous air occupied the ſpace of two meaſures and a quarter.

PART II.

OF THE PROPERTIES OF NITROUS AIR.

SECTION I.

Of nitrous Air as the Test of the Purity of respirable Air.

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 ſuppoſed he had obſerved, but of about one fourth of the common air, and as much of of the nitrous air as is neceſſary to produce that effect; which, as I have found by many trials, is about one third 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 effervefcence 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, there will remain only one fifth of the original quantity of common air. This farther diminution, by long ſtanding, I had not obſerved at the time of my firſt publication on this ſubject.

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.

In order to judge whether the water contributed to the diminution of this mixture of nitrous and com-

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 mixture had been made in water, there remaining one seventh more than the original quantity of common air.

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

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

It is evident from these experiments, that the diminution is in part owing to the absorption by the water; but that when the mixture is kept a long time, in a situation in which there is no water to absorb any part of it, it acquires a constitution,

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

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

It is alſo remarkable that, on whatever account air is unfit for reſpiration, this ſame teſt is equally applicable. Thus there is not the leaſt effervesſcence 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 fourth of the whole of any quantity of air, we are, by this means, in poſſeſſion of a prodigiouſly large *ſcale*, by which we may diſtinguiſh very ſmall degrees of difference in the goodneſs of air.

I have not attended much to this circumſtance, having uſed this teſt chiefly for greater differences; but, if I did not deceive myſelf, I have perceived a real difference in the air of my ſtudy, after a few perſons have been with me in it, and the air on the out ſide of the houſe.

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

trous air to each of them, the latter was diminished rather more than the former.

It agrees with this observation, that air in which a candle has *burned* is farther diminished both by putrefaction, and a mixture of iron filings and sulphur; and, I therefore take it for granted, by every other cause of the diminution of air. It is probable, therefore, that this 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.

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

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

I found, very unexpectedly, that a conſiderable difference would be made in the dimenſions of the mixture of air by a circumſtance in the *manner of mixing* them that one would not readily ſuſpect, and I was not at firſt able to account for it. My uſual method, as I have obſerved in the Introduction, has been to mix equal meaſures of nitrous and common air in a low jar, and then to transfer the air into a graduated tube, three or four feet long. What I obſerved is, that I could make a difference of five hundred parts of a meaſure by making the air run up the long tube quickly or ſlowly. The more ſlowly it aſcended, the leſs ſpace it occupied. To aſcertain whether it depended merely upon the two kinds of air being ſo much longer together in the wider veſſel, or in the funnel through which it was poured into the tube, I made the mixtures over night, and tranſferred them into the graduated tube the next morning; but I ſtill found the ſame difference, depending upon the circumſtance above-mentioned. It has been obſerved by Mr. Cavendiſh, that *agi-*

tation

tation brings a mixture of common air and nitrous air into much leſs compaſs than a mixture of them without agitation. The difference is indeed very great, and therefore ſhould always be mentioned. But in this caſe there was no proper agitation.

The fact above mentioned, I now conclude aroſe from what remained of the nitrous air, not decompoſed in the mixture, being diminiſhed by paſſing through ſo much ſpace of water, which is more expoſed to its influence in a ſlow than in a quick paſſage. But I own I ſhould not have ſuſpected that nitrous air would have been diminiſhed ſo very much by being ſimply poured from one veſſel of water into another, if I had not obſerved it in the following manner.

Having mixed a quantity of air, which I knew to be thoroughly phlogiſticated by the putrefaction of fiſhes, with an equal quantity of nitrous air, I transferred the mixture into my graduated tube; when, inſtead of occupying two whole meaſures, as I had expected, they only occupied 1.95 meaſures. Suſpecting that the five hundred parts of a meaſure which had diſappeared had been abſorbed by the water, I poured the air back again into the wide jar; and transferring it once more into the graduated tube, found it to be only 1.8 meaſures; and pouring it about ten times backwards and forwards,

wards, without any unneceſſary agitation, it was reduced to 1.6. Having ſtood in water all night, I meaſured it again the next morning, when I found it to be 1.5; and by meaſuring three times more it was reduced to 1.4.

I then poured two meaſures of nitrous air only from the wide jar into the graduated tube, and found that it was diminiſhed even in a greater proportion than the former mixture.

In applying the teſt of nitrous air, I have lately preferred equal meaſures of nitrous and of common air, or of any air which may be conjectured *a priori* to be nearly in the ſtate of common air, in order that there might be phlogiſton enough to ſaturate it entirely; and if the remaining nitrous air was not affected by water, this method would be perfectly unexceptionable; and with due precaution, it is not liable to much objection. But the moſt accurate method would be to uſe no more nitrous air than the air to be examined is able completely to decompoſe. But then it cannot be known before hand how much this is. Perhaps, in order to guard againſt the inconvenience above mentioned, it might be moſt adviſeable, in common caſes, that is, when the air to be examined is about the ſtandard of common air, to uſe ſomething leſs than an equal quantity of nitrous air, but more than one half, which

which was the quantity that I first confined myself to.

I rather suspect that when nitrous air is mixed with common air, in a greater proportion than is requisite to the complete saturation of the common air with phlogiston, the superfluous nitrous air is more disposed to be absorbed by water than pure nitrous air. It appears, however, that, in no great length of time, such mixtures are brought to the same dimensions as if only half the quantity of nitrous air had been mixed with the common air. This, I think, may be inferred from an experiment which I made to try the difference between *old* and *fresh made* nitrous air, both having been made in the same manner, and, I believe, having been originally of equal strength.

October 25, 1777, I mixed equal quantities of the same common air with equal quantities of the old and fresh made nitrous air. What space they occupied at that time, and in several subsequent periods, is represented at one view, as follows:

	With the old nitrous air.	With the new.
Oct. 27, 1777,	1.22	1.05
Nov. 10,	1.07	0.93
24,	0.96	0.86
Feb. 2, 1778,	0.84	0.8

The last is one fifth less than the original bulk of the common air, and consequently very near to the

the utmoſt limit of the diminution of common air by any proper phlogiſtic proceſs. An accident prevented my obſerving this progreſs any farther.

SECTION II.

Of the Impregnation of Water with nitrous Air.

HAVING, among other kinds of air, expoſed a quantity of nitrous air to water, out of which all air had been well boiled, in the experiment to which I may more than once refer (as having been the occaſion of ſeveral new and important obſervations) I found that nineteen twentieths 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 reduced 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 aſtringent taſte from it. The ſmell of water thus impregnated is at firſt peculiarly pungent. I did not chuſe to ſwallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in ſome caſes, ſalutary.

This kind of air is retained very obſtinately by water. In an exhauſted receiver a quantity of water thus ſaturated emitted a whitiſh fume, ſuch as ſometimes iſſues from bubbles of this air when it is firſt generated,

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

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

In the beginning of May 1776, I ſaturated a quantity of diſtilled water with nitrous air produced from *biſmuth*, and it happened to ſtand ten days, or a fortnight, in the phial in which the impregnation was made, the ſuperfluous nitrous air lying upon the ſurface of it. Then, mindful of the caution ſuggeſted by Mr. Bewly, not to admit the common air to this nitrous air in contact with the water, I very carefully, and as quickly as poſſible, ſlipped a ſmall funnel into the mouth of the phial, in the inſtant that I turned it upſide down; and immediately I filled it up with ſome of the water in the baſon in which it had been inverted, ſo that the nitrous air, in its eſcape mixed with the common air, and was decompoſed, on the outſide of the phial, and not within it. I have forgotten with what particular view I had made this impregnation, but I had no expectation of the reſult, till, obſerving it the day following, I found that it had depoſited a conſiderable quantity of very *white matter*, and that the water did not retain the leaſt ſenſible degree of acidity, not even turning the juice of turnſole red. This

* 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.

experiment

experiment I have endeavoured to repeat, but always without ſucceſs.

At one time, in order to determine whether the precipitate from water impregnated with nitrous air was different according to the *metal* made uſe of in procuring the air, I impregnated three quantities of diſtilled water with nitrous air, of which one was procured from *biſmuth*, another from *copper*, and a third from *iron*, each in an eight ounce phial. In all theſe caſes the water imbibed about one ſixth of its bulk of this air; and when the impregnation was completed, I, as quickly as poſſible, and in the manner deſcribed in the laſt mentioned experiment, filled all the phials with water from their reſpective baſons. But very little depoſit was obſerved for a conſiderable time, and the water in all the phials turned the juice of turnſole red. This impregnation was made on the 28th of May, and the depoſit having been made gradually, and as far as I could obſerve equally, the quantity of it was, in the beginning of October, pretty conſiderable; but ſtill not more than half of what was depoſited in the firſt mentioned experiment. In all the phials, alſo, the colour of the depoſit was the ſame, *viz.* a *dark brown*. The water alſo in them all was ſtill acid, but not, I think, in ſo great a degree as at firſt.

Imagining

Imagining that the difference might depend upon the *time* that the ſuperfluous nitrous air had remained upon the ſurface of the water (during which I had never obſerved any depoſit to be made) I let ſeveral of theſe impregnations remain a fortnight, and ſome more than a month, before I inverted the phials; but ſtill the depoſit was made as ſlowly as before, and was always of a browniſh colour. In ſome caſes this depoſit was very inconſiderable.

When I heated this water, or when I put it into an exhauſted receiver, and thereby expelled from it all the air that I could, very little more depoſit was made than there would have been if no air had been extracted from it. Alſo whether the phials containing the impregnated water were cloſely ſtopped, or left quite open, there was no difference with reſpect to the depoſit.

I imagined that I ſhould have procured a conſiderable quantity of this depoſit by decompoſing a large quantity of nitrous air, which I did by means of common air, in a ſmall quantity of water. But though I repeated this proceſs till the water was become exceedingly acid, it made no more depoſit in a few days than would have been made from water ſimply impregnated with nitrous air. One phial of this water I put under an exhauſted receiver; but though, by this means, a conſiderable quantity of air was difcharged from it, it made no more depofit than the reft.

Imagining that the *acid* which remained in this water might prevent the depofit from being made, efpecially as in the firft experiment, in which the depofit was fo confiderable, the water did not retain any fenfible acidity, I put a little cauftic alkali to the impregnated water ; but no vifible effect followed from it. To prevent all acidity as much as poffible, I did not always depend upon my addrefs in applying the funnel, in the manner defcribed above, but I let out the fuperfluous nitrous air in a trough of the fame water that had been impregnated with it, fo that it was impoffible for it to be in the leaft affected by the decompofition of it with the common air. But ftill the refult was not at all different from what it had been in the other cafes in which this precaution had not been taken.

Mr. Bewly has very well obferved, that that acidity of water impregnated with nitrous air which is *fenfible to the tafte*, is given to it by the decompofition of the nitrous air in contact with the impregnated water; but I have found, that a flight degree of acidity, not indeed fenfible to the tafte, but difcoverable by the juice of turnfole, is always communicated to water by its impregnation with nitrous air. For if a phial be filled with water tinged blue with the juice of turnfole, and then the nitrous air be be admitted to it, and agitated in it, in order to promote the impregnation, a change of colour will presently be perceived in the water. But rain-water so impregnated (the superfluous air being let out under water) retains so little acidity, as hardly to be discovered by mixing it with other water tinged blue.

I once imagined that nitrous air might possibly undergo some change in its constitution in consequence of its being imbibed by water; and for some time I always expelled a proportion of *fixed air* along with the nitrous, from water so impregnated; but by using the following precaution I discovered my mistake. I carefully pumped all the air out of a quantity of rain water, letting it stand twenty four hours in a very good vacuum, and then impregnated it with nitrous air; when, immediately expelling all that I could of it by the heat of boiling water, I found no part of it fixed air, but all pure nitrous air, though not more than one fourth of the quantity that had been imbibed by it.

I wish I could have given my reader more satisfaction with respect to this *deposit* made by nitrous air; but though I have given more attention to it than perhaps to any other subject relating to air, I have not hitherto succeeded to my wish. Perhaps I may be more fortunate hereafter. I have little doubt, however, but that this precipitate consists of the calx of the metal, by the dissolution of which the nitrous air is procured, and the white colour of the first deposit from bismuth may arise from a less portion of phlogiston adhering to it than to the *brown* precipitates. But what I want is a method of making the precipitate *at pleasure*, that a quantity might be procured for a careful examination, and that the proportion of it, in a given quantity of air, might be ascertained.

SECTION III.

Of the Absorption of nitrous Air by Oils, Spirit of Wine and caustic Alkali.

THAT *water* would imbibe a certain portion of nitrous air, I discovered pretty early; but that *oils* would do it, and especially in such a prodigious quantity, and so very rapidly, as I afterwards found they do, I did not so much as suspect at the time of my last publication; and the experiments will shew that the decomposition is effected by means of the affinity which oils, and especially the essential oils, are known to have with the nitrous acid, or its base.

For

For it is evidently this part of the nitrous air that they imbibe.

Having ſeen ſome reaſon to ſuſpect what would be the conſequence of admitting nitrous air to oil of turpentine, from what I had obſerved in my impregnation of oils with the *nitrous vapour*, as will be ſeen hereafter, I filled a ſmall glaſs jar with oil of turpentine, inverted in a baſon of the ſame; and on expelling that fluid by filling it with nitrous air, I obſerved that, without any agitation, the nitrous air was diminiſhed ſo faſt, that in about ſix hours three fourths of it quite diſappeared. What remained extinguiſhed a candle, being, in all reſpects, the ſame with *phlogiſticated air*, or that to which nitrous air is reduced by iron filings and brimſtone, long agitation in water, and other proceſſes.

When I *agitated* nitrous air in oil of turpentine, it was abſorbed quite as readily as fixed air is abſorbed by water; but the quantity of nitrous air that oil of turpentine will imbibe is vaſtly greater than the quantity of fixed air that water can be made to receive.

What is the *limit* in this caſe I cannot tell; but throwing away the reſiduum, which could not be imbibed by oil of turpentine (which was generally about one fourth of the whole, the ſame as in the proceſs with iron filings and brimſtone) I made a quantity of this oil imbibe, at different times, eleven times its bulk of nitrous air, and with very great eafe, even at the laft; but not quite fo readily as at the firft.

During this procefs, the oil, from being tranfparent, prefently became of a light orange colour, and then had a yellowifh caft, and was a little glutinous; but towards the end of the procefs part of the oil became of a very deep orange, and, feparating from the reft, funk to the bottom of the veffel. It muft have been the nitrous acid formed by the nitrous air, and the acidifying principle of pure air, contained perhaps in the oil of turpentine; and it would probably have decompofed more nitrous air, till the whole of it had been converted into this thick orange coloured mafs; which is the fame thing, as will be feen in its proper place, with this oil after it has been fully impregnated with *nitrous vapour*.

I endeavoured to expel air from the oil of turpentine which had imbibed fuch a quantity of nitrous air, but though I applied a confiderable degree of heat, no air came from it.

Willing to know the *laft ftate* to which the impregnation with nitrous air would bring oil of turpentine, I put a fmall quantity of it into a thin phial, ballancing it fo that it would fwim upright in water, and then introduced it into a large jar of nitrous

nitrous air ſtanding in water. It abſorbed, in all, two thirds of it; at firſt very ſlowly, but afterwards more rapidly; the water riſing more in one day than it had done in ſeveral days before, and the whole proceſs laſted a week; after which part of the oil of turpentine was become orange coloured and thick, ſinking to the bottom of the phial, but the change of colour was made at the ſurface. After about ten days I took it out of this jar, and put it into another jar of freſh nitrous air, when it began to abſorb the air very faſt, having imbibed about one fourth of it in one night.

Seeing no other appearance than the change of the oil of turpentine into this dark orange coloured maſs, I at length diſcontinued the proceſs, and expoſed the ſubſtance I had procured to the open air, when it became gradually thicker, till, in a month or ſix weeks, it became almoſt as hard as glue. The inſide of the jar in which this experiment was made was nearly covered with ſmall ſpecks of the ſame glutinous orange coloured matter; the impregnated oil having, no doubt, been exhaled, and having ſettled on the ſides of the glaſs, where, the more limpid part being evaporated, the reſt became of the conſiſtence above mentioned.

I alſo made a quantity of oil of turpentine imbibe nitrous acid from nitrous air, by ſaturating with it the common air in the phial in which it was contained. It presently became very hot; was first green, and then of an orange colour, and parts of it becoming very thick and glutinous, sunk to the bottom, exactly as the oil of turpentine which had imbibed the nitrous air in the preceding process, or the nitrous vapour. N. B. After I had made some progress in this operation, it went on very rapidly. For immediately after I had applied the bladder of nitrous air to the phial, it rushed into it, and all the nitrous air was decomposed in a few seconds. In this circumstance, also, there is a remarkable resemblance between the two processes; the decomposition of the nitrous air in both cases not having been effected so rapidly at the first, as some time afterwards.

Ether has the same power of absorbing nitrous air that oil of turpentine possesses. Having filled a phial with ether, and inverted it in a bason of the same, I introduced a quantity of nitrous air into it, in the same manner as I had done to the oil of turpentine; and presently found that, with a very little agitation, three fourths of it disappeared, and the remainder possessed no nitrous property.

Willing to see the whole effect of nitrous air upon ether, I introduced a small quantity of it into a large jar of nitrous air, in the same manner as I had done with oil of turpentine in the above-

mentioned

mentioned experiment. For ſeveral days air kept bubbling out at the bottom of the jar (the effect of ether on all kinds of air, as I have obſerved, being to increaſe, and almoſt double the whole quantity of it) but after this time the air in the jar began to be diminiſhed, and the water to riſe in it, the phial containing the ether always ſwimming on its ſurface. But at the end of the proceſs, which continued about three weeks, one third of the air in the jar remained. After this I perceived no alteration in its quantity: but, letting it remain a fortnight longer, I examined it, and found the ether very much diminiſhed in quantity, though not changed in its appearance; but it did not evaporate on being expoſed to the open air as ether does. What was moſt remarkable was, that the nitrous air had loſt hardly any thing of its peculiar property of diminiſhing common air. But it may be ſuppoſed that there was not a quantity of ether ſufficient to produce any conſiderable change in ſo large a quantity of nitrous air; and the reaſon why what remained of the ether, after this experiment, did not evaporate, might be, that the exhalation of the water within the jar had mixed with it, and diluted it very much.

Olive oil, likewiſe, imbibes nitrous air, but not rapidly, perhaps about half as faſt as water imbibes fixed air without agitation; which makes very little

little difference in the caſe of *oil*, on account of its viſcidity, and conſequently its not being much divided by that operation. By long ſtanding, a quantity of olive oil imbibed almoſt the whole of a ſmall quantity of nitrous air.

Olive oil, by which a quantity of nitrous air had been confined in a phial ſeveral months, had abſorbed almoſt the whole of it, and that part of the oil which was contiguous to the air was coagulated in lumps, as if it had been frozen, and remained a long time at the top of the oil. But afterwards, being looſened, I ſuppoſe, by the warmth of the weather, it all ſunk to the bottom, as the ice of oil always does.

This property of diminiſhing nitrous air, is not peculiar to oils. It is likewiſe found in *cauſtic alkali*, though not in the ſame degree. Imagining that the preceding oils ſeized upon the acid of nitrous air, and thereby decompoſed it, I thought that alkalies, having the ſtrongeſt affinity with acids, cauſtic alkaline liquors, fixed and volatile, muſt have the ſame effect; and the experiments ſeemed to verify my conjecture. Having put a quantity of nitrous air to a phial of cauſtic fixed alkali, immerſed in a baſon of the ſame, I obſerved that, in the ſpace of three days, and without agitation, ſo much of it had been abſorbed, that not more than one ſixth of the quantity remained, and after

ſix

ſix days about one twelfth part of it only was left.

An equal quantity of *volatile alkali*, in ſimilar circumſtances, imbibed, at the ſame time, but little of the nitrous air. But at another time, after waiting about a week, I obſerved that a quantity of it had abſorbed about one third of a ſmall quantity of nitrous air that had reſted upon its ſurface.

At another time I obſerved that a quantity of fixed alkali abſorbed almoſt the whole of about one fourth of its bulk of nitrous air; for the remainder could not be more than one twentieth part. But when a phial was quite filled with nitrous air, and placed with its mouth in a baſon of the ſame fluid, the abſorption went on very ſlowly.

When, by means of agitation, I had made a quantity of fixed cauſtic alkali imbibe its bulk of nitrous air, I obſerved that the colour of it was not in the leaſt ſenſibly changed; alſo it had no more effect upon iron than it had before this proceſs.

Cauſtic alkali had no ſenſible effect either on common or inflammable air, though only a ſmall quantity of theſe kinds of air was kept in contact with a large quantity of this liquor about a week.

At the ſame time that I firſt expoſed nitrous air to oil of turpentine, I, in the ſame manner, brought

brought it into contact with *ſpirit of wine*; and at that time the abſorption, without agitation, ſeemed to be almoſt as conſiderable as that with the oil of turpentine. But though this fluid imbibed the nitrous air very faſt at the firſt, it was ſoon ſaturated, which is not the caſe with oil of turpentine. By repeating the proceſs ſeveral times, I made a quantity of ſpirit of wine imbibe its bulk of nitrous air. But after this it received more air with great difficulty; and though I did not urge it to the utmoſt, I do not think that it would have taken much more. No change was produced in the appearance of the ſpirit of wine, it being as tranſparent as at firſt; and, what I thought a little remarkable, it did not affect the juice of turnſole in any other manner than ſpirit of wine always does. The application of *heat* to the ſpirit of wine thus impregnated did not expel any air from it, any more than it had done from the oil of turpentine impregnated in the ſame manner.

In order to compare the abſorption of nitrous air by ſpirit of wine and by oil of turpentine, I filled two cylindrical glaſs veſſels, nine inches in length, one with oil of turpentine, and the other with ſpirit of wine, inverting them in baſons of the ſame. Then, expelling the liquors, I filled them both completely with nitrous air, and obſerved that in leſs than a day the oil of turpentine had abſorbed three fourths of its air, while the ſpirit of

wine

wine had not risen in the jar more than three quarters of an inch, and it never advanced any higher.

SECTION IV.

Of the Phenomena attending the Absorption of nitrous Air by acid Liquors.

AS nitrous air is liable to be decomposed by any substance that has a near affinity either with its phlogiston, or with any other of its constituent parts, it was natural to think of trying the effect of the several *acids*, which are known to have a considerable affinity with phlogiston. Accordingly, about the same time that I made the experiments described in the last section, I conveyed a quantity of nitrous air into phials previously filled with the *vitriolic*, *nitrous* and *marine acids*; and it presently appeared that all of them got phlogiston from this air; but the quantity of it which the nitrous acid decomposed, the quickness of the process, and the effect of it upon the nitrous acid itself,

itſelf, were appearances that I viewed with aſtoniſhment, having had no expectation of any ſuch reſult; and ſeveral good chemiſts of my acquaintance have expreſſed no leſs ſurprize at them than myſelf, though theſe facts will appear leſs extraordinary, when it is conſidered how very ſtrong is the affinity between this acid and phlogiſton. This, however, is perhaps a more evident proof of the peculiar ſtrength of this affinity than any other fact that chemiſtry has hitherto furniſhed.

Having, in all the other caſes, had occaſion to agitate this ſpecies of air in the fluids which I expected to abſorb it; the moment that I introduced *this* air to the nitrous acid, I was, as uſual, beginning to agitate it; but with the leaſt motion the abſorption was almoſt inſtantaneous, being nearly as quick as the abſorption of acid or alkaline air by water; and the quantity of nitrous air that a very ſmall proportion of this acid is able to decompoſe, and to appearance abſorb, almoſt exceeds belief.

Finding this abſorption ſo very rapid, I had no occaſion to introduce the air into phials previouſly filled with nitrous acid, in the manner in which I had done with reſpect to other fluids, but only filled the phials with nitrous air, and covering the mouths of them with my finger, placed them inverted in a baſon of the acid, when the abſorption

would

would inſtantly commence, and the fluid, without any agitation, would riſe gradually and viſibly, till the greateſt part of the air diſappeared. Making the experiments in this manner, I obſerved that the upper part of the acid, on which the nitrous air reſted, became firſt of a deep orange, and then of a green colour.

In order to obſerve the full effect of nitrous air on a given quantity of ſtrong nitrous acid, I filled a ſmall phial with it, and then introduced it through the water into a large jar previouſly filled with nitrous air, and ſupported the phial in ſuch a manner, as that the water could never riſe ſo high as to get into it. In theſe circumſtances, the ſurface of the liqour, which was at firſt of a pale yellow, preſently aſſumed a deep orange colour, and the quantity of air abſorbed was indeed very great. I was ſo much ſtruck with this experiment, that I repeated it very often; and the following is a diſtinct recital of all the remarkable appearances attending one of them, which I ſelect from the reſt, as I noted them more minutely than in any other proceſs of the kind.

Having filled a phial, containing exactly the quantity of four pennyweights of water, with a ſtrong pale yellow ſpirit of nitre, with its mouth quite cloſe to the top of a pretty large receiver, ſtanding in water, I carefully drew out almoſt all the common air, and then filled it with nitrous air; and as this was abſorbed, I kept putting in more, till, in leſs than two days, it had completely abſorbed 130 ounce meaſures.

Preſently after this proceſs began, the ſurface of the acid aſſumed a deep orange colour, and when twenty or thirty ounce meaſures of air were abſorbed, it began to be ſenſibly green at the top; and this green kept deſcending lower and lower, till it reached the bottom of the phial. Towards the end of the proceſs, the evaporation of the acid was perceived to be very great; and when I took it out, the quantity was found to have been diminiſhed exactly one half: for there remained no more than the quantity of two pennyweights of water. Alſo it had become, by means of this proceſs and the evaporation together, exceedingly weak, and was rather *blue* than green.

The phial of ſpirit of nitre in this experiment was ſupported by an iron wire, riſing from a flat piece of braſs; and having at one time filled the receiver quite full of nitrous air, ſo as to leave the whole ſtand quite bare, I obſerved that great quantities of air iſſued from them, the moiſture on their ſurface rendering this effect very apparent. This muſt have been an additional quantity of nitrous air, produced by the nitrous vapour which had been exhaled from the phial, or depoſited in the

the decompoſition of the nitrous air, and muſt be conſidered as having been abſorbed by the acid in the phial, beſides that which I threw into it. How much was the amount of this additional quantity of nitrous air, decompoſed by the acid in the phial, I cannot certainly tell; but ſhould gueſs, from the circumſtances, that it could not be leſs than twenty ounce meaſures; which, added to the 130 above-mentioned, makes the whole quantity abſorbed to have been 150 ounce meaſures. Beſides, I withdrew the phial before the abſorption had quite ceaſed.

At another time I was determined that the nitrous acid ſhould continue in nitrous air till it could not poſſibly abſorb any more of it, in order to obſerve what the *acid itſelf* would become after being fully ſaturated with phlogiſton in this manner, and when it had at the ſame time exhaled as much as it could of its own acid in thoſe circumſtances. The conſequence was, that, in four or five days, when the proceſs terminated, the acid was become of a very light blue colour, and, as in the former caſe, was reduced to half its dimenſions; ſo that the evaporation of the acid in this confined ſituation ceaſes before it becomes quite tranſparent, as it does by long expoſure to the open air, though it is very poſſible, that a much longer continuance, even in these circumstances, would have the same effect.

The above-mentioned experiments were made with the strongest yellow spirit of nitre. When I exposed to the nitrous air a quantity of *blue* spirit of nitre, the air was absorbed, but by no means in so great a quantity as by the other acid; and the surface of this acid became of a deeper blue in these circumstances. Had it been continued longer, it would, I suppose, have returned to a lighter blue by the evaporation of its acid; in which state it would have lost its power of attracting phlogiston from the nitrous air, as in the last-mentioned experiment. The nitrous air which had been exposed to this blue spirit of nitre was diminished a little by fresh nitrous air.

Having observed the change that took place in nitrous air by means of spirit of nitre, I was desirous of knowing whether the spirit of nitre in which it was agitated *acquired* or *lost* strength; when I soon found that, in consequence of getting more phlogiston, its power of dissolving metals was diminished, though it will be seen that the acid must have been weakened a little in the course of the experiment.

In order to effect my purpose, I first filled a phial with the nitrous acid, which was very strong, and

and of a pale yellow colour; and placing it inverted in a bason of the same, I introduced to it, by means of a bladder, a quantity of nitrous air; and when the acid had absorbed as much as it could of this, I threw out the residuum of phlogisticated air, and filling it up again with spirit of nitre from the same bason, I supplied it with more nitrous air.

This I continued to do for a considerable time, and observed that by the process the acid became very brown, and smoking; in consequence, no doubt, of having acquired phlogiston from the nitrous air. In dissolving copper with this acid, immediately after the process, I found that it was become weaker in the proportion of five and a half to seven. It must be observed, however, that the evaporation during the process (though I made it as expeditiously as I possibly could) must have weakened the acid a little, and also the end of a wet glass tube (though I never failed to wipe it as well as I could) being dipped into it every time that I supplied it with more air, must have diluted it a little more.

Observing the readiness with which nitrous acid decomposed nitrous air, by depriving it of its phlogiston, I had the curiosity to try how far the agitation of a quantity of this air in strong spirit of nitre would depurate it; and it was not without ſurprize that I found that, when this proceſs had continued but a very ſhort time, the air had become ſo far pure by the loſs of its phlogiſton; that two meaſures of it and one of freſh nitrous air occupied the ſpace of two meaſures and two thirds. I then tried the effect of this proceſs on air phlogiſticated by nitrous air, and found that this alſo was conſiderably improved by this means.

In both theſe caſes the air was far from being ſo pure as to be fit for reſpiration; but that any kind of air ſhould be reduced by this proceſs to a ſtate that is at all *better* than perfectly phlogiſticated, will appear extraordinary, when it is conſidered, that, notwithſtanding the affinity there is between this acid and phlogiſton, yet that the vapour of it never fails to impart phlogiſton to common air, ſo as to deprave it conſiderably. In ſeveral caſes I have obſerved that common air thus expoſed to the influence of nitrous vapour has become perfectly phlogiſticated in a very ſhort ſpace of time. It ſhould ſeem that the nitrous acid, when combined with water, has a ſtronger affinity with phlogiſton than it retains in the form of vapour, free from water.

The effect of oil of vitriol, and ſpirit of ſalt, on nitrous air is by no means ſo remarkable as the effect of the nitrous acid upon it; but it is ſufficiently fufficiently evident that both thefe mineral acids do really decompofe this air in part; and the impregnation they receive from the phlogifton they take from it is worth notice.

Oil of vitriol imbibes almoft as much nitrous air as water can do, and requires about the fame degree of agitation, or rather more, to effect it. Two thirds of the quantity of the air admitted to about four times as much of the acid was imbibed, and the oil of vitriol, which was before quite colourlefs, affumed a beautiful purple hue.

Spirit of falt imbibes nitrous air very flowly, and in a fmall quantity; but by this fmall impregnation, from being of a light ftraw colour, it became of a beautiful fky blue, very vifible when held up to the light. The quantity abforbed was about one twentieth of its own bulk, and one third of the nitrous air employed in the experiment. In order to obferve what proportion of nitrous air a quantity of fpirit of falt would abforb with *long ftanding*, I fuffered them to continue in contact in one cafe about two months; and after that time about two thirds of the air, which was originally about one fourth of the bulk of acid, was imbibed; and I imagine that with more time, ftill more of the air will difappear.

Nitrous air was readily abforbed without agitation by water impregnated both with *vitriolic acid* *air* and *fluor acid air*. Each took more than its bulk, and not more than one twentieth part of the nitrous air remained unabſorbed. How much more would have been abſorbed I did not try. No change of colour was produced by the proceſs. N. B. Agitation only ſet looſe the vapour of theſe acid liquors, and thereby increaſed the apparent bulk of the air. Theſe two kinds of acid air imbibing nitrous air in the ſame manner is an argument for their being ultimately the ſame thing.

Both *radical vinegar*, and *concentrated vegetable acid*, abſorbed nitrous air conſiderably faſter than water. Of theſe acid liquors the former retained its tranſparency; for though, during the agitation it ſuddenly became of a turbid white, that change took place on the accidental admiſſion of a bubble or two of common air, though I do not underſtand how this circumſtance could produce that effect. The *concentrated vegetable acid* aſſumed a dark purple in conſequence of this impregnation, very much reſembling the oil of vitriol after the ſame proceſs.

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SECTION V.

Of the antiseptic Power of nitrous Air.

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 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 five and a quarter to three and a quarter; 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 moufe out, I perceived, to my very great furprize, that it had no offenfive fmell.

Upon this I took two other mice, one of them juft killed, and the other foft and putrid, and put them both into the fame jar of nitrous air, ftanding in the ufual temperature of the weather, in the months of July and Auguft of 1772; and after twenty five days, having obferved 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 fweet, even when cut through in feveral places. That which had been put into the air when juft dead was quite firm; and the flefh of the other, which had been putrid and foft, was ftill foft, but perfectly fweet.

In order to compare the antifeptic power of this kind of air with that of fixed air, I examined a moufe which I had inclofed in a phial full of fixed air, as pure as I could make it, and which I had corked very clofe. 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 rufhed with violence out of the phial; and the fmell that came from it, the moment the cork was taken out, was infufferably offenfive. Indeed Dr. Macbride fays, that he could only reftore very thin pieces of putrid flefh by means of fixed air.

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I once thought that 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, and eſpecially that anatomiſts might perhaps avail themſelves of it; but Mr. Hey, who made the trial, found that, after ſome months, various animal ſubſtances were ſhriveled, and did not preſerve their natural forms in this kind of air.

I have made a few experiments, in order to aſcertain whether it be poſſible to derive any advantage from this property of nitrous air for *culinary purpoſes*. But I cannot ſay that my obſervations have been very favourable to it in this reſpect. Nitrous air will, indeed, preſerve fleſh meat from putrefaction; but after long keeping in this manner it becomes very offenſive, both to the noſtrils, and the palate, though the ſmell is not altogether that of putrefaction; and indeed the ſubſtance continuing quite firm, it could not be properly putrid. Though theſe experiments were not quite fair, becauſe the nitrous air had not been renewed ſo often as it ought to have been, ſeveral of the phenomena may be worth mentioning.

On the 28th of April 1777, I put two pigeons into two jars of nitrous air, juſt wide enough to contain

contain them, with about as much nitrous air in the jars, as the bulk of the pigeons. From this time till the 4th of June following, I had renewed the nitrous air but once, and then, taking them out, I found them both free from all ſmell of putrefaction. One of them was broiled, when the fleſh was found to be ſweet, but it had not the natural taſte of the pigeon, and was, on the whole, unpleaſant. The fleſh was quite red throughout, and a little harder than that of a pigeon generally is. The water contained in the cups, in which the jars with the pigeons had ſtood, had generally been very offenſive, ſo that it ſhould ſeem that the putrid effluvium (containing, probably, much phlogiſton, and perhaps the moſt nutritive part of the fleſh) had paſſed through the nitrous air, and the water, into the ſurrounding atmoſphere.

I replaced the pigeon that was not uſed, and let it remain, along with two others which had been kept the ſame time, till the 13th of September following, in all, near ſix months, or the whole ſummer ſeaſon; but I had not been careful to change the air very often, though I did it two days before I took them out the laſt time. The pigeons had now certainly a very bad ſmell, though their fleſh was firm, and ſo were even the bowels of one of them which had not been drawn. When they were dreſſed, they were much more offenſive, and had a

ſtrong

ſtrong ſmell of putrefaction, or ſomething very much reſembling it. The fleſh was red throughout, ſtill firm, and excluſive of the ſmell, had little or no taſte. My friend, Mr. Magellan, who was with me at the preparation of them, had not ſo bad an opinion of this piece of cookery as I had.

On the 10th of May I put into a jar of nitrous air a large wood pigeon; and taking it out on the 18th of June following, obſerved that it had a ſtrong and offenſive ſmell, but the fleſh was perfectly firm. Though a very great part of the air had been abſorbed, and during the fortnight preceding the examination it had not been ſupplied with freſh air, as it had been occaſionally before, the air to which it had been expoſed all that time diminiſhed common air quite as much as freſh made nitrous air. It was this obſervation that gave me the firſt ſuſpicion of the manner in which nitrous air is diminiſhed in this and in other proceſſes. Having replaced the pigeon in the jar, I found on the 7th of Auguſt following, that the air was but ſlightly nitrous, and on the 22d of the ſame month it was mere phlogiſticated air. After this I neglected to attend to it, and at laſt threw it away. Whether, in this proceſs, the nitrous air ever comes into a ſtate in which a candle will burn in it, or not, I cannot tell. The experiment is a very unpleaſant one, and I ſhall hardly repeat it.

In all theſe caſes the fleſh was kept a long time, *viz.* through the ſix ſummer months; and though nitrous air failed to preſerve meat in a ſtate fit for eating ſo very long, it may poſſibly anſwer the purpoſe for a few days tolerably well, as it will certainly reſtore meat that has begun to turn putrid. One trial of this kind I did make.

On the 14th of June 1777, I took a fowl which had been killed a week, and which had been purpoſely kept till it was offenſive; and putting it into a jar of nitrous air, obſerved that the air began immediately to be abſorbed, and on the 16th I took the fowl out, when it had no ſmell of putrefaction at all; but when it was boiled, though myſelf and ſeveral other perſons taſted of it, and perceived nothing diſagreeable in the taſte itſelf, we were diſguſted with a faint ſmell that came from the body of the fowl, when we held it to our noſtrils. Perhaps it had not been expoſed to the nitrous air quite long enough.

Though part of this air had been abſorbed, the remainder diminiſhed common air quite as much as any freſh made nitrous air.

On the ſubject of this ſection I ſhall obſerve that Dr. Millman having been ſo obliging as to inform me that he had found that *bile* is prevented from becoming putrid much longer by being impregnated with fixed air, than it could otherwiſe be; I was deſirous

desirous of trying what effect the impregnation with nitrous air would have upon it. Accordingly, on the 19th of February 1777, I impregnated a quantity of ox bile, with which he supplied me with nitrous air; when, from being viscid, it presently became limpid like water, and assumed a brownish hue, without depositing any thing that I could perceive. This bile continued perfectly sweet till the the 20th of March following, when it was packed up, along with other things, and removed from London into the country. Examining it some time afterwards, I found it had contracted a smell of putrefaction, and on the 23d of April, it was quite putrid. The same brown colour continued, but it had deposited something of a whitish colour.

SECTION VI.

Of the Formation of nitrous Ammoniac by nitrous Air.

IN the mixture of this kind of air with common air, in a trough of water which had been putrid, but which at that time ſeemed to have recovered its former ſweetneſs (for it was not in the leaſt degree offenſive to the ſmell) a phenomenon ſometimes occurred, which for a long time exceedingly delighted and puzzled me.

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

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

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

When, inſtead of the ſmall phial, I uſed a large and tall glaſs jar, this appearance was truly fine and ſtriking, eſpecially when the water in the trough was very tranſparent. For I had only to put the ſmalleſt drop of a volatile alkaline liquor, or the ſmalleſt bit of the ſolid ſalt, into the jar, and the moment that the mouth of it was opened in a jar of nitrous air,

air, the white clouds above mentioned began to be formed at the mouth, and presently descended to the bottom, so as to fill the whole, were it ever so large, as with fine snow.

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

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

If the salt be not put to the mixture of these two kinds of air till it has perfectly recovered its transparency,

parency, the effervescence being completely over, no white cloud will be formed; and, what is rather more remarkable, there is nothing of this appearance when the salt is put into the nitrous air itself. The reason of this must be, that till common air be admitted to the nitrous, no acid is formed, to unite with the alkali, and make the nitrous ammoniac.

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

SECTION VII.

Explanation of ſome Phenomena attending the Solution of Metals in nitrous Acid.

AS the diſcovery of fixed air in calcareous ſubſtances threw new light upon many phenomena in chemiſtry, in like manner the diſcovery of every other kind of air, and indeed of every property of any of them, muſt throw light upon thoſe proceſſes in which they are concerned. Not being a profeſſed chemiſt, and attending only to ſuch articles in that branch of knowledge as my own purſuits are particularly connected with (though theſe neceſſarily grow more various and extenſive continually) ſuch illuſtrations of chemical proceſſes are not ſo likely to occur to me, as they are to others, who by their profeſſion give a general attention to every thing within the whole compaſs of chemiſtry. Such, however, as I have had occaſion to attend to, and which I imagine I can throw any light upon, I ſhall not fail to mention.

There

There are many facts relating to the solution of metals in spirit of nitre, which could not have been understood without the knowledge of nitrous air; and yet, though several of them are very remarkable, I do not find that even the phenomena themselves, and much less the difficulties attending the solution of them, have been so much as noticed. I am persuaded, however, that an attention to the nature of this remarkable kind of air will contribute greatly to the investigation of the constitution of the several metals, and the explanation of many phenomena attending their decomposition, and consequently their composition.

Having had frequent occasion to dissolve mercury in strong spirit of nitre, in order to procure from it nitrous and dephlogisticated air, and to note the quantity of the metal revivified afterwards, I could not help being very particularly struck with some phenomena in the solution, which are as follows.

The moment that strong spirit of nitre is poured upon quicksilver, the solution is instantly very rapid. But though it is known that one method of procuring nitrous air is by the solution of this metal in the nitrous acid, not a single bubble of any kind of air is seen to be formed; at least none rises through the acid. Presently, however, one may perceive, that very large bubbles of air *are* formed, but they inſtantly diſappear, and nothing remains of them but the ſmalleſt ſpecks imaginable, to riſe to the top of the acid. By degrees, the acid near the mercury becomes of a deep orange colour, and then through this part of the acid the bubbles of air aſcend freely; but the moment they come to the ſuperincumbent pale coloured acid, they collapſe into thoſe ſmall and barely perceivable points, yielding no air that can be collected in any ſenſible quantity. And it is not till the whole quantity of the acid is changed from a pale to an orange colour, that any nitrous air can be collected. Then, however, the bubbles riſe freely to the top of the acid, and, mixing with the incumbent common air, exhibit an orange colour by their decompoſition on mixing with it. Then, alſo, a ſtrong ſmell of ſpirit of nitre is perceived, as it always happens when nitrous air is let looſe to mix with the air of the room in which we are breathing. Whereas, immediately before, no ſmell was perceived, and the common air incumbent on the mixture was quite colourleſs.

Had theſe ſingular phenomena been noticed by any chemiſt before the diſcovery of nitrous air, I cannot imagine what hypotheſis he would have formed for the explanation of them. Whatever it had been, it muſt have been very wide of the truth; whereas

whereas the whole procefs admits of the eafieft explanation imaginable by the help of my obfervations on the decompofition of nitrous air by the nitrous acid.

Nitrous air is actually formed the moment that the folution begins, but it is inftantly decompofed by the ftrong fpirit of nitre in contact with it. By the addition of the phlogifton contained in the nitrous air, the pale fpirit of nitre affumes an orange colour, and it is then much lefs able to decompofe the nitrous air; which, therefore, rifes in bubbles through it, and is not decompofed till it comes to the region of the pale acid lying upon it. But when the whole body of the acid is faturated with phlogifton, then, and not before, the bubbles of nitrous air pafs freely through it, and may be collected.

On this account, it is not eafy to afcertain the exact quantity of nitrous air yielded by the folution of mercury, and, for the fame reafon, of other metals too, in ftrong fpirit of nitre; becaufe allowance muft be made for the quantity that will be imbibed by the acid itfelf, which muft be faturated before any can be collected; whereas, when the acid is much diluted with water, it is not fo capable of decompofing this air, and therefore, in general, it may be collected from the moment that the folution begins.

It

It is very remarkable, that when copper is diſſolved in pale ſpirit of nitre, even diluted with much water, though the ſolution is evidently the moſt rapid at the firſt, the produce of air is very trifling for a conſiderable time, and the quantity collected increaſes very gradually; whereas when the orange coloured acid is employed, in the ſame diluted ſtate, the nitrous air is collected immediately, and the production is the moſt copious at the firſt.

When I diſſolved a quantity of copper in ſtrong ſpirit of nitre half diluted with water, no air whatever was produced, though the metal was completely diſſolved.

When, in the ſolution of mercury, I uſed the green ſpirit of nitre, inſtead of the pale coloured and ſtrongeſt acid, the phenomena were not materially different from thoſe deſcribed above. The lower part of the acid next to the mercury aſſumed a deeper green, but it never became orange coloured.

SEC-

SECTION VIII.

Miscellaneous Properties of nitrous Air.

1. *Of the freezing of Water impregnated with nitrous Air.*

I HAVE observed, that water discharges all the fixed air it had imbibed the moment that it is converted into ice. The same is the case with water impregnated with nitrous air, as appears by the following experiment, made with a view both to this circumstance, and also to the earthy precipitate deposited by water thus impregnated.

Having impregnated a quantity of water with nitrous air, I exposed it to the frost, and observed that it did not freeze quite so soon as a quantity of the same water which had not been so impregnated, exposed in the same manner. The ice of the impregnated water was full of very small bubbles, and when it was thawed did not turn the juice of turnsole red in the smallest degree. It also made a considerable precipitate of a very white matter, exactly like that which I procured from the water impregnated with nitrous air from bismuth. This nitrous air, however, had been procured from copper.

Having exposed to the frost a quantity of water which had been a long time before impregnated with nitrous air, and which had spontaneously deposited a brownish sediment, it now deposited more of the same colour.

2. *Of the burning of a Mixture of nitrous and inflammable Air.*

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. When a candle is extinguished, as it never fails to be, in nitrous air, the flame seems to be a little

little enlarged at its edges, by another bluiſh flame added to it juſt before its extinction.

3. *Of Plants and Animals in nitrous Air.*

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

This kind of air is as noxious as any whatever, a mouſe dying the moment it is put into it; but frogs and ſnails (and therefore, probably, other animals whoſe reſpiration is not frequent) will bear being expoſed to it a conſiderable time, though they die at length. A frog put into nitrous air ſtruggled much for two or three minutes, and moved now and then for a quarter of an hour, after which it was taken out, but did not recover.

There is ſomething remarkable in the effect of nitrous air on *inſects* that are put into it. *Waſps* always died the moment they were put into the nitrous air. I could never obſerve that they made the leaſt motion in it, nor could they be recovered to life afterwards. This was alſo the caſe in general with *ſpiders*, *flies*, and *butterflies*. Sometimes, however, ſpiders would recover after being expoſed about a minute to this kind of air.

4. *Of the Use of nitrous Air in Clysters.*

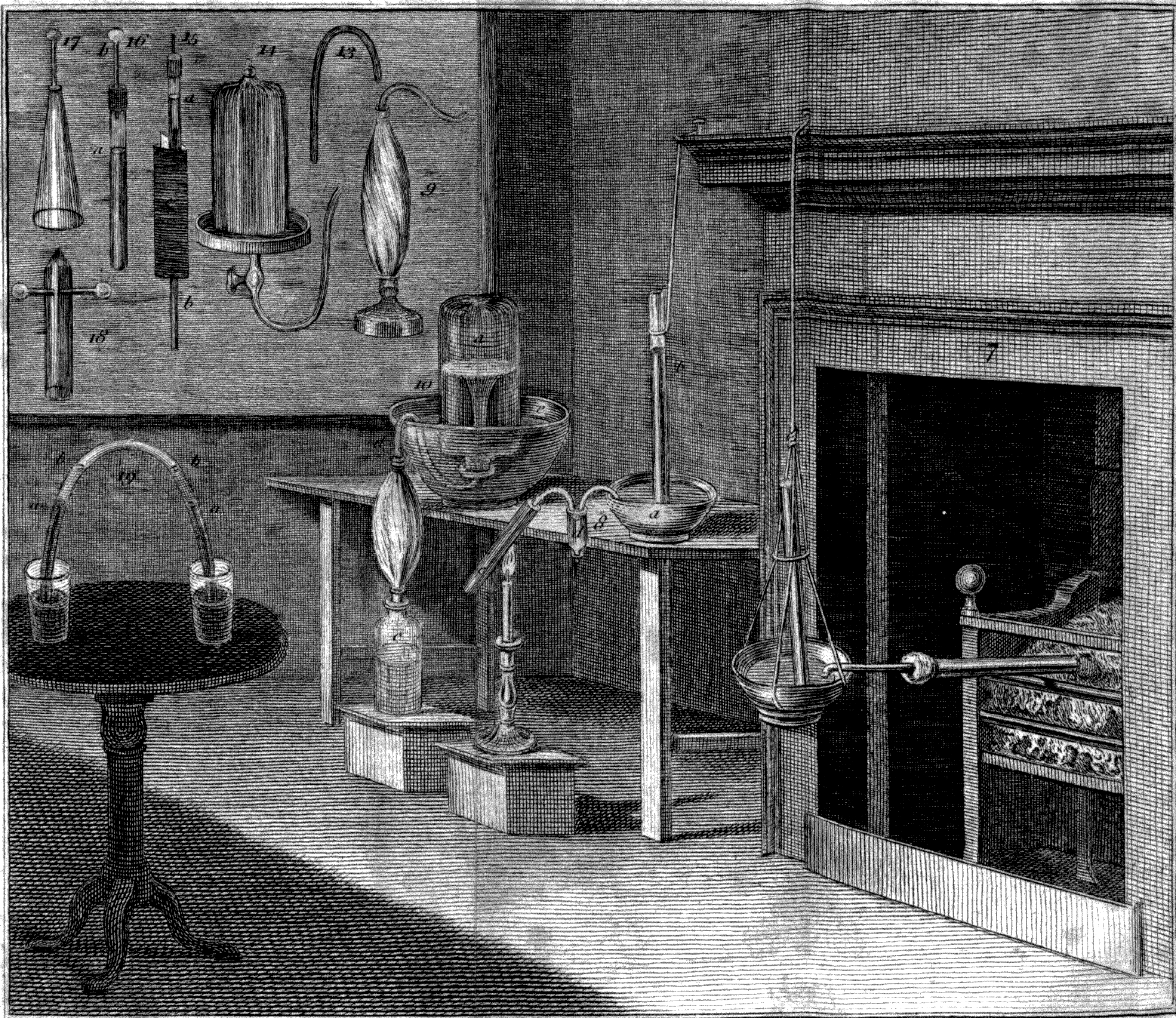
Confidering how fatal nitrous air is to infects, and likewife its great antifeptic power, I conceived that confiderable ufe might be made of it in medicine, efpecially in the form of *clyfters*, in which fixed air had been applied with fome fuccefs; and in order to try whether the bowels of an animal would bear the injection of it, I contrived, with the help of Mr. Hey, to convey a quantity of it up the anus of a dog. But he gave manifeft figns of uneafinefs as long as he retained it, which was a confiderable time, though in a few hours afterwards he was as lively as ever, and feemed to have fuffered nothing from the operation.

Perhaps if nitrous air was diluted either with common air, or fixed air, the bowels might bear it better, and ftill it might be deftructive to *worms* of all kinds, and be of ufe to check, or correct, putrefaction in the inteftinal canal, or other parts of the fyftem. I repeat it once more, that, being no phyfician, I run no rifk by fuch propofals as thefe; and I cannot help flattering myfelf that, in time, very great medicinal ufe will be made of the application of thefe different kinds of air to the animal fyftem.

ſyſtem. Let ingenious phyſicians attend to this ſubject, and endeavour to lay hold of the new *handle* which is now preſented them, before it be ſeized by raſh emperics; who, by an indiſcriminate and injudicious application, often ruin the credit of things and proceſſes, which might otherwiſe make an uſeful addition to the *materia* and *ars medica.*

END OF THE FIRST VOLUME.

Pl. II.



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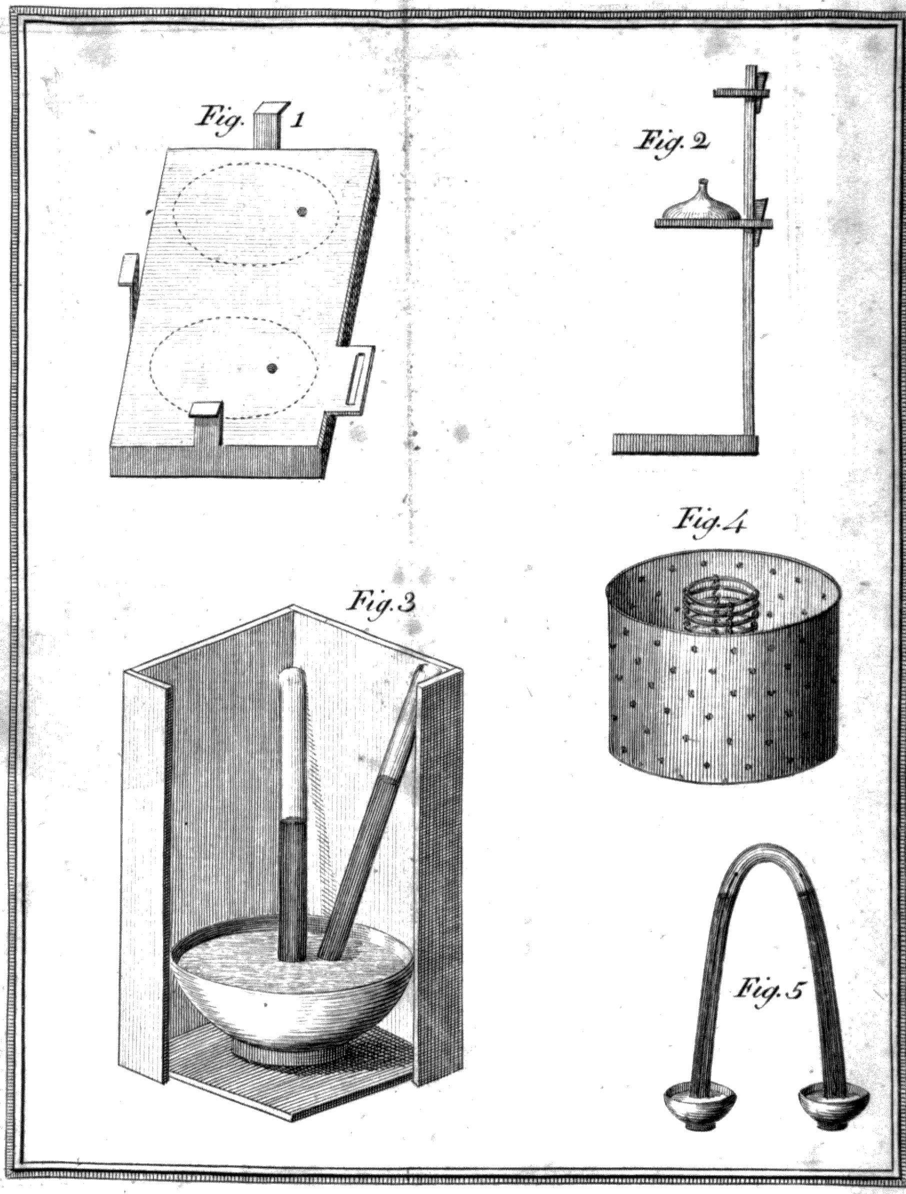

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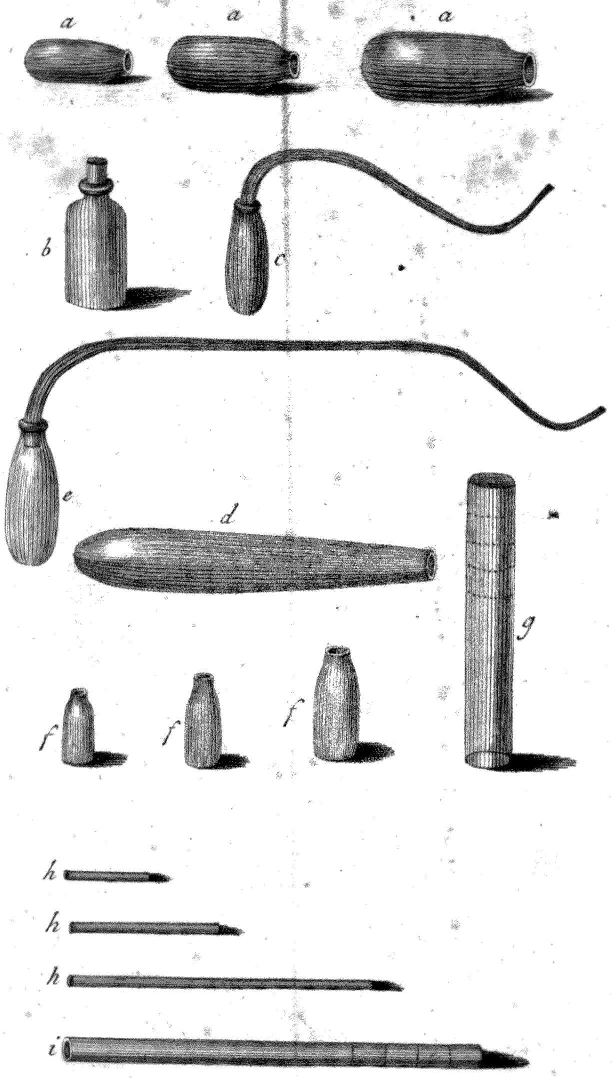

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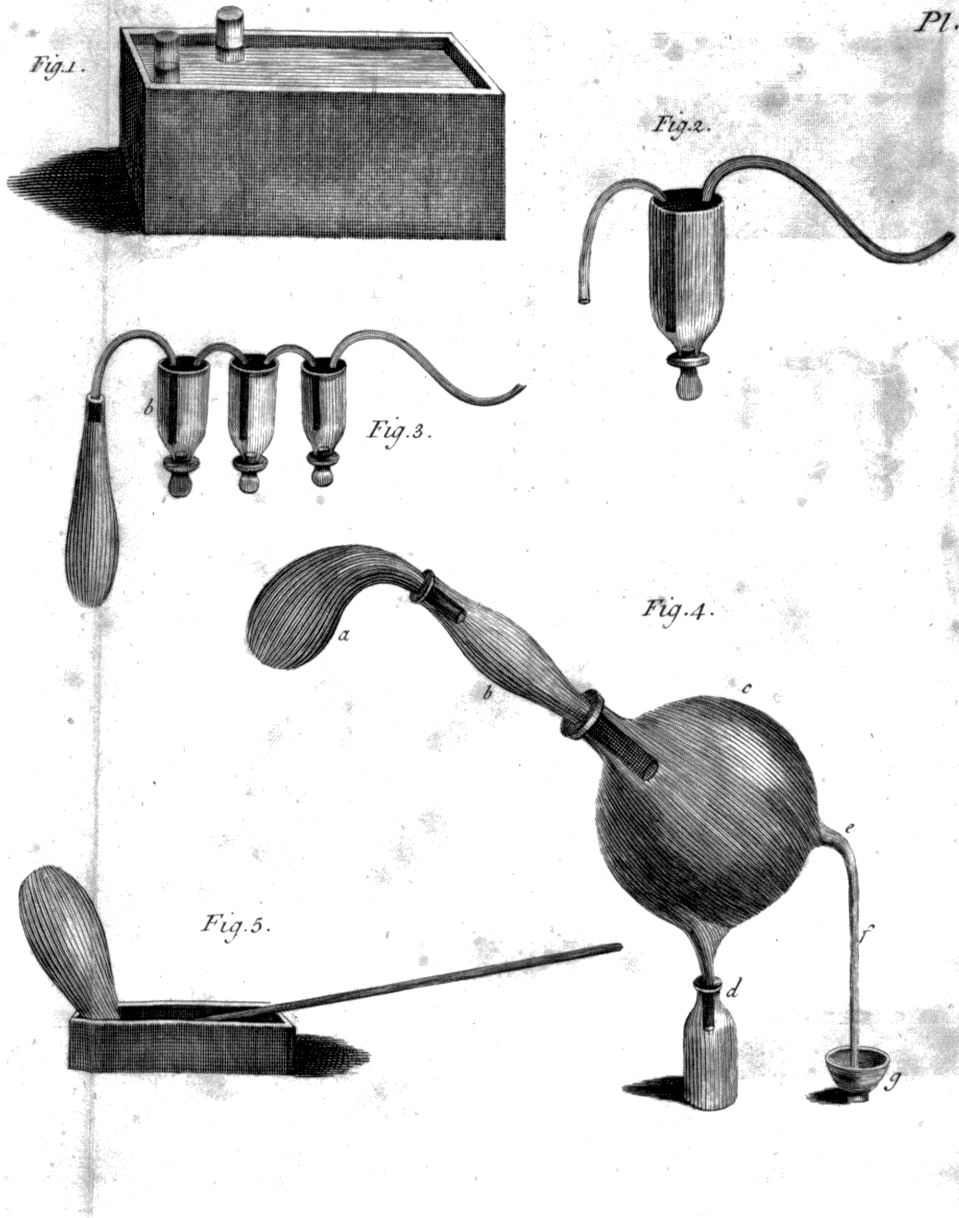


Pl. VI.

Fig. 1.

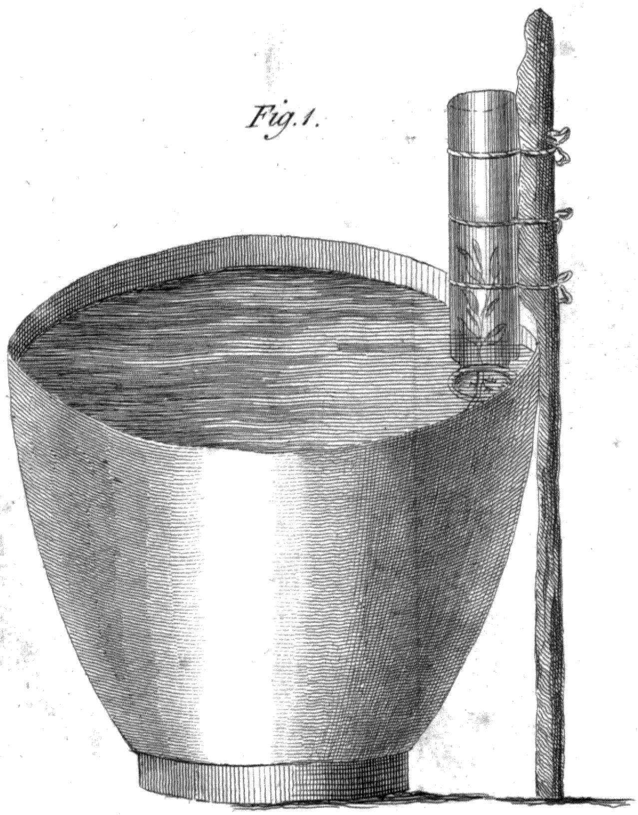
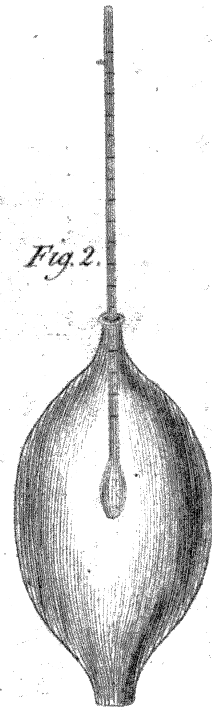
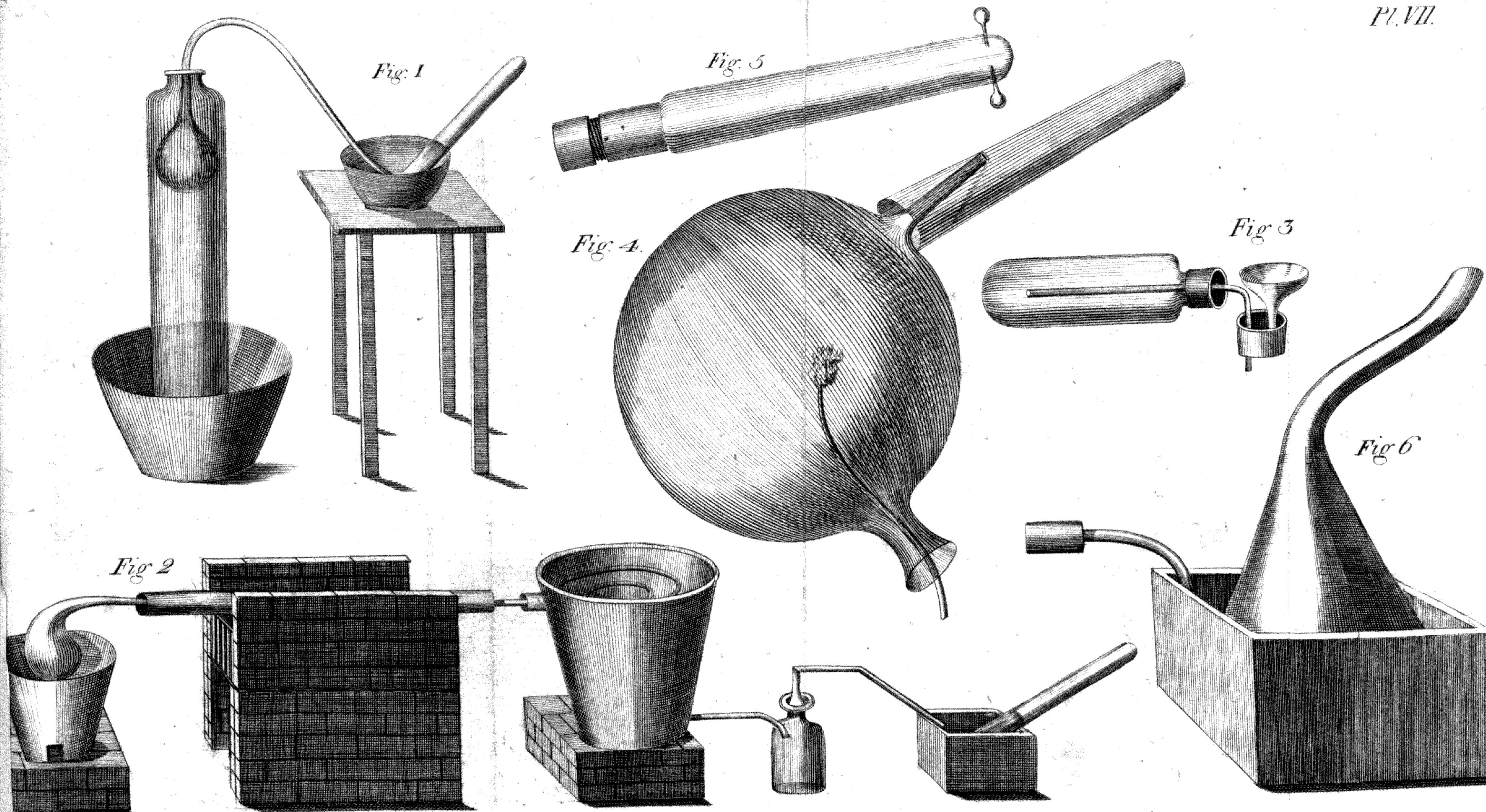
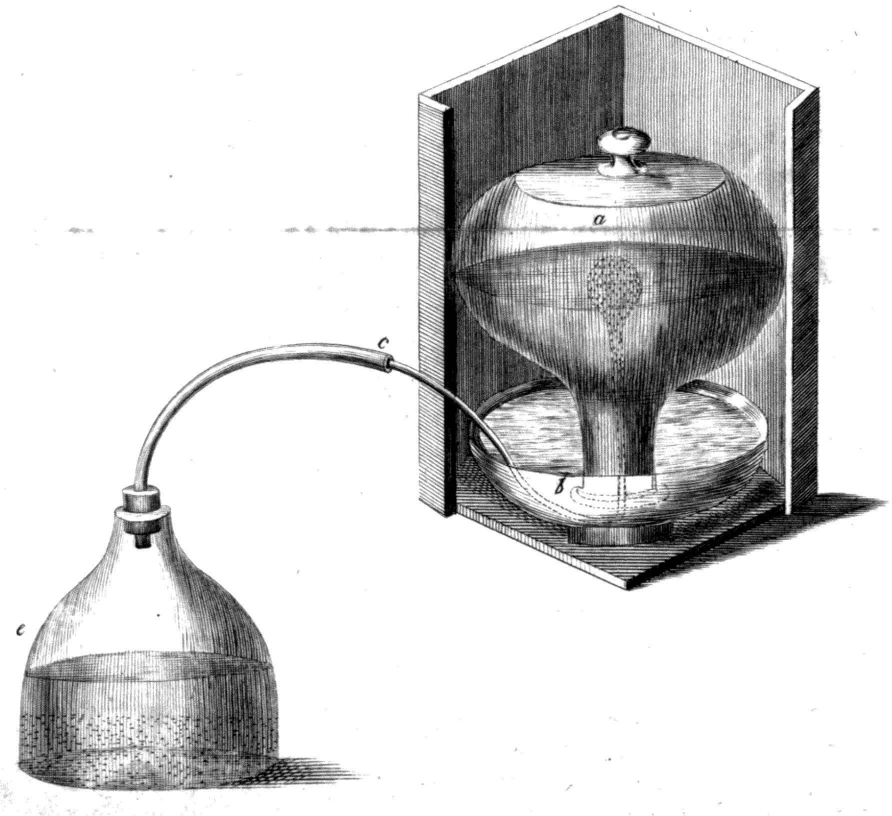


Fig. 2.




Pl. VII.
Fig. 1
Fig. 2
Fig. 3
Fig. 4.
Fig. 5
Fig. 6

Pl. VIII.



Pl. IX.