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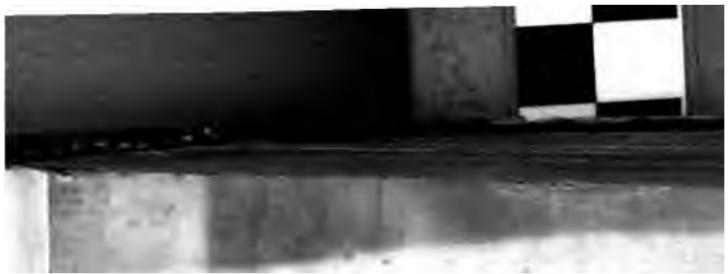
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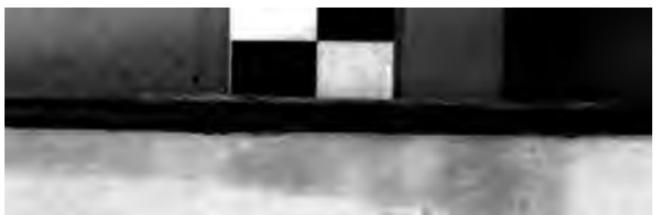
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29

E S S A Y S

Wall, 1780 —

PHYSICAL AND CHEMICAL,

B Y

M. L A V O I S I E R,

Member of the ROYAL ACADEMY OF SCIENCES at PARIS, &c.

VOLUME THE FIRST.

TRANSLATED FROM THE FRENCH,

WITH NOTES,

AND AN

A P P E N D I X,

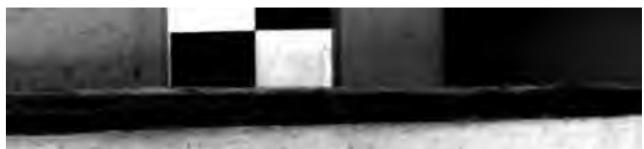
By THOMAS HENRY, F.R.S.

L O N D O N :

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M D C C L X X V I .

H 497



T O

SIR JOHN PRINGLE,
BARONET,

PRESIDENT of the ROYAL SOCIETY,

A N D

PHYSICIAN IN ORDINARY TO
HIS MAJESTY.

S I R,

THE subject of M. LAVOISIER's Philosophical and Chemical Essays, has already been honoured with your attention; and the praise which you have so justly bestowed on our very ingenious

iv DEDICATION.

countryman Dr. PRIESTLEY, in your elegant and learned oration, on delivering to him the honourable reward of his researches into the nature and properties of different kinds of air, evinces the degree of importance in which you esteem such inquiries. The recent discoveries, which have resulted from them, fully justify the decision of the ROYAL SOCIETY; and it must be pleasing to every individual of it to observe, in perusing the historical part of M. LAVOISIER's Treatise, the very large share which their brother-members have sustained in this interesting investigation.

To

DEDICATION. v

To you therefore, Sir, as President of the first Philosophical Society in Europe, as a most competent judge of the merit of the original work, I beg leave to dedicate this Translation, and request you to accept it as a testimony of the gratitude, esteem and respect with which I am,

SIR,

Your most devote

and obliged

humble Servant,

THOMAS HENRY.

MANCHESTER,
Feb. 1st. 1776.



THE TRANSLATOR's

P R E F A C E.

IT has been observed by one of the greatest Philosophers of the present age, that "if those who unhappily spent their time and substance in search after an imaginary production that was to reduce all things to gold, had, instead of that fruitless pursuit, bestowed their labour in searching after that much neglected volatile *Hermes*, who has so often escaped through their burst receivers in the disguise of a subtle spirit, a mere flatulent explosive matter; they would then, instead of reaping vanity, have found their researches

viii P R E F A C E.

rewarded with very considerable discoveries." *

IT will appear that this observation has been strongly verified. Since men have ceased to pay attention to the arrogant pretensions and idle dreams of the old Alchemists, and have directed their inquiries on physical principles, a very rapid progress has been made in the improvement of Chemistry. Mystery and empiricism have given place to systematical perspicuity; men of the first character in Philosophy, and of the highest rank and opulence, have become cultivators of the science, and Chemistry, instead of confining her pursuits to the transmutation of metals or the discovery of panaceas, has now taken a more liberal and enlarged field of action, and has greatly contributed, and, it is hoped, from the progress which she every day makes, will still more extensively contribute

* Hales's Statics, Vol. I. p. 516.

P R E F A C E.

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tribute to the improvement of the other arts and sciences. By her assistance Philosophers have been enabled to make greater discoveries in a few years, than they were before capable of effecting in an age. Instead of building on the sandy foundation of hypothesis, they now establish more durable systems supported by experiment and rational induction. These are the trials to which every new opinion is to be submitted; and, however plausible its appearance or respectable its inventor, no theory can be admitted which will not stand the test of this examination.

THE advantages arising from the aid which Chemistry affords to the other branches of Physics, have been in nothing more conspicuous than in the very important discoveries which have been lately made relative to the nature and constitution of Air. By the assiduous application, and unremitting attention of several eminent Philosophers;

phers, and particularly among our own countrymen of Messieurs Hales, Black, Macbride, Cavendish and Priestley, a number of experiments have been made, by which the analysis of this fluid has been pursued much farther than could possibly have been expected; and hence many new and curious discoveries have resulted, which were wholly unforeseen even by the ingenious experimentalists themselves.

BESIDES the most convincing demonstration of the existence of air, in a fixed state, in several of the hardest and most solid bodies, capable notwithstanding of recovering its elasticity whenever it is let loose from its basis by the action of fire, or by fermentation or effervescence; besides the proof that it is contained in great abundance in calcareous earths and alkaline salts, and, that as on its presence depend some of their distinguishing properties, so they acquire new ones by being deprived

deprived of it; that this air has various degrees of affinity with different bodies, and is capable of being transferred from one substance to another; it has also been discovered that fixed air, when restored to a state of expansibility, is different from common air, fatal to animals who breathe it, yet strongly antiseptic, not only resisting the putrefaction of animal substances, but even restoring them to sweetness and firmness when actually putrid, and that, contrary to common air, it may be absorbed in considerable quantity by water.

THESE last properties of fixed air, suggested the idea of its utility as a medicine in putrid diseases; the trials that have been made have fully justified the expectations that had been formed of its efficacy. We are arrived at the power of imitating the acidulous and chalybeate waters in great perfection, and fixed air has been administered

nistered in various forms, and with considerable success, in several diseases.

HOWEVER firmly the doctrine of fixed air might appear to be established in Great Britain, some formidable opponents to it arose in Germany. These Philosophers, instead of fixed air, substituted another agent which they denominated *Acidum pingue*, with this difference, that as the causticity of alkalies and quick lime, and the solubility of the latter in water, depend, according to Dr. Black's system, on their being deprived of fixed air, the German theory represents these properties as depending on those bodies possessing the *acidum pingue*, which neutralizes them, and thereby gives solubility to the one, and causticity to both.

THE decision of this controversy, of such importance to science, has been undertaken by M. Lavoisier of Paris, a Gentleman

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tleman of distinguished rank, and an Intendant of the Finances, who, amidst the various public avocations to which the nature of his office subjects him, pursues a number of philosophical and chemical researches with almost unequalled abilities and perseverance. The history which he has given us of the gradual discoveries which have been made, relative to the subject of elastic vapours, is so entertaining and instructive, and his experiments so well conducted, his inferences, in general, so justly, so judiciously deduced, that I thought I could not render a greater service to the public, than by translating M. Lavoisier's Treatise into English, being sensible that the reading of foreign books of science in their original language, is, from various causes, confined to a very narrow circle.

MANY of my readers will perhaps be convinced that this remark is not ill founded,

ed, when they see how much has been done abroad on the subject of fixed air. I am apt to think that the modern foreign authors, of whose works M. Lavoisier has given a detail, are far from being generally known in this island ; yet they seem to be distinguished by an ingenuity, and a degree of physical knowledge well deserving of our candid attention.

M. LAVOISIER, in his account of the discoveries of our great English Philosopher, has, as I imagine from an insufficient acquaintance with our language, sometimes misrepresented that Author's meaning. Where this was the case, I have either altered the text by restoring Dr. Priestley's own words, or, where that could not be so conveniently done, have pointed out and corrected the mistakes by notes. I have likewise, as M. Lavoisier's history only recounts Dr. Priestley's experiments as published in the Philosophical Transactions,

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tions, with a view to make the historical part more complete, added a short account of his more recent discoveries contained in the first volume of his Experiments and Observations on different kinds of Air. In order to distinguish these notes from those of M. Lavoisier, I have marked them with the initial letters of my name.

I SHOULD have been apprehensive, from my very short acquaintance with the French language, of publishing this Translation, had not my friend Mr. Aikin of Warrington kindly undertaken to revise the sheets. His perfect knowledge of the language and of the subject, have certainly contributed to make it more free from error, than I fear it would otherwise have been.

A MEMOIR of M. Lavoisier's, read before the Royal Academy of Sciences, on the nature of the principle which combines with metals during their calcination
and

and increases their weight, has been, lately, put into my hands, and I thought it proper to add this by way of Appendix; and the publication of Dr. Priestley's second volume on the subject of Air, has enabled me, with that Gentleman's approbation, not only to give his sentiments on the nature of that principle, but also his ideas of the constitution of common air. But so much interesting information, such important discoveries abound in that work, that nothing but a perusal of the whole can satisfy any person possessed of the least philosophical taste.

ADVER-

ADVERTISEMENT

PREFIXED TO THE
FRENCH EDITION.

DURING more than ten years that I have applied myself to Physics and Chemistry, and have devoted, to these two sciences, the time of which my other occupations have permitted me to dispose, my materials have so greatly accumulated, that I cannot possibly expect they will find a place in the Collection of Memoirs of the Royal Academy of Sciences. Besides, most of the objects which have employed my attention have required too great a number of experiments, and too great an extent of discussion, to leave it possible for me to

b

confine

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confine them to the bounds prescribed to our Memoirs, and I therefore thought myself indispensably obliged to form them into distinct Treatises.

THE diversity of subjects which I have to offer to the public, the uncertainty also, under which I remain, to determine in what order I shall publish my Memoirs, have imposed on me the necessity of chusing a general title applicable to the whole, and that of Physical and Chemical Essays appeared to me more proper than any other to answer my intention. This title will apprise the reader of the indulgence for which I have occasion; it will give me the liberty of presenting him with detached observations; and lastly, it will even render excusable the very disorder which may appear in the arrangement of the materials.

WE easily become interested for the subject on which we are employed, and the last

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last labour in which we have been engaged is commonly the favourite object. This weakness, from which it would be difficult, and perhaps dangerous to guard ourselves, has doubtless induced me to publish first what I have collected concerning the existence of an elastic fixable fluid in some substances, and its disengagement from them, although this work is the last I have written; besides the kind of interest, which men of science seem to take in this subject, and the researches which multiply on every side, would certainly have been a sufficient motive to determine me, and it is unnecessary therefore to search for any other.

I DESIGNED to have inserted, in this volume, some much more elaborate details on the precipitation of metals dissolved in acids, and the considerable augmentation in weight which they acquire in that operation; but the necessity of first investigating the nature of the acids themselves,

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of knowing the principles of which they are composed, and the cases in which their decomposition is effected, &c. prevented me; and I have been convinced that many things were previously to be done. From these and other similar motives, I have also deferred the publication of my experiments on fermentation in general, and on the acid fermentation in particular.

This first volume will, I hope, be followed by several others, and in these I shall successively enter on a train of experiments which are already numerous, and which I still intend to increase; 1st, on the existence of the same elastic fluid in a great number of bodies in nature, in which it has not been hitherto suspected; 2dly, on the total decomposition of the three mineral acids; 3dly, on the ebullition of fluids in the vacuum of an air pump; 4thly, on a method of determining the quantity of saline matter contained in mineral

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neral waters, from the knowledge of their specific gravity; 5thly, on the application of the use, either of pure spirit of wine, or of spirit of wine mixed with water, in certain proportions, to the analysis of the very complicated mineral waters; 6thly, on the cause of the cold which is observed in the evaporation of fluids; 7thly, on different points of optics, on which I have had occasion to be employed in a Memoir relative to the lighting of the streets of Paris; a work which the Academy have been pleased to reward, in their public meeting at Easter 1766, with a gold medal, and in which I have since had occasion to make considerable alterations and additions; 8thly, on the height of the principal mountains in the environs of Paris, compared with the level of the river Seine, measured both by the aid of a good quadrant belonging to M. le Chevalier de Borda, and also of an excellent level with a bubble of air and a glass, constructed by

xxii ADVERTISEMENT.

M. de Chezy, and the property of M. Perronet : Lastly, I shall add a numerous train of observations on the barometer, made in different provinces of France ; I shall give a sketch of the inner part of the earth in these provinces to a pretty considerable depth, the order which is observed in the strata, the constant level at which certain substances and certain shells are found, and the remarkable inclination which some strata always have in the same direction.

THESE different works are mostly far advanced, several of them have even been long since signed by M. de Fouchy, perpetual Secretary to the Academy, I therefore expect that it will soon be in my power to submit them to the judgment of the public.

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B R R A T A.

- Page 47, line 14, after matter place a comma.
50, 12, for primarium read primarium.
117, 6, in the note, for it read them.
128, note, dele inverted commas.
159, line last, in the note, for contain read contains.
183, 4, in the note, after water read with fixed air.
284, 21, ditto, for nitrous read noxious.
— 5, upwards ditto, dele inverted commas after words
honour and remarks.
185, 10, ditto, after years add after.
197, 11 and 19, for del read delle.
200, 3, for be read is.
— 3, last note, dele other.
213, 2 from the bottom, after burns place a semicolon.
221, 10, after salta, instead of a period a comma.
227, 32, dele and.
235, 7, for a read but.
260, 12, for contains read contain.
326, 2 from the bottom, far as read a.
352, Line last, for come read comes.

T H E
F I R S T P A R T.

A SUMMARY HISTORICAL ACCOUNT
OF THE ELASTIC VAPOURS WHICH
ARE SEPARATED FROM BODIES, DU-
RING EITHER THEIR COMBUSTION,
FERMENTATION OR EFFERVESCENCE.

* B



A SUMMARY
HISTORICAL ACCOUNT
OF THE *ELASTIC VAPOURS* WHICH
ARE SEPARATED FROM BODIES DURING
EITHER THEIR COMBUSTION, FERMENTA-
TION OR EFFERVESCENCE.

T H E
INTRODUCTION.

A GREAT number of foreign Philosophers and Chemists are at this time employed in researches concerning the fixation of Air in bodies, and the elastic vapours which are separated in the combination as well as in the decomposition and resolution of their principles. Various Memoirs, Theses and Dissertations have appeared on this subject in England, Germany, and Holland.

B The

2 I N T R O D U C T I O N.

The French Chemists alone seem not to take any part in these important inquiries; and while the discoveries of other nations increase every year, our modern publications, the most complete, in many respects, of any that have been written in Chemistry, are almost totally silent upon this subject.

THESE considerations induced me to think it necessary to give the public a short account of every thing, which has hitherto been done, relative to the combination of Air with bodies, and to give an accurate description of the discoveries which have been made in this subject. This I propose to do in the first part of this Treatise. I have endeavoured to perform it with the utmost impartiality, and I have confined myself, as much as possible, to the simple character of an Historian.

IN the second part, an account will be given of my own experiments. Those which are related in the first Chapter, are intended to fix the opinion of Chemists in regard to the different systems of Dr. Black and Mr. Meyer. I believe I have

INTRODUCTION. 3

I have arrived to as great a degree of certainty, in this respect, as can be hoped for in Physics. The following Chapters treat of the union of the elastic fluid with the metallic calces, of the burning of Phosphorus, of the formation of its acid, of the nature of the elastic fluid discharged in the solutions of metals, &c. &c.

IT must be acknowledged that this last part of my work is not so complete as I could wish, and that it is not even without some degree of regret that I publish it; But as it is easy to lose one's way in a road but little travelled, I reflected how important it might be for me to benefit myself by the remarks of the learned, though it might expose me at the same time to their criticisms. It is principally with this view that the latter part of this work is published in this state of imperfection; and I am already aware that I have occasion for all the indulgence of my Reader.

HISTORICAL ACCOUNT OF

CHAPTER I.

OF THE ELASTIC FLUID DESCRIBED, TO THE TIME OF PARACELSIUS, UNDER THE NAME OF SPIRITUS SILVESTRIS, AND UNDER THE NAME OF GAS BY VAN HELMONT.

THE different Authors, previous to Paracelsus, who have spoken of the elastic matter which is separated from bodies either by means of Fire, Fermentation or Effervescence, do not seem to have formed very clear ideas of its nature and properties. They have given it the name of Spiritus Silvestris.

PARACELSIUS, and some Authors cotemporary with him, imagined this matter to be the same as the air which we breathe; but it does not appear that this opinion was supported among them by any argument, much less by Experiments. Van Helmont the disciple of Paracelsus,
and

ELASTIC VAPOURS. 5

and frequently differing in opinion from him, appears to be the first who undertook to make the following inquiries into the nature of this substance. He gave it the name of Gas*, Gas Silvestre†, and he defined it a spirit, an incor-ecible vapour, which could neither be collected in vessels nor be reduced under a visible form. He observes that some bodies resolve themselves, almost entirely, into this substance; "Not, adds he, that it had actually been contained under that form in the bodies from which it was separated; otherwise nothing could have retained it, and all its parts would have been dissipated. But it was contained under a concrete form, as if fixed or coagulated". This substance, according to Van Helmont's experiments, is separated from every fermenting body; from wine, from meath, from

* Gas is derived from the Dutch word Ghoast, which signifies Spirit. The English express the same idea by the word Ghost, and the Germans by the word Geist, which is pronounced Gaistre. These words have too much affinity with that of Gas, to leave any doubt of their derivation.

† Complexionum atque mixtionum. Elementalium Fig-mentum. No. 13, 14, & seq.

* HISTORICAL ACCOUNT OF

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PARACELSIUS, and some Authors cotemporary with him, imagined this matter to be the same as the air which we breathe; but it does not appear that this opinion was supported among them by any argument, much less by Experiments. Van Helmont the disciple of Paracelsus, and

ELASTIC VAPOURS. 5

and frequently differing in opinion from him, appears to be the first who undertook to make the following inquiries into the nature of this substance. He gave it the name of Gas*, Gas Silvestre†, and he defined it a spirit, an incor-ecible vapour, which could neither be collected in vessels nor be reduced under a visible form. He observes that some bodies resolve themselves, almost entirely, into this substance; "Not, adds he, that it had actually been contained under that form in the bodies from which it was separated; otherwise nothing could have retained it, and all its parts would have been dissipated. But it was contained under a concrete form, as if fixed or coagulated". This substance, according to Van Helmont's experiments, is separated from every fermenting body; from wine, from meath, from

* GAS is derived from the Dutch word Ghoast, which signifies Spirit. The English express the same idea by the word Ghost, and the Germans by the word Geist, which is pronounced Gaist. These words have too much affinity with that of Gas, to leave any doubt of their derivation.

† Complexionum atque mixtionum Elementarium Fig-
mentum. No. 13, 14, & seq.

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IT is in the same Treatise * that Van Helmont examines whether this Gas or Spiritus Silvestris of the Ancients be not, according to Paracelsus's opinion, the same air which we breathe reduced to its elementary parts and combined with bodies. Although the arguments and experiments, by which he supports his opinion, be not very decisive, he believes, however, that it may be determined † that the Gas is different from the air which we breathe; that it has a greater affinity with water; that it probably might consist of water reduced to vapours. Next moment ‡ he thinks that this air might perhaps result from a combination of a very subtle acid with the volatile alkali.

THE passages of Van Helmont's works which I have quoted, are not the only ones in which he speaks of the Gas: he has taken notice of it in many others, and particularly in his Treatise *de Litibias*, Cap. 4. No. 7. and in his *Tremulus pestis*. He also ascribes the propagation of epidemic diseases to the vapours with which Gas is infected.

C H A P.

* *De Flatibus*, No. 19.

† *Idem*.

‡ *Idem*, 67 & 68.

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CHAPTER II.

OF MR. BOYLE's ARTIFICIAL AIR.

WHAT Van Helmont called *Gas*, Boyle denominated *artificial Air*. Furnished with the new instruments with which he has enriched Natural Philosophy, he repeated all Van Helmont's experiments in Vacuo, in condensed Air, and in open Air. The greater part of his Experiments may be found in a work entitled; *Continuatio novorum Experimentorum physico mechanicorum de gravitate et elatere aeris*. Some others are dispersed in many parts of his works.

Boyle was sensible, as well as Van Helmont, that most vegetables, when diluted with a certain proportion of water, and placed in a situation proper for fermentation, discharge a considerable quantity of Air; that this Air discharges itself with more facility in the exhausted receiver of an Air pump, than in compressed Air;

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Air; that every thing which impedes the progress of fermentation suspends also the separation of the air, and that spirit of wine possesses this property in a particular and eminent degree.

THESE experiments, repeated in air much more condensed than that of the atmosphere, afforded nearly the same results. He then tried the effect of placing the fermenting matter in an atmosphere of artificial air, and he found that in some circumstances it accelerated, and in others retarded the fermentation. But one essential difference, between this air and that of the atmosphere, before observed by Van Helmont and recognised by Boyle, is that the latter is necessary to the existence of many animals, whereas the other, when breathed by them, proves instantly destructive. Mr. Boyle's experiments on this subject demonstrate that artificial air is not always the same, from whatever vegetable substance it may be separated; and that the air which is produced by the explosion of gunpowder presents phenomena peculiar to itself.

IT is easy to see that almost all the discoveries
of

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of this kind, which we have usually attributed to Mr. Boyle, really belong to Van Helmont, and that the latter had even carried his theory much farther. But one observation which is particularly Boyle's, does not seem to have been even suspected by Van Helmont, viz. That there are bodies, such as sulphur, amber, camphor, &c. which diminish the volume of air, in which they burn.

CHAPTER III.

DR. HALE'S EXPERIMENTS ON THE QUANTITY OF ELASTIC FLUID SEPARATED DURING THE COMBINATIONS OR DECOMPOSITIONS OF BODIES.

THE united experiments of Van Helmont and of Boyle had made it evident that a great quantity of elastic fluid analogous to air was separated from bodies in many operations; that also in some other operations a portion of atmospheric air was absorbed, or at least deprived of

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of its elasticity; but nobody had conceived any idea of the quantities either produced or absorbed. Dr. Hales was the first person who has seen the object in this point of view; he invented various methods, equally simple and commodious, to measure with exactness the bulk of air. I shall not at present attempt to describe the different instruments which he made use of. Particular attention will be paid to this object in the sequel; I shall then describe the alterations which have been made in them by some Philosophers, and those which I think they are capable of receiving.

THE great number of experiments made by Doctor Hales, which may be found in Chap. 6. of his Vegetable Statics, comprehends almost every substance in nature. He has examined into the effects of combustion, of fermentation and of combinations, &c. As these experiments are, even at this time, the most complete of their kind, it may be proper to give, in this place, an abstract of them. The form of a Table appeared to me to be the most clear, the most convenient, and the least voluminous.

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EXPERIMENTA

BY DISTILLATION.

The Names of the Materials used in the Experiments.	The number of cubic inches of air produced by distillation.
ON VEGETABLES.	
One cubic inch or 270 grains of oak timber,	256
One cubic inch or 398 grains of pease,	396
142 grains of dry tobacco, — — —	153
One cubic inch of oil of aniseeds,	22
One cubic inch of oil of olives,	80
One cubic inch of tartar,	504
One cubic inch or 270 grains of amber,	270
OF ANIMAL SUBSTANCES.	
One cubic inch of hog's blood distilled to dryness, — — —	33
Rather less than a cubic inch of tallow,	18
One cubic inch or 482 grains of hartshorn,	234
One cubic inch or 532 grains of oyster shells,	324
One cubic inch of honey, — — —	144
One cubic inch or 253 grains of yellow wax,	54
Three-quarters of a cubic inch of human calculus weighing 230 grains, — — —	516

Or

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Names of Materials, &c.	Number of cu- bic inches of Air, &c.
O R M I N E R A L S.	
One cubic inch or 316 grains of pit-coal,	360 •
One cubic inch of fresh untried earth, —	43
One cubic inch of antimony, — —	28
Half an inch of sea salt and the same quan- tity of calcined bones, — —	64
Half a cubic inch or 211 grains of nitre, with the calx of calcined bones, —	90

E X P E R I M E N T S

ON[°] FERMENTATION.

Forty-two inches of small beer in seven days,	639
Twenty-six inches of bruised apples in thir- teen days, — — —	968

E X P E-

* About 102 grains of Air, according to Dr. Hales, or a third of the whole weight.

ELASTIC VAPOURS.

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EXPERIMENTS ON DISSOLUTIONS AND COMBINATIONS.

The Names of the Materials used in the Experiments.	Cubic inches of Air produced.	Cubic inches of Air absorbed.
Half a cubic inch of sal ammoniac with one cubic inch of oil of vitriol, the first day, — The next day it had absorbed 15. Six cubic inches of oyster shells and as much vinegar in some hours, — — —	5 to 6	
In nine days 21 inches were destroyed, and the other eight disappeared on pouring warm water into the vessel ".	29	

Two.

" The ninth day, says Dr. Hales, I poured warm water into the vessel, and the following day, when all was cool, I found that it had resorbed the remaining eight cubic inches. Hence we see that warmth will sometimes promote a resorbing as well as a generating state, viz. by raising the resorbing fumes as will appear hereafter."

Dr. HALS here ascribes to the warmth what was really owing to the water simply as such, which from its affinity with fixed air imbibed it with rapidity. And indeed from the Doctor's own relation it does not appear that the resorption took place while the mixture continued warm. T. H.

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Names of Materials, &c.	Inches of Air produced.	Inches of Air absorbed.
Two cubic inches of aqua regia poured on a gold ring flattened,	4	
Two cubic inches of aqua regia with a quarter of an inch of antimony, in three or four hours,	38	
After some hours 14 inches were resorbed.		
A cubic inch of aqua fortis poured on a quarter of an inch of antimony, at different intervals,	130	
A cubic inch of aqua-fortis, with $\frac{1}{4}$ of an inch of iron filings,	43	
Quarter of an inch of iron filings, and one cubic inch of powdered sulphur, — — — — —	— —	19
A cubic inch of aqua-fortis on an equal quantity of powdered marcasite, or Walton pyrites,	— —	85
A cubic inch of aqua-fortis, with the same quantity of pit-coal, absorbed in three days 18 inches of air; and in three days more remitted and generated 12 cubic inches, — — — — —	— —	18
Two cubic inches of quick-lime, with four of vinegar, — — — — —	— —	22

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Names of Materials, &c.	Inches of Air produced.	Inches of Air absorbed.
Two cubic inches of lime and as much sal ammoniac, — — —		115
By the burning of linen-rags, dipped in melted brimstone, in a large vessel, — — —		198
In a smaller vessel, — — —		150
Two grains of Kunkel's phosphorus, — — —		28
After inflammation it had lost only half a grain; some time after its weight was augmented one grain.		
A piece of brown paper soaked in a strong solution of nitre, and set on fire under a receiver, by means of a burning lens, produced — — —	80	
In a few hours this quantity of air was diminished.		

C E X P E-

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 EXPERIMENTS
 ON BURNING BODIES,
 AND ON THE
 RESPIRATION OF ANIMALS.

The Names of the Materials used in the Experiments.	Cubic inches of Air produced.	Cubic inches of Air absorbed.
A lighted candle, of $\frac{2}{3}$ of an English inch in diameter,		78
A rat confined in a receiver,		78
73 cubic inches of air, breathed by a man till he was nearly suffocated, was reduced to 20 inches.		

These are not the only experiments contained in the sixth chapter of Dr. Hales's Vegetable Statics; we meet with several others which are not capable of being reduced to the form of a table. The Author has every where joined to them views entirely new and excellent reflections. Too much cannot be said to induce the reader to peruse

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peruse that Author's own work. He will find a most inexhaustible fund of meditation. I shall now proceed and endeavour to give an abstract of the sixth chapter of his book, though it be not very capable of abridgement.

IT is in this work, that we perceive the first traces of the existence of air in those waters which are, even now, improperly called acidulous. Dr. Hales has not only remarked that these waters contained double the quantity of air to common water, but he also suspected that they owed their remarkable sparkling and briskness to this air.

THOUGH Dr. Hales suspected that the acids in general, and particularly spirit of nitre, contained air, yet the distillation of aqua fortis gave a contrary product. Instead of the augmentation which he expected, he observed a remarkable diminution of the volume of air. The consequence which he drew, from this observation, was that acid vapours absorb air; and from hence he concluded, that what we obtain by the combination of acids with alkaline substances may not wholly appertain to the latter, but that the

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acid may also afford a part of it, and that it is very probable that it is this substance which produces the air which is separated in solutions of metals by acids.

IT is to the great quantity of air which is let loose by the detonation of nitre, that Dr. Hales attributes the effects of gunpowder; to which he, notwithstanding, thinks should be added the expansion of the watery particles which are reduced into vapour. If tartar, which, as well as nitre, contains a very large quantity of air, does not explode in the same manner, Dr. Hales accounts for this difference, by supposing that the air is more intimately united to it, and requires a greater degree of heat to detach it; and it is from this great quantity of air and its close adherence to the tartar that he deduces an explanation of the effects of the pulvis fulminans.

DR. Hales has attempted to determine the specific gravity of Air, which had been separated from tartar, by distillation; but he did not find that it differed at all, in that respect, from atmospheric air; the event was the same, whether

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he employed air *recently* extracted from tartar, or air which had been separated more than ten days before.

It did not escape the knowledge of Dr. Hales that the quantity of air absorbed either by the burning of sulphur or of candles, or by the respiration of animals, presented different appearances, accordingly as he employed receivers of a larger or smaller capacity. He observed, in this respect, that the quantity of air absorbed is generally greater in large than in small vessels; that, however, it is more considerable in small than in large vessels, *in proportion to their different capacities*. He also remarked that this absorption of air is limited; and that it cannot proceed beyond a certain point.

DR. Hales, in the course of his experiments, has observed the singular, alternate production and absorption of air, of which he does not seem to have understood the true cause: the detonation of nitre, for example, furnished him with a great quantity of air; but that air diminished daily in its elasticity and bulk: he has observed

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the same thing in regard to a great number of different factitious kinds of air. This phenomenon depended on the water which the Doctor always used in his experiments : it will be shewn hereafter, that most of these vapours, and particularly those which we are accustomed to denominate Fixed Air, have a great affinity with water, which is capable of absorbing more than its own bulk of this air. Hence it was, that Dr. Hales did not always draw just conclusions from his experiments, and this was the source of errors, of which he was by no means apprised, and which will make it necessary to repeat his trials, some time, with particular precautions.

It is to this tendency of fixed air, to unite itself with water, that we may attribute the phenomenon, observed by Dr. Hales, in the burning of candles. He remarked that the absorption of air continued, not only during their burning, but even several days after. It will be seen, hereafter, in the Chapter which treats of the Experiments made by Dr. Priestley, that the air in which candles have burned, is, in a great measure, in the state of fixed air; that it is consequently

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frequently susceptible of combination with water; and that it is in proportion to this combination that the volume of air continues to diminish. It was, also, owing to this cause that the different kinds of air, which he obtained, did not appear to be susceptible of farther reduction, after they had passed through water. In fact, all the fixable part had combined itself with the water.

THE air in which sulphur has burned is incapable of recovering its elasticity; it remains in the same state, however long a time it may be kept.

Dr. Hales, persuaded that the air separated from bodies, as well as that which had been burned, or been respired by animals, was not different from that of the atmosphere, and that it produced such particular effects, only on account of its being infected and rendered noxious by vapours, which were foreign to its nature, attempted to filter it through flannel which had been steeped in a solution of salt of tartar; and, by this means, he perfectly restored it. The air, when passed through the filter, was found

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fit for respiration. A candle, likewise, placed under a receiver lined with a flannel which had been dipped in a solution of salt of tartar, burned a longer time than it could have done under a receiver which was not lined, although the flannel considerably diminished its capacity. We shall, in future, see what was the effect of the salt of tartar in this experiment, and in what manner it rendered the air salubrious; but one interesting fact should be, here, remarked, viz. that the flannel diaphragms, through which the air had been filtered, were sensibly increased in their weight.

IT is to Dr. Hales also that we are obliged for the information, that a great number of substances, such as pease, wax, oyster shells, amber, &c. furnish, by distillation, air susceptible of inflammation, and which even retains that quality, after it has been washed in water.

ALL the Philosophers of his time believed, that fire became fixed, and combined itself with metals, and that to this addition they owed their reduction to the state of a calx. Dr. Hales has
not

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not discarded this opinion ; but he has moreover advanced that the air contributed to produce that effect, and that to it, in some degree, must be attributed the augmentation in the weight of metallic calxes. He founded this opinion, on his having obtained from 1922 grains of lead, only seven inches of air, by distillation, whereas an equal quantity of minium afforded him 34 inches.

THE Doctor has also remarked that the phosphorus, or rather the pirophorus of M. Homberg, diminished the bulk of air in which it burned ; that nitre could not explode in vacuo ; that air was necessary for the crystallization of most salts ; that vegetables in fermentation, yielded, at first, a great quantity of air which they afterwards resorbed, &c. &c. As to the diminution in the bulk of air which takes place during the burning of some bodies, he sometimes ascribes it to the loss of its elasticity ; sometimes he seems to think that the air is really fixed and absorbed during the combustion, and his work seems to leave this matter in some degree of uncertainty.

HOWEVER

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However this may be, Dr. Hales finishes his sixth Chapter of Vegetable Statics, with concluding, that the air of the atmosphere which is the same which serves us for respiration, enters into the composition of the greater part of bodies, where it exists under a solid form, deprived of its elasticity and of most of those properties by which it is distinguished; that this air is, in some manner, the universal bond of nature, that it is the cement of bodies; that to this is owing the great hardness of some bodies, as also a great part of the gravity of others; that it is composed of parts so durable, that the action of fire, however violent, is not capable of changing them, and, likewise, that having existed for ages in a solid and concrete form, and having passed through trials of every kind, it is capable, under some circumstances, of recovering all its elasticity, and becoming again an elastic, thin fluid, wholly resembling that of our atmosphere*. And he compares air to a true Proteus, now fixed, now volatile, which should

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* Dr. Hales excepts the case of vitrification, "when with the vegetable salt and nitre in which the air is incorporated,

be adopted among the chemical principles, and possess a rank which has, hitherto, been denied to it.

CHAPTER IV.

THE OPINION OF BOERHAAVE ON THE FIXATION OF AIR IN BODIES, AND ON ELASTIC VAPOURS.

THE celebrated Boerhaave, to whom we are indebted for an excellent Treatise on the Elements, does not seem to have been always of one uniform opinion, as to the combination and fixation of air. Sometimes he appears to deny that air can be combined with bodies so as to contribute to the formation of their solid parts; sometimes he seems to adopt the contrary opinion, and to enlist under the banner of Dr. Hales. In short, on reviewing what this author has

porated, he supposes, it may perhaps some of it, with other chymical principles, be immutably fixt." Hales's Vegetable Statics, p. 316.

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has said in different passages of his works, one may plainly discern, that the publication of Dr. Hales's experiments had induced him to change his opinion, and that he adopted, to a certain degree, the system of the fixation of air in bodies; but it is also evident, that this theory did not appear to him sufficiently proved to oblige him to retract, from his works, what he had said on the contrary side of the question.

HOWEVER this be, it is at the end of his treatise on air, that he explains himself in the most particular manner, on the opinion of Dr. Hales; we find there a train of experiments, made with that accuracy which characterises the works of Boerhaave, on the air disengaged from bodies by combination, and we cannot but agree also, that the apparatus he made use of had some advantage over that of Dr. Hales. This advantage consisted in his not permitting the factitious air to have any immediate communication with the surface of the water. We have already observed, that for want of such precaution, it is possible to fall into considerable errors, as to the quantity of air produced or absorbed.

IT

It was in the vacuum of an air pump, and under a receiver of known dimensions that Boerhaave always operated; he took care exactly to pump out all the air, before he made the mixture; and he afterwards judged of the quantity of air produced, by a good barometer. By the aid of this apparatus he discovered that one drachm and half of crab's eyes, dissolved in one ounce and a half of distilled vinegar, yielded eighty one inches of air: that one drachm of chalk yielded, with two ounces of the same acid, one hundred and fifty one inches. That the combination of the ley of tartar, whether with vinegar, or vitriolic acid, also furnished him with a very considerable quantity; that there were other combinations, such as the solution of iron by the nitrous acid, which though attended with considerable effervescence, did not produce any separation of elastic fluid in vacuo; and lastly, that the smoaking spirit of nitre and oil of caraway yielded a separation of air so very considerable, that the experiment was dangerous, unless the operator had the precaution to employ exceedingly large vessels, and to make his trials with only very small quantities.

THESE

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THESE experiments are followed by some accounts of the separation of air which takes place in combustion, in fermentation, in putrefaction, and in some distillations; and the author has finished his Treatise with the following reflections, which, I think, ought to be transcribed at large.

" ALL these different methods, which resemble one another in their acting by means of fire, prove that elastic air enters into the composition of bodies, as a constituent, and, even, " very considerable part. If any one still doubt, " he must at least acknowledge, that by means " of fire, a matter may be extracted from every " known body, which, when once separated, is " fluid and elastic; which is compressible by " weights; which contracts in the cold, and di- " lates either by heat, or by the diminution of " the weight with which it is pressed: but when " what we call elastic air is separated from bo- " dies with which it has been mixed, we are un- " acquainted with any other properties of it than " the above. It must then be acknowledged " that fire separates, from all bodies, an elastic " vapour, and, consequently, that this aerial
" matter

" matter resides in bodies, but in such a manner
" as not to produce the effects of air, as long as
" it is combined and united with them; but
" that whenever it is detached, and joins itself
" with other parts similar to itself, it then re-
" sumes its former nature, and continues to be
" air, till, divided again into its elements, it re-
" unites with other parts of a different kind,
" where it may remain quiet for some time, and
" form one and the same mass without, however,
" losing any of its former nature; for it shews it-
" self always the same, whenever it is freed from
" the bonds which retain it, and is joined with
" other aerial particles of the same nature. It is,
" then, unchangeable in all these different circum-
" stances; disengaged from a body, it becomes
" true air, as before its union, and disposed to
" join with other matter, so as to form a new body,
" such as it had just quitted. No art demon-
" strates this resolution and composition more
" clearly than chemistry; and I should have
" given several examples of it, had I not lately
" read the celebrated Dr. Hales's excellent trea-
" tise on vegetable staticks; in the sixth chapter
" of that book, the author has collected with
" much

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“ much labour and justness, and has related in
“ the best possible order, the experiments which
“ have been made on this subject, and he has
“ exhausted the matter. To these I refer my
“ readers, they will there see how art has arrived
“ at the power of unveiling nature.”

CHAPTER V.

THE OPINION OF M. STALH, ON THE FIXATION OF AIR IN BODIES.

ALTHOUGH some of M. Stalh's writings are posterior to the publication of Dr. Hales's experiments, he does not appear to have adopted any part of his system of the fixation of air in bodies. There is not even any appearance, of his having been acquainted with the Doctor's experiments. At least we find the following passage, written in the year 1731, in his work entitled, *Experimenta, observationes et animadversiones*, §. 47. “ *Elastica illa expansio
“ aëri,*

"aéri, ita per effentiam propria est, ut nunquam
ad vere densam aggregationem nec ipse in se, nec
in ulla mixtionibus coivisse sentiri posset."

CHAPTER VI.

M. VENEL's EXPERIMENTS ON THE
WATERS IMPROPERLY CALLED ACIDU-
LOUS, AND ON THE ELASTIC FLUID
CONTAINED IN THEM.

THUS the impression which Dr. Hales's Treatise had made upon the learned at the time of its publication, did not produce in the theory of Physics and Chemistry, the immediate reformation which it was reasonable to expect. His experiments formed, as it were, only the connecting stones, which it was necessary to join to the edifice of physical knowledge.

M. VENEL, the present Professor of Chemistry in the University of Montpellier, laid the first foundation of this enterprise in two Memoirs

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read

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read in 1750, before the Royal Academy of Sciences; they may be found in the second volume of *Mémoires présentés par les Savans étrangers*. The intention of these Memoirs is to prove, contrary to the opinions of the ancients, and to those of Hoffman and Slarre, that the Seltzer waters, and most of those which we have been accustomed to denominate acidulous, are neither acid nor alkaline; that they owe their sharp, brisk and penetrating taste, and the bubbles which mount to their surface, and which imitate the appearance of champagne, of beer or of cyder, to a considerable quantity of elastic fluid or air combined with these waters in a state of dissolution; M. Venel proceeded so far as to be able to separate this air by simple agitation, to receive it in a moistened bladder, and to measure the quantity. Whatever means he made use of to obtain the same end — whether he employed the air-pump, heat, or Dr. Hales's apparatus, the result was constantly the same, and he uniformly observed that Seltzer water contained about a fifth of its bulk of fixed air.

WHEN the Seltzer water has been deprived of
the

the air which it contained in a state of dissolution, whether by agitation, heat, or any other means, it no longer possesses the properties which constituted it acidulous ; instead of the pungent taste which it before impressed, it becomes flat, and ceases to sparkle ; in short, it returns to the state of common water, only, as M. Venel observes, it contains a small quantity of sea-salt.

M. VENEL thought proper to pursue his researches still further, and having proved that the Seltzer water owed its properties to the air which it contained, he endeavoured to combine air with water, and to form anew an aerial water similar to that of Seltzer. The following are nearly the reflexions by which he was guided in his experiments.

AIR, says he, is soluble in water * ; this is demonstrated by the example of brisk wines and Seltzer water ; but the particles of this air are

* M. VENEL has always supposed the elastic fluid contained in mineral waters to be common atmospheric air ; it will appear in the sequel how far this opinion is to be embraced.

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to be considered as having a stronger affinity with each other, than with the fluid employed to dissolve them; from whence it follows that the solvent cannot, of itself, have sufficient force to destroy the aggregation of the air; and that a previous condition to its dissolution is the breaking of this aggregation.

No method appeared to M. Venel more likely to produce this effect, than to decompose salts in the water intended to dissolve them. Thus he might excite an effervescence, and consequently let loose a large quantity of air; and this air being in a state of absolute division, it would therefore be under circumstances the most favourable to its dissolution.

M. VENEL was still further confirmed in this opinion, by the following mode of reasoning. Effervescence is, according to him, nothing else than a true precipitation of air: Two bodies, when uniting, produce an effervescence from this cause only, that they have a greater affinity with each other, than the one or both of them have with the air to which they were united; but it is

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is well known that in many chemical precipitations, if the operation be made in much water and the precipitate be soluble in that fluid, it will be redissolved as fast as it is precipitated: The same thing should happen to air under similar circumstances.

AFTER all these reflexions M. Venel dissolved in a pint of water two drachms of fossile alkali, to which he added an equal quantity of marine acid; being previously assured of the following facts: 1st. that this was precisely the proportion necessary to a perfect saturation: 2dly, that it was also the same which he had observed in the Seltzer water. He had the precaution to make use of a vessel with a narrow neck, and to prevent the escape of the air, by disposing the ingredients in such a manner, that they could not communicate with each other till after the bottle was corked. By this method he was able to compose a water, not only analogous to that of Seltzer, but much more strongly impregnated with air. We have already seen that the Seltzer water does not contain more than a *fourth* or *fifth*, whereas M. Venel could introduce into his factitious water nearly *half* its bulk of air.

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THESE

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THESE experiments of M. Venel, however, were insufficient to explain a singular phenomenon which seemed contradictory to his opinion : M. Hoffman had observed that the waters of Troplitz and Piperine in Germany, as well as many others which are spirituous or acidulous, do not contain any portion of saline matter: it was therefore evident that these waters do not acquire their air by the method employed by M. Venel ; and it plainly follows that in such case, *this* process is not that of nature.

THE explanation of this phenomenon was reserved for Messrs. Cavendish and Priestley ; but previous to my recital of their experiments, which are much more modern, the order of facts obliges me to give an account of those made by Dr. Black, Professor in the University of Glasgow. This author may be truly regarded as the person who first introduced Fixed Air into Chemistry.

C H A P.

CHAPTER VII.

DR. BLACK'S THEORY CONCERNING THE
FIXED OR FIXABLE AIR CONTAINED IN
CALCAREOUS EARTHS, AND THE
PHENOMENA PRODUCED IN THEM BY
DEPRIVING THEM OF THIS AIR.

MAGNESIA, calcareous earth, and all the earths in general which are reduced to quick-lime by calcination, consist only, according to Dr. Black, of a combination of a large quantity of fixed air with an alkaline earth, naturally soluble in water. By the term *fixed air* Dr. Black means a species of air different from common elastic air, dispersed nevertheless in the atmosphere. He informs the reader, that it may appear wrong to make use of that name, but he rather chose to retain a word already known in natural philosophy, than to invent a new one, while we were imperfectly acquainted with the

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nature and properties of the substance so deno-
minated.

FIXED AIR, according to Dr. Black's experiments, may be separated, in two different ways, from calcareous earth; either by fire, or by dissolution in acids. The calcareous earth, in the first case, viz. by calcination, loses more than half its weight---The remainder is an earth, absolutely deprived of air, and which, consequently, no longer effervesces with acids. Lime (which is the name we usually give to calcareous earth in this state) owes its causticity, according to Dr. Black, to the great affinity which it has with the air of which it has been deprived by calcination; thus, as soon as it is applied to any animal or vegetable substance, it attracts eagerly the air contained in it, effects a decomposition; and it is this decomposition or kind of destruction which is improperly denominated burning or cauterising.

THIS property, which is possessed by lime, of attracting the air from other bodies, furnishes us with the means of communicating causticity to fixed

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fixed and volatile alkaline salts. If to a solution of fixed alkali, be added a certain quantity of lime, it attracts to itself the whole of the air contained in the alkali; at the same time it loses all the constituent properties of lime; it becomes capable of effervescing with acids, and insoluble in water; in short, it returns to the state of common calcareous earth: on the other hand, the fixed alkali which has been deprived of its air, no longer ferments with acids, is incapable of being crystallized, becomes caustic, and when dried by fire, and reduced to a concrete form, it is called the *stronger common caustic*.

THE same thing happens to the volatile alkali. If we distill sal ammoniac with chalk, we obtain a volatile alkali in a solid form, which effervesces with acids; but if instead of chalk, we employ a calcareous earth deprived of its air, otherwise called lime, the volatile alkali, as fast as it is separated, is robbed of its air by the lime, and passing into the receiver in a fluid form, is a caustic volatile alkali which neither effervesces with acids, nor can be formed into crystals. It appears from Dr. Black's experiments, that the attach-

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attachment of fixed air is not equal to all bodies; but that it has a greater affinity with calcareous earth than with fixed alkali, with fixed than with volatile alkali, &c.

ANOTHER method of detaching, from calcareous earth, the air with which it is combined, is by the addition of acids. If we dissolve lime-stone or chalk in any acid, a brisk effervescence, or which is the same thing, a considerable separation of fixed air, ensues; the earth, which has a greater affinity with the acid than with the air, abandons the latter, which, recovering its elasticity, immediately escapes, and is dissipated and mixed with the common air of the atmosphere. If, afterwards, we precipitate the earth from this solution, we may obtain it, as we please, either in the form of chalk or of lime; of chalk if we employ a common alkali for the precipitation, but of lime if it be effected by means of a caustic alkali, or alkali deprived of its air. What is here particularly to be remarked is, that, as Dr. Black informs us, the lime-stone loses nearly the same weight in this process as by calcination; and that it recovers its former weight

weight when it has been precipitated in the form of a calcareous earth, that is to say, with all its air.

DR. BLACK explains on the same principle, why lime is not totally soluble in water; why the part which is dissolved is so easily converted into a pellicle, insoluble in water, and known by the name of *Cream of Lime*. Calcareous earths, according to his opinion, have a stronger affinity with air, than they have with water; consequently, on adding lime to water, one part of the lime will attract from the water the fixed air contained by the latter, and be precipitated as a calcareous earth; but, at the same time, another portion of the lime, not finding a sufficient quantity of fixed air to saturate it, will be dissolved in the water, and form lime-water; if we then expose this water to the air, presently the particles of lime nearest to the surface attract the fixed air floating in the atmosphere; they again become insoluble, and collecting together on the surface, form an insoluble pellicle, which no longer retains the properties of lime, nor differs from calcareous earth. A proof of the truth

of

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of this theory is, that the reduction of the lime into a calcareous earth may be prevented, by keeping the lime-water in bottles well stopped, so as to prevent any contact with the circumambient air.

DR. BLACK has also observed, that Magnesia, the basis of Epsom salt, has the property of sweetening lime-water; and, therefore, that fixed air has a stronger affinity with calcareous earth than with the basis of Epsom salt. Lastly, from all these experiments, Dr. Black concludes, that we may make the following alterations in the column of acids, in Mr. Geoffroy's Table of Affinities; and that we may also add a new column, considering alkaline substances in their natural state, and deprived of their fixed air, as follows:

ACIDS.

Fixed Alkali.

Calcareous Earth.

Volatile Alkali.

Magnesia.

FIXED AIR.

Calcareous Earth.

Fixed Alkali.

Magnesia.

Volatile Alkali.

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THE limits of an abstract do not permit me to enter here into a detail of the great number of interesting experiments relative to the diminution of weight, which alkalis suffer after they have been dissolved in acids; to the manner of rendering alkalis caustic by fire, &c.

I MUST not however omit to add, before this article be concluded, that Dr. Black suspected, that the fixed air of alkaline salts unites itself to metals, during their precipitation from acids; and that to this cause, the augmentation of weight, observable in these precipitates, and perhaps the surprising effects of the pulvis fulminans, may be attributed*.

C H A P.

* It is thought necessary to acquaint the reader, that the theory of fixed air, had not acquired the perfection and confidence which is given to it in this article, when it came from the hands of Dr. Black. It did not arrive to this point till after the publication of M. Jacquin's work, of which we shall shortly give an account. I judged it proper to make this remark here, not with a view to take in the least from the sentiments of respect and admiration which

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CHAPTER VIII.

ON THE ELASTIC FLUID SEPARATED FROM
GUN-POWDER; BY THE COUNT DE
SALUCES.

WHILE Dr. Black was publishing in England, the theory of which we have just now given an account, the Count de Saluces employed himself at Turin, in some very interesting inquiries into the nature of the elastic fluid which is detached from gun-powder, during its detonation. He has discovered, that this fluid,

which are due to the desert and genius of Dr. Black, to whom, without equivocation or division, the merit of the invention belongs, but to render to M. Jaquin the justice that is due to him, and to avoid a remonstrance on his part, which would have been well founded. Further, we shall see presently, that M. Jaquin has departed from the opinion of Dr. Black, so far as to suppose the fixed air to be the same with that which composes our atmosphere.

fluid, when at liberty, occupies a space two hundred times greater than that of the powder from which it has been disengaged. A number of experiments convinced him, that this fluid was, like the air of the atmosphere, elastic ; that, like the latter, it was compressible in proportion to the weight with which it was loaded ; but that it differed, notwithstanding, in these particulars, that it extinguished the flame of a candle, and also was mortal to those animals who breathed it. He attempted to filter this air, through linen or through gauze, well impregnated with a solution of fixed alkali ; there remained in the filtre, a small quantity of coaly matter of fixed alkali, and some traces of vitriolated tartar. The air, after this operation, had lost all its noxious properties, and did not seem to differ in any respect from common air.

ANOTHER method which the Count points out to restore to the air, separated from gun-powder, all the properties of common air, is to keep it, for twelve hours in a degree of cold, equal to that in which water freezes. He assures us, that he had repeated the same experiment on air detached

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tached by the effervescence of an acid with an alkaline substance, and with the same success.

INDEPENDENT of these experiments, which tend essentially to the object which the Count de Saluces had in view, his memoirs contain several others, which throw light on the theory of the combination of air with bodies. He observed, that the air disengaged from effervescing substances, for the most part, extinguished flame; that what was separated in the combination of the volatile alkali with vinegar, was an exception to this general rule; that the nitrous acid, when mixed, in *vacuo*, with a fixed alkali, produced no air; that this combination remained chiefly deliquescent, as long as it was continued in *vacuo*, but that it crystallized when it had been exposed for some time to the air. This experiment, joined to those of Dr. Black on the crystallization of fixed alkali, seems to give occasion to suspect, that the combination of air is necessary to the formation of saline crystals.

THE Count de Saluces further observes, that gun-powder explodes, in air however infected, whether

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whether it be by the burning of sulphur, whether candles have been extinguished in it, or whether it have been disengaged by the explosion of another portion of the same powder. He then shews that the phænomena of the pulvis fulminans are the same with those of gun-powder; that they are occasioned by the separation of the same elastic fluid; but, what is very singular, is that the quantity of that fluid which is detached from the pulvis fulminans is less than what is disengaged from gun-powder; from whence the Count concludes that the effects are less in proportion to the quantity of air separated, than to the rapidity, and if I may be allowed the expression, the *instantaneity* of the separation. I take no notice here of an infinite number of interesting facts with which the Count's memoir is replete, because they are rather foreign to my subject; I shall only add, before I finish this article, that the Count de Saluces only admits of one species of air, in which opinion he differs essentially from that of Dr. Black.

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CHAPTER IX.

DR. MACBRIDE's APPLICATION OF DR.
BLACK's DOCTRINE OF FIXED AIR TO
THE EXPLANATION OF THE PRINCIPAL
PHENOMENA OF THE ANIMAL ECO-
NOMY.

HITHERTO the existence of fixed air, and its combination with bodies was but a physical opinion, founded on particular experiments; but no physiologist, since Van Helmont, had adopted it. M. Haller was the first who, from Dr. Hales's experiments, has instructed us, that the air was the real cement of bodies, that this was the principle which fixing itself in the solids and fluids served as a bond to the elementary parts, and united them to each other.

*Videtur aer vinculum elementorum primarium
constituere, cum non prius ea elementa a se invicem
discendant*

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discendant quam der expulsus fuerit. Haller, *Elementa Physiologie*, Tit. I. Cap. I.

Gluten præstat verum moleculis terreis adunandis, ut constat exemplo calculorum lapidum, aliorum corporum durorum; in his omnibus solvitur tunc deum partium vinculum quando aer educitur. Ibid. Scel. 244.

A very numerous and accurate series of experiments appeared in 1764 in support of this doctrine. Dr. Macbride of Dublin, the author of the essays which contain them, possesses too distinguished a rank among the writers on this subject, for us to omit entering into a detail of the important facts in natural philosophy and physiology, the discovery of which we owe to him.

It is demonstrated by Dr. Macbride's experiments, that fixed air is separated, not only from effervescing substances and from vegetable matter during fermentation, but also from all animal substances as soon as they begin to putrefy; and to prove the very great facility with which

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this air is capable of uniting itself, either with lime or with the fixed or volatile alkalis ; he made use of what is now called Dr. Macbride's apparatus, though the original idea is due to Dr. Black. We shall endeavour to describe the manner in which he operated. He placed successively in a bottle, saline substances in the act of effervescence, vegetable substances in fermentation, and lastly, animal substances which had begun to putrefy ; he obliged the air which was separated from them to pass through a bended tube, into a bottle or flask which successively contained lime water, and the fixed and volatile alkalis in a caustic state. As soon as the fixed air, detached from these different bodies, came into contact with the surface of the lime water, it became turbid, and soon after the earth gradually subsided in the form of calcareous earth, viz. possessed of all its air and without any signs of causticity. So likewise the fixed and the volatile caustic alkalis, in proportion as they became combined with the fixed air, recovered their property of effervesing with acids ; and as soon as they were in a sufficiently concentrated state, reassumed their solid form and crystallized

in

in the bottle. This experiment shews us, that if the fixed vegetable alkali has not the property of crystallizing, it is because, being prepared by a strong fire, we generally procure it deprived of some part of the air which it naturally possesses. If therefore we restore to it this quantity of air, it at the same time recovers its property of crystallizing. We find the first traces of this last discovery in Dr. Black's Memoirs.

THE different experiments made by Dr. Macbride on the great quantity of fixed air which is separated from animal substances during their putrefaction, led him to conclude, that on the presence of this elastic fluid or fixed air in flesh depend its firmness, its cohesion and state of soundness; that in proportion as the fixed air is separated from it by the fermentation, its texture is destroyed, its constituent parts become disunited, and separate to reunite in a different manner, and to form new combinations very different from the former.

IT is easily perceived that this doctrine is nearly the same as that which had been taught by

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Van Helmont ; but one important discovery, supposing the fact to be sufficiently proved, * belongs wholly to Dr. Macbride, viz. that flesh which is half putrid and has lost a portion of the fixed air which entered into its composition, may recover its former sweetness, by restoring to it the fixed air of which it had been deprived; to produce this effect, it will be sufficient to expose it to the vapours of any fermenting matter, or rather to a current of fixed air from an effervescing mixture; in short, to introduce the fixed air by any means whatsoever.

DR. MACBRIDE applies these different discoveries to explain the phenomena of the theory of digestion; he shews that all the alimentary mixtures which we commonly use, are susceptible of fermenting in a short time; that animal and vegetable substances when mixed, have a greater aptitude to ferment, than either of these substances possess separately; and that from all the alimentary mixtures, on which he has made a numerous train of experiments, he always separated a considerable quantity of fixed air.

Dr.

* Vide Henry's Experiments and Observations, page 114 & seq.

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Dr. Macbride supposes that the same separation must take place in the stomach of an animal; but what becomes of this fixed air? He imagines that it is either absorbed and combined with the chyle, and passes, in that state, into the circulation of the blood, or rather that it is absorbed in the intestinal canal, by particular vessels, adapted to this kind of secretion: in either case the air is carried off either by perspiration or urine. This theory leads Dr. Macbride to engage in a long train of experiments on the greater or less quantity of fixed air, contained in the different animal secretions. Lime water appeared to him to be the proper test in this inquiry; for as lime has a great affinity with fixed air, whenever any liquor containing that air is mixed with it, it greedily absorbs it, saturates itself with it, and now no longer soluble, it is precipitated and subsides in the form of a calcareous earth. By this mode of trial, Dr. Macbride discovered that blood, newly drawn, contained a great quantity of fixed air; pursuing his experiments still futher he discovered that this air resided in the red part of the blood while the serum was free from it. By experiments of a similar kind, he also was informed that the sweat and urine

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contained

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contained much fixed air, whereas on the contrary the bile and especially the saliva, so far from containing it, had a tendency to absorb it.

It would be too tedious, here to relate the several experiments made by Dr. Macbride on the fermentation of alimentary mixtures, and on the means of accelerating or retarding fermentation. Let it suffice to say that they led the author to the most important reflections on putrid diseases, and the sea-scurvy, which according to Dr. Macbride's theory of putrefaction, owe their origin to the privation of that certain quantity of air which is necessary to a state of salubrity. It is also observable that the diet, which is most unsuitable in these diseases, is that composed of animal food, which, by Dr. Macbride's account, yields much less air by fermentation than vegetables; the method of cure on the contrary consists in the use of a vegetable diet, and of all those substances which are capable of furnishing an abundance of fixed air. On these principles Dr. Macbride recommends the use of malt as a cure for the sea-scurvy; this substance furnishes a decoction very proper for fermentation

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fermentation and which supplies a larger quantity of fixed air than any other vegetable body. With the same view he prescribes sugar and water, and other similar liquors.

THE antiseptic quality which is so universally known to belong to acids, Dr. Macbride attributes solely to the particular property which they have of uniting with the alkaline particles of the putrefying substance, and thereby neutralizing them; but as a remedy, he esteems them to be rather palliative than curative, because they do not like fixed air, restore the part to its natural state.

INDEPENDENT of the experiments already recited, which are essentially connected with Dr. Macbride's theory, his Treatise contains many others of which the following are the principal:

1st. THE separation of fixed air from fermentative mixtures is accelerated in Boyle's vacuum.

2dly. CALCAREOUS earths have the property of hastening putrefaction.

3dly. LIME

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3dly. LIME produces a very particular effect on animal substances. It decomposes them by absorbing the fixed air which they contain, and thereby produces an effect, in some measure, analogous to that of putrefaction.

4thly. THE union of oil and fixed alkali takes place according as the latter is deprived of its air. If the vapours, arising from an effervescing or a fermentative mixture, be suffered to pass into a solution of soap, the fixed air which is separated, combines itself gradually with the fixed alkali of the soap, and the oil being set at liberty, swims on the surface.

5thly. ARDENT rectified spirits absorb fixed air when exposed to it.

THE Doctor has also proved that the volatile alkali, which is discharged from animal substances, in the progress of putrefaction, is sometimes in its natural state or saturated with air, sometimes, on the contrary, wholly divested of its air, and in a caustic state. For example, he discovered by a series of experiments, that putrefied

putrefied blood, as well as the spirit drawn from it, effervesced with acids; whereas bile, though equally putrid, and also the liquor which runs from putrefied flesh, did not produce any effervescence; neither did the spirit distilled from them differ, in this respect, from the liquors themselves.

FROM all these experiments, Dr. Macbride concludes that fixed air is an elastic fluid, very different from atmospheric air; that the former may be introduced without danger into the intestinal canal, as well as into other parts of the animal economy, without occasioning any disorder; whereas atmospheric air under the same circumstances, would be fatal in its effects: but yet, on the contrary, it is impossible for animals to live without respiring continually the fluid which forms our atmosphere, whereas fixed air inspired into the lungs, is a subtle poison occasioning immediate death: That fixed air very easily unites with lime or with alkaline salts, whereas we cannot, by the same methods, combine them with the air of our atmosphere. Lastly, he adds, that fixed air is found to be dispersed

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dispersed in our atmosphere, since lime and caustic alkalis, in time, lose their distinguishing properties, and acquire that of effervescent with acids. These conclusions are nearly the same as those of Van Helmont.

CHAPTER X.

EXPERIMENTS MADE BY THE HONOURABLE
MR. CAVENDISH ON THE COMBINATION
OF FIXED AIR WITH DIFFERENT SUB-
STANCES.

SOON after the publication of Dr. Macbride's Treatise, Mr. Cavendish communicated to the Royal Society of London some new experiments which tended equally to confirm Dr. Black's doctrine; they are to be found in the Philosophical Transactions for the years 1766 and 1767. Mr. Cavendish has proved that the quantity of fixed air contained in fixed alkali when fully saturated with air, is five-twelfths of its weight, and seven-twelfths in volatile alkali,
which

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which great quantity of air is sometimes the cause of the brisk effervescence which appears in a calcareous earth after it has been dissolved in the nitrous acid and precipitated by an alkali thus saturated with air; for in fact, as the precipitant furnishes more air than the precipitated matter can absorb, there is necessarily a portion at liberty, which, recovering its elasticity, occasions the effervescence.

MR. CAVENDISH farther shows that water is capable of absorbing a volume of air more than equal to itself; that this quantity is proportionably greater as the water is colder, and is compressed by a heavier atmosphere; that water thus impregnated with fixed air has an acidulous, spirituous, and not disagreeable taste; and lastly, that it has the property of dissolving calcareous earth and Magnesia. It follows, as a consequence of this property of water impregnated with fixed air, that if after precipitating the lime from lime-water by throwing fixed air into it, still more of the same air be added, the water becomes capable of redissolving a part of the earth which had been precipitated.

WATER

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WATER impregnated with fixed air has also the property of dissolving almost all the metals, and especially iron and zinc; a very small quantity of these metals is sufficient to communicate to water their taste and virtues*.

THESE circumstances seem to explain, in the most natural manner, how the most pure distilled water dissolves iron, as appears from Mr. Monet's observations, and why that combination takes place more readily in cold than in hot water: the reason is this, the water acts on the metal only in proportion to the fixed air which it contains; and it has been already observed that it contains less in proportion to its heat. For this reason we are not able to obtain the least particle of vitriol from most of the mineral, ferruginous waters.

WE are also informed by Mr. Cavendish, that
fixed

* THOUGH M. Lavoisier has placed this discovery here, he acknowledges it does not belong to Mr. Cavendish. It is, in reality, the property of my very ingenious friend Mr. Lane of Aldersgate-street; and was made in consequence of a conversation with Dr. Watson, junior. Vide Philosophical Transactions, Ann. 1769, and Sir John Pringle's Discourse on the different kinds of Air, page 11. T. H.

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fixed air may be combined with spirit of wine and with expressed oils, but that these substances do not, in other respects, obtain any new properties by the union ; that the vapour of burning charcoal occasions a remarkable diminution of air, and, that at the same time, a quantity of fixed air is produced in the operation which is capable of being absorbed by soap ley. And to conclude, Mr. Cavendish is the first who has remarked, that a solution of copper in spirit of salt, instead of yielding inflammable air, like that of iron or zinc, afforded a particular species of air, which lost its elasticity as soon as it came into contact with water.

C H A P T E R XI.

M. MEYER's THEORY CONCERNING THE CALCINATION OF CALCAREOUS EARTHS, AND THE CAUSE OF CAUSTICITY IN LIME AND IN ALKALIS.

WHILE the doctrine of fixed air was peaceably established in England, a formidable opponent to it arose in Germany.
Nearly

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Nearly at the same time that Dr. Macbride published, in English, the Essays of which we have just given an account, a very elaborate Treatise was published, written in German, by M. Meyer, Apothecary at Osnabruck, entitled, *Essays in Chemistry, on Quick-lime, the Elastic and Electric Matter, Fire, and the universal primitive Acid.* This Treatise contains a great number of experiments, for the most part accurate and true, from which the author has deduced consequences totally opposite to those drawn by Doctors Hales, Black and Macbride. There are few modern books of chemistry which display more genius than this of M. Meyer; and if his ideas were to be adopted, the consequence would be nothing less than a new theory directly contrary to that of Stalh, and of all the modern chemists.

MR. MEYER, first examines the nature of the calcareous stones of the sparry kind, and of substances proper to make lime. He remarks that these substances are rarely pure, that they are commonly mixed with sand and other foreign matter; but that the part really proper to make lime, is nothing more than a pure earthy alkali, insoluble

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insoluble in water, susceptible of combination with acids, in which it dissolves with effervescence, &c. He observes, that as soon as these substances have been exposed, a sufficient time, to the action of a strong fire, they suffer a large quantity of water to escape; that after this operation they come out with the property of being wholly soluble in water, and of no longer effervescing with acids. From these new properties M. Meyer concludes, that the lime, while in the fire, has been neutralized by a particular acid, to which, as a medium, its solubility in water is due, and whose union with it has deprived it of its property of effervescing with acids. To confirm this theory, Mr. Meyer poured into lime-water, drop by drop, some lixivium of fixed alkali. The lime-water presently became turbid, and the lime subsided under the form of a calcareous earth, insoluble in water as before its calcination; the alkali, on the other hand, had acquired the causticity of the lime, and a part of its other properties. From whence Mr. Meyer concludes, that the acid which was united to the lime, and rendered it soluble, has more affinity with fixed alkali than with lime; that it aban-

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dons the latter, and unites with the fixed alkali. The same thing happens when lime-water is precipitated by the volatile alkali, or on separating, by means of lime, the volatile alkali from sal ammoniac: in all these cases, the acid of the lime neutralizes the salt, renders it caustic, uncrystallizable, and deprives it of the property of effervescing with acids. The acid substance, which the lime thus attracts in the fire, Mr. Meyer calls *acidum pingue*; he supposes it to be a substance nearly approaching to that of fire and of light; that it is by the aid of this acid, that lime unites with oil, dissolves sulphur, &c. Lastly, it is Mr. Meyer's opinion, that the *acidum pingue* enters very abundantly into the composition of vegetables and animals; that it is this which escapes from charcoal when burning, from wood when it consumes, &c.

MR. MEYER goes on to point out its combination with a great number of bodies; he supposes it to exist in metallic calces, in minium, and that it may be made to pass from them into either the fixed or volatile alkalis, which thereby acquire a state of causticity. It is principally in this

this article that Mr. Meyer's theory seems to have the advantage over the English system. In fact the theory of the acidum pingue explains in the most natural and most simple manner the augmentation in the weight of the metallic calces, their action on sal ammoniac, the separation of the volatile alkali from that salt by minium, litharge, and many other metallic calces. In every case, it is the *caustic* of fire, the *acidum pingue* which unites with the metals by calcination, which passes afterwards into the volatile alkali, and forms a species of neutral salt similar to that which is obtained with lime.

MR. MEYER obviates one capital objection which might be made to his theory on Dr. Black's system. The latter has advanced, that if a pure calcareous earth be dissolved in the nitrous acid, and afterwards precipitated by an alkali, we may have, as we chuse, the earth precipitated either in the state of calcareous earth or of lime. If it be precipitated by the common fixed, or concrete volatile alkali, it is obtained in the state of calcareous earth; but in that of quick-lime, on the contrary, if it be precipitated by either the

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fixed or volatile *caustic alkali*. Dr. Black has explained this phenomenon in the following manner: Calcareous earth, dissolved in spirit of nitre, no longer contains any air, for it has been dissipated, during the combination, by the effervescence. If, then, the earth be precipitated from this solution by a common fixed alkali saturated with air, in proportion as the alkali unites with the acid, it abandons all its air, which is conveyed to the earth, and precipitates it under the form of a calcareous earth; if on the contrary it be precipitated by a caustic alkali, viz. by an alkali deprived of its air, the earth, not finding, in the mixture, any thing which can supply it with air, subsides in the state of lime.

THE simplicity of this explanation does not at all disconcert Mr. Meyer, and he answers in a manner also quite natural. When we precipitate a solution of calcareous earth by a caustic alkali, we mix, as it were, by his account, two neutral salts together; the one a nitre with an earthy basis, the other composed of the acidum pingue and fixed alkali. A double decomposi-

tion

xion, then, should take place in the mixture. The nitrous acid should quit its basis to unite with the fixed alkali, and, at the same time, the *acidum pingue* being at liberty, should attach itself to the calcareous earth and precipitate with it in the form of lime, viz. soluble in water, and deprived of the property of effervescing with acids. But the event must be different when we precipitate by a common alkali; for as that does not contain the *acidum pingue*, the precipitate falls as a calcareous earth.

IT would be too tedious to follow Mr. Meyer in the comparison which he makes between the *acidum pingue* and the matter of fire, that of light, the electric, and the phlogistic matter. I should be led, besides, into details too extensive for my design. This chemist, it must be confessed, gives himself up too much to a propensity which all those have, who believe they have discovered a new agent, and apply it indiscriminately to every thing.

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C H A P T E R XII.

AN EXPLANATION OF DR. BLACK's THEORY
OF FIXED AIR, BY MR. JACQUIN.

THE English doctrine attacked by Mr. Meyer, soon found a defender. Mr. Jacquin, Botanical Professor at Vienna, published in 1769, a Latin dissertation in its favour intitled ; *A Chemical examination of Mr. Meyer's Doctrine, of his Acidum pingue, and of Dr. Black's Doctrine concerning the Phenomena of Fixed Air, with regard to lime.* This dissertation, though it may not have added much to what had before been done by Messrs. Black and Macbride, may be considered as an excellent work, on account of the method and clearness with which the facts are there related, the choice of experiments which it contains, the simplicity and accuracy of the processes, and lastly of the right manner of philosophizing observable in it.

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THE first observation which struck Mr. Jacquin, was that lime loses, during its calcination nearly one half of its weight. This peculiarity, which rendered Mr. Meyer's theory suspected by him, engaged him to perform the calcination of lime-stone in close vessels. For this purpose, he took a stone-retort capable of bearing the action of fire; into this he put thirty-two ounces of lime-stone; he then adapted to it a large tubulated receiver, and proceeded to distillation.

AT first he employed but a moderate fire, and obtained only some phlegm; but, presently, having raised the fire, an elastic vapour began to separate very plentifully, which continued to fly off during an hour and half, with an hissing noise, through the tube of the receiver: this vapour Mr. Jacquin supposes, to have been nothing but air. The operation being finished he found no more than seventeen ounces of calcareous earth in the state of lime, in the cucurbit, and two ounces of phlegm containing some slight traces of volatile alkali in the receiver.

THE thirteen deficient ounces Mr. Jacquin at-

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tributes

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tributes to the air; hence it follows, according to him, that lime-stone contains six or seven hundred times its bulk of air.

THE object of several experiments, which are related after this, is to prove that lime-stone becomes quick-lime, only in proportion to the quantity of elastic fluid which is disengaged from it; and that if, for example, we draw off nothing but the phlegm, and then extinguish the fire, the lime-stone will be found in the retort, nearly in the same state in which it was put into it. What, according to Mr. Jacquin, proves still more satisfactorily, that it is not the deprivation of water only which constitutes the lime, is that if instead of stopping the operation as soon as the air begins to be disengaged, we continue it somewhat longer, the limestone is reduced into quicklime on its surface, without being so inferiorly.

THESE first experiments led Mr. Jacquin to some reflections on the manner in which air may exist in bodies; and he makes a distinction between the air which only enters into their pores and

and that which enters into their composition. The first may be rendered sensible by the mere experiment of the air pump; the latter, on the contrary is in a state of division or dissolution which does not permit it to enjoy its elasticity.

We know that lime is capable of being dissolved in water; that lime-water exposed to the air affords a pellicle which is no longer lime, but a calcareous earth which effervesces with acids. Mr. Jacquin agrees in opinion with all Dr. Black's disciples, that this substance is nothing more than lime which has recovered the air of which it has been deprived, and he shews that it recovers, in proportion, the weight which it had lost by calcination. This tremor calcis calcined again loses $\frac{13}{32}$ of its weight; its air is separated during the calcination; in short, every thing declares it to have repassed to the state of lime-stone.

MR. JACQUIN, afterwards, examines the action of water upon lime; he demonstrates that it extinguishes the lime without restoring to it its air, because we may preserve the lime under the water, as long as we please, without its ceasing

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ing to be lime, provided that we can keep the surface of the water from contact with the open air, otherwise the whole must be converted, successively and in process of time, into a tremor calcis. He shews also, that if we evaporate lime-water, by way of distillation, the earth which remains in the cucurbit is still lime, and by no means calcareous earth. All these experiments prove further that it is not the absence or presence of water which constitutes the state of lime or of calcareous earth.

MR. JACQUIN afterwards reviews all the experiments of Doctors Black and Macbride; and he has also added some, which are new, with the same intentions. He shews that every mixture of chalk or of common alkali, with an acid, produces air which has the property of precipitating lime-water; viz. of uniting with the lime which is dissolved in the water, of converting it into a calcareous earth, rendering it insoluble, and making it crystallize immediately. The air which separates from lime-stone, during its calcination, has the same property.

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MR. JACQUIN opposes these experiments and those of Messrs. Black and Macbride to Mr. Meyer's theory, and he draws, from most of them, objections which to him appeared insurmountable.

MR. JACQUIN had before observed, that whenever the air is dissolved, and combined with certain substances, it has, as in all the chemical combinations, 1st. a point of saturation; 2dly. a certain degree of adhesion, which is greater or smaller in proportion to the difference of affinity which it has with these different substances. He applies these reflections, in the clearest manner, to the formation of caustic alkalis; he maintains that lime only acts upon them by means of the greater affinity which fixed air has with it; and he also establishes it as a principle, with Black and Macbride, that lime, the common caustic, and all caustics of that kind, act so powerfully on animal substances, only by attracting their air of which they are extremely greedy, and that as this air is essential to their combination, a decomposition ensues.

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MR. JACQUIN has also repeated the experiments of Messrs. Black and Macbride on the means of making lime in the moist way. If calcareous earth be combined with nitrous acid in a long necked bottle, we perceive, after the effervescence, that the chalk has lost nearly half its weight, viz. that it has lost all the air which constituted it calcareous earth; it is then in the state of lime. If we wish to obtain it alone in the same state and separated from the nitrous acid, we have only to precipitate it by a caustic alkali; the earth which remains, when washed from the salt, is a true lime soluble in water.

THIS dissertation of Mr. Jacquin, as has already been said, contains only a small number of new facts, the foundation belongs, entirely, to Messrs. Black and Macbride; but there is to be discovered in his experiments much more order than in those of the two English authors; and it may be regarded as a complete treatise on the causticity of lime, and of alkalis, upon the hypothesis of Dr. Black. The fear of falling into repetitions does not permit me to avail myself of an infinite number of interesting details which constitute

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constitute a part of the merit of this work, and which announce very great perspicuity in his ideas, and much method in the manner of communicating them.

C H A P T E R XIII.

A REFUTATION OF THE THEORY OF MESSIEURS BLACK, MACBRIDE, AND JACQUIN, BY MR. CRANS.

DEATH removed Mr. Meyer from among the learned about the time that Mr. Jaquin's work appeared; but his doctrine had already made a rapid progress in Germany, had been adopted by chymists of reputation, and had begun to receive public notice in the schools. Mr. Jacquin's work, therefore, did not meet with a friendly reception there; and in 1770, Mr. Crans, physician to his Majesty the King of Prussia, published against him at Leipsick, a Latin work, entitled, *A Refutation of the chemical Examination of Meyer's Doctrine concerning the Acidum Pingue; and of Black's Doctrine concerning*

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cerning fixed Air, as relative to Quick-lime, in octavo, 212 pages.

THE experiments, related by Mr. Crans, are so very numerous, that I should exceed the bounds to which I have confined myself, if I were to enter into a detail of them all: I shall only endeavour to give some idea of the principal ones, and I shall especially select those which seem more directly to oppose the doctrine of fixed air.

MR. CRANS first inquires into the action of fire on lime-stone. He agrees with Dr. Black's disciples, that this substance loses a considerable portion of its weight in the fire, but he attributes this loss to the great quantity of water which it contained, and which was driven off by the force of the fire. It is also to the water reduced to vapours, or in a state of expansion, that he attributes, chiefly, the elastic separation observed by Mr. Jacquin, during the calcination of lime-stone in close vessels; but he has not produced, beyond this, any decisive proof of this assertion.

MR. CRANS is of opinion, that limestone when calcined

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calcined is not deprived of the property of effervescing with acids, as Dr. Black's followers pretend, and he quotes, on this subject, the testimonies of Messrs. Duhamel, Geoffroy, Homberg, and Pott, who have all declared that lime effervesces with acids. He here adds some experiments of his own; these were made on lime under different circumstances, and which had especially been carefully preserved from contact with the air, yet he uniformly observed an effervescence.

He objects on this occasion, that if lime only differ from calcareous earth in being deprived of its air, and in the great affinity which it has with it, it ought, in a short time to reabsorb in the open air, the whole of the air of which it has been deprived, and to become again a calcareous earth; he has observed however that lime may be preserved a long time in the air without losing its nature; he even assures us that after a considerable length of time, it acquires greater causticity.

AFTER having examined the appearances
which

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which limestone exhibits during its calcination, Mr. Crans proceeds to the flaking of lime. He observes that the sudden swelling, and that considerable heat which is remarkable during the operation, and which is so natural a consequence by Mr. Meyer's system, is absolutely inexplicable by Dr. Black's hypothesis, which also affords no better reason why calcareous earth dissolves with very little heat in the nitrous acid, whereas the dissolution of lime in the same acid produces a degree of heat superior to that of boiling water; that, in fine, the partisans of fixed air are not able to give any satisfactory reason for that acrid and corrosive vapour which exhales from lime and occasions us to cough, for the danger attending buildings newly plastered with lime, nor for several other effects produced by it.

MR. CRANS proceeds to examine the phenomena which lime presents on its dissolution in water, and during its crystallization. We have seen above that the pellicle which forms on the surface of lime water, when it has been exposed, for some time, to the air and which is known among Chemists, by the name of *Cremor Calcis*,
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is nothing else, according to Mr. Jacquin, than lime which has recovered its air, and which by this union has also recovered the state of calcareous earth, viz. has become insoluble in water, and susceptible of effervescence; in a word, the same as before calcination. Mr. Crans, on the contrary, supposes, with Mr. Meyer, that the tremor calcis is lime which has lost the caustic principle or acidum pingue; he assures us that he has frequently seen this substance form itself at the bottom of the fluid, and not on its surface, and that it is deposited on the interior sides of the vessel, and in places where the lime could have had no contact with the air; and lastly, that it even forms while the lime-water is covered with a pellicle which cuts off all communication with the air. Moreover, all the lime, according to Mr. Crans, is not soluble in water, nor can the whole be converted into cream, which ought to follow from the principles of Dr. Black and his disciples.

MR. CRANS does not quit the subject of lime-water till after he has dwelt a considerable time on its properties, and he deduces from thence,

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almost every objection against Dr. Black's theory. Lime-water dissolves sulphur, camphor, and resins, nearly in the same manner as spirit of wine; Dr. Black's disciples, to reason consistently, ought then to go so far as to declare, that it renders these substances soluble in water by attracting their air from them, as they say of calcareous earth converted into quick-lime; but then they will find themselves necessitated to pronounce, that spirit of wine dissolves the resins by attracting the air which they contain; which, according to Mr. Crans, would throw them into a labyrinth of difficulties, if not of absurdities.

BESIDES, adds Mr. Crans, if it were the absence of air which constitutes causticity, it would follow, that all the neutral salts must be caustic, as the air has been expelled in their combination, by the effervescence; we, however, find, that they are more mild, than either of the ingredients, of which they are composed, was in its separate state.

MR. CRANS then proceeds to the dissolution
both

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body of limestone and chalk in acids. He observes, that we may have an effervescence or not, in these operations, as we please. The effervescence is very brisk if we employ an acid moderately concentrated; and there is none, if the same acid be diluted in a great quantity of water. However, says Mr. Crans, if fixed air be one of the constituent principles of calcareous earths and stones, why does it not disengage itself under the last circumstance? And if it be disengaged, what becomes of it, since it does not announce its separation by an effervescence?

MR. CRANS shews afterwards, that we may obtain a brisk effervescence, by mixing together the caustic lixivium and an acid; although, according to Messrs. Black and Jacquin, it contains no air. His method is to pour, gently, some caustic lixivium into a solution of calcareous earth, the alkali trickles down the sides of the bottle, and reaches the bottom: if we afterwards agitate the two liquors suddenly, to mix them together, a brisk effervescence ensues, and the precipitation is formed in an instant.

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MESSRS.

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MESSRS. Black and Jacquin had declared that quick lime might be made, in the humid way, by precipitating calcareous earth, dissolved in the nitrous acid, by means of a caustic alkali; in fact, according to their doctrine, the calcareous earth not meeting, in the process, with any substance which can supply it with air, must remain in the state of quick lime. Mr. Crans denies these experiments, and opposes them with some of a contrary kind: he tells us, that, in whatever way he operated, the calcareous earth precipitated from a solution of it in the nitrous acid, whether he employed a mild or a caustic fixed alkali, afforded no difference; that in every case it effervesced with acids, and was but a common calcareous earth, except that it had some degree of solubility in water, and turned syrup of violets green. He tried to dissolve lime itself in the nitrous acid, and to precipitate it with the caustic alkali; and, notwithstanding Dr. Black declares, that there is nothing in this combination which can supply the lime with air, he, nevertheless, obtained a true calcareous earth, which effervesced with acids.

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ANOTHER kind of proof, of which Dr. Black and his followers avail themselves, is the precipitation of lime-water by air separated either from an effervescing or fermenting mixture; but Mr. Crans pretends, that there is no absolute proof that the precipitation is made by the air; that there are other causes which may produce similar effects, and supposing the air to act in no other manner on the water than to render it more rare, that circumstance alone would be sufficient to cause the precipitation. Besides, adds Mr. Crans, how can we conceive that the air, which in the aerial waters is the solvent of iron, should here have a quite contrary property, of rendering the lime insoluble in water*.

MR. CRANS then takes notice of the arguments which the partisans of fixed air draw from the loss of weight which calcareous earth suffers when dissolved in acids. Dr. Black and Mr. Jacquin have advanced that when lime-stone was dissolved in an acid, a diminution of weight was

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* MR. CRANS might add that the aerial waters even dissolve calcareous earth.

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perceived equal to what would have taken place if the same stone had been reduced to lime by calcination ; that in both cases the fixed air contained in the lime-stone escaped, in the first case by the effervescence, and in the second because it was driven off by the force of the fire.

MR. CRANS opposes again, here, experiment to experiment ; he dissolved several kinds of calcareous stones in the nitrous acid ; he also made a solution of lime, keeping an exact account of the weights of the acid and of the earths to be dissolved in it. He commonly observed, in these processes, that there was a sufficiently remarkable diminution of weight, but without any rule ; sometimes the lime appeared more diminished than the calcareous earth ; at other times the calcareous earth appeared to receive some augmentation of weight in its dissolution. All these results are directly contrary to Dr. Black's doctrine. But it may be objected against Mr. Crans, that he made use of vessels which were too shallow in these last experiments, and more especially that he operated on such small quantities, that an error in the scales might occasion
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the greater part of the inequalities which he has remarked.

AFTER some other objections, which I shall omit to relate, Mr. Crans proceeded to the decomposition of sal ammoniac by lime. He first observes, that if, according to Dr. Black's hypothesis, the fire drives from the lime-stone, during its calcination, the fixed air with which it was saturated, it is impossible for the lime, in the decomposition of the sal ammoniac, which is made in a retort and in a considerable degree of heat, to attract to itself the air of the volatile alkali, and he pretends, that so far should the lime be from absorbing it under such circumstances, that it should rather undergo a new calcination, and lose that which might still adhere to it; but admitting also Dr. Black's hypothesis, the lime, says Mr. Crans, ought to cease to be lime after the operation; he assures us, however, that the residuum after the decomposition of sal ammoniac by lime, uniformly afforded him a calcareous earth in the state of lime, and consequently deprived of its air; from whence he concludes that it has not attracted the air which

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was contained in the volatile alkali, and therefore it is not the absence of air which causes its causticity. Lastly, he asserts that sal ammoniac contains much air; that this air, according to Dr. Black, should serve to saturate the lime; and therefore it must be incapable of any further action on the volatile alkali.

MR. CRANS adds to these experiments, that if the action of caustics really depends on their absorption of air, animals ought to be cauterised whenever they are placed in an air pump; an infant ought to cauterise its mother's nipples, &c. as in each of these cases there is a privation of air.

MR. CRANS goes on to relate a numerous train of experiments made with Dr. Macbride's apparatus; the reader may recollect that it consists of two bottles which have a communication by means of a glass syphon; into the one we put either some substance capable of fermentation, or a mixture of an effervescent kind; in the other is to be placed the liquor or whatever matter is to be exposed to the action of the fixed

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air which is separated. Mr. Crans, successively raised an effervescence, with the vitriolic and with the nitrous acid and fixed alkali, in one of these bottles, and he procured from lime-water, placed in the other, a precipitation, such as Dr. Black and Mr. Jacquin mention. He produced the same effect with air which had served the purposes of respiration.

MR. CRANS submitted, to the same apparatus, the caustic Lixivium made after Mr. Meyer's method; the air detached from an effervescent mixture precipitated from it a white sediment which collected at the bottom of the bottle; the liquor also acquired, after a certain time, the property of effervescing with acids, but he has also observed, that when exposed to the open air, it recovered that property nearly as soon: that it also recovered it much more speedily, if it were placed over a moderate fire; and that it was at the instant when the fumes begin to arise that it regained the property of effervescing; hence Mr. Crans concluded that it acquires this property only in proportion as the caustic

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caustic principle or *acidum pingue*, to which it
was united, evaporates.

MR. CRANS observed the same thing in respect to the caustic volatile alkali detached from sal ammoniac. He placed one portion of it in a stove, another on hot cinders, and he submitted the third to Dr. Macbride's apparatus. At the end of eight hours, all the three effervesced; because, says Mr. Crans, of the evaporation of the *acidum pingue*: Dr. Macbride's apparatus therefore, according to him, operated only in these experiments, in a manner which might very naturally have been effected in the open air.

MR. CRANS has extended his inquiries further, and has made a great number of experiments in the same apparatus, keeping the vessels close, and observing the weight of the matters employed both before and after the operations. He had always a considerable loss of weight in the bottle which contained the mixture designed for effervescence; he always obtained, on the contrary, an augmentation of weight, of some grains, in the other bottle,

Mr,

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MR. MEYER's caustic lixivium, submitted to this trial, acquired an augmentation in weight of ten grains.

A SOLUTION of salt of tartar obtained five grains.

SPIRIT of hartshorn acquired nearly twenty-two grains.

COMMON spirit of sal ammoniac, three grains.

THE caustic volatile alkali, twenty grains.

MR. CRANS repeated the same experiments, leaving the recipient bottle open, whereas in the former experiments it had been accurately luted.

THE salt of tartar thus exposed received an augmentation of five grains in weight, and a small quantity of the salt concreted at the bottom of the vessel.

MR. MEYER's caustic lixivium, on the contrary,

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trary, lost two grains in three hours, and deposited a sediment.

THE liquor, afterwards, and the sediment at the bottom, effervesced with acids.

THE common volatile alkali lost something of its weight.

THE caustic volatile alkali, on the other hand, acquired some grains, it was no longer caustic, but quite mild, and effervesced.

THESE augmentations in the weights, observed in most of the experiments made with the caustic alkalis, and, in general, almost all the experiments made in Dr. Macbride's apparatus, seem to furnish very strong arguments in favour of Dr. Black's opinion. Mr. Crans, however, is not at all embarrassed in forming an answer: he agrees, readily, that the fixed air combines with the liquors placed in the receiving bottle, and that to this cause is owing the augmentation of weight which they receive; but he adds, that these liquors are impregnated in the same manner

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not as common water ; he denies, that there is a real combination, or that to such a combination is owing the rendering mild the caustic salts, and he persists in the belief, that such changes depend on the evaporation of the *caustic*, or *acidum pingue*, which neutralized the alkali.

Such are most of the principal arguments, which Mr. Crans's work contains, against Dr. Black's doctrine. I have used my utmost endeavours to deliver them with all their force. It had, perhaps, been desirable, that the author had brought them into less compass ; that he had made a selection of his experiments ; and, especially, that he had not entered into personalities against Mr. Jacquin, which are entirely foreign to his subject.

CHAPTER XIV.

THE OPINION OF MR. DE SMETH OF
THE ELASTIC VAPOURS WHICH ARE
SEPARATED FROM BODIES, AND OF THE
PHENOMENA OF LIME, AND THE CAUS-
TIC ALKALIS.

WHILE Mr. Crans attacked Dr. Black's doctrine of fixed air in the calcareous earths and in alkalis; while he shook the foundations on which this doctrine was established, two learned gentlemen, Mr. de Smeth at Utrecht, and Dr. Priestley at London, were each employed on their parts in throwing light on this matter by new experiments. They published, nearly at the same time, two dissertations, replete with interesting facts and important discoveries. Although Dr. Priestley's experiments, having been read at the meetings of the Royal Society of London, some months before the publica-

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publication of Mr. de Smeth's work, have acquired, thereby, a considerable priority of date, yet as Dr. Priestley has further exceeded the bounds of our knowledge on this subject, and as it is to him we owe some facts which seem to discover a new order of things, the natural arrangement of ideas obliges me first to give an account of Mr. de Smeth's experiments, and I shall terminate this historical essay with those of Dr. Priestley.

MR. DE SMETH's dissertation is written in Latin, and in form of a thesis; he printed it at Utrecht in the Month of October 1772, under the title of a dissertation on fixed air. Small 4to. 101. pages.

MR. DE SMETH, first asserts, that we have no knowledge of the common air which composes our atmosphere, except by some physical effects; but that we have no idea of its nature, composition, and chemical combination; from whence he concludes that it is contrary to the principle of sound philosophy, to affirm that any substance is air, because it affords us one

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one or two properties which it may possess in common with air ; that all those who have spoken of the elastic vapours, separated from bodies whether by fermentation, by fire, or by the effervescence produced by the mixture of acid with alkaline substances, have fallen into this error, that they have only considered the subtlety, the elasticity, the specific gravity of these vapours ; but they seem to have forgotton and disregarded many other properties which are not less essential to air ; that by this method of philosophising, water reduced into vapours, ought also be denominated air, that we should give the same name to the elastic fluid, and to an infinite number of incoercible vapours which have no property of air but its elasticity and its subtlety ; in fine, Mr. de Smeth goes so far as to say that elasticity is a very equivocal characteristic of air ; that we may say the same of each particular property of it with which we are acquainted, and he undertakes to prove this in the course of his work.

AFTER having shewn by experiments, already known, that air is a true solvent, in the chemi-
eal

cal acceptation of that term; that it dissolves water and vapours, in the same manner that water dissolves salts, and that it keeps these bodies suspended, contrary to the laws of hydrostatics, Mr. de Smeth proceeds to some experiments on the effect of air on some bodies, which if they be not entirely new, are at least but little known.

MR. SZATHMAR had demonstrated in 1771, in a dissertation on Homberg's pirophorus, or phosphorus, that this substance increased sensibly in weight even during the time in which it fumed, grew hot, and took fire; Mr. de Smeth has examined the circumstances of this phenomenon, in conjunction with Mr. Hann, Professor in Medicine, in the University of Utrecht, and the following is the result of their experiments.

ON the 22d. of November 1771, Mr. Hann placed 272 grains of pirophorus in an exact and sensible balance; the pirophorus took fire immediately; and in half an hour, its weight received an increase of twenty grains; the next day it was increased to twenty-one grains; seven days

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after it had acquired fifteen grains more, and the whole augmentation was then nearly a fifth, after which it had no sensible increase except what depended on the variations in the cold, heat, and moisture of the atmosphere.

Two hundred grains of pirophorus which had been kept a long time, and which had lost the power of spontaneous inflammability, having been submitted to this trial, at the end of three days, had augmented one-tenth of its weight: Mr. Smeth observes that the augmentation was not greater in this experiment, because not having been inflamed, it had undergone less heat, and consequently fewer of its parts had been dissipated and reduced to vapour.

THESE observations on the increase in the weight of the pirophorus, led Mr. de Smeth to that which takes place in quick lime: twelve ounces of this substance exposed to the air in a balance, augmented, almost visibly, in weight, during the first month: after this period, its attractive power diminished insensibly, and at the end of a year or thirteen months, it was absolutely

lutely lost. The lime, in this time, had acquired an augmentation in weight of four ounces, three drachms and forty grains, was reduced to a fine powder, and no longer separated the volatile alkali from sal ammoniac, but in a concrete form.

THE whole weight then of this lime, after a space of three months, was sixteen ounces, three drachms and forty grains; Mr. de Smeth then weighed separately twelve ounces, three drachms and forty grains. After which he made the following calculation; if sixteen ounces, three drachms and forty grains of lime flaked by the air, contain four ounces, three drachms, forty grains of matter drawn from the atmosphere, how much should twelve ounces, three drachms and forty grains contain? He found that the quantity should be three ounces, two drachms, fifty four grains and half. It was natural to believe that this matter thus attracted from the atmosphere would easily be dissipated by fire; to assure himself of this, he put these twelve ounces, three drachms and forty grains of lime into an earthen retort, such as is commonly made use of for the

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distillation of phosphorus, and he exposed it to a very strong fire during two hours: during the operation, there passed over into the receiver, one ounce, four drachms, forty grains of pure phlegm, in which not the least vestige of any saline matter could be discovered, by any kind of trial. However closely Mr. Smeth attended, he could not perceive, during the whole time of the process, any separation of elastic matter; but as after the fire was extinguished the retort was found to be cracked, no certain inference is to be drawn from this experiment. The lime, when taken out of the retort, weighed ten ounces, five drachms; which added to one ounce, four drachms, forty grains of phlegm, amounts in the whole to twelve ounces, one drachm, forty grains; whence it follows that the loss of weight during the calcination was only two drachms. It is evident then that if there had been a detachment of air, during the distillation, it was by no means so considerable as it should have been according to Dr. Black's system; for we may recollect, in fact, that, from his account, it should be nearly one half of the weight of the calcareous earth employed. Mr. de Smeth assures us, further,

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ther, that the residuum in the retort was a true quick lime.

THIS experiment gave Mr. de Smeth room to remark that lime flaked in the open air and afterwards calcined in close vessels, does not lose again the whole which it had attracted from the atmosphere. It has been seen, in fact, that the flaked lime contained, before it was submitted to distillation, three ounces, two drachms, fifty-four grains and a half of matter attracted from the atmosphere; it lost, by distillation, no more than one ounce, seven drachms, forty grains; there remained then one ounce, three drachms, fourteen grains which the degree of fire employed was not able to separate. Mr. Duhamel had observed the same thing in a Memoir on Lime, read before the Academy of Sciences in 1747, and which may be found among the papers of that year; I shall presently give an account of his experiments; I have only deferred it hitherto, that the thread of the history of fixed air might meet with no interruption.

THIS singular circumstance engaged Mr. de
H 3 Smeth

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Smeth to repeat this experiment in open vessels; for this purpose, he placed in a crucible the remaining four ounces of the same lime which had been flaked by the air. It ought to have contained, according to the above proportions, eight drachms, forty-seven grains of matter attracted from the atmosphere: however, the lime having been urged by a very strong fire, in a wind furnace, lost no more than seven drachms, thirty-six grains; and, therefore, had retained one drachm, eleven grains of the matter which had been attracted. The same lime, being again exposed to the air, regained an increase of weight, of four drachms, twenty-eight grains.

MR. DE SMETH concludes from these experiments: 1st, That lime attracts, from the atmosphere, a substance which it is impossible to deprive it of again. 2dly, That the principal increase in weight which it acquires by exposure to the air, is owing to the water only, and that the air does not sensibly contribute to this increase by the combination of its proper substance. He thinks, with Mr. Szathmár, that it is the same case with regard to the augmentation of weight in

in the pirophorus, and that this is equally owing to humidity alone. It is obvious, that these assertions are directly contrary to those of Doctor Black and his disciples.

AFTER some reflections on the manner in which air exists in water, and on the cause of ebullition in the latter, Mr. de Smeth undertakes to prove, that if the caustic alkalis do not effervesce with acids, it is probably not to the deficiency of air, or elastic matter, that we should attribute this phenomenon; and the following is his manner of reasoning :

" DR. BLACK, and the partisans of fixed air, suppose, that caustic alkalis no longer effervesce with acids, because the lime, which has a strong affinity with fixed air, has deprived them of that which they contained. If this principle were true, two things must necessarily follow : 1st, That the caustic alkalis ought to be entirely free from the matter proper for effervescence or ebullition. 2dly, That on restoring to them a sufficient quantity of air, they ought instantly to recover their property

" of effervescing; but experience, adds Mr. de
" Simeth, demonstrates, that both these conse-
" quences of Dr. Black's theory are equally
" false." And this is what he undertakes to
prove by the following experiments:

E X P E R I M E N T I.

He placed some volatile spirit of sal ammoniac, prepared with lime, under the receiver of an air pump. To the apparatus was affixed an accurate barometer, constructed in such a manner, that the mercury rose, at each stroke of the piston, instead of falling as in the air pumps used in France; as soon as the mercury had risen to twenty-five inches, the volatile spirit began to boil briskly.

E X P E R I M E N T II.

HAVING repeated the same experiment with the common volatile alkali drawn from sal ammoniac

moniac with fixed alkali, and having made a much more perfect vacuum, only a few bubbles appeared, which were scarcely perceptible.

EXPERIMENT III.

SOME soap-ley was placed under the same receiver. As soon as the mercury arrived at nineteen inches it began to yeild some bubbles: these bubbles insensibly acquired the appearance of pearls; they did not, however, burst at the surface, but when the mercury was risen to the height of $28\frac{3}{4}$ inches, they became much larger, and arrived at the surface, but without lifting it up: there were many which remained attached to the interior sides of the vessel.

EXPERIMENT IV.

THE common alkalis, how long soever they were kept in the vacuum, did not suffer the least bubble of air to escape, unless they had been strongly heated.

MR.

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MR. DE SMETH concludes from these experiments, that the caustic alkalis are more disposed to ebullition than the common alkalis; but it is very apparent, that he supposes the property of effervescence to depend on the same principle which occasions liquors to boil, which is not proved: I shall have occasion to recur some time to this article.

MR. DE SMETH endeavours to prove, next, that the introduction of air into caustic alkalis, does not restore to them the property of effervescing with acids. To prove this, he procured two bent tubes of glass to be soldered to the large bulb of a thermometer; he filled the bulb with volatile caustic alkali, and he blew through one of the tubes in such a manner as to make the air bubble into the liquor; but although he continued this trial a long time, the alkali had not acquired the property of effervescing.

He tried to keep the fixed and volatile caustic alkalis in the machine for condensing air, described

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cribed in Gravesande's philosophy, and he did not observe that they underwent any change*.

MR. DE SMETH concludes, from these experiments, that the non-effervescent property of the caustic alkalis proceeds rather from a substance added to them, than from any thing taken from them; unless, adds he, that the lime takes one thing from them and gives them another, which he thinks is a matter difficult to decide upon.

MR. DE SMETH has also repeated most of Dr. Macbride's experiments, on the effect which the vapours arising from fermenting or effervescent substances produce on lime water and the caustic alkalis; but instead of Dr. Macbride's apparatus he substituted a simple glass cucurbit to which a tubulated head was adapted; at the bottom of the cucurbit he placed some chalk or alkaline salts; on which he poured through the tube, by means of a funnel, some kind of acid,

* It appears, that Mr. de Smeth supposes here, that the elastic fluid, which affords the power of effervescence to the fixed and volatile alkalis, is the same with that we breathe, which is contrary to his own opinion, as we shall presently see.

and

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and immediately closed the tube, and then fastened to the extremity of the beak of the head a phial which contained the lime water, caustic alkali or other matter which he chose to expose to the vapours arising from the fermenting or effervescent mixtures.

THE caustic volatile alkali, exposed in this apparatus to the vapours arising from an effervescence occasioned by a solution of fixed alkali, either in the vitriolic, nitrous or marine acid, acquired in all the three cases the property of effervescent, and recovered a concrete form.

FIXED caustic alkali became effervescent in the same apparatus, but did not crystallize.

THE acid of vinegar combined with the different absorbent earths produced the same effects.

QUICK-LIME being substituted to calcareous earths, its combination with acids did not render the caustic alkalis effervescent, nor capable of crystallizing.

MR.

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MR. DE SMETH repeated the same experiments with sugar and water which he had placed to ferment in the same cucurbit; at another time he made use of rye flower diluted with a certain quantity of water; the vapour which separated while the fermentation was strong, produced precisely the same effects as that from effervescent mixtures.

EVERY time the caustic volatile alkali was submitted to this trial, there always formed at the top of the bottle which contained it, concretions of volatile alkali in different forms, and resembling vegetation. One might see these concretions appear in the liquor; and if the fermentation were brisk, the operation was finished, and the volatile alkali rendered mild in two or three hours.

MR. DE SMETH has further observed that in this experiment, a small cloud always arose from the caustic volatile alkali which directed itself towards the beak of the alembic; that at the same time an intestine motion was observable in the liquor, nearly proportionable to the thickness

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ness of the cloud, and which seemed directed upwards. The crystals of volatile alkali, obtained in these different operations, dried easily on filtering paper, and their penetrating odour was nearly dissipated.

WHEN the fermentation is finished, the elastic vapour can still render caustic alkalis capable of effervesing, but it has no longer the power of making them crystallize.

LIME-WATER, exposed to the same trials, became turbid, and the lime, which it contained, precipitated.

MR. DE SMETH suffered flesh to putrefy in the same apparatus, and the vapour which proceeded from it precipitated lime, and restored to the caustic alkalis the property of effervesing; only the effects were produced more slowly. As to their property of occasioning the salts to crystallize, he could not possibly form any judgment of it, because the animal substances when in fermentation threw off moist vapours, which would have dissolved the salt, even supposing that it had been disposed to crystallize.

MR.

ELASTIC VAPOURS.

III

MR. DE SMETH proposes to prove, in the next place, that the elastic vapours, which arise from either fermenting or effervescing substances, differ essentially from atmospheric air. I shall relate, in few words, the principal distinctions which, according to him, characterise these emanations.

1st, THE vapour from effervescing, or fermenting bodies, restores to caustic alkalis the property of effervescing with acids, and enables volatile alkalis to crystallize; but atmospheric air, under the same circumstances, does not produce the same effects.

2dly, ATMOSPHERIC air supports, nourishes, and excites fire; it is even so essential to the formation of flame, that the latter cannot exist without it: on the contrary, the air from effervescence, or fermentation, is hostile to flame, and immediately extinguishes it. Mr. de Smeth was assured of this fact by a great number of experiments; besides, it is well known to all those who make wine, that candles are instantly extinguished in cellars where that liquor is fermenting, if there be not a sufficient renovation of air.

3dly,

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3dly, ATMOSPHERIC air is equally necessary to the support of *animal* life; that from fermentation, on the contrary, is so noxious to animals, that, like a subtle poison, it is fatal to those who breathe it in sufficient abundance; and it is from this cause, that frequent accidents happen in cellars, when they have been shut up too soon after the vintage; also that care is taken not to enter them without precaution, and even not without first putting down a lighted candle into them.

THE air, arising from effervescence, is not less fatal to animals than that from fermentation; it differs, however, from the latter, in not occasioning intoxication, and in not communicating to bodies the same vigour, when taken in small doses.

4thly, THE air of the atmosphere rather favours than resists putrefaction; whereas, the vapour from fermenting or from effervescent substances is a powerful antiseptic, as Boyle first observed; as Mr. Cotes has remarked in his lectures, and as Dr. Macbride has since confirmed by several experiments.

5thly,

5thly, THE vapour from fermentation is, sometimes, wonderfully elastic, but that elasticity is not permanent. At first it is very considerable; it afterwards becomes more languid, till at last it is entirely lost. The case is nearly the same with the vapour from effervescence. Though the occasion of these differences be not well known, it may however be compared to the case of water, which, when reduced to vapour, is rarefied by heat to a great degree, and affords appearances similar to those of air; but when cooled and condensed, is reduced to a simple drop of water.

6thly, THE vapour of fermentation is much more subtle than common air; it passes through bodies which would be an impenetrable obstacle to the latter. Mr. de Smeth was not able to retain it by the aid of lutes; a moistened bladder, tied over the mouth of a vessel, which contained some fermenting matter, was not at all inflated during the height of the fermentation; although it was certain, from other experiments, that a great quantity of elastic fluid was discharged.

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From all these experiments, and the reflexions accompanying them, Mr. de Smeth concludes; that the appellation of *fixed air* is very improperly given to the vapours arising from fermentation and from effervescence; that this substance was long since known to Van Helmont by the name of Gas, to Boyle by the name of Factitious Air, and by that of *Aësus* to the ancients. That it is this which was meant to be described by the dangerous air of Avernus, by the pestiferous blast of the furies; and that to this we are to ascribe the cause of the fatal effects of the Grotto del Cani, and some other subterraneous places.

LASTLY, Mr. de Smeth concludes, that fixed air or gas is not always one and the same substance, but, on the contrary, is very various, multiplied, and different in itself. That so far from being a particular element, or being *simple* in the sense which chemists give to that word, it did not exist, primitively, in the bodies from which it is separated, but that it is a miasma formed by the attrition consequent upon the collision of all the solid and fluid parts; that it is, therefore, never produced but in cases where the bodies

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bodies suffer violent intestine motions, and tumultuous shocks, when their parts, striking against each other, are altered, broken, attenuated, as in fermentation, effervescence, combustion, &c. Mr. de Smeth thinks, that it should therefore be distinguished into

- Gas vinificationis,
- Gas acetificationis,
- Gas septicum,
- Gas salinum seu effervescentiarum,
- Gas aquæ et terræ seu subterraneum.

He assigns, however, nothing to authorize these distinctions, besides the several odours, excepting the gas vinificationis, which occasions particular phenomena in the animal œconomy.

MR. DE SMETH, afterwards, briefly examines into the opinion of those who esteem fixed air to be the universal bond of the elements, the cement of bodies. It may be easily apprehended, after what has been declared, that this opinion is not adopted by him. He does not deny that

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fixed air is an antiseptic, but yet, according to him, it does not follow from thence, either that fixed air must originally have existed in bodies from which it has been disengaged, or that it must have contributed to the cohesion of their parts, or their state of salubrity; on the contrary, he observes, that this antiseptic virtue is not particular to fixed air; but that all the products of fermentation possess the same property; that tartar, vinegar, and spirit of wine, are equally antiseptic with fixed air. Lastly, he adds, that every thing which Dr. Black's disciples advance, with respect to fixed air, is equally applicable to spirit of wine; and that we may, by the same arguments, maintain that *it* is the cement of bodies and the bond of the elements, which would, notwithstanding, be absurd.

DR. MACBRIDE had advanced a new argument in favour of fixed air, from the manner in which astringents act: their antiseptic virtue, according to his account, depends on the property which they possess of contracting the pores of bodies, when putrefying, and by that means preventing the separation of the fixed air which is

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is ready to escape. Mr. de Smeth refutes this argument, and asserts, that we know too little of the manner in which astringents act, to be able to form the least induction from thence*.

FROM the whole of this work, Mr. de Smeth concludes, that the doctrine of fixed air is erected

* THE following experiments, which were made by the translator, induced him to suppose, that the sweetening properties of fixed air may possibly depend on an affinity between fixed air and the septic particles arising from putrid bodies, and that this air may act as a *sanctuarium* on the effluvia emitted by it. A piece of putrid beef, fastened, by a string, to a cork, was confined in three pints of fixed air for thirteen hours, and was, thereby, considerably, though not entirely, sweetened. But the air in the bottle seemed to have acquired all the putrid smell of which the flesh had been deprived. So that the septic effluvium did not appear to be destroyed, but to have changed place. Slips of linen cloth, also, dipped in very rancid oil, had their rancidity much diminished by exposure to a stream of fixed air from an effervescent mixture. But a pint bottle of the same oil being saturated with this vapour, was not at all sweetened, though it absorbed much air. *Henry's Experiments and Observations*, page 127 and seq. T. H.

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ed upon uncertain and weak foundations; that from the manner in which it is delivered by its favourers, it cannot bear a serious examination; and that it will prove to be the mere opinion of a moment.

To this examination of Dr. Black's system, Mr. de Smeth adds two interesting observations, on the air of the wells at Utrecht, and on that which proceeds from burning charcoal.

THE wells at Utrecht are from eight to twenty feet in depth; it has been the custom to make use of pumps to raise the water, and they are then covered over with a kind of arch. When, after a certain period of time, the wells are opened, on any account, it is necessary to leave them uncovered for twelve hours, before any person descend into them; whoever should venture to go down into them sooner, would expose himself to immediate death. The air of these wells extinguishes candles, like that acquired from fermentation or effervescence; it precipitates, also, the lime from lime-water, and changes it to a calcareous earth; in short, it has all the properties

ties of that which we call fixed air; the water, however, from these wells, is not the less salubrious.

MR. DE SMETH has also proved, that the air which has passed through burning charcoal, has much the same properties as fixed air; it precipitates lime-water, and restores the property of effervescent with acids to caustic alkalis. He shews the manner of combining this air with different substances, in the vacuum of an air pump; and he observes, that when the volatile caustic alkali is employed, at the instant the air from the charcoal enters the receiver, a very considerable aggregation of smoke is observable, arising from the volatile alkali.

IT is easy to perceive, from the account which has been given of Mr. de Smeth's work, that he has endeavoured to embrace an intermediate opinion between those of Dr. Black, and of Mr. Meyer: but that his system, at the same time, does not always agree with his own experiments. His treatise, otherwise, is clear, methodical, and well written. His experiments are properly

made, and, in general, accurate and true. I speak, at least, of such as I have had occasion to repeat; and those are the greater part of them.

CHAPTER XV.

DR. PRIESTLEY'S INQUIRIES CONCERNING
DIFFERENT KINDS OF AIR.

NOTHING now remains to complete the object which I proposed to myself in this first part, but to give an account of a numerous series of experiments, communicated last year to the Royal Society of London by Dr. Priestley *. This work may be regarded as the most elaborate, and most interesting, of any which has appeared

* THESE experiments of Dr. Priestley, were published in English at the latter end of the year 1772. I had already been, some time, employed on the same subject, and I had declared, in a paper deposited with the Royal Academy of Sciences, on the first of November 1772, that a very great quantity of air was separated, during the reduction of metals.

peared since that of Dr. Hales, on the fixation and separation of Air. No modern work has seemed to me more adapted to evince how many new roads Philosophy and Chemistry still point out to travel over.

DR. PRIESTLEY's work, being, as it were, a train of experiments, not much interrupted by any reasoning, an assemblage of facts, mostly new, whether considered by themselves, or by the circumstances which accompany them, it may be imagined that it is little capable of abridgment; I shall therefore be obliged to follow him, step by step, in the account which I am going to give of his experiments, and my extract will be nearly as long as *his* treatise.

SECTION I.

OF FIXED AIR.

DR. PRIESTLEY first examines that which is properly called Fixed Air, which is the product of spirituous fermentation or of some effervescence,

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cence. The breweries afforded him a plain and easy method of obtaining a great quantity of this air in an almost perfect state of purity*. There is always a body of it, nine inches in depth over the tubs in which the beer is fermented, and as there is a constant fresh supply from the fermenting liquor, it is but little mixed, for that thickness, with the neighbouring air.

THIS air, according to Dr. Black's experiments, is heavier than atmospheric air, and it is doubtless for this reason, that it continues, in a manner, attached to the surface of the beer without separating from it; it is also on account of its superior gravity, that it may be conveyed from one room to another in an open bottle, provided the mouth of the bottle be kept upwards; the fixed

* DR. PRIESTLEY speaks of this kind of air as sufficiently pure for many purposes, but, from its continually mixing with common air, far from being perfectly pure. Mr. Lavoisier has, in some other instances, expressed himself in stronger terms than those which Dr. Priestley has used, and sometimes mistaken his meaning; I have, in these cases, often taken the liberty of restoring the Doctor's expressions, instead of translating the words of my author. T. H.

fixed air, for some moments, not mixing much with that of the atmosphere. Though this superiority of weight appear to be well established by these experiments, Dr. Priestley relates others, also, which might almost induce us to change our opinion. He informs us that we may place a lighted candle in a receiver filled with atmospheric air, and then plunge it, with its mouth upwards into an atmosphere of fixed air, and yet the candle shall continue to burn. The fixed air, in this experiment, does not, then, displace the atmospheric air, and consequently is not heavier: if on the contrary the mouth of the jar, instead of being upwards, be turned downwards, and especially if a narrow necked vessel be employed, the two kinds of air will, in time, be perfectly mixed *. Supposing that these experiments do not prove an excess of gravity in atmospheric air; we may at least conclude that they are very nearly

* MR. LAVOISIER has misinterpreted this passage; Dr. Priestley's words are, "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*, with their mouths downwards in the fixed air, will, in time, have the common air, which they contain, perfectly mixed with it." T. H.

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nearly equi-ponderant, and this seems to have been confirmed by Dr. Hales's experiments on air separated from tartar, and those of Mr. Bucquet on that produced from effervescent mixtures.

DR. PRIESTLEY has also observed that a candle, charcoal, and a piece of red-hot wood were extinguished, as soon as they were plunged into the atmosphere of fixed air, which occupied the surface of a tub of fermenting beer: but it is most worthy of remark that this air seems to retain the smoke, which floats upon the surface of it without separation; it there forms a bed smooth and well defined, above, but ragged, beneath, several parts hanging down to a considerable distance within the body of the fixed air. When this fixed air is very strong, the smoke of a small quantity of gun-powder fired in it will be wholly retained by it, no part escaping into the common air.

DR. PRIESTLEY has also observed, that the fixed air from fermenting beer, combines easily with the vapour of water, as also with the smoke

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of resin, sulphur and other electrical substances; yet by holding the wire of a charged phial among these fumes, he could not make any electrical atmosphere.

A SHORT time before the publication of these papers, Dr. Priestley had published a small pamphlet * on the manner of impregnating water with fixed air, and communicating to it the properties of those acidulous or aerial waters which are so frequently met with in a natural state. His process consists in receiving the air, produced by an effervescence from chalk and oil of vitriol, into a bladder; making it pass from thence, by means of a syphon, into a bottle filled with water and inverted into a vessel in the form of a basin with a little water in it, and then agitating the bottle strongly: the water, by this process, absorbs almost all the air introduced into the bottle, and by repeating the operation, a quantity of air, something more than equal to the bulk of the water may be united to it. Dr.
Priestley

* DIRECTIONS for impregnating water with fixed air,
&c. London, 1772.

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Priestley here gives us a still more simple method of producing this union, viz. to place an open vessel, filled with water, in an atmosphere of fixed air arising from fermenting wort; and it becomes, in a short time, similar to the acidulous waters. This combination is accelerated, by pouring the water from one vessel to another, exposed to the same atmosphere; in a few minutes, the Doctor has thus given it an appearance hardly distinguishable from very good Pyrmont, or rather Seltzer water. The same effect may also be produced by filling a receiver with fixed air at the breweries, and inverting it into a basin full of water; the water insensibly absorbs and dissolves the fixed air, and rises proportionably in the receiver: this is a very commodious method of uniting fixed air with all kinds of liquors, and may serve to restore briskness to flat wines and spirituous liquors which are become vapid.

It appears from Dr. Priestley's experiments, that water cannot absorb the whole of the air separated by effervescence or by fermentation; however pure it may be, a portion remains unabsorbed,

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absorbed; in which though burning bodies be extinguished, animals can, however, respire.

It has been already seen, from Dr. Hales's experiments, that a mixture of iron filings and brimstone, placed under an inverted glass bell, diminished the bulk of air in which it was confined. Dr. Priestley has observed the same diminution to take place when he employed fixed, instead of common, air; and that the fixed air, thus diminished in its volume, did not appear to be so noxious to animals as before its diminution, nor to differ so much from common air, but he remarks that this change may be attributed to his having inadvertently agitated the diminished air in water. Dr. Priestley was once led to conclude from hence, that it is phlogiston which fixed air wants to make it common air*, though

* In the account which Dr. Priestley has since published, of further experiments made on different kinds of air, we find him confirmed in the conjecture, that fixed air is capable of forming an union with phlogiston, and thereby becoming immiscible with water. This effect he had before produced by means of iron filings and brimstone, but has since had a much more decisive and elegant proof of it by electricity. Vide Doctor Priestley's Experiments and Observations on different kinds of air, page 248. T. H.

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though he acknowledged himself ignorant of the method of combining them, and that when he calcined a quantity of lead in fixed air, it did not seem to have been less soluble in water than it was before.

DR. PRIESTLEY has also repeated most of the Honourable Mr. Cavendish's experiments on the dissolving power of water impregnated with fixed air. He has observed, with that gentleman *, that it readily dissolves iron, but that it does not make a complete solution of soap; that it changes the blue juice of tournsole into red. This last observation seems declarative of its containing some portion of acid; some experiments, however, will appear, in the sequel, contradictory to this opinion †. Water, thus impregnated with fixed

* " Or rather with Mr. Lane. It appears, however, that fixed air, without water, has no such power." Ibid. page 250.

† MR. HEY's experiments, to which MR. Lavoisier here alludes, tend only to prove that water impregnated with fixed air is not made acid by the vitriolic acid being volatilized and mixed with it.

fixed air, easily parts with it when heated, when freezing, and in the vacuum of an air pump.

DR. PRIESTLEY was desirous of knowing, from his own observation, the effect of fixed air on animals: those which breathed it, perished instantly; he remarked, that their lungs were white and collapsed, and he could not perceive in them any other cause of death. Insects, such as butterflies, and flies of other kinds, soon become torpid and apparently dead, but may easily be revived, by being exposed to a current of common air. The effect is nearly the same on frogs; but a snail, treated in the same manner, died presently.

FIXED air is not less fatal to vegetable than to animal life. A sprig of mint, growing in water, placed over some fermenting liquor, became quite dead in one day; a red rose became of a purple colour, on being exposed to the vapour from fermentation, in twenty-four hours; but various other flowers were little affected*.

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* DR. PRIESTLEY relates that another rose turned perfectly white in the same situation; but he acknowledges

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When Dr. Priestley had separated fixed air from chalk by its combination with acids, he tried also to detach it by fire; and, for this purpose, made use of a gun-barrel. One half of the air which he obtained, by this process, was capable of being absorbed by water, the other half inflammable.

S E C T I O N . II.

OF AIR IN WHICH A CANDLE, OR BRIMSTONE, HAS BURNED OUT.

AFTER having examined the properties of fixed air, Dr. Priestley relates the experiments he

that the experiments were not repeated, and expresses a wish that it might be done, in pure fixed air extracted from chalk by means of oil of vitriol. In the summer of 1774 the translator of this treatise, exposed several roses, and other flowers to the vapour arising from an effervescent mixture, formed with those ingredients, without perceiving any change in their colour; nor does fixed air so obtained prove fatal to vegetation, as appears from some ingenious experiments with which Dr. Percival intends to favour the public. T. H.

he has made on portions of atmospheric air, which he confined in glass receivers, and in which candles or sulphur had burned out.

THE air, thus confined, diminished about one-fifteenth, or one-sixteenth, in its bulk, which is one-third as much, according to Dr. Priestley, as can be induced, either by the respiration of animals, the corruption of animal or vegetable substances, the calcination of metals, or by a mixture of sulphur and iron filings. One singular circumstance, and which may throw some light on this phenomenon, is that this diminution does not, always, immediately take place; but that it is sometimes not reduced till it has passed several times through a quantity of water, and been agitated with it; the fixed part combines with the water, and it is not till then that the diminution is effected. But the diminution is generally inconsiderable, if the air have stood in quicksilver, there not being any substance exposed to the air that could absorb any part of it.

THESE experiments of Dr. Priestley confirm

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what Dr Hales had suspected, viz. that air, confined in a receiver, does not diminish in bulk, in proportion to the quantity of sulphur burned in it. Dr Priestley has proved that this diminution has bounds which cannot be exceeded, and that, whenever he used a sufficient quantity of sulphur, it was always the same in proportion to the size of the receiver.

ATMOSPHERIC air confined in a receiver, acquires the property of uniting with lime-water and precipitating the lime, when either wax candles, tallow candles, spirit of wine, ether or any other substance, except brimstone, is burned in it*; and the reason why the last named body does not produce the same effect, Dr. Priestley thinks, may be deduced from the acid vapour of the sulphur uniting with the lime, dissolving it, and preventing its precipitation.

DR.

* DR. PRIESTLEY suspected, at that time, that the phenomena produced in common air by these methods, were caused by the action of phlogiston let loose from these bodies, in the process, overcharging the air, and subsequent experiments have tended to confirm him in this opinion,

DR. HALES, in his vegetable statics, attributes the diminution of the volume of air to the loss of its elasticity; in this case, the air so reduced, ought to have a greater degree of specific gravity than it before possessed; Dr. Priestley, however, believes, that it certainly becomes, on the contrary, rather lighter; and he therefore concludes that it is the fixed part of the air and its heavier portion which is precipitated.

IT is universally known that a wax or tallow candle, lighted and placed under a receiver, cannot burn long; it goes out; and if we attempt to place fresh ones there, they are immediately extinguished. M. de Saluces, in the Turin Memoirs, vol. I. page 41, attributes this effect to the rarefaction caused by the heat, and he supposes that the air may be recovered by compressing

opinion, having found that every body which emits phlogiston, and among others, the electric spark, diminishes common air. He conjectures that the phlogiston having a nearer affinity with some of the constituent parts of the air, than the fixed air which enters into the composition of it, the latter is consequently precipitated.

pressing it in bladders. Dr. Priestley agrees with him as to the truth of the experiments, but he denies the consequences : he imagines that the effect cannot be owing to compression alone, because the experiment will not succeed, except in bladders : and he assures us that he had tried, in vain, a very strong degree of compression, in glass vessels, without the quantity of the air being restored. He also brings another experiment in aid of the above ; he filled an exhausted receiver with air which had passed through a glass tube made red-hot, and found that a candle would burn in it perfectly well. The extinction, therefore of wax and tallow candles, confined in close vessels, cannot be owing to the rarefaction of the air only.

ANIMALS, from Dr. Priestley's experiments, live as long in air in which candles have burned out as in common air. He also observed the same of air in which brimstone has burned, after the vapours have had time to subside. Neither is this air more noxious to vegetables ; Dr. Pristley has kept different kinds of plants growing in it, without their being particularly affected

ed by it. But the most remarkable circumstance was, that the air was afterwards re-established in the state of common air, and candles again burned in it as at first *.

SECTION III.

OF INFLAMMABLE AIR.

DR. PRIESTLEY first describes the method by which he obtains inflammable air, which is the same as that described by Mr. Cavendish in the Philosophical Transactions: it consists in forming solutions of iron, zinc or tin, but especially the two former, in the vitriolic acid, and catching the air or rather elastic fluid, separated by the effervescence, either in bladders or otherwise. But when he extracted it from vegetable or animal substances, or from pit-coals, he put them into a gun barrel, to the orifice of which

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* DR. PRIESTLEY conjectures that this recovery of the air to its former state, is effected by the plants imbibing the phlogistic matter with which it is overloaded by the burning of inflammable bodies.

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he luted a glass-tube or the stem of a tobacco-pipe to the other end of which he tied a flaccid bladder.

THE quantity of inflammable air which is obtained by the process, depends very essentially on the degree of heat employed. A sudden and violent heat procures six or seven times as much as when the heat is applied more gradually, though increased to ever so great a degree, at the end of the operation. A bit of dry oak weighing ten or twelve grains, will generally yield about a sheep's bladder full of inflammable air, supposing the heat to be sufficiently brisk.

DR. PRIESTLEY has also remarked, in this respect, that the air obtained from solutions is more inflammable in proportion as the effervescence is more brisk; but in this experiment, as in all the others, he made use of bladders; and it must be acknowledged, that this circumstance is capable of throwing some uncertainty on the results. The doubts which may be formed, on this head, seem also confirmed by other passages in his memoir; he owns that inflammable air penetrates

penetrates bladders, and even cork, and that there is no other way of preserving it, than by corking closely the bottles which contain it, and inverting them with their mouths downwards, into a vessel of water.

HAVING instructed us how to obtain and preserve inflammable air, Dr. Priestley examines into the degree of its affinity with water; he first remarks, that if it be kept in a bottle inverted into a basin of water, the surface of the water becomes covered with a fixed matter, which is a red okre when it has been generated from iron, but whitish when extracted from zinc.

THOUGH the combination of this air with water, be not any thing near so easy as that of fixed air, it may however be accomplished by strong agitation. About a fourth part of inflammable air is absorbed in this operation; if the agitation be long continued the air loses its inflammability, and the remainder does not seem to differ from common air*.

INFLAMMABLE

* DR. PRIESTLEY says, "it admitted a candle to burn in it like common air, only more faintly; and indeed by the

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INFLAMMABLE air, produced from oak, had this peculiarity, that water absorbed about one half of its bulk ; but this circumstance is probably to be accounted for, from the mixture of a portion of *fixed*, with the *inflammable*, air. The residuum also in this, as in the preceding experiment, differs not from common air.

DR. PRIESTLEY did not fail to examine into the effect of inflammable air on animals, and vegetables ; the first were thrown into convulsions, which soon terminated in death †, nearly in the same manner as when they are plunged into fixed air. Whatever number of animals thus perished in it, the noxious quality of the air was not diminished, and its action was as great
on

the test of nitrous air, it did not appear to be near so good as common air." T. H.

† Two wasps, having been put into inflammable air, and suffered to remain in it near an hour, presently ceased to move and seemed to be quite dead for about half an hour after they were taken into the open air, but then they revived, and, were soon, quite recovered. Priestley's Experiments and Observations on Air, page 247. T. H.

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on the last as on the first. As to vegetables, it did not appear that inflammable air, remarkably, prejudiced their growth; these last experiments were made with air drawn from zinc and from oak.

THESE different experiments induced Doctor Priestley to imagine, that different kinds of air, mixed together, might correct each other: he, accordingly, proceeded to mix inflammable air, with air which animals had respired, and he found that the mixture was not inflammable. The same event did not result from a mixture of fixed with inflammable air, for the latter retained its inflammability; they even appeared to have had very little influence on each other, for though they were kept, thus mixed, during three years, they were easily separated by simple agitation in water. All the fixed air was absorbed, and the remainder was equally inflammable as at first.

IT was natural to suppose that inflammable air must be loaded with phlogiston; yet it could not be absorbed either by oil of vitriol or spirit of nitre, notwithstanding the great affinity which those acids have with phlogiston; it does not unite

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unite with the fumes of smoking spirit of nitre,
nor is its inflammability even diminished*.

S E C T I O N IV.

OF AIR INFECTED WITH ANIMAL RESPIRATION OR PUTREFACTION.

AIR which has served, some time, for the respiration of animals, has lost the property of supporting the lives of other animals. When an animal has died in this air and another is substituted to the first, it perishes instantly and in the first attempt to respire. It should seem however that animals may, by habit, become capable, to a certain point, of breathing noxious air:

Dr.

* DR. PRIESTLEY has since discovered that the electric spark, taken in any kind of oil, produces inflammable air; that inflammable air is not changed by being made to pass several times through a red hot iron tube; and that it is no more diminished or changed by the fumes of liver of sulphur, or by the electric spark, than he had before observed it to be by a mixture of iron filings and brimstone. Priestley's Experiments and Observations on Air, page 247.

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Dr. Priestley has actually observed that although an animal will live tolerably in air in which it has continued for a considerable time, yet if another animal be placed in the same air, it will perish immediately, though the former will live for several minutes longer. Young animals, *ceteris paribus*, bear this trial longer than old ones. These circumstances occasion such frequent variations in the result of the experiments, that nothing can be precisely determined from them, without repeating them very frequently.

AIR which has thus served the purpose of animal respiration is no longer common air; it approaches to the nature of fixed air, in as much as it is capable of combining with lime-water* and precipitating the lime from it, in form of a calcareous earth; but it differs from fixed air, 1st. That being mixed with common air it diminishes its bulk †, whereas fixed air increases it; 2dly.

* THE whole of the air which has been respired does not combine with lime-water. It is the fixed principle which has been precipitated, by this process, from the air which possesses this property.

† THE phlogiston emitted by animal respiration diminishes

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2dly. That it can remain in contact with water without being absorbed by it; **3dly.** That insects and vegetables can live in it, whereas they perish in fixed air.

DR. PRIESTLEY, afterwards, shews, that there is a very perfect analogy between this air, and that in which animal or vegetable substances have putrefied: both extinguish flame, and are fatal to animals; both equally precipitate lime-water; the gravity of each is equal, and each of them may be restored to the state of common air by the same methods: the Doctor concludes, from this analogy, that the principal use of the lungs, in animals, is to procure the evacuation of a putrid effluvium, which would corrupt the living body in the same manner that it produces corruption in those which are dead.

DR. PRIESTLEY had the curiosity to inquire into

air's common air; but air, so diminished, is not capable of producing any diminution in a quantity of fresh air.
T. H.

into the diminution of air which is produced either by the putrefaction of animal substances, or by the respiration of animals. A mouse having putrefied in a given quantity of air, its volume was increased for a few days, but it afterwards diminished, and in about eight or ten days, if the weather were warm, the diminution was found to be $\frac{1}{6}$ or $\frac{1}{7}$. Sometimes the diminution does not appear, till after the air has been made to pass, two or three times, through water. The same is the case with air which animals have breathed; also, air in which candles have burned out may, almost always, be further reduced by this means.

DR. PRIESTLEY has repeated the same experiments, only making use of mercury instead of water; he remarked an augmentation in the bulk of the air, for the first days, perhaps about $\frac{1}{20}$. After this it stood in the quicksilver two days without any sensible alteration; and then, on admitting water to it, it began to be absorbed, and its quantity was diminished about $\frac{1}{7}$. If he made use of lime-water in this experiment, it became turbid, and precipitated, which shewed

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shewed that this air was, in part, in the state of fixed air.

HAVING also placed some mice in a vessel, the mouth of which was inverted into quicksilver, Dr. Priestley did not perceive, when they were dead, that the air had been much diminished, but having withdrawn the mice, and introduced some lime-water into the vessel, the volume of air became diminished, and the lime was precipitated.

HITHERTO Dr. Priestley had operated only on common air, corrupted by the effluvia of putrefied animal substances, or; which is the same thing, on a mixture of common air, and air separated by the putrid fermentation. He thought proper to operate on the same effluvia, without any mixture of common air, and his experiments afforded him some remarkable phenomena. He put dead-mice into vessels full of water, and inverted the mouths of the jars into basons also filled with water ; they produced a considerable quantity of elastic matter which was not absorbed by water ; after a short time the water contracted an extremely fetid and offensive

five smell, which seems to indicate that the putrid effluvium pervades the water, and affects the neighbouring air*. He repeated this experiment in a jar filled with quicksilver, and he procured a considerable quantity of air which was absorbed by lime-water in the same manner as if it had been fixed air. These two last experiments seem contradictory to the former; it has been seen, in fact, that the putrefaction of animal substances diminished the bulk of common air in which they were confined, we see here, on the contrary, a considerable production of elastic matter.

To reconcile these phenomena, Dr. Priestley persuaded himself that the putrid effluvium is fixed air mixed with some other vapour which has the property of diminishing common air, in proportion as it is combined with it. This conjecture, however, is not confirmed by experiment;

* MR. LAVOISIER makes Dr. Priestley say that water did not absorb this air, whereas he expressly mentions the water being saturated with it. *Priestley's Experiments*, page 84. T. H.

ment; for having attempted to mix air separated by putrefaction with common air, no common air having been previously suffered to communicate with the former, he did not remark any diminution in its bulk *.

AGAIN, according to Dr. Priestley, we may vary all these phenomena, by varying the circumstances of the experiment: If, for example, a piece of beef or mutton, raw or boiled, be placed under an inverted jar, filled with, and standing in mercury, and so near to the fire that the heat to which it is exposed be at least equal to that of the blood; a considerable quantity of air will be generated in a day or two, about $\frac{1}{7}$ of which will be absorbed by water, while all the rest will be inflammable. A mouse, under this circumstance, and with the same degree of heat, furnishes a putrid vapour which extinguishes the flame of wax or tallow candles.

THE

* DR. PRIESTLEY tells us that in the way he made the experiment, he was obliged to let the putrid air pass through a body of water which might instantly absorb the phlogistic matter that diminished the common air. Experiments, &c. p. 85. T. H.

THE air which is produced from vegetables, under similar circumstances, is almost wholly fixed air, and does not contain any inflammable part. Putrid cabbage, green or boiled, yields products exactly similar in every point, to those which are obtained from animal substances.

ANIMAL respiration, fermentation, combustion, in short, effluvia of every kind would infect the air of the atmosphere, and render it mortal to all animals, if nature had not a method of reducing the corrupted air to the state of common air. This object engaged much of Dr. Priestley's attention, and the following are nearly the results of his experiments. He first proved that simple agitation with water was not capable of depriving air, thus infected, of its noxious quality, unless the agitation were continued for a very long time, a circumstance which could not happen in the common order of nature. He then tried to mix this air with that discharged from salt-petre, by explosion, and with the fumes of sulphur; he submitted it to the tests of heat, rarefaction, and condensation; but they all proved unsuccessful. One method only ap-

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peared, to him, to succeed, in restoring air to a salubrious state, and he suspected it to be the method employed by nature; it is the vegetation of plants. With this view, he has made many experiments, from whence it is evinced, that plants, confined in receivers filled with infected air, vegetate in that situation, and, after some time, the air becomes as proper as that of the atmosphere for the respiration of animals.

DR. PRIESTLEY has demonstrated that four parts of fixed air, mixed with one of corrupted air, form air proper for respiration; but as this mixture is not made without being several times decanted, in water, from one jar to another, he apprehended that its being thus passed might contribute as much, if not more than the mixture of the fixed air, to render the air wholesome.

DR. PRIESTLEY observes further, upon this subject, that every species of noxious air, whether infected by respiration or putrefaction; whether proceeding from the vapour of burning charcoal, or having served in the calcination of metals; whether it have contained, for a long time,

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time, a mixture of iron filings and brimstone, or of oil and white lead; may be always restored to salubrity by agitating them long with water. The volume of air is diminished by this process, when water is used which has been deprived of its air: but it is augmented, on the contrary, when well-water is employed which contains much air. This general assertion seems to contradict what Dr. Priestley had advanced, in another place, viz. that agitation with water was not sufficient to deprive corrupted air of its noxious qualities *.

S E C T I O N V.

OF AIR IN WHICH A MIXTURE OF BRIMSTONE AND IRON FILINGS HAS STOOD.

FROM Dr. Hales's experiments, we had learned, that a paste made with powdered brimstone

* It was not agitation, but merely decantation from one vessel to another, which had proved insufficient in other experiments. T. H.

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stone and iron filings moistened with water, occasions a considerable diminution of the volume of air in which it is placed. Dr. Priestley has repeated this experiment, in receivers immersed both in quicksilver and in water : the diminution was equal in both cases, but he observed, that it could not exceed a fourth or a fifth of the whole volume of air contained in the receiver. Air, thus diminished, is lighter than common air, but it does not precipitate lime-water.

DR. PRIESTLEY attributes this last circumstance to the acid vapour, which exhales from the mixture, during the operation, combining with the air, and forming a selenitic salt with the lime, instead of precipitating it. As a proof of this he observes, that the water, which has been used in the process, has a manifest smell of volatile spirit of vitriol. If, instead of making this experiment in common air, it be made in air which has been already diminished, whether by candles burning in it, or by putrefaction, though it never fails to diminish it something more, it is, however, no further than this process alone would have done it. Dr. Priestley has remarked,

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ed, that air thus reduced, by a mixture of brimstone and filings of iron, was very noxious to animals, and he did not perceive that it was rendered more salutary by being placed in contact with water*.

S E C T I O N VI.

O F N I T R O U S A I R.

DR. PRIESTLEY gives the name of nitrous air to the elastic fluid which is separated during the dissolution of iron, copper, brass, tin, silver, quicksilver, bismuth, or nickel, in the nitrous acid, and of gold, or regulus of antimony, in aqua regia.

THIS

* DR. PRIESTLEY has given us some curious experiments, in the second part of his ingenious treatise on air, tending to prove, from the diminution of common air by the electric spark, that the latter either is, or contains, phlogiston, since it does the very same thing that phlogiston does. And further proving that the diminution of common air by bodies emitting phlogiston is owing to the precipitation of its fixable part. T. H.

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THIS air has a strong disagreeable smell, differing but little from that of smoking spirit of nitre. It has the singular property of becoming turbid when it is mixed with common air, of assuming a red or deep orange colour, and of producing a strong heat; at the same time the mixture diminishes considerably in its bulk.

DR. PRIESTLEY supposes, that this diminution principally takes place in the common air. Not that the whole is to be ascribed to it, but that the nitrous air likewise contributes in some degré. He proves this by the greater or smaller diminution, which he has experienced in the volume of these two kinds of air, according to the different proportions in which they were mixed. When, for instance, he mixed one measure of nitrous air with two of common air, after some minutes, when the effervescence was ended, the whole volume, instead of being three measures, which it should have been, in proportion to the sum of the volume, was found, on the contrary, a ninth less than the two measures, viz. less by a ninth in measure than the quantity of common air introduced into the mixture.

When,

When, on the contrary, he employed more of the nitrous than of common air, the volume of the mixture was less than that of the two separately, but greater than that of the nitrous air alone. These appearances Dr. Priestley was able to explain on no other principles, than the supposing, that the greater diminution took place in the common air.

DR. PRIESTLEY next proceeded to mix twenty parts of nitrous air with one part of common air. The diminution was about a fortieth, viz. half the bulk of the common air: for as we have seen above, that the diminution of common air, in all cases, never exceeded from a fourth to a fifth, it follows that the remainder of the diminution must be in the nitrous air.

THE proportion of about two thirds of common air, to one third of nitrous will nearly form the point of saturation. After we have arrived at this point if we add more nitrous air, neither the redness nor effervescence will be produced, and it makes an addition equal to its own bulk.

THERE

THERE is every appearance that the water which is used to confine the air in the receiver, when this mixture is formed, absorbs a part of the air; and in fact the diminution in the bulk is less when quicksilver is employed instead of water. Two parts of common, with one part of nitrous air, give, in this case, by their combination, two parts and a seventh, instead of a ninth less than two parts; if afterwards water be introduced into the receiver, it absorbs some part of the air, but it never proceeds so far as if the mixture had been, originally, made in water.

NITROUS air makes no effervescence either with fixed or with inflammable air, nor in general with any air which has been reduced by any method whatsoever; neither do we remark any diminution in their bulk. On the contrary, the more salubrious the air, the more considerable is the diminution of its bulk; and this circumstance has afforded Dr. Priestley a certain method of distinguishing wholesome air from that which is not so. From the moment he made this discovery, he preferred this test to that of experiments made on animals.

NITROUS

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NITROUS air is susceptible of being absorbed by water, especially by such as is free from air. As to the quantity of this absorption, Dr. Priestley has given results which do not seem to agree exactly with each other*. When this air has been once combined with water it is difficult to separate them; a quantity of water, thus saturated, yielded a few bubbles in an exhausted receiver, and though it was suffered to stand there, a long time, it still retained its peculiar taste†. Dr. Priestley has, however, found by experiment, that this water, when it had stood all night near the fire, had become quite vapid, and a filmy kind of matter had been separated, which he supposes to be a calx of the metal from which the air had been obtained. Water, impregnated with nitrous air, may be easily preserved in bottles, even uncorked, in a cold place. Dr. Priestley has never observed the least alteration in it.

* THESE differences depended on the variety in the circumstances under which the experiments were made, and are not to be attributed to any inaccuracy in Dr. Priestley's manner of conducting or relating them. T. H.

† DR. PRIESTLEY has 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. T. H.

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

" THE effect of this process is generally perceived in five or six hours, about which time the visible effervescence of the mixture begins ;

" and

* MR. LAVOISIER having mistaken Dr. Priestley's meaning, in imagining that the Doctor had declared that the diminution of common air effected by means of brimstone and iron filings might be carried still further by an addition of nitrous air to it ; I have thought proper to substitute Dr. Priestley's own account instead of the paragraph which Mr. Lavoisier intended as an abstract of it. I have marked the quotation, so far as it proceeds, with inverted commas. T. H.

" and in very short time it advances so rapidly,
" that in about an hour almost the whole effect
" will have taken place. If it be suffered to
" stand a day or two longer, the air will still be
" diminished farther, but only a very little far-
" ther, in proportion to the first diminution.
" The glass jar, in which the air and this mix-
" ture have been confined, has generally been so
" much heated in this process, that I have not
" been able to touch it."

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

DR. PRIESTLEY has attempted to mix nitrous with inflammable air, and the mixture was inflammable. The flame which proceeded from it, had this peculiarity, that it was of a green colour; this circumstance, Dr. Priestley supposes to depend on the nature of the air itself, and not on the metal from which it has been extracted.

A VERY

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A VERY singular, and almost incredible, phenomenon was that nitrous air, whether by itself, or combined with common air, always retained a specific gravity, sensibly equal to that of atmospheric air. Dr. Priestley in a quantity of three pints, never observed more than the difference of half a grain, which was sometimes more, and at other times less. How is it to be conceived, however, that two fluids should penetrate each other in such a manner, that there shall result a diminution of a third of their bulk, without the specific gravity of the mixture being greater, than that of each of the fluids had been separately?

NITROUS air is exceedingly fatal to vegetables. Whether the air were pure, or mixed with common air to the point of saturation, the plants which were confined in it, perished in a short time.

METALS, calcined in this air, produced no sensible effect. Dr. Priestley has discovered that its antiseptic powers are much greater than those
of

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of fixed air, and that it is capable of preserving flesh, for a long time, from corruption.

DR. PRIESTLEY terminates this article by a table of the quantities of nitrous air, obtainable from different metals; it appears from hence, that iron afforded the most, next to it brass, then copper and silver. The other metals furnished much less *.

S E C-

* WHEN Dr. Priestley published an account of his Experiments in the Philosophical Transactions, he had endeavoured to determine whether lime-water was precipitated in the diminution of common by nitrous air, in order to know if the fixed part of the former were deposited; but on putting a bottle of lime-water into the jar in which the process was made, no precipitation of the lime ensued, though it was easily effected, afterwards, by breathing into it. This experiment not having succeeded, Mr. Lavoisier has taken no notice of it. But Dr. Priestley has since informed us that when the whole process was made in lime-water, the precipitation was sufficiently sensible. Experiments and Observations on Air, p. 114.

THE second part of Dr. Priestley's Experiments and Observations on different kinds of Air contain many curious

SECTION VII.

OF AIR INFECTED WITH THE FUMES OF BURNING
CHARCOAL.

THE honourable Mr. Cavendish had informed us, in a memoir communicated to the Royal Society,

rious discoveries, but as it would exceed my limits to enter into a particular detail, I shall therefore confine myself to a general account of them. From the green powder which remained after the evaporation of a solution of copper in nitrous acid, he obtained by means of a burning lens a quantity of nitrous air. When any volatile alkali was added to a mixture of nitrous and common air while effervescent, the jar was filled with white clouds which, precipitating in form of snow, made a most beautiful appearance, and proved to be a nitrous ammoniac. At the same time a diminution of the common air took place. So that the Doctor imagines the salt to have been formed by the acid of the nitrous air, let 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, was the cause of its diminution. And to this last cause he attributes all the diminutions of common by nitrous air.

HAVING

Society, which is inserted in the Philosophical Transactions, that air passing through a red hot iron tube, filled with the dust of charcoal, was diminished about a tenth part in its bulk; he had also observed that some fixed air was obtained in that process. Dr. Priestley repeated these experiments, and with similar success.

DR.

DR. PRIESTLEY having placed some iron nails in a bottle of nitrous air, when they had stood with the mouth of the bottle immersed in quicksilver above a month, the nitrous air was found to be transformed into a very singular kind of air, in which a candle burned naturally and freely, and yet it was, in the highest degree, noxious to animals: whereas, in general, animals will live in air in which candles have burned out. When the nitrous air was longer continued in these circumstances, the flame of a candle became twice, and sometimes five or six times larger in it, yet without any explosion. Liver of sulphur produced this effect on nitrous air much more expeditiously, and diminished it even to nineteen-twentieths of its original quantity. Dr. Priestley did not proceed to determine whether it were capable of farther diminution. In this state, and, in general, when the diminution had reached beyond three fourths of the original quantity, a candle would not burn in it. Agitation in water deprived it of its noxious qualities. In one instance nitrous air, in which iron had stood, fired and exploded like weak inflammable air.

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DR. PRIESTLEY varied the experiment, by repeating it under a receiver, by the assistance of the focus of a burning lens, and he was able to produce a diminution of one-fifth in the volume of air; the remaining four-fifths were partly fixed, and partly inflammable air*. It is very worthy of remark, in this experiment, that, sometimes, though the charcoal which was used, had been prepared by a fire so strong as to be capable of vitrifying part of the crucible in which it was confined; no diminution was perceptible

THE electric spark taken in nitrous air diminishes it to one-fourth of its former quantity.

FROM all these, and many other concurring circumstances, Dr. Priestley concludes that all the difference between fresh nitrous air, and the various states in which he has described it, depends upon some difference in the mode of the *combination* of its acid with phlogiston, or on the proportion between these two ingredients in its composition.

FROM the fatality of nitrous air to insects, Dr. Priestley recommends the trial of it, diluted either with common or fixed air, in the form of clysters, as a remedy against worms in the intestines. T. H.

* The fixed air Dr. Priestley supposes to have been deposited by the common air, and not to have proceeded from the charcoal; the other part is not said to have been inflammable. Priestley's Experiments p. 130. T. H.

ceivable in the volume of air in which it was burned *. Dr. Priestley attributes this effect to the inflammable air which is detached from the charcoal in this last case, and which supplies the place of the air which is absorbed. He observes, in support of this explanation, that charcoal which had not been made with a considerable degree of heat seldom failed to yield a permanent addition of inflammable air, and supposes, that in converting dry wood into charcoal, the greatest part is changed into inflammable air. If instead of burning the charcoal over water, the operation was performed in quicksilver, there was then no diminution, but rather an augmentation in the volume of air, either on account of the fixed air, which was separated, or of the inflammable air, but principally of the former. When lime-water was afterwards exposed to this air, it immediately precipitated, and the air was diminished one-fifth. But it was a singular circumstance, that the charcoal which Dr. Priestley

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* In general, however, Dr. Priestley seems to think, that the greater the heat, and the longer its continuance, the purer will be the phlogiston which the charcoal contains. T. H.

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made use of in this experiment, and which weighed exactly two grains, was found not to be sensibly diminished in weight at the end of the operation.

WHEN air has been reduced by the burning of charcoal, it extinguishes flame, is mortal to animals, makes no effervescence with nitrous air, and is capable of no farther diminution, either by burning fresh charcoal in it, by a mixture of brimstone and iron filings, or by any other known cause of the diminution of air.

S E C T I O N V I I I .

ON THE EFFECT OF THE CALCINATION OF METALS, AND OF THE EFFLUVIA OF PAINT MADE WITH WHITE LEAD AND OIL, ON AIR.

FROM the experiments on burning charcoal, which we have just reviewed, Dr. Priestley was led to suspect, that the diminution of air was the consequence of its having more than its usual quantity

quantity of phlogiston. The calcination of metals afforded him another method of producing the same effect, or, according to his ideas, an emanation of phlogiston. Accordingly he suspended pieces of lead and tin in given quantities of air, and threw the focus of a burning mirror upon them. The air, in this process, was diminished one-fourth; the remaining portion did not effervesce with nitrous air, was destructive to animals, like air in which charcoal had burned, and it was incapable of farther diminution by the mixture of iron filings and brimstone; it was rendered innoxious, and, in a great measure, recovered the other properties of common air, by washing in water. Whether Dr. Priestley calcined tin or lead, in making this experiment, he found no sensible difference in the properties of the air. He observed in both cases, that the water acquired a yellowish tinge, and that a thin, and whitish pellicle covered both the surface of the water, and the sides of the phial in which the calcination was made.

If instead of immersing the mouth of the jar, which contained the metals, into common water, it was placed in lime-water, no precipitation fol-

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lowed *, but its colour, smell and taste were sensibly changed. If, instead of lime-water, quicksilver was used, the air was only diminished one-fifth instead of one-fourth, and upon water being admitted to it, no more was absorbed.

IT appears that Dr. Priestley attempted to calcine metals in inflammable, in fixed, and in nitrous air, without being able to accomplish it: but he remarks that they may be calcined in air in which charcoal can no longer burn.

DR. PRIESTLEY accounts for all these phenomena, by the emanation of phlogiston; that substance which is separated from burning charcoal, and from metals during their calcination, unites, according to his doctrine, with the air, and diminishes its volume. Water being, afterwards, agitated with this air, deprives it of its phlogiston, and the air becomes restored to its natural state. He, also, presumes, that vegetation corrects noxious air, by absorbing the superabundant phlogiston.

THESE

* THE fixed air being more strongly attracted by the metallic calx, than by the lime. T. H.



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THESE considerations led Dr. Priestley to a method of explaining the cause of the mischief which arises from fresh paint made with white lead and oil, which, according to his account, is an imperfect calx of lead; and having painted several pieces of paper and placed them under a receiver, a fourth or fifth of the air was found to be absorbed in twenty-four hours. The remainder resembled air in which metals have been calcined; it did not effervesce with nitrous air, it was no farther diminished by a mixture of brimstone and filings of iron, and it was easily restored by simple agitation in water.

S E C T I O N IX.

O F A I R E X T R A C T E D B Y M E A N S O F S P I R I T O F S A L T , O R A C I D A I R .

DR. PRIESTLEY has experienced, after Mr. Cavendish, that the dissolution of copper, by spirit of salt, yielded an elastic vapour. He caught this vapour in a vessel filled with quick-silver, and inverted into a basin of the same; but having afterwards introduced some water to

it*, almost the whole of it disappeared, and nothing remained but a portion of inflammable air.

† This air turns lime water white; but Dr. Priestley does not think that the milky colour is owing to the precipitation of the lime, but to some particular circumstance, which he has not had an opportunity of ascertaining.

THE solution of lead in the marine acid is attended with the same phenomena: about three-fourths of the generated air disappears, on contact with water; the remaining one-fourth is inflammable. In the solution of iron in the marine acid, one-eighth only of the elastic vapour disappears on the admission of water; and in that of tin, a sixth, and of zinc, a tenth only. The remainder of the air, detached from iron, burns with a green or very light blue flame. Dr. Priestley thinks, that this vapour is really absorbed by

* Dr. Priestley mentions that three-fourths disappeared.

† This remark is omitted in Dr. Priestley's treatise on air, since published. T. H.

by the water, and he is persuaded, that there is a point of saturation, beyond which, the water is not capable of receiving an additional quantity.

IT is evident, from Dr. Priestley's experiments, that the air, which is treated of in this article, is merely spirit of salt reduced into vapours; in fact, an elastic vapour, entirely similar to this, is obtainable by means of spirit of salt alone, without the necessity of making any metallic solution. It is therefore easy to judge, from hence, that water impregnated with this vapour, is nothing but spirit of salt, and that it has all the same properties*.

DR. PRIESTLEY is satisfied that this elastic vapour is much heavier than common air. Two grains and one-half of rain water are capable of absorbing

* SINCE the publication of Dr. Priestley's Memoir in the Philosophical Transactions, he has discovered, that acid air may be obtained, by having recourse to the process by which the spirit of salt is originally made.—Vide Dr. Priestley's Experiments and Observations on air, page 229. T. H.

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sorbing three ounce measures of it; after which the water is increased double in weight, and one-third in its bulk. This vapour, we are told by Dr. Priestley, has a very strong affinity with phlogiston, it attracts it from all other substances, and forms with it inflammable air. This circumstance led Dr. Priestley to believe, that inflammable air consists of an acid reduced to vapours united with phlogiston. He was confirmed in his opinion, from having poured spirit of wine, oil of olives, and oil of turpentine, to this air, and having exposed charcoal, phosphorus, and even sulphur to it, he obtained inflammable air from them; this last experiment seems to shew, that the marine acid, in this case, has the power of decomposing sulphur.

DR. PRIESTLEY also suspended a piece of salt-petre in this air: it was presently surrounded with a white fume, in the same manner as if the air had been mixed with nitrous air: this experiment proves likewise, that the vapour of spirit of salt is, in some circumstances, stronger than the nitrous acid, as it is able to decompose it, and dispossess it from its basis.

MOST

ELASTIC VAPOURS. 171

Most kinds of liquors very readily absorb the vapour of spirit of salt ; linseed oil absorbs it more slowly than others, and becomes black and glutinous *.

* THE second part of Dr. Priestley's work contains also a great number of experiments in which he united this acid air with several different substances, and with various success. Ether and acid air, when combined and suffered to escape into the open air, formed a visible fume like a white cloud, having the smell of ether, but peculiarly offensive. Liver of sulphur did not unite with acid air, so as to form inflammable air. It imbibed half of the air to which it was exposed ; one-fourth of the remainder was imbibed by water, and the remainder extinguished a candle. This experiment, the Doctor thinks, seems to prove, that acid air and phlogiston may form a permanent air, not inflammable. And he suspects, that it may be air in such a state as common air loaded with phlogiston, and deprived of its fixed air ; or rather the same thing as inflammable air, which has lost its inflammability by long standing in water.

QUICK-LIME being put to acid air yielded a portion of air which was strongly inflammable ; a proof that some part of the phlogiston, which escapes from the fuel, in contact with which the lime is burned, adheres to it. But Dr. Priestley, by no means, imagines the causticity of quick-lime to depend on this circumstance.

THE specific gravity of acid air, does not appear to be far different from that of the atmosphere.

SECTION

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SECTION X.

MISCELLANEOUS OBSERVATIONS.

DR. PRIESTLEY places under this head, some experiments which he could not arrange under the preceding divisions. He filled a bottle with small beer, and placed it under a jar standing in water; during some of the first days there was an increase of the quantity of air, and afterwards a gradual diminution, which proceeded to about a tenth of the original quantity of air. The beer, after this period, was sour; the air, which remained, extinguished candles, but having mixed it with four times the quantity of fixed air, a mouse lived in it as in common air.

OUR author establishes it, as a principle, that every kind of factitious air is noxious to animals, except that which is extracted from saltpetre by detonation, in which a candle not only burned*, but the flame was even augmented, with a kind

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* SUBSEQUENT experiments made it probable, that though a candle burned in the manner related, an animal would not have lived in it. Vide Priestley's Experiments and observations, page 245.

of hissing, when the air had been newly separated. Without doubt it, at that time, still contained some particles of nitre which were not decomposed. Dr. Priestley having kept this air, for a year, found, at the end of that time, that it was extremely noxious to animals; but having washed it in rain water, it recovered its wholesomeness, and effervesced with nitrous air in the same manner as common air does.

DR. PRIESTLEY has also tried the effect of the vapour of camphor, and of volatile alkali on animals. A mouse being placed in a bottle filled with this air, was not very much incommoded; it sneezed and coughed, especially after it was taken out, but there did not remain any sensible injury.

THIS work is terminated by some very remarkable experiments on common air, agitated a long time with water; he inverted jars, filled with common air, into boiling water; in a short time, four-sevenths of that air was absorbed; the remaining part extinguished flame, but it was not detrimental

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detrimental to animals. The quantities absorbed were not always, exactly, the same, but this circumstance, doubtless, depended on the state of the water employed.

THE air, of which a part had been thus absorbed, could not easily be restored, even by the vegetation of plants.

DR. PRIESTLEY has observed, that a pint of water from his own pump contained about one-fourth of an ounce measure of air; this air extinguished candles, but was not fatal to a mouse. He also kept common air in receivers for a very long time, with a view of being satisfied whether its remaining in a state of stagnation would make any alteration in it. After several months he tried it, and found it as salubrious, as it was the moment he first confined it, and it fermented as strongly with nitrous air.

DR. PRIESTLEY's paper is followed by some experiments of Mr. Hey *, the object of which

* An ingenious Surgeon-Apothecary at Leeds in Yorkshire.

is

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is to prove that water impregnated with fixed air, separated from chalk by oil of vitriol, does not contain any other part of the ingredients which have contributed to furnish the air. This water does not change the colour of syrup of violets, whereas a single drop of the vitriolic acid diluted in a pint of water, turned it, very sensibly, of a purplish cast.

THIS water rather curdled a solution of soap in water; but this effect Mr. Hey attributes to the combination of the fixed air, with the caustic alkali of the soap, which occasions a separation of part of the oil. It also renders turbid a solution of sugar of lead.

As a sequel to these experiments, is added a letter from Mr. Hey to Dr Priestley, on the effects of fixed air, applied by way of clyster, in putrid diseases *.

C H A P.

* It is with particular pleasure that I can congratulate the public on the acquisition of so valuable an addition to the *materia medica*. Dr. Percival has favoured the public with some cases in which fixed air thrown into the intestines,

CHAPTER XVI.

EXPERIMENTS ON LIME, BY MR.
DUHAMEL*.

I HAVE already declared, that I deferred giving an account of Duhamel's experiments on lime, that I might not interrupt the thread of my

* THIS Chapter is extracted from the memoirs of the Royal Academy of Sciences, for the year 1747.

intestines, by way of clyster, produced considerable benefit, and that gentleman's practice, and the observation of other ingenious physicians have since furnished some additional cases in which the good effects of this medicine were indisputable. It has also been applied with great success, as a fumigation to foul ulcers, and I have used it with the most happy event, in putrid sore throats, and ulcerated mouths and gums, attended with putrid symptoms.

The instrument which Mr. Hey used for conveying the fixed air, was that employed for transmitting the fumes of tobacco into the intestines, one end of a bladder was tied

to

my narration, relative to fixed air. I now hasten to do justice to that celebrated academician; it is

to the tube of this apparatus, detached from the box, and the other end round the neck of the bottle, which contained the chalk and vitriolic acid. It is better to fasten one end of the bladder to a perforated cork*, and the other to a flexible leather tube, to which another bladder and clyster pipe may be affixed. By this means the effervescence may be kept up as long as the operator thinks proper, by uncorking the bottle, and adding more acid or chalk. And the second bladder serves to unite the clyster pipe and leathern tube. By this means I have injected fixed air into the bowels with great facility and convenience.

SINCE the publication of this memoir in the Philosophical Transactions, Dr. Priestley, as I have before observed, has republished it, with additions to each article, and one entirely new section, containing observations on alkaline air, which he procured, with the most ease and convenience, from the materials used for the production of the caustic spirit of sal ammoniac. Many observations are to be met with in this part of the Doctor's work, and another part contains many ingenious queries, speculations and hints. I have selected from that volume, such passages as were necessary to elucidate Mr. Lavoisier's de-

* *Vide Dr. Priestley's directions for impregnating water with fixed air.*

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is well known that there are few branches of science which have not been enriched by him.

MR. DUHAMEL has remarked, that white marble calcined in a very strong fire, lost about a third of its weight; but still it was not calcined quite to the center, and there remained a nucleus in the middle, which partook as much of the nature of marble as of lime. The lime-stone of Courcelles, from whence is brought almost all the lime which we use in our buildings, was not nearly so difficult to calcine, and it appeared, in general, that the calcination was more readily and easily performed, in proportion to the softness of the stone. The courcelles stones lose, by calcination, about eight ounces, four drachms, from a pound weight, viz. something more than half their weight. Exposed, afterwards, to the air, they crack, fall into powder, and regain, by degrees, part of the weight which they had lost; but there still want five ounces

tail of the first memoir, but it would be doing injustice to the ingenious author's performance to attempt any farther abridgement of it. T. H.

and

and an half in the pound, to make up the weight which they possessed before calcination.

MR. DUHAMEL has made some inquiries into the quantity of water necessary to flake lime. He took sixteen ounces of Courcelles lime; he flaked it with water, till it was of the consistence of a thin paste, and he left it to dry in the air, after which it weighed twenty-six ounces, and had therefore acquired an augmentation of ten ounces. This lime, having been kept, a very considerable time, in the heat of a stove, was not perceptibly diminished in weight.

THE quantity of water absorbed by lime prepared from marble, is much more considerable than that absorbed by the Courcelles lime.

MR. DUHAMEL tried to drive off, by fire, the same water which he had introduced into the lime, but he found great difficulty in performing it; and although he made use of a melting furnace, in which the fire was excited by a strong blast, the lime, uniformly, retained an increase of weight amounting to four drachms and an

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half per pound. It was, without doubt, occasioned by the remainder of the water which could not be separated. This lime was then in the state of quick-lime, and presented the same phenomena.

MR. DUHAMEL's memoir, moreover, contains very numerous and interesting experiments on quick-lime, and its combination with acids; but as they would be foreign to my subject, I shall not enter into a detail of them here. I shall content myself with relating, that lime, combined with the three mineral acids, did not afford compounds different from those obtained from chalk, and all the other pure calcareous earths. Mr. Duhamel has remarked, that a quick and penetrating vapour is discharged, in all these combinations, which precipitates a solution of silver, and this circumstance added to its odour, induced him to suspect that it was the marine acid.

MR. DUHAMEL closes this memoir, with a particular observation, which was entirely new at the time of its publication: he dissolved salt
of

of tartar in distilled water, and, on evaporation, he obtained crystals from it; from whence it is evident, that the original discovery of the crystallization of alkalis, belongs to Mr. Duhamel.

CHAPTER XVII.

OBSERVATIONS CONCERNING FIXED AIR,
AND THE EFFECTS OF CERTAIN MINERAL
WATERS, BY MR. ROUELLE, LECTU-
RER IN CHEMISTRY AT THE ROYAL
BOTANICAL GARDEN IN PARIS*.

FIXED air becomes daily the object of che-
mical, and still more, of physical investigation.
The celebrated Mr. Hales, was in some degree,
the

* THE work which I am now publishing on Elastic Va-
pours and the Fixation of Air in bodies, was nearly finish-
ed, and I was preparing to read it at the Academy when
these observations of Mr. Rouelle made their appearance.
As they are short, and as they are, in other respects, very

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the first who has pointed out the road, by the work which he has left us on that subject. Messrs. Black and Macbride have added a very interesting series of experiments affording much light. And lastly ; Dr. Priestley at London, and Mr. Jacquin at Vienna, have so ably supported Dr. Black's doctrine, that this subject is become one of the most interesting in chemistry and physics, from the immediate relation which this recently known substance may, and, must have with an infinite number of the phenomena of nature.

I SHALL confine myself, at present, to the relation which fixed air appears to have with certain mineral waters, and some grand phenomena in nature, and I shall relate, as succinctly as possible,

interesting and not very capable of abridgment, I thought that the public would approve of my giving them at large. I have therefore only transcribed, verbatim, the article, from Mr. Roux's *Journal de Medicine*, for the month of May last *, where these observations are printed ; and it is no longer I, but Mr. Rouelle who speaks in this chapter.

* *Mr. Lavoisier's Work was published 1774.*

possible, some experiments, which give us a knowledge of its use, and of its effects on the iron which is found in these waters, and afford a solution of some facts which cannot, in my opinion, be explained without it.

DISTILLED water, river water, in short, the purest waters, as Dr. Priestley has marked are easily impregnated with fixed air; and then they have the same taste, the same flavour, and afford the same appearances, as those mineral waters which are properly called acidulous. This has been, already, first completely demonstrated by Mr. Venel*. The experiments in proof of it

are

* MR. ROUELLE, in this place, and Mr. Lavoisier in his account of Mr. Venel's experiments, page 33, have attributed the first discovery of the natural impregnation of water to this ingenious professor. I should think myself inexcuseable, therefore, if I did not take this opportunity of doing justice to the merits of our very worthy and learned countryman Dr. Brownrigg, and this cannot be done more effectually than by transcribing a passage from Sir John Pringle's elegant *Discourse on the different kinds of air, delivered before the Royal Society, November 30, 1773.* I

are known, and I have only repeated them, that they might lead me more certainly, to those which

shall only observe, that the memoir containing the account of Mr. Venel's experiments was not read before the Royal Academy of Sciences till the year 1750, and that he has supposed the air, contained in the waters which have been called acidulous, to be common atmospherical air.

THE Learned President having first informed us that he believes Dr. Seip of Pyrmont had, originally, suggested the notion concerning the impregnation of the mineral waters by the mephitis, in a treatise published in the German language, and afterwards in a communication to the Royal Society in the year 1736, though that author seems not to have rightly understood the nature of that vapour, proceeds in the following manner. T. H.

" THUS the fuller discovery of this principle we owe to
" Dr. Brownrigg of Whitehaven, who about thirty years
" ago, began clearly to unfold this mystery. But those cu-
" rious papers were not then inserted in the Transactions,
" as the too modest author had requested a delay, till he
" should be able to make them more worthy of that ho-
" nour." In that communication he remarks " that a more
" intimate acquaintance with those nitrous airs in mines
" called *damps*, might lead to the discovery of that princi-
" ple of mineral waters, known by the name of their *spirit* ;
" that

which I afterwards made, and of which I shall proceed to give an account.

1st. Some distilled water was impregnated with fixed air after Dr. Priestley's manner. I then immediately took a bottle of it, to which I added a small quantity of iron ore, of the nature of the eagle stone, finely powdered. This ore is not attracted by the magnet, at least not sensibly.

" that the mephitic exhalations termed the *choak damp*, he
" had found to be a fluid permanently elastic; and from
" various experiments, he had reason to conclude, that it
" entered the composition of the waters of Pyrmont, Spa,
" and others; imparting to them that pungent taste, from
" which they are denominated *acidulez*, and likewise that
" volatile principle, on which their virtues chiefly depend."

" In order to ascertain a fact of so much consequence,
" Dr. Brownrigg took the opportunity, when at Spa sever-
" al years, to make some experiments for this purpose;
" when he had the satisfaction to find those waters preg-
" nant with the *artificial or factitious air* of Mr. Boyle, the
" same with that of the suffocating grotta near Naples,
" and the same with the *choak damp* of our coal-mines; for
" as much as this air instantly extinguished flame, and the
" life of those animals he had inclosed in it."

sensibly so. The bottle was corked in the closest manner possible, and then set by, inverted for twenty four hours*. It had then dissolved a sufficient quantity of iron, to produce with an infusion of galls, a strong vinous violet tint, rather tending to black.

THE liquor which is prepared to precipitate the Prussian blue, or phlogistigated alkali, struck with it a greenish blue colour, and after some days a portion of precipitate was formed, which was true Prussian blue.

THIS water thus impregnated with air having been boiled, lost all its properties. It became turbid, deposited an okery matter, and no longer produced the violet, green, or blue tint with galls, or the phlogisticated alkali.

EXPOSED to the open air for several days, it lost, likewise, all its properties, and precisely in the

* If some iron filings be placed in the basin, which is used in Dr. Priestley process for impregnating water with fixed air, the water dissolves a sufficient quantity of iron during the time that the other operation is performing. T. H.

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the same manner as those mineral waters which Mr. Monnet calls ferruginous.

I AM not the first person who has thought of dissolving pure iron into water by means of fixed air. Dr. Priestley informs us, *that his friend Mr. Lane has put iron filings into this mixed water, and that he made a strong and agreeable chalybeate or iron water, similar to some natural waters which contain iron in solution, by means of fixed air only without any acid.*

BUT it is well known that iron is very rarely found in the bosom of the earth, joined with all its phlogiston; and that nature seldom has iron filings in her possession. I therefore thought that I ought to direct my experiments to a martial substance more commonly to be found; and, for this reason, I preferred the iron ores of the eagle-stone kind, which are very plentiful, and to be met with every where.

2dly, A PINT of distilled water, four grains of sea-salt, with an earthy basis, twelve grains of Epsom salt, and as much iron ore as you please,
for

for the water takes up but a small quantity, not being capable of dissolving more.

THIS water being impregnated with air, yields, with gall, a strong violet, or red wine, color, and with the liquor of Prussian blue, a deepish green color tending towards blue.

3dly, WATER, impregnated with air, to a pint of which twelve grains of sea-salt, and eighteen grains of fixed mineral alkali, had been added, took up less iron than the preceding. The violet color, by the gall, and the greenish blue, by the phlogisticated alkali were more pale and languid. It is true, that both these colors shewed themselves, a little, after some time.

THIS water lost, by boiling, the property of turning green with the phlogisticated fixed alkali; but an infusion of gall still shewed some traces in it of iron.

RIVER water impregnated with fixed air, and to which a small quantity of iron ore was added, struck,

struck, with galls, a very deep violet color, and a beautiful blue with the phlogisticated alkali.

Some of this ore being mixed with the same pure river water unimpregnated with air, and the bottle being well stopped, and frequently shaken up, at the end of twenty four hours the water yielded no signs of the presence of iron, by either of the tests.

MR. MONNET, in his treatise on mineral waters, proposes, as an approved method of making a chalybeate water without air; to inclose fresh iron filings in a bottle, well corked, and to shake it frequently for several days.

I SHALL have an opportunity of speaking, on another occasion, of this manner of making ferruginous waters without fixed air. There are several natural waters, in fact, which contain iron, without that intermedium, as Mr. Monnet has demonstrated.

gthly, Pure Arcueil water, unimpregnated with air, to which the same ore had been added, gave

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gave no marks of the presence of iron, when tried by the tests. I furnished it with air, and the iron was presently dissolved: galls afforded me a violet colour, which shewed itself gradually; and the phlogisticated alkali struck, immediately, a deepish green colour.

SOME spirit of salt was added to this water, with an intention of saturating, in part, the absorbent earth which it keeps in a state of solution. I afterwards impregnated it with fixed air, and I obtained, by the tests, the common violet, and blue or green colors; but each of them was less deep than in the preceding waters. It seems as if the presence of the salts and earth, with which some waters are loaded, impedes greatly the solution of the iron. I have however found, that my own well water suspends a small quantity of iron without any air. This water, being boiled, deposites all the iron, so as not to be affected by the tests.

6thly, PURE Sein water, furnished with air, by the common apparatus, from the vapour which is separated during the precipitation of hepar

hepar sulphuris, by acids, and mixed with the same ore, scarcely changed colour with infusion of galls, and not at all with the phlogisticated alkali. However, it must be observed, that not only iron ore, but also calcined saffrons of iron, not attractive by the magnet, as the saffron of the residuum of corrosive sublimate, and that which is called the Berlin Red, turn black pretty readily when they are mixed with that water impregnated with this vapour.

WATER furnished with this vapour, receives the taste and a strong smell of the hepar; and retains them both for a considerable time, even in the open air; but it grows turbid, and becomes like whey which has not been clarified; this is to be attributed to a very minutely divided portion of sulphur, which separates from the water and precipitates.

THIS vapour, which arises, in the precipitation of hepar by all the acids, is very inflammable*. It is still so after having passed through water,

This Note is Mr. Rouelle's.

* I thought that I had been the first who had seen this phenomenon, but on recollection, Mr. Meyer has mentioned

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water, with which it forms scarcely any union; which induces me to believe that it contains very little true and pure fixed air, although it be very copiously discharged by the effervescence of the acids with the alkali of the hepar; but I see from the phenomena which it presents, that it is here, as in the solutions of the metals by acids,

in

ed it. It was chance which offered it to him as well as to me. My brother and I were employed in the year 1774 to examine some gold coin which was supposed to be so alloyed, that none of the usual methods used for the assaying, and purification of gold could separate it. We had at that time four ounces in solution by the hepar. The precipitation was made at night; the candle was near, and I beheld myself surrounded on every side with a large flame, of which I soon knew the cause. Mr. Meyer seems to attribute the inflammability of this vapour to a portion of real sulphur, which is divided in such a manner, that it is volatilized, and carried away with the torrent of the vapour, in which, I presume, he was mistaken. The vapour itself is inflammable, and any portion of sulphur it may contain, burns with it, and is only an accessory to its inflammation; for if we agitate this vapour, thus loaded with sulphur, in water, the sulphur is separated, as I have remarked above; but the vapour, though deprived of this foreign sulphur, does not cease, on that account, to be inflammable.

in a very different state from common fixed air. Thus water is but slightly, and with great difficulty, impregnated with that vapour. Dr. Priestley has made the same observation.

7thly, I took a pint of pure river water, I then added, according to Mr. Venel's process, two drachms of fixed mineral alkali, and six drachms of spirit of salt, which from some previous experiments, was the quantity necessary to saturate that quantity of alkali. The bottle was closely corked during the time of effervescence. Twenty-four hours after I opened it carefully, that I might introduce some iron ore, and I corked the bottle again immediately,

WHEN forty-eight hours had elapsed, the water, still, shewed marks of air both to the eye and taste; but only became moderately brown with infusion of galls, and scarcely turned green, some time after, by the addition of phlogisticated alkali.

8thly, I CAUGHT, in a bladder, the vapour which arises from a solution of iron in spirit of
O salt,

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salt. This vapour which is inflammable, and remains so a long time, incorporates very difficultly with water ; but however small the quantity may be which the water receives, it, nevertheless, contracts a very sensible odour like hepar sulphuris, or rotten* eggs.

WATER, also, absorbs but a very small quantity of the vapour which arises on the solution of iron in the vitriolic acid ; but it does not contract the same sulphurous smell as that in the last experiment.

THE air which is separated from bodies, is, then, in two very different states. In some, it is pure fixed air, which combines with water in a quantity at least equal to it in bulk, and communicates to it many properties, and among others those of dissolving iron, and of precipitating lime-water, in the same manner as fixed air itself, &c. Such is the air which is detached by the combination of acids with alkaline and calcareous substances, the vapour which arises from spirituous liquors in actual fermentation, and

and that of charcoal. In all these cases this vapour, or fixed air, is not inflammable.

On the contrary, that which is separated in the precipitation of liver of sulphur, by any of the three mineral acids, or by the acetous acid, that which is plentifully supplied from the solutions of iron and zinc in the vitriolic and also in the marine acids, are very inflammable. This vapour passes through water without incorporating with it, and without losing its inflammability, which it retains for a long time. It communicates to water very sensibly the taste and smell which attends the precipitation of liver of sulphur. But yet it differs from common fixed air, in not precipitating lime-water; and to speak *en passant*, it may be compared to the air obtained by the distillation of animals and vegetables, which Dr. Hales first examined, and which he has recorded as being inflammable a long time after.

Not that there is not much air discharged both in the precipitation of hepar sulphuris, and in metallic solutions; but it is evidently com-

bined with a great quantity of phlogiston, and it is in proportion to this combination, that the air is more or less immiscible or insoluble in water, and becomes inflammable.

If we now cast a view upon what passes in the great works of nature; I believe that the same difference exists between that incoercible being, if I may be allowed the expression, which flies off from the cold mineral waters, which are falsely called acidulous, as those of Buffans, Seltzer, &c. and the sulphureous vapour which arises from the warm waters, as those of Aix-la-Chapelle, Bareges, Canterets, &c. In the first it appears, that this being is nothing but fixed air, the same as is obtained by Dr. Priestley's method. Instead of which, the sulphureous vapour of the Aix-la-Chapelle waters, &c. must have a great resemblance to that which is separated in the precipitation of the hepars.

It might be hoped that the chemists, who are more nearly situated to those waters, would verify this conjecture, and also inform us whether that vapour be inflammable like this of the hepars.

separa. It is certain, that the latter has precisely the same colour as that which proceeds from the mineral waters. It has also the property, even when introduced into water, of turning silver black; as well as the metallic calces, and even crocus martis perfectly calcined, and not attractable by the magnet.

We may also observe the same relations and the same differences in mephitic exhalations, of which we know there are two sorts. Those of one species, such as those of the Grotto del Cani, are not inflammable; they neither make silver nor the metallic calces black; they extinguish candles, &c. in the same manner as the vapours which are disengaged in spirituous fermentation. Those from charcoal, and the fixed air arising from the combination of acids and alkali, after Dr. Priestley's manner, produce the same phenomena as the Grotto del Cani, and may be compared to it on every account.

THERE is then separated from the earth a fixed air similar to that produced in certain chemical experiments, and in the fermentation of

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spirituous liquors; since this, as Dr. Priestley remarks, has also the property of dissolving in water. It is principally on account of this air, that the cold mineral springs keep a larger quantity of iron dissolved, and that after the manner of our artificial aerial waters, they readily deposit it, either by standing exposed to the open air, or else by being boiled.

THE volume of fixed air which is introduced into water, is, according to Dr. Priestley, equal to that of the water which is impregnated by it. This air is not merely interposed among the particles of water, but is in a state of actual combination; and the water may even be filtered, without being sensibly deprived of it. However, the water does not acquire, by it, any increase either in bulk or weight, proportionable to the great volume of air which it has absorbed.

MAY we not suspect, from all the effects of fixed air, that it is this which passes from the earth in vegetation, by means of that motion of universal fermentation, which the return of the sun excites in nature, in the beginning of spring?

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In fact, the air which is combined with vegetables, according to Dr. Hales's experiments, has lost all its elastic properties, although it be in quantity very large and ponderous.

As to the other species of mephitic exhalations, we know that there are separated in the galleries of mines, and in coal, and salt-pits, &c. two sorts of vapours, one of which is frequently even visible. It is immiscible in water, and takes fire, and explodes frequently with a loud crack. The other, on the contrary, is not inflammable, but extinguishes lamps and candles, like the vapour of the Grotto del Cani, like that of spirituous fermentation, and that from charcoal. But they are equally fatal to animals which are exposed to them.

We know that there are vapours arising from certain waters, both subterraneous, and even above ground, which take fire and burn very rapidly.

DR. PRIESTLEY has concluded, from some salutary effects of which he has been informed,

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that fixed air is not noxious, and may be respired*. For my part, I strongly suspect that wherever it be collected in quantity, and without communication with the air of the atmosphere, it may become dangerous, and perhaps fatal, like the vapours of which we have been speaking; of this I shall give an account, from a train of experiments which may decide the question.†

As to the vapour of hepar sulphuris, I dare assert that it is also pernicious like that of charcoal. It was at my own expence I obtained this knowledge,

* MR. ROUELLE certainly mistakes Dr. Priestley. The latter only meant that it might be respired in small quantities and with caution; not that it was totally innoxious in whatever quantity it might be respired. Dr. Priestley's expression is, that "he was satisfied that fixed air is not noxious *per se* any more than heat;" but no one can suppose that heat, in the extreme, is innoxious. Directions to impregnate Water, &c. page 18. T. H.

† It appears that Mr. Rouelle, at the time of writing this Memoir, had seen no other part of Dr. Priestley's work on this subject except his Directions for impregnating Water with Fixed Air, for he has inserted a note in this place, in which he says, that he is just informed that Dr. Priestley had decided the question. T. H.

knowledge, having been once nearly suffocated by it.

The following are the symptoms which that vapour occasioned. Having been desirous to respire it strongly, to discover the character of this odour, I held my nostrils and open mouth over the vessel, while I made a very large precipitation of hepar. I was immediately affected by it, and suddenly found myself absolutely unable to inspire, and especially to expire. I perceived my breast to be in a state of dilatation; joined with an insupportable oppression. In this situation, notwithstanding all my efforts, I could neither introduce fresh air, nor expel that already in my lungs. I directly threw myself out of the laboratory of the King's garden, where I made this experiment; I got into the open air, and supported myself by the court-wall, for I felt myself universally faint; and it was not till after I had made several very strong efforts to respire in the open air, that I recovered that function, together with that of motion. But I continued all the afternoon in a state of uneasiness and oppression,

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preffion, attended with a weight in my head
which I cannot easily describe *.

IT is known that fixed air, separated after Dr. Priestley's process, also possesses properties in common with ordinary air. If it be introduced into a vacuum, the vacuum is destroyed, and the vessels are detached. The same phenomenon is produced by inflammable air. It is therefore able to counterbalance the pressure of the atmosphere; which, in my opinion, proves, among other things, that this vapour is not phlogiston only or acidum pingue, as has been advanced on mere speculation, but, on the contrary, that it is some kind of air, which, however combined, still retains the principal properties of common air, although it may differ from it in other respects †.

C H A P-

* MR. MEYER has related a similar accident which happened to his assistant, in his presence, when making a precipitation of a large quantity of hepar.

† I AM just informed that an English dissertation by Dr. Priestley has lately appeared, which contains a very beautiful

CHAPTER XVIII.

EXTRACT OF A MEMOIR, BY MR. BUCQUET, DOCTOR-REGENT OF THE FACULTY OF PHYSIC IN PARIS, ENTITLED, PHYSICO-CHEMICAL EXPERIMENTS ON THE AIR, WHICH IS SEPARATED FROM BODIES AT THE TIME OF THEIR DECOMPOSITION, KNOWN BY THE NAME OF FIXED AIR, READ AT THE ROYAL ACADEMY OF SCIENCES, APRIL 24, 1773,

MR. BUCQUET, after having given, in a very concise abridgement, an account of the experiments of Van Helmont, Boyle, Black, Macbride and Jacquin, on the nature of the elastic emanations which are disengaged from bodies,

tiful series of experiments on fixed air, inflammable air, and mephitic air or that arising from putrefaction. I regret that I did not know of them sooner; the manner in which the experiments are made which we have already had from him, assures us of the excellent use which may be made of every thing which comes from his hands,

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bodies, and on fixed air, attempts to determine, 1st, Whether fixed air be the same as that of the atmosphere; 2dly, Whether it be the same from whatever bodies it may have been extracted.

MR. BUCQUET, in a great part of his experiments, made use of Dr. Macbride's apparatus, of which we have given a description in another place*. It may be recollect'd, that it consists of two bottles, between which a communication is made by a bent glass tube. This apparatus, as made use of by Dr. Macbride, has the great inconvenience of not permitting us to operate on fixed air without a very considerable quantity of atmospheric air being mixed with it, and this circumstance induced Mr. Bucquet to make some alterations in it. He added cocks to it, he disposed it in such a manner that he could adapt it to an air pump; lastly, he cut one of the bottles through the middle, so that the upper part might be separated, and an accurate barometer be introduced. That which was designed to receive the substances which were to be combined together

* Chapter IX. page 52.

ther to produce the air, Mr. Bucquet called *the bottle of mixture*, and that which was to receive the bodies which he proposed to expose to the vapour of the separated air, he named *the bottle of reception*.

THE result of the experiments, made with this apparatus, was that the air separated by all the acids, without exception, whether from chalk or alkaline salts, was absolutely the same; he only observed that the air drawn from volatile alkalis, retained a smell like that of stinking flesh. He also found a perfect sameness between the air detached from fermenting, and that from effervescent substances. This air has a penetrating smell, which Mr. Bucquet calls *odeur gaseuse*: it has the property of precipitating lime dissolved in water, of changing it into calcareous earth, and of restoring to it the property of effervescent with acids. It produces nearly the same effects on the caustic alkalis; it restores to them the power of effervescent, and of crystallising.

FIXED air, in all these cases, does not contain
any

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any thing of the saline substances from which it has been obtained. Syrup of violets, though exposed to its action, for above twelve hours, in the apparatus which has been just described, was in no wise altered.

MR. BUCQUET submitted this air to the usual experiments, to determine its weight and compressibility; his results did not differ sensibly from those which are obtained when common air is employed.

MR. BUCQUET afterwards examines the air produced by the solution of metallic substances, and he found it very different from that which is separated either by effervescence (from calcareous earths and alkalis) or by fermentation. This air is not at all susceptible of being combined with water; it is equally incapable of combination, either with lime or with the caustic alkalis; however long a time they were exposed to its action, they did not recover the property of effervescing with acids.

FIXED air discharged from an effervescent mixture,

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tare, combined afterwards with wine, does not convert it into vinegar; it only communicates to it an acerb taste, which may however be the first stage of the acetous fermentation.

MR. BUCQUET then proceeds to examine, if the air produced; either by effervescence or fermentation be inflammable, like that separated from zinc or iron, by the vitriolic or marine acids, as Dr. Hales has advanced; but he was not able to make it burn.

FROM these experiments, Mr. Bucquet concludes, that air produced, either by effervescence, fermentation, or metallic solution, is not precisely the same as that of the atmosphere, though equal in weight and elasticity; that air produced by effervescence and fermentation differs from that of the atmosphere, and that of metallic solutions, in the very great aptitude which it has to combine with lime, with alkalis, and even with water; and lastly, that the air from metallic solutions has the distinguishing character of being inflammable.

THOUGH

THOUGH these experiments be very similar to those which had been published before Mr. Bucquet's, and especially to Dr. Priestley's, they are not less valuable in physics. Experiments on a subject so intricate, and which is still left in obscurity, cannot be too much multiplied. It is also of some importance to know that we may arrive at the same results by different processes.

CHAPTER XIX.

AN APPENDIX ON FIXED AIR, BY MR.
BAUME, MASTER-APOTHECARY AT
PARIS, OF THE ROYAL ACADEMY OF
SCIENCES *.

SOME philosophers are of opinion, that there are properties discovered in fixed air, that should

* THE apprehension that I might be accused of having shewn a spirit of partiality, in having brought to view, as I have,

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should lead to the rejection of phlogiston, and the substitution of fixed air, in its place*. According to these philosophers, fixed air ought to bring about a total revolution in chemistry, and change the order of all the knowledge which has been acquired. But the experiments which have been published, hitherto, have appeared to me to afford phenomena, concerning the cause of which, I apprehend, mistaken notions have been received, as it will be easily judged by the following reflections.

We have proved in many places of this work, and agreeably to the most celebrated philosophers, that air is an element which enters into the composition of most bodies. Hales, in his Vegetable and Animal Statics, has demonstrated this truth by a great number of well executed experi-

* We are ignorant who these philosophers are.

I have, whatever has been hitherto written on the subject of fixed air, has engaged me to transcribe, here, this appendix, which is to be found at the end of the third volume of Mr. Baumé's Chemistry, page 693.

P

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experiments. He has calculated the weight and the volume of air contained in different bodies, and he has given the name of fixed air to that which enters into their composition; that, in short, which is become one of their constituent principles, and which has lost its elasticity and all the properties of pure and aggregated air; and he has given the name of elastic air to that which is disengaged from bodies.

AIR, as we have observed, under that head, is one and the same: there is only one species of air. This element may, and in fact does enter into an infinite number of combinations; but when it is disengaged from bodies with which it was combined, it recovers all its properties, and when it is properly purified it in no wise differs from that which we respire.

WHAT many chemists call fixed air at present, appears to be that which has been separated from bodies by different methods: but it ought rather to be denominated, as Dr. Hales has called it, detached or elastic air. In fact, air thus separated from bodies is no more fixed than that
which

which we breathe, since it recovers all its elastic properties as that philosopher has demonstrated,

Air, as we have declared in many parts of this work, not only dissolves water and is saturated with it, but it also dissolves oily substances, &c.

When air has been detached from bodies, by submitting them to distillation in an apparatus similar to that described by Dr. Hales, real philosophers call it *fixed air*. This air, in disengaging itself from the bodies, to which it was united, carries along with it different substances which it actually holds in solution, and properties are attributed to it, which do not belong to the air, but merely to the foreign substances with which it is loaded. It appears that this distinction has not been made, which however ought to present itself naturally.

When an acid is combined with a calcareous earth, with an alkaline salt or with a metallic substance, there is separated, as we have remarked, a considerable quantity of air and of fire in

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a state nearly pure, which could by no means enter into the composition of the neutral salt which results from this union. If we collect, by a suitable apparatus, the air which is detached while this combination is formed, the air thus detached is still named *fixed air*. Properties are found in this air different from atmospheric air, and it is therefore concluded that fixed air is not the same in all bodies; but the different properties which are discovered in it, ought to be attributed, as we have just said, to the foreign substances with which it is loaded.

AIR which is separated from bodies, during either the spirituous, acetose or putrefactive fermentation, is also denominated *fixed air*; and these several kinds of air differ from each other, like the bodies from which they have been produced. These observations alone might be sufficient to shew that these different properties ought to be attributed to the matter with which the air is impregnated, and not to the air itself, which is an element incapable of any alteration. But instead of making these reflections, there appears a disposition to establish as many species
of

of air, as there are bodies which can supply it*, which can answer no purpose, but to obscure the theory of chemistry. Some persons are already willing to admit of fixed air which is inflammable; of fixed air which reduces the metals to a calx, and causes the augmentation in their weight; of antiseptic fixed air, which restores putrefied flesh to sweetnes, &c. &c.

THERE is not the least doubt, but when an highly rectified oily substance is dissolved by air, and is collected in a proper space, it will burn, as Dr. Hales has remarked in many parts of his Vegetable Statics, and particularly in his analysis of pease, oyster shells, amber and wax, though he afterwards washed the air, separated from these substances, eleven times. Oily substances, thus dissolved by the air, or reduced to the state of vapours, almost always take fire, with an explosion, on the approach of a candle; but it is, by no means, the air which burns that element is not combustible.

* MR. BAUME certainly makes the advocates for fixed air, appear as holding absurd notions, which never entered into their heads. T. H.

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IT has been a received doctrine among chemists, that metals are reduced to calces, only by the loss of a portion of phlogiston, and that they recover their metallic brilliancy, such as before calcination, whenever this inflammable principle is restored to them; but some philosophers, partisans of fixed air, assert, on the contrary, that it is to the air which becomes fixed in the metal, during its reduction into a calx, that this change of state and the cause of the augmentation in weight ought to be attributed. These philosophers suppose, that by detaching from the metallic calces the fixed air with which they are loaded, they may be restored to their metallic form, without any addition, even without fire; but this opinion relative to their reduction appears founded on mistake, as phlogistic vapours are, imperceptibly, made use of in these operations.

We have said, under the article of liver of sulphur precipitated by an acid, that the vapours which arise from it are not at all inflammable, but that they restore without fire the metallic calces to their brilliancy. It is, by no means,

the

the air which produces this effect, but merely the phlogistic principle which it contains.

WITH regard to fixed air as an antiseptic, it is very probable, that there are many substances possessing antiseptic properties, which air may dissolve, and which may also counteract putrefaction, such as the Peruvian bark and other astringent bodies, which also have antiseptic qualities when immediately applied to putrefied flesh.

FROM these reflections we may conclude, 1st, That what is called *fixed air* is improperly named; the appellation of *detached* or *elastic* air, which Dr. Hales has given it, is better adapted to it.

2dly, THAT fixed air, under this denomination which has been given to it, is common air, but impregnated with foreign substances which it holds in solution; air, which may frequently be purified and restored to the state of good air, similar to that of the atmosphere, by causing it to pass through different liquors, proper for filtering it, and for retaining the foreign matters, which destroy its purity.

P 4

3dly,

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3dly, FIXED AIR, according to this theory, ought no longer to be examined, in the point of view in which it has hitherto been considered, but merely with respect to the substances which air is capable of dissolving or of being impregnated with.

4thly, THERE is therefore a field for a curious train of experiments which may acquaint us what substances may be dissolved in air, and what may be the properties of these substances when reduced to this state. These experiments made, in this point of view, would lead to more clear, and certain knowledge, than any which have hitherto been published.

5thly, THE case is the same with air as with water; both elements have the quality of dissolving, and being saturated with several substances; each of these elements acquires new properties which belong neither to water nor to air, but merely to the substances with which they are impregnated. As there are certain bodies which water is capable of dissolving, and which cannot be separated from it; the same should be the case with air: this last element may

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may be impregnated with substances as volatile, and as dilatable as itself, and which can never be separated from it, either by distillation, filtration, or any other method; but it does not less follow, but that these new properties which are to be found in the air, should always be attributed to the foreign substances, and not to the air itself*.

* THESE nineteen Chapters contain all that is most interesting relative to fixed air, which I have been able to obtain. I might have added here an extract from a very well written thesis, in favour of Dr. Black's theory, which was maintained at Edinburgh, September 12th, 1772, by Mr. Rutherford; but as it contains only a summary of what has been written on this subject by Messrs. Black, Cavendish and Lane, I was apprehensive it might lead me into unnecessary repetitions.

I AM also acquainted that there has appeared lately a Collection of Chemical Dissertations, by Dr. Wiegel, a Physician at Grieswald, in one of which he treats of fixed air and of the acidum pingue; but it has not been in my power to procure that work.

END OF THE FIRST PART.

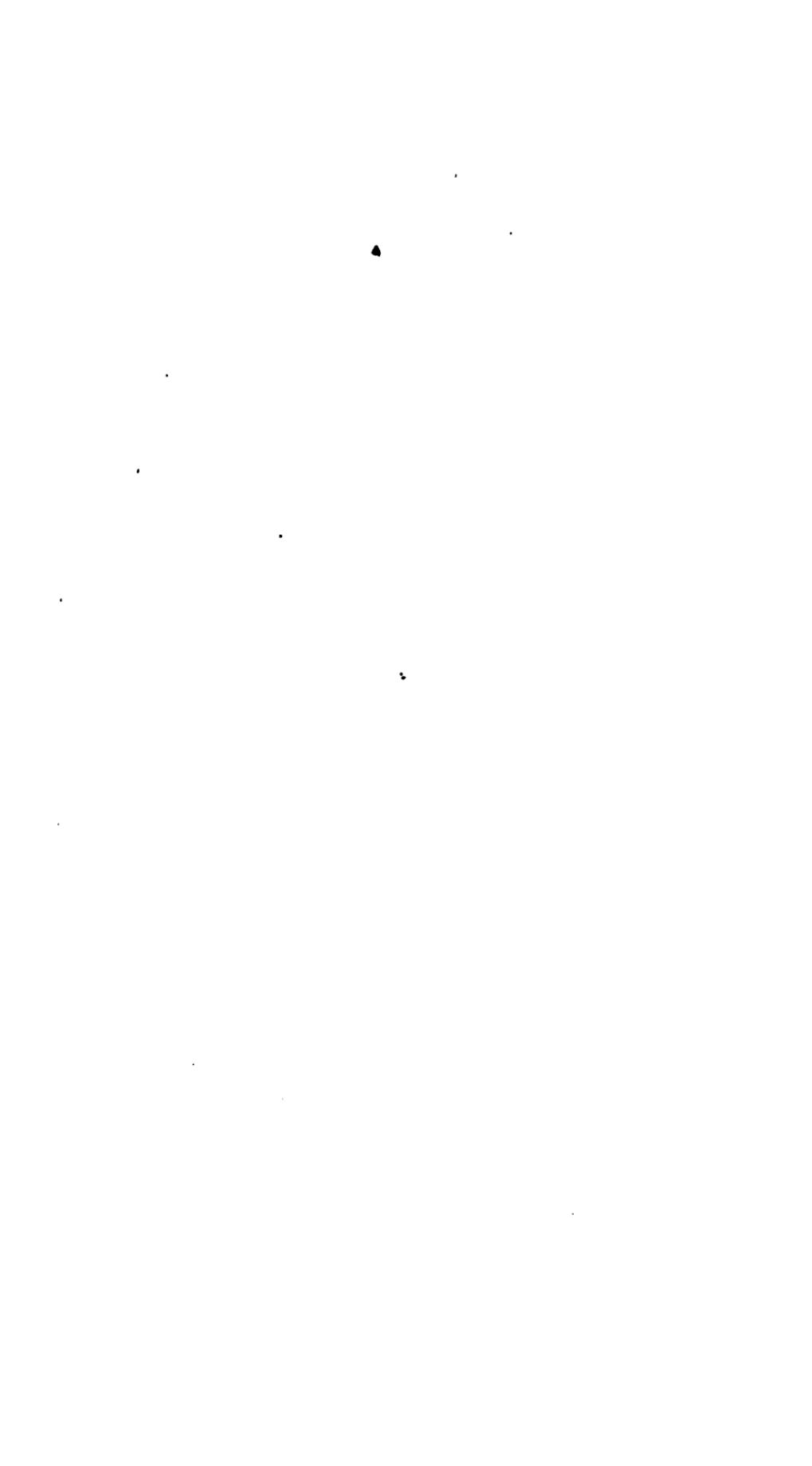
the following observations
and conclusions may be drawn:
1. The first point to note is
that the mean age of the
population was 25 years.
2. The second point is that
there were more males than
females in the population.
3. The third point is that
there was a high proportion
of illiterates in the popula-
tion.

T. A. D. S. C. G. O. D. S. P.

T H E

S E C O N D P A R T.

NEW INQUIRIES RELATIVE TO THE EXISTENCE OF AN ELASTIC FIXABLE FLUID
IN CERTAIN SUBSTANCES, AND THE PHENOMENA RESULTING FROM ITS DISENGAGEMENT,
OR ITS FIXATION.



T H E

S E C O N D P A R T.

**NEW INQUIRIES RELATIVE TO THE EXIS-
TENCE OF AN ELASTIC FIXABLE FLUID
IN CERTAIN SUBSTANCES, AND THE PHENOME-
NA RESULTING FROM ITS DISENGAGEMENT,
OR ITS FIXATION.**

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of philosophers as to the merit of these different
systems.

SUCH is the object which I propose to complete in the three first chapters of this second part. As the experiments which are here related, are all closely connected with each other, the unremitting attention of the reader will be requisite.

E X P E R I M E N T I.

THE SOLUTION OF CHALK IN THE NITROUS
ACID.

I POURED into a small matras, with a long narrow neck, six ounces of the nitrous acid, the weight of which was to that of water as 129,895 to 100,000. I added, by degrees, some chalk in powder dried in a degree of fire, long continued, nearly equal to that in which mercury boils.

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THE solution was effected by a quick effervescence, but with scarcely any heat. I took care to keep the matras as much stopped as possible; only opened it from time to time, to give vent to the vapours which were discharged impetuoufly; these precautions were taken that the evaporation might be as small as possible. Two ounces, three drachms, and thirty-six grains of chalk were employed to saturate the acid; the whole weight, therefore, of both substances, amounted to eight ounces, three drachms, and thirty-six grains: but having weighed them again, after their combination, the weight was no more than seven ounces, three drachms, and thirty-six grains; which made the loss in weight exactly one ounce. This could only be attributed to the elastic fluid which was separated, and the watery or other vapours which it had carried off with it: a method was, therefore, to be discovered to retain and examine them. This I proposed to do by the following experiment.

EXPE.

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EXPERIMENT II.

TO MEASURE THE QUANTITY OF THE ELASTIC
FLUID, SEPARATED DURING THE SOLUTION
OF CHALK IN THE NITROUS ACID.

A. B. Figure 1st, is a brass dish of ten inches diameter; in its center, at C. is a shaft which supports another dish, E. F. round like the former, and of $5\frac{1}{2}$ inches diameter. On this second dish another shaft is erected, G. H. which supports a frame represented separately in figure second. This frame is designed to bear a glass phial I. in form of a pear. It should have a spout to prevent the fluid, which it is designed to pour, from trickling down its outside; in default of a spout, one may be made of wax. The phial I. instead of being suspended by two pivots, ought rather to be sustained by two hemispherical leather caps, with a screw, which may separate or bring them nearer according as the phial employed is more or less thick. The inferior part K. of this phial, ought to be ballasted with lead, in order to keep it straight. It should also have

have in this part a button K. to which a thread should be connected. This last should pass above the frame, and be introduced through the hole M. in the lower dish, which is furnished with a small pulley. This thread serves to make a swing for the bottle I. when it is thought proper. All this apparatus is covered with a large receiver N. N. O. O. of six inches diameter, and from two and a half to three feet high. Lastly, the whole should be placed in an earthen-ware cistern V. V. S. S. of one foot diameter in its bottom, and nearly of the same height. It is represented as transparent in the figure, that the whole detail of the apparatus may be better understood.

WHEN this machine is to be made use of, a certain quantity of acid, or any other liquor is to be placed in the bottle I. In the basin Q. is placed some chalk, alkali, or other substance of which you would make your solution. The cistern V. V. S. S. is to be filled with water. The water is then to be raised by suction, by means of the hole R. at the top of the receiver, and to be made to mount nearly to Y. Y. high-

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er or lower according to the nature of the experiments which are proposed to be made. In fine, by means of the funnel represented in figure third, some oil is to be introduced into the receiver. The oil being lighter than the water mounts to its surface Y. Y. and by its interposition prevents the elastic fluid, separated in the combination of the acid and alkali, from being absorbed by the water. As soon as every thing is thus prepared, the thread R. is to be drawn, which passes through the three pulleys marked p. M. n. and gives a swing to the bottle I.

To this apparatus may be joined a pump, and this precaution is even indispensably necessary where substances are employed, whose vapours are noxious, and will not permit the air to be sucked without inconvenience. Such a pump is represented, adapted to the apparatus in figure first. P. P. represents the body of the pump, Z. the ring which serves to raise the piston. At each stroke the air is drawn in by the pipe X. L. whose extremity X. must rise within some lines of the dish: it is then filled again, and the air driven out through the body of the pump

pump by the pipe T. As the lower extremity L. of the pipe X. L. as well as the extremity s. of the pipe which sustains the pump, is designed to stand in the water, the screws in those parts should be guarded with leather well greased. We shall see in the sequel, other uses to which this pump may be applied.

INTO the bottle I. fig. 1st. I put an ounce and half of the same nitrous acid as that employed in experiment the first. In the bason Q. were placed four drachms and sixty three grains of the same chalk, and dried in a degree of heat in which mercury boils. The water was raised up to Y. Y. as above mentioned, and I introduced a covering of oil upon the surface of the water, I then mixed the acid and chalk by means of the swing, taking care to do it gradually, least by the briskness of the effervescence the liquor should run over the sides of the bason.

THE water sank suddenly in the receiver N. N. O. O. and settled seven and a half inches beneath the surface Y. Y. The receiver in this place was seventy lines $\frac{85}{100}$; whence it follows

Q. 2 that

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that the quantity of elastic fluid separated, amounted to two hundred and six cubic inches; but in about a quarter of an hour, the small degree of heat raised in the combination being dissipated, the quantity of elastic fluid was reduced to two hundred inches. After which there was no farther sensible variation, even for several days. The thermometer * during this interval of time, stood from sixteen to seventeen degrees, and the barometer at about twenty-eight inches.

THE quantities of nitrous acid and chalk employed in this experiment were only a fourth of those used in the former one; whence it follows that if six ounces of the nitrous acid, and two ounces three drachms and thirty-six grains of chalk had been employed, as in the first experiment, there would have been a separation of eight hundred cubic inches of elastic fluid: but the loss of weight in the first experiment was exactly one ounce; therefore eight hundred cubic inches of elastic fluid, such as is separated from

* Reaumur's thermometer was used in all these experiments.

from chalk, loaded no doubt with a large quantity of watery vapours which are carried off with it, weigh exactly one ounce, in a temperature from sixteen to seventeen degrees of the thermometer. A cubic foot, therefore, or one thousand seven hundred and twenty-eight cubic inches of this elastic fluid, weighs two ounces, one drachm and twenty grains; but a cubic foot of *common* air in that temperature does not weigh, according to the observations of Mr. de Luc, more than one ounce, two drachms and sixty-six grains*. From whence we may already draw one or other of these conclusions, either that the elastic fluid discharged from chalk by effervescence weighs about one-third more than common air, or, what is more probable, that by the violence of the effervescence, it carries off with it a considerable quantity of watery or other vapours, which contribute to increase the loss of weight observed in experiment first, and which occasion the elastic fluid to appear heavier than it really is.

Q. 3

EXPE-

* The French drachm is divided into 72 grains.

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EXPERIMENT III.

TO DETERMINE THE PROPORTION OF WATER
NECESSARY TO SATURATE A GIVEN QUAN-
TITY OF QUICK-LIME.

I PUT into an iron kettle twenty-eight ounces and six drachms of quick-lime, and by degrees poured on it a sufficient quantity of water to reduce it to a paste of a moderate consistence. After the ebullition was over, the kettle was placed on a gentle fire, to evaporate the super-abundant moisture. Care was taken to stir the mixture frequently with an iron spatula, to prevent its uniting and forming into large masses; towards the end the fire was increased, and gradually raised to a degree equal to that of boiling mercury; and it was kept up to this for several hours. At length, when the matter appeared to be perfectly dry, I withdrew it from the fire, and weighing it, while hot, the whole weight was found to be exactly thirty-seven ounces. I then quickly reduced into powder all the lime,
in

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in a mortar which was kept continually hot; it was passed through a silken sieve, and kept closely stopped in a glass bottle for use.

It appears from this experiment, that the relative weight of quick lime to that of slaked lime is as 1000 to 1287; that is to say, that 1000 parts of quick lime can absorb $\frac{287}{1000}$ of water, or, in other words, that every pound of this substance can imbibe four ounces, four drachms and fifty-three grains of water,

It might perhaps be imagined that the lime not only absorbs the water during its extinction, but that the air itself, or some matter suspended in the air, combines with it during the operation, and contributes to the augmentation of weight which has been observed. The following experiment will destroy these conjectures, and make it evident that the external air has no concern in the phenomena attending the extinction.

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EXPERIMENT IV.

LIME SLAKED IN THE VACUUM OF AN
AIR PUMP.

AN ounce and half of quick lime broken into pieces of a moderate size, was placed in a glass vessel, and a sufficient quantity of water poured on it; after which the vessel was placed under the receiver of an air pump, and a vacuum made as quickly as possible.

THE phenomena attending the extinction were in no wise different from those which were observed in the open air. During a space of some minutes the mixture swelled, attended with ebullition and heat. The lime was reduced to a white paste, which was dried, and found to have received an increase of weight nearly equal to that observed in the former experiment. I do not deny, that the lime might absorb a little elastic air during the operations of slaking and drying it; but the quantity is very inconsiderable,

able, and almost nothing in comparison of the quantity of water which it absorbs.

EXPERIMENT V.

THE SOLUTION OF LIME IN THE NITROUS ACID.

INTO a small matras, with a long narrow neck, were poured six ounces of the same nitrous acid as that used in the former experiments. I then added, by degrees, some of the flaked lime saturated with water and dried as in experiment the third.

THE first portions were dissolved with scarcely any motion, but as the acid became more saturated, the effervescence grew proportionably greater; but yet this effervescence was different from that arising from the solution of chalk: the bubbles were frequent, but small, and the swelling inconsiderable; the heat, on the contrary, was very strong, and even such, that it appeared

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ed as if the phenomena of boiling were joined to those of effervescence. The quantity of lime necessary for saturation was one ounce, five drachms, and thirty-six grains; the weight of the same materials, after combination, was found to be seven ounces, four drachms, and seventy grains. The loss of weight, therefore, was only thirty-eight grains.

It was of some importance to compare, as in experiment the first and second, the loss of weight observable during the effervescence with the quantity of elastic fluid which is separated. To effect this, the apparatus used in experiment the first was employed in manner following:

EXPERIMENT VI.

TO DETERMINE THE QUANTITY OF ELASTIC
FLUID WHICH SEPARATES FROM LIME DUR-
ING ITS SOLUTION IN THE NITROUS ACID.

IN the bottle I. fig. 1st. was placed an ounce and a half of the nitrous acid, before employed, and

and three drachms, twenty-seven grains of lime, slaked and dried (which appears to be the proportion necessary to saturate that quantity of acid) were put into the basin Q. The rest was disposed as in experiment the second.

THE combination was formed, as in the preceding experiment, with a little motion of effervescence or ebullition. In the first instant, the water descended suddenly three or four inches in the receiver N. N. O. O. but in a few seconds it recovered its level, and settled about an inch below the former mark. The receiver was luke-warm, and as it contained a considerable body of air, a very sensible dilatation would naturally result, accordingly in proportion as that air returned to the same degree of heat with that of the laboratory, the water remounted, and the separated air was found reduced to a cylindrical space of four lines in height, by seventy lines $\frac{42}{100}$ in diameter, i. e. of nine cubic inches. Had this experiment been made with the same quantities of materials as in experiment the fifth, there is no doubt but a quantity of elastic fluid, four times as great, would have been separated, i. e.

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i. e. thirty-six cubic inches. But supposing, as we may here do without any sensible error, that this fluid was exactly equiponderant with air, these thirty-six inches should, in the temperature of the laboratory, weigh $16\frac{1}{3}$ grains. The total loss of weight in experiment the fifth was but thirty-eight grains; from whence it follows, that notwithstanding the great heat undergone during the dissolution, the loss of weight caused by the evaporation was only twenty-one grains two thirds.

General conclusions deducible from the six preceding Experiments.

IT is immediately evident from the third experiment, 1st, That the quantity of one ounce, five drachms, and thirty-six grains of flaked lime, employed in experiment the fifth, and necessary to saturate six ounces of nitrous acid, contained three drachms and three quarters of a grain of water. 2dly, From experiment the sixth it appears, that the same quantity of flaked lime contained sixteen grains and a half of elastic fluid. It contained therefore only, in reality, one ounce,

ounce, two drachms, eighteen grains, and three quarters of alkaline earth; but in experiment the first, two ounces, three drachms, and thirty-six grains of chalk, were necessary to saturate an equal quantity, viz. six ounces of the nitrous acid; from whence it seems, that we may conclude, that two ounces, three drachms, and thirty-six grains of chalk, in like manner, do not contain more than one ounce, two drachms, eighteen grains and three quarters of alkaline earth; and that they contain besides, three drachms and three quarters of a grain of water, and six drachms, sixteen grains and a half of elastic fluid, which six drachms, sixteen grains and a half, by experiment the fifth, are equivalent to eight hundred cubic inches. From whence it follows, that a cubic inch of the elastic fluid contained in chalk weighs $\frac{561}{2000}$ or something above half a grain, in a heat of sixteen or seventeen degrees of Reaumur's thermometer. Whereas a cubic inch of common air, in the same temperament, weighs, according to Mr. De Luc, no more than $\frac{455}{1000}$ or something less than half a grain. This difference happens either because the elastic fluid discharged from chalk

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chalk is actually a little heavier than atmospheric air; or because it is charged with vapours in its separation from the chalk; or, lastly, because chalk contains more water than flaked lime.

If, as we have just said, two ounces, three drachms and thirty-six grains of chalk are really composed of one ounce, two drachms, eighteen grains and three quarters of alkaline earth---three drachms and three quarters of a grain of water, and six drachms, sixteen grains and a half of elastic fluid; it must necessarily follow that these different substances combined with each other in the same proportions, should make a calcareous earth or chalk. To obtain a full proof of this, the following experiment was made.

EXPERIMENT.

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EXPERIMENT VII.

TO REPRODUCE CALCAREOUS EARTH OR CHALK,
BY RESTORING TO LIME THE WATER AND
ELASTIC FLUID OF WHICH IT HAS BEEN DE-
PRIVED BY CALCINATION.

I WEIGHED five drachms and twenty-two grains of quick lime. We may recollect that this quantity is precisely adequate to one ounce, one drachm and fifty-four grains of chalk. The lime was thrown into eight pints of distilled water. It was very soon divided by the water, and in part dissolved; but a considerable portion remained deposited at the bottom of the vessel.

I TOOK, on the other hand, a glass bottle A. fig. 4. tubulated at E; and filled it up to B.C. fig. 5. i. e. nearly about a third, with chalk in gross powder. I then fitted the funnel G. to it, which was luted well to the neck of the bottle, so that there could be no communication of air by

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by the juncture; and fixed to the end of a small rod O. P. a cork P. of such a size as exactly to stop the neck of the funnel G. I luted to the orifice E, a glass syphon E. H. I. and brought its extremity I. to the bottom of a delph bucket K. L. M. N. which contained the lime water. Lastly, I filled the funnel G. with dilute vitriolic acid, and lifted up, from time to time, the cork P. that some portion of the acid might be introduced into the bottle A.

THE air which was detached in the effervescence, occasioned by the dissolution of the chalk in the vitriolic acid, passed through the syphon E. H. I. and bubbled in the lime-water contained in the vessel K. L. M. N. At the same time the lime-water became turbid, and having continued so for a considerable time, I proceeded so far as to precipitate all the lime, and to render the water which floated above it absolutely sweet. I then decanted the liquor; the earth which remained at the bottom was dried in a heat equal to that in which quicksilver boils; after which it was found to weigh one ounce, one drachm and thirty-six grains. Its weight, according to the

the foregoing results, should have been one ounce, one drachm and fifty-four grains. This difference of eighteen grains, which cannot be deemed very considerable, happened either from the inevitable loss which must attend almost every experiment, were it only from a small quantity of the earth remaining attached to the vessels; or perhaps the lime in this experiment was not so perfectly saturated with the elastic vapour as it might have been.

THIS calcareous earth moreover differed in no respect from chalk. It yielded, by dissolving it in the nitrous acid, a quantity of elastic fluid nearly equal to what chalk affords; the loss of weight which it suffered in the process was also the same; it no longer separated the volatile alkali of sal ammoniac in the cold: in a word, it was not in any respect distinguishable from common powdered chalk.

EXPERIMENT VIII.

TO DETERMINE THE SPECIFIC GRAVITY OF
LIME-WATER BEFORE AND AFTER PRECIP-
ITATION.

INTO some distilled water which was of a temperature of seventeen degrees of Reaumur's thermometer, I plunged a silver water-gage represented figure 6th. This instrument is constructed on the same principles as that described by Farenheit in the Philosophical Transactions; viz. the shaft D.E. instead of being graduated in the same manner as Boyle's areometer, had only a small mark engraved at E. nearly about the middle. This shaft is but three inches long; at the top is a basin proper to receive the weights; the instrument is to be loaded so as to sink it in the fluid to the mark E, by which the specific gravity may be determined. The waterpoise is ballasted at its lower end, viz. at B.C. with tin. Its weight is nine ounces and sixty-four grains. That it might sink to the mark E. in distilled water,

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water, at the temperature of seventeen degrees of Reaumur's thermometer, I was obliged to load it with twenty grains and a half. Hence it follows that it displaces a volume of distilled water of nine ounces, one drachm, twelve grains and a half.

HAVING withdrawn the water-gage from the distilled water, I threw in a good deal more lime than it could dissolve; it was then filtered, and as soon as it was found to be of the temperature mentioned in the last experiment, I plunged the water-gage into it; but it could not be made to sink to the same mark without loading it with thirty-two grains. Therefore the weight of the volume of lime-water displaced by the waterpoise was nine ounces, one drachm and twenty-four grains; which settles the relative specific gravity of distilled water to lime-water to be as 1000000 to 1002135.

EXPERIMENT IX.

TO DETERMINE THE SPECIFIC GRAVITY OF
LIME-WATER INTO WHICH THE ELASTIC
FLUID DISCHARGED BY EFFERVESCENCE HAS
BEEN SUFFERED TO PASS.

ELASTIC air detached by the effervescence of chalk and spirit of vitriol was made to bubble, as in experiment VI. into saturated lime-water; as soon as the precipitation was completely finished, the liquor was suffered to stand still, after which it was decanted, and the water-gage was cast into it. The weight of the fluid displaced was found to be nine ounces, one drachm, twelve grains and three quarters, or sensibly the same as that of distilled water; hence it is apparent that the elastic fluid had precipitated all the lime, and rendered it insoluble in water.

EXPE.

EXPERIMENT X.

TO IMPREGNATE WATER, OR ANY OTHER FLUID, WITH FIXED AIR OR ELASTIC FLUID.

FIGURE 7th. represents the apparatus by which I performed all experiments of this sort; it does not differ from that of figure 5th. excepting that I have substituted the bottle I. tubulated at R. instead of the bucket K. L. M. N. Some chalk grossly powdered was placed in the bottle A. as in the seventh experiment, and the vitriolic acid was added gradually through the funnel G.*

In proportion as the elastic fluid was discharged by the effervescence, it was obliged to enter the syphon

* Dr. NOOTH has communicated to the Royal Society, a description of a very convenient apparatus for impregnating water with fixed air, and of the manner of conducting that process. Vide Philosophical Transactions, Vol. LXV. Part I. page 59.

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syphon E. H. I. to pass into the bottle I. and to bubble through the distilled water or other liquor confined in it. It is necessary that all the junctures of the vessels be exactly luted in this experiment. The orifice R. must also be stopped with a good cork. By this means we keep an atmosphere of elastic fluid, much more condensed than atmospheric air, in the bottle I., and the liquor absorbs it more readily and in greater abundance than if it were not compressed. It is necessary to unstop the tube R. now and then, for fear the vessels should break, or that the vapours being too much condensed should escape by the junctures; there is always, besides, a large quantity of elastic fluid separated in the effervescence, which is not capable of being combined with water, and which it is necessary to let out occasionally.

EXPE-

EXPERIMENT XI.

TO COMPARE THE SPECIFIC GRAVITY OF WATER IMPREGNATED WITH ELASTIC FLUID WITH THAT OF DISTILLED WATER.

SOME distilled water was saturated with elastic fluid by the method described in the last experiment. The water had, very sensibly, an acidulous taste, and more considerably so, in my opinion, than when prepared by Dr. Priestley's process.

HAVING plunged in the water-gage, represented in figure 6th. the fluid displaced was found to weigh nine ounces, one drachm and thirteen grains in a temperature of $19\frac{1}{2}$ degrees. An equal bulk of distilled water of the same temperature weighed only nine ounces, one drachm, eleven grains and a quarter. The difference is one grain and three quarters. So that the specific gravity of water impregnated with fixed air is to that of distilled water as 1000332 to 1000000.

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THE same water, having been agitated by pouring it five or six times from one vessel to another, lost its acidulous taste; proceeding then to prove the weight of the water in this state, the bulk displaced was only nine ounces, one drachm, eleven grains and one third, viz. nearly the same as that of distilled water.

IT is probable, that if the experiment were repeated in cold weather, one might charge the water with a much larger quantity of elastic fluid; but I shall postpone this experiment for a future occasion.

EXPERIMENT XII.

TO PRECIPITATE LIME-WATER, BY ADDING
WATER IMPREGNATED WITH ELASTIC FLUID.

A QUANTITY of the same lime-water, the specific gravity of which was determined in experiment VIII. was placed in a receiver, and mixed gradually with some water which had been

been impregnated with elastic fluid. The lime-water became instantly turbid, and the earth precipitated to the bottom of the vessel: more of the water impregnated with elastic fluid was added, till I was assured that the precipitation was completed. The liquor was then suffered to stand awhile, and as soon as it was perfectly clear, the water-gage was thrown in; the specific gravity was found to be nearly the same as that of distilled water. The difference was only about 0,000095; and even this small variation might proceed, from my not having employed precisely the proportions of lime-water, and water impregnated with elastic fluid, necessary to make the precipitation perfect. We may easily, in fact, conclude, from comparing this experiment with the following, that on exceeding ever so little in the quantity of either kind of water, there will remain alike, in each case, a portion of earth united with the water. The earth precipitated in this experiment was no longer in a state of quick lime, it effervesced with acids, and did not separate the volatile alkali from sal ammoniac without the aid of heat. It was become a true chalk.

EXPE-

EXPERIMENT XIII.

TO REDISSOLVE THE LIME AFTER IT HAS BEEN PRECIPITATED, BY A FRESH ADDITION OF WATER IMPREGNATED WITH FIXED AIR.

THE precipitation of lime water, by mixing with it water impregnated with fixed air, affords a singular phenomenon; for if, after having precipitated all the lime, as in the last experiment, one continue to add still more of the water impregnated with fixed air, all the calcareous earth which had been precipitated will be redissolved, and the liquor become perfectly transparent.

I SHALL examine, in a particular chapter, the effects of water thus charged with a solution of calcareous earth combined with elastic fluid.

Conclusion of this Chapter.

IN reviewing the different experiments of which I have given an account in the preceding chapter,

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chapter, we cannot refuse our assent to the following consequences.

FIRST, That there exists in calcareous stones and earths an elastic fluid, a species of air under a fixed form, and that this air, when it has recovered its elasticity, possesses the principal physical properties of air.

SECONDLY, That a hundred pounds weight of chalk, according to the above proportions, contains about thirty-one pounds, fifteen ounces of this elastic fluid; fifteen pounds, seven ounces of water; and only fifty-two pounds, ten ounces of alkaline earth.

THIRDLY, That it is even possible, that the chalk may contain still less alkaline earth, and more elastic fluid, but that hitherto we are not acquainted with any method of depriving it of more, or of carrying its analysis farther.

FOURTHLY, That alkaline earth may exist in three different states: first, saturated with water and elastic fluid, as in chalk: secondly, deprived

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ed of its elastic fluid, but saturated with water, as in flaked lime: and, thirdly, deprived both of its elastic fluid and water as in quick-lime.

FIFTHLY, That quick lime, or alkaline earth, deprived both of its water and elastic fluid, contains a great quantity of the matter of pure fire, which it has probably acquired during its calcination, and that to this matter is owing the great heat which is observable during the extinction of lime, and its dissolution in acids.

SIXTHLY, That it is not sufficient to saturate quick lime with water, in order to deprive it of the superabundant quantity of igneous particles; but that they remain after this operation; since flaked lime communicates a considerable degree of heat to the nitrous acid, in which it is dissolved; a phenomenon which is not produced by calcareous earth or chalk.

SEVENTHLY, That it is by no means this superabundant igneous matter which reduces the alkaline earth into the state of lime, since flaked lime, when deprived by the flaking of a great part

part of this fire, is, notwithstanding, no less soluble in water, still continues to decompose sal ammoniac without the assistance of heat, and does not communicate a less degree of causticity, to either the fixed or volatile alkalis. In a word, it is no less lime than before it has been flaked.

LASTLY, That it is sufficient that we restore to lime, by any means whatsoever, the elastic fluid of which it has been deprived, to render it mild, insoluble in water, and capable of effervescing with acids; in short, to re-establish it in the state of calcareous earth or chalk *.

C H A P-

* I HAVE only spoken, in this chapter, of one species of calcareous earth, that I might not throw any confusion on the experiments, and lose sight of the principal object. All the pure calcareous earths, which I have had an opportunity of examining, present the same phenomena as chalk. They are all composed of an alkaline earth and water combined with elastic fluid in a fixed state; but they, almost all, differ in the proportions in which these three substances enter into their composition.

SOME experiments even lead me to believe, that it is partly from the difference of these proportions, that the
spars

CHAPTER II.

OF THE EXISTENCE OF AN ELASTIC FIXABLE FLUID IN THE FIXED AND VOLATILE ALKALIS, AND OF THE MEANS BY WHICH THEY MAY BE DEPRIVED OF IT.

AFTER having proved the existence of the elastic fluid, under a fixed form, in calcareous earths; that the fluid constitutes a consi-

spars possess their diversity of figure. I have found, for instance, that the species, described by Valerius, by the name of *Spatium pellucidum flavescens*, contained less alkaline earth, and more fixed air, than an equal quantity of chalk. The piece which I made my experiments upon, was taken from the lime-stone quarries between Chaumont en Bassigny and Vignory. Two ounces, six drachms, thirty-three grains of it, were necessary to saturate six ounces of the above mentioned nitrous acid; whereas, two ounces,

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considerable part of their weight; and that the causticity of lime is principally owing to the absence of it; it remains for me to trace the combination of this fluid with different substances in nature, and particularly with alkaline substances and with metals.

THE fixed vegetable alkali, which is produced by the burning of vegetables, and commonly known by the name of salt of tartar, appeared to

ounces, three drachms, thirty-six grains of chalk, were sufficient for the same purpose. On the other side, the loss of weight, after the combination, instead of being exactly an ounce, as in chalk, was an ounce and two drachms. The solution of this spar had a greenish cast, and there remained a small white sediment, insoluble in acids.

A SPAR from St. Marie-aux mines, in white crystalline groups, of the Drusæ species, which has much affinity to that represented in fig. 7. of Valerius's Mineralogy, afforded nearly the same results as chalk. The quantity necessary to saturate six ounces of nitrous acid was two ounces three drachms; and the loss of weight, in the combination, one ounce and three grains. This spar deposited a yellow sediment, which would not dissolve in acids. I intend sometime to pursue these experiments farther.

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to me not well adapted to be employed in the experiments of which I am going to give an account. 1st, Because it is difficult to bring it always to one fixed and determinate point of dryness, and the greater or less quantity of water which it retains, may occasion very considerable errors, 2dly, That having a very strong propensity to attract the moisture from the air, it changes its weight almost every moment. The crystals of soda, or mineral alkali purified, crystallized, and dried on brown paper, appeared preferable; provided that care were taken to keep them always in bottles, well stopped, to prevent their effervescence. I therefore made use of this alkali in the following experiments:

EXPERIMENT I.

THE SOLUTION OF CRYSTALS OF SODA IN THE
NITROUS ACID.

INTO a long narrow necked matras were put six ounces of the same nitrous acid as that used in the first experiment, Chap. I. A given weight

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weight of the crystals of soda was also dissolved in a given quantity of distilled water, six ounces of nitrous acid were gradually saturated with this alkaline liquor, and to arrive at that point, it was necessary to employ ten ounces, six drachms, sixty-three grains of water, and six ounces, two drachms, fifteen grains $\frac{3}{4}$ of the mineral alkali; still the acid was rather predominant. The total of the ingredients, employed in the combination, weighed twenty-three ounces, one drachm, six grains $\frac{3}{4}$.

THE effervescence was brisk, but unattended by heat; after which the same ingredients weighed no more than twenty-two ounces, sixty-two grains $\frac{1}{4}$. The loss of weight, therefore, was one ounce, $16\frac{1}{4}$ grains.

S

EXPE-

EXPERIMENT II.

TO MEASURE THE QUANTITY OF ELASTIC
FLUID, SEPARATED FROM SODA DURING
ITS DISSOLUTION IN THE NITROUS ACID.

I MADE use, in this experiment, of a sixth part of the quantities employed in the preceding one. I, therefore, placed, in the phial I. fig. 1st, one ounce of nitrous acid, and in the basin Q. one ounce, twenty-six grains $\frac{1}{2}$ of crystals of soda, dissolved in two ounces of water. The whole was covered with the large Receiver N. N. O. O. and the water being raised to a suitable height, and its surface covered with oil, the swing was set in motion.

THE effervescence was brisk, and the quantity of elastic fluid separated was 135 cubic inches. If then equal quantities had been used in this, as in the preceding experiment, there would have been a separation of 810 cubic inches of elastic fluid.

THE

THE barometer, during this experiment, was at twenty-eight inches one line $\frac{1}{2}$, and Mr. Reaumur's spirits of wine thermometer at fifteen degrees $\frac{1}{2}$; from whence it may be concluded, after the rules established by Mr. de Luc, that the atmospheric air weighed, at that instant, about $\frac{46}{100}$ of a grain to the cubic inch. If, therefore, the elastic fluid which was disengaged had been only pure air, its weight would have been no more than five drachms, twelve grains $\frac{2}{3}$, but the loss of weight was found to be one ounce, sixteen grains $\frac{1}{2}$; from whence an excess results of three drachms, three grains $\frac{1}{2}$. This difference happens, as has been before observed, with respect to chalk, either because the fluid, discharged by the effervescence, is heavier than atmospheric air, or because it carries up with it some watery vapours.

FROM this experiment it is evident, 1st, That more mineral alkali, than chalk, is requisite to saturate a given quantity of nitrous acid; which shews, that the salt retains much water in its crystallization and composition. 2dly, That if, on one side, the soda, in equal weights, contains

a much less quantity of elastic fluid than chalk; on the other hand, it contains a quantity in sufficiently exact proportion to its quantity of alkaline matter; for we may recollect, that in saturating six ounces of nitrous acid with chalk, 800 cubic inches of elastic fluid were obtained, and the separation of the same fluid from soda amounted to 810; but those two quantities may be regarded as not sensibly different.

We might, perhaps, from hence, suppose that six ounces, two drachms and 15 grains $\frac{1}{2}$ of soda, contains an equal weight of alkaline matter to that contained in two ounces, three drachms and thirty-six grains of chalk, and might make a tolerably probable calculation of the proportion of water, of elastic fluid and alkaline substance which the soda contains; but I acknowledge, at the same time, that a greater number of experiments are necessary to give a certain degree of evidence to this calculation. According to this mode of reckoning it would follow, that six ounces, two drachms and fifteen grains $\frac{1}{2}$ of soda contain only one ounce, two drachms and eighteen grains $\frac{3}{4}$ of alkaline matter, one ounce of elastic

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elastic fluid; and three ounces, seven drachms and sixty-nine grains of water; at least this calculation cannot be far from the truth. When these quantities are brought to the hundred, the result will then be that one hundred weight of soda contains sixty three pounds ten ounces of water, fifteen pounds fifteen ounces of elastic fluid, and twenty pounds seven ounces of alkaline matter.

E X P E R I M E N T III.

THE DIMINUTION OF SPECIFIC GRAVITY, IN A SOLUTION OF THE CRYSTALS OF SODA, BY THE ADDITION OF LIME.

TWO ounces of crystals of soda were dissolved in fourteen ounces of distilled water. The silver hydrometer, represented in fig. 6. which displaces, as we have before seen, nine ounces, one drachm, twelve $\frac{1}{4}$ grains of distilled water, in a temperature of seventeen degrees of Reaumur's thermometer, was then immersed into the solution; the weight of an equal bulk of the solution

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of soda, was found to be nine ounces, four drachms, fifty-six $\frac{1}{2}$ grains, which gives the proportion between the specific gravity of distilled water, and that of the solution of soda, as 1000000 to 1049350.

To this solution was added, one ounce of lime flaked and dried (Exp. III. chap. I.) viz. an alkaline earth saturated with water, but deprived of its elastic fluid; I shook the liquor, for some moments, to give the lime time to act upon the soda, after which I left it to settle. In a short time the lime gained the bottom of the vessel, and formed there a body; while the liquor which swam above it appeared clear and transparent. The hydrometer was cast in; but the fluid, which was now displaced, instead of weighing, as before, nine ounces, four drachms, fifty-six $\frac{1}{2}$ grains, only weighed nine ounces, four drachms, forty $\frac{1}{2}$ grains; which fixes the relative specific gravity of the solution to that of distilled water, as 1000000 to 1046313.

ANOTHER ounce of lime was then added to the same solution; it was shaken up as before, and

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and left to settle; the weight of the liquor, displaced by the hydrometer, was found to be no more than nine ounces, four drachms, twenty-one grains, viz. in the proportion of 1000000 to 1042612.

I STILL added a third ounce of lime; it precipitated more slowly, and did not collect into a body, as in the preceding experiments; the solution, however, was yet sensibly diminished in its specific gravity; the bulk displaced by the hydrometer, did not now exceed, in weight, nine ounces, four drachms, fourteen grains, and the proportionate specific gravity between it and distilled water was as 1000000 to 1041093.

ON each addition of lime, the alkaline solution effervesced less with acids; in short, after the third, it did not effervesce at all; the only appearance that could be observed, by attending very closely, was some very small bubbles which rose on the surface of the liquor, or were attached to the sides of the vessels in which the precipitation was made. Whatever quantity of lime was added afterwards, no further diminution

could be made in the specific gravity of the solution, nor could it be brought to the state of not discharging any more small bubbles on mixing it with acids.

THIS experiment affords us the proportion of flaked lime, requisite to reduce the soda to a caustic state: it appears to be three parts of lime to two of the crystallized soda. This quantity of lime is, indeed, more than is indispensably necessary: but it seems better, when we wish to obtain a lixivium in the highest degree of causticity, to employ too much than too little. If quick lime be used instead of flaked lime, equal parts will be sufficient. We have seen, in fact, by Experiment III. Chap. I. that flaked lime contains something more than $\frac{1}{4}$ of its weight of water.

HOWEVER favourable this experiment may appear to Dr. Black's system, it might, nevertheless, be explained also on that of Mr. Meyer. The partizans of the last might say, that the diminution of specific gravity, observed in the alkaline solution, in proportion as the lime was added,

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and left to settle; the weight of the liquor, displaced by the hydrometer, was found to be no more than nine ounces, four drachms, twenty-one grains, viz. in the proportion of 1000000 to 1042612.

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EXPERIMENT IV.

THE AUGMENTATION IN WEIGHT OF LIME
WHICH HAS PASSED THROUGH AN ALKALINE
SOLUTION.

FOUR ounces of crystals of soda were dissolved in fourteen ounces of distilled water; to which were added two ounces of flaked and dried lime, and the liquor was agitated for some moments; when all the lime was deposited, I decanted the liquor; the earth which remained at the bottom was washed with several waters, and then dried in a degree of heat in which mercury boils; when afterwards weighed, it was found to be three ounces six grains.

It is evident from this experiment, that flaked lime attracts from the alkaline solution, some substance which it takes possession of, and which increases its weight about $\frac{1}{2}$. This substance cannot be water, 1st, because the lime was already

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ready saturated with it. 2dly, That in attracting the water from the alkaline solution, it would concentrate the latter, so that the specific gravity of the solution should be augmented, instead of being diminished, as we have seen in the preceding experiment. The following experiments will inform us what that matter is which lime attracts from the solution of soda.

EXPERIMENT V.

TO MAKE WHATEVER PORTION IS DESIRED OF THE ELASTIC FLUID OF SODA, TO PASS INTO LIME, AND AFTERWARDS TO DEMONSTRATE IT IN THE LIME.

I DISSOLVED one ounce $26\frac{5}{8}$ grains of soda in crystals, in two ounces of distilled water, which according to Experiment II, ought to contain one hundred and thirty five inches of elastic fluid; I added to this solution two drachms of flaked lime, which, by Experiment V. chapter I. should contain about six cubic inches of air. The whole quantity

quantity of elastic fluid, therefore, employed in this experiment, was one hundred and forty one inches.

If these two drachms of lime had actually attracted from the soda a portion of the elastic fluid which it contained, it must necessarily follow, first, that the soda should contain less of it than before. Secondly, that the quantity abstracted from the soda should be found united with the lime. To verify this conjecture, I decanted, on one part, the alkaline solution swimming above the lime, to the very last drop; on the other side, I carefully washed the lime which was at the bottom, and lastly I saturated each of them, separately, with nitrous acid, in the apparatus designed to measure the quantity of air discharged, represented in figure 1.

THE alkaline solution, instead of 135 inches, now only yielded 64; the lime, on the contrary, which should have only produced 6 inches, afforded 80, total 144, which was within three inches of the total quantity employed.

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E X P E R I M E N T VI.

THE same experiment was repeated, employing the same quantities of mineral alkali and of water, only adding four drachms, instead of two, of lime. The alkaline solution was decanted, and the lime washed with a little water; after which I submitted them both separately and successively to the apparatus represented in figure 1.

THE separation of air from the alkaline lixivium was only 18 cubic inches. The lime, on the contrary, yielded 132; the whole amount, 150 inches; which is equal, again, within about eight inches, to the total quantity of elastic fluid employed in this experiment.

FOUR drachms of flaked lime are capable, according to the experiments related in the preceding Chapter, of absorbing more than 200 cubic inches of elastic fluid; however it wanted 18 inches of attracting the 135 inches of air which the soda contained. This circumstance proves,

proves, on one side, that the last portions of elastic fluid have a strong adherence to alkaline substances with which they are united; on the other hand, that lime, after it is combined with a certain portion of elastic fluid, no longer acts so powerfully, as before, in absorbing more.

I SHALL now proceed to the phenomena observable in the volatile alkali.

EXPERIMENT VII.

THE DISSOLUTION OF CONCRETE VOLATILE ALKALI IN NITROUS ACID.

INTO a small matras, with a long neck, were poured six ounces of nitrous acid, and a quantity of concrete volatile alkali was gradually added till the acid was thoroughly saturated.

A VERY brisk effervescence ensued, and the quantity of volatile alkali, necessary to completely saturate the nitrous acid, was 2 ounces, 6 drachms, 36 grains. The total weight, therefore, of the ingredients employed, was, before their mixture, 8 ounces, 6 drachms, 36 grains.

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The combination being effected, there only were found 7 ounces, 3 drachms, 60 grains; from whence it follows that the loss, during the effervescence, amounted to 1 ounce, 2 drachms, 48 grains.

EXPERIMENT VIII.

TO MEASURE THE QUANTITY OF ELASTIC FLUID DISCHARGED FROM A GIVEN QUANTITY OF CONCRETE VOLATILE ALKALI.

I EMPLOYED in this experiment $\frac{1}{4}$ of the ingredients of the preceding one, viz. $1\frac{1}{2}$ ounce of nitrous acid, and 5 drachms 45 grains of concrete volatile alkali. The combination being made in the apparatus described fig. 1st. yielded me $270\frac{1}{2}$ cubic inches of elastic fluid, which being multiplied by 4, makes 1080 cubic inches for the quantity of elastic fluid contained in 2 ounces, 6 drachms, 36 grains of concrete volatile alkali. The height of the barometer, at the time of this operation, was 28 inches, $1\frac{1}{2}$ line, and of the thermometer 19 degrees. The weight, therefore, of a cubic inch of atmospheric air, was,

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was, as Mr. de Luc has determined it, about the $\frac{45}{100}$ of a grain; consequently, if the elastic fluid, separated from concrete volatile alkali, were no heavier than atmospheric air, 1080 cubic inches ought not to weigh above 6 drachms 54 grains; but the loss of weight amounted (Exp. VII.) to 1 écuée, 2 drachms, 48 grains; on which the same reflections may be made as those with respect to chalk and the alkali of soda*.

EXPERIMENT IX.

THE COMBINATION OF LIME WITH A SOLUTION OF CONCRETE VOLATILE ALKALI.

EIGHTEEN ounces of distilled water, and two ounces of concrete volatile alkali were mixed together in a vessel which was immediately corked closely. The solution produced cold, as is the case with most salts. When the saline li-

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* Vide Exp. II. Chap. I. &c.

quor had recovered the temperature of the laboratory, which was about 17 degrees of Reaumur's thermometer, I immersed the same silver hydrometer, which I used in the former experiments; the weight of the fluid displaced by it, was found to be 9 ounces, 3 drachms, $65\frac{3}{4}$ grains, so that the specific gravity of this solution was to that of distilled water as 1037440 to 1000000.

I RETURNED this solution into a bottle which was well corked; I then added an ounce of lime flaked and dried; the vessel was shaken for some moments, then left to settle, and the liquor being decanted, the hydrometer was again thrown into it: the bulk of fluid displaced by the instrument, was found to be sensibly lighter than before the addition of the lime. Its weight was only 9 ounces, 2 drachms, 59 grains; that is to say, the specific gravity of the solution now exceeded that of distilled water only in the proportion of 1022492 to 1000000. This solution, which, before the lime was added, had only a faint odour of the volatile alkali, became now very penetrating.

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To this solution four drachms of fresh lime were added, the weight of the volume of fluid displaced, was now found to be reduced to nine ounces, one drachm, and fifty seven grains, viz. its specific gravity was to that of distilled water in the proportion of 1008446 to 1000000.

Four drachms more of lime, reduced its weight to nine ounces sixty-nine grains, consequently the liquor was lighter than that of distilled water * in the proportion of 997058 to 1000000.

THE solution was then extremely penetrating; the vapours were even so suffocating that it was impossible to proceed in the operation to determine the specific gravity, without taking some precautions to avoid them.

Four drachms of fresh lime being again added, the liquor appeared to be lighter than distilled water in the proportion of 990790 to 1000000.

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* THE superior levity of the fluid volatile alkali to that of water has been already observed by Mr. Baumé, relatively to that drawn from sal ammoniac by means of quick-lime. See Chymie experimentale et raisonnée, p. 112.

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THIS is the point at which the volatile alkali is deprived of its elastic fluid as much as it can be by lime; for on adding afterwards four drachms of lime to the alkaline solution, no further diminution was produced in its specific gravity*.

THIS experiment evinces, that the greatest quantity of *slaked lime*, necessary to render the volatile alkali as caustic as it can be made by that means, is two parts and half; according to this proportion, rather less than two parts of *quick-lime* will be sufficient to produce the same effect; but it is much preferable to use *slaked lime*; because the great heat which the liquor acquires in the extinction of the lime, would dissipate a portion of the volatile alkali.

* The total quantity of lime made use of in this experiment was exactly three ounces.

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EXPERIMENT X.

**THE INCREASE OF WEIGHT IN LIME WHICH
HAS BEEN COMBINED WITH A SOLUTION OF
CONCRETE VOLATILE ALKALI.**

To prove, as in Experiment IV. that lime attracts something from the volatile alkali, the solution, which had been diminished in its weight, in the last experiment, was decanted, and all the lime which had settled to the bottom was carefully separated. I dried it, by keeping it exposed, for a long time, in a sand bath, to a degree of heat rather greater than that in which mercury boils, and therefore capable of driving off any volatile alkali which might remain interposed among its particles; after which having weighed it, I found its weight to be 3 ounces, 4 drachms, 60 grains, whereas it only weighed exactly 3 ounces before the operation.

If, now, we calculate, after the proportions
of

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of Experiment VIII. we shall find that the two ounces of concrete volatile alkali employed in Experiment IX. should contain 768 cubic inches of elastic fluid; but these 768 inches of elastic fluid, by passing into the lime, occasioned in it an augmentation in weight of 4 drachms 60 grains; each inch, then, of elastic fluid weighed $\frac{45}{100}$ of a grain; which is precisely the same as the weight of a cubic inch of atmospheric air.

It may be here objected, that I suppose, in this experiment, that the elastic fluid has passed from the volatile alkali into the lime, without demonstrating it. The following experiment will remove this objection.

EXPERIMENT XI.

TO DEMONSTRATE IN THE LIME THE QUANTITY OF ELASTIC FLUID WHICH IT HAS ATTRACTED FROM THE VOLATILE ALKALI.

FIVE drachms, 45 grains of concrete volatile alkali were dissolved in a sufficient quantity of

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distilled water; I then added half its weight, or 2 drachms, 58 grains of flaked lime; I agitated the liquor, and when I thought the lime had exerted all its force, the liquor which swam above it was decanted, and I, separately, submitted both the lime which was deposited at the bottom of the vessel, and also the volatile alkali to the same apparatus, figure 1st. The separation of air afforded by the lime was 163 inches, that furnished by the volatile alkali was nearly, as much as it should be to make up the 270 cubic inches of elastic fluid contained in five drachms, 45 grains of volatile alkali; I say, nearly as much, because one circumstance of the experiment, which it is insignificant to repeat, occasioned an uncertainty of some inches as to the product obtained from the volatile alkali,

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EXPERIMENT XII.

TO RESTORE TO A CAUSTIC ALKALINE LIXIVIUM OF SODA, THE AIR OF WHICH IT HAS BEEN DEPRIVED BY LIME, AND AT THE SAME TIME TO RESTORE IT TO ITS ORIGINAL SPECIFIC GRAVITY AND THE PROPERTY OF EFFERVESCENT WITH ACIDS.

I TOOK the alkaline lixivium of Experiment III. which had been deprived of its air by lime, and placed it in the apparatus represented figure 7th, and I caused the elastic fluid detached from chalk by the vitriolic acid to bubble into it.

WHEN only a small quantity of the caustic alkaline lixivium was put into the bottle I, it recovered its effervescent property in three or four minutes; a longer time was necessary in proportion as the bulk of the liquor was more considerable; but in each case its specific gravity was sensibly augmented, and at the end of the experiment it was nearly the same as before its combination with the lime.

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EXPERIMENT XIII.

TO RESTORE TO THE CAUSTIC VOLATILE ALKALI THE AIR WHICH HAD BEEN ATTRACTED UPON IT BY LIME, AND AT THE SAME TIME TO ALL THE PROPERTIES DEPENDING ON THAT AIR.

The volatile alkali of Experiment IX. of this Chapter, rendered caustic by lime, was placed in the bottle I. figure 7th. and the elastic fluid discharged from chalk by the vitriolic acid was made to pass through it. The liquor was gradually increased in specific gravity; its quick and penetrating odour was rendered mild; and, lastly, it recovered the property, which it had lost, of effervescing with acids, and of precipitating calcareous earth dissolved in the nitrous acid *.

* This last circumstance is connected with Experiment I. of the following Chapter.

In this book there is a chapter on the composition of lime, which I have omitted, as it is very well described by Dr. Black in his *Elements of Chemistry*; and I have also omitted the chapter on the composition of lime, as it is very well described by Dr. Black in his *Elements of Chemistry*.

CHAPTER III.

OF THE PRECIPITATION OF CALCAROUS EARTH, DISSOLVED IN NITROUS ACID BY ALKALIS IN A CAUSTIC, AND IN A MILD STATE.

HAVING made a three-fold combination of nitrous acid and calcareous earth, as also the fixed and volatile alkalis, with elastic fluid; and having shewn how the last passes from alkalis into calcareous earth, and how it may be driven from the latter by means of acids; I thought it necessary, after the examples of Messrs. Black and Jacquin, to render these combinations more complex, to make them quadruplicate; and I shall now relate the phenomena which these experiments presented to me.

I FIRST dissolved one ounce, five drachms, thirty-six grains of flaked lime, in six ounces of nitrous

nitrous acid *. This solution was again divided into four equal parts, and placed in as many separate vessels ; it is evident that each of these contained one ounce and a half of nitrous acid, and three drachms, twenty-seven grains of flaked lime. I kept them to make the four following Experiments.

EXPERIMENT I.

THE PRECIPITATION OF LIME DISSOLVED IN NITROUS ACID BY FOSSILE ALKALIS.

TO one of the four portions of the above solution, was added, drop by drop, some alkali of soda in a liquid form, which was continued till no more precipitation was produced ; there was neither motion, nor effervescence in the mixture, and the precipitate collected together in a white form. The liquor which was above it was decanted, the powder washed in several portions of distilled water, and, having dried it in the heat

* See Chapter I. Experiment V.

of boiling mercury, it weighed four drachms, sixty grains.

THIS earth effervesced briskly with acids ; it had very little taste, and did not separate the volatile alkali from sal ammoniac without heat ; in a word, it was no longer in the state of lime, but in that of calcareous earth or chalk.

EXPERIMENT II.

THE PRECIPITATION OF CALCAREOUS EARTH DISSOLVED IN NITROUS ACID BY CAUSTIC FOSSILE ALKALI.

INTO the second portion of the same solution I poured some lixivium of alkali of soda, which had been deprived of its air by lime*. The precipitation was made in the common manner ; having afterwards washed and dried the precipitate, its weight was three drachms forty-eight grains. This earth was a true lime ; it dissolved

* See Chapter II. Experiment III.

dissolved in water, in the same proportion as lime; the lime-water prepared with it, threw up a tremor calcis on its surface; it made scarcely any effervescence with acids; it communicated causticity to alkalis; it decomposed sal ammoniac without heat; in short, no difference could be perceived between this and real lime prepared by calcination.

EXPERIMENT III.

THE PRECIPITATION OF CALCAREOUS EARTH,
DISSOLVED IN NITROUS ACID, BY A SOLUTION
OF CONCRETE VOLATILE ALKALI.

THE precipitation, in this Experiment, was made with a sufficiently sensible effervescent motion, and this circumstance furnishes also a new confirmation of the theory. It has been seen, in fact, Chapter II. Experiment VIII. and Chapter I. Experiment II. that the volatile alkali contains more elastic fluid than calcareous earth; the latter, therefore, cannot absorb, during its precipitation, the whole of what is discharged from the volatile

volatile alkali in its solution, and there must necessarily be an overplus which having recovered its elasticity, must be dissipated by effervescence. The precipitate was of a yellowish white; and being dried in the same manner as in the former experiments, it weighed 4 drachms, 49 grains. This earth, like that of Experiment I. of this Chapter, was in the state of calcareous earth; it was insoluble in water; it effervesced with acids, and had none of the characteristics of lime.

EXPERIMENT IV.

THE PRECIPITATION OF CALCAREOUS EARTH,
DISSOLVED IN NITROUS ACID, BY CAUSTIC VOLA-
TILE ALKALI.

I ATTEMPTED this precipitation in vain, both with the volatile alkali of sal ammoniac separated by lime, and with concrete volatile alkali deprived of its elastic fluid by the addition of lime, as also by a volatile alkali separated from sal ammoniac by metallic substances and well freed from elastic fluid; in all these cases the calcareous

ous earth was not precipitated; I only observed that, sometimes, the liquor lost something of its transparency, that a yellow matter, in time, collected, like very fine rust of iron, which, when dried, weighed but a few grains*.

IT appears from these four experiments, 1st, That we may, at pleasure, precipitate an alkaline earth from its solution in nitrous acid, either in the form of chalk, viz. saturated with elastic fluid, or in the form of lime. It is lime, if it be precipitated by a caustic alkali, or an alkali deprived of its elastic fluid: and if precipitated by a mild alkali, it is chalk. 2dly, That when it has been precipitated under the form of lime, its weight is nearly the same as that of the original lime employed in the solution, whereas, on the contrary, when it is precipitated under the form of calcareous earth or chalk, i. e. saturated with elastic fluid, it is obtained with an increase of weight nearly equal to that which lime, converted

* It has been already seen, Chapter II. Experiment XIII. that by restoring the elastic fluid to the caustic volatile alkali; the property of precipitating calcareous earth was also restored to it.

verted into chalk, acquires. 3dly, That something was however wanting to render this augmentation as great as it should be; for it appears from the experiments related at the beginning of Chapter I. that 3 drachms, 27 grains of flaked lime, afterwards saturated with elastic fluid, ought to weigh 4 drachms, 63 grains. We had, however, by the fossile alkali, Experiment I. but 4 drachms, 60 grains, and by the concrete volatile alkali, Experiment III. only 4 drachms, 49 grains; which farther confirms, what was above advanced, that lime, which very powerfully attracts the first portions of elastic fluid which are presented to it, acts but weakly on the last.

THE Experiments related in these two chapters, give almost the strongest proofs that can be adduced in physics, that the same elastic fluid, which has been seen to exist in chalk, Chapter I. is also found in the fixed and in the volatile alkalies; that it may be driven off from them by dissolving them in acids; and that the effervescence, observable at the instant of their combination, is caused by the separation of this fluid. That the
same

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some fluid has also more repulsion, more affinity with lime, than with alkaline salts; and that it is for this reason, that if we mix lime with an alkaline ley, it attracts the elastic fluid which the alkali contained, unites with it, is converted into calcareous earth, and reduces the alkali to a caustic state.

At present, however, I have not time to pursue this subject further. This might, perhaps, be the time to relate the experiments which I have made on the nature of elastic fluid separated from alkaline salts and earths; but other considerations oblige me, first, to inquire into the combination of this fluid with metallic substances.

C H A P.

CHAPTER IV.OF THE COMBINATION OF THE ELASTIC
FLUID OF CALCAREOUS EARTH AND
ALKALIS WITH METALLIC SUB-
STANCES BY PRECIPITATION.

EXPERIMENTS, sufficiently numerous, lead me to believe, that the elastic fluid, the same whose existence I have endeavoured to prove in calcareous earth and alkalis, is capable of uniting, by precipitation, with most metallic substances; that it is, in a great measure, this principle which forms the augmentation of weight in metallic calces, which deprives them of their brilliancy, which reduces them to the form of a calx, &c. Though my experiments on this subject are very numerous, yet as it is not to be doubted, that the precipitates retain with them something, both of their solvents and of the sub-

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stances which have been used to precipitate them; and that to this circumstance, also, some particular phenomena are joined, which are caused by the decomposition of the acids; I thought it better to reserve, for a particular memoir, the greater part of my experiments; and shall, therefore, content myself with delivering, at present, those which are more essentially connected with the subject^{*} which I am now treating; but with this caution, however, to the reader, that they are related but as facts, the consequences of which are not yet sufficiently proved.

EXPERIMENT I.

THE SOLUTION OF QUICK-SILVER IN THE NITROUS ACID.

EXACTLY twelve ounces of quick-silver were put into a matras, and twelve ounces of spirit of nitre* poured on it; immediately a

* See Chapter I. Experiment I.

spontaneous

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spontaneous effervescence ensued attended with heat. The red vapours of the nitrous acid arose from the mixture, and the liquor assumed a greenish colour. I did not wait till the solution was entirely accomplished, before I weighed it; it had lost one drachm, eighteen grains: three hours after, the mercury was nearly all dissolved; but having again weighed the solution, I was much astonished to perceive, that it had increased, instead of being diminished in weight, and that the loss, which was one drachm eighteen grains, at first, was now only fifty-four grains. The next day the solution of the mercury was entirely finished, and the loss of weight reduced to eighteen grains; so that in twelve hours, the solution, though confined in a narrow necked matras, had acquired an augmentation in weight of one drachm. Not having leisure at that time to pursue this phenomenon farther, I postponed my inquiry to a future opportunity; I added some distilled water to my solution, to prevent it from crystallizing, the total weight of it was then found to be forty-eight ounces, one drachm, eighteen grains.

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EXPERIMENT II.

**The Precipitation of Quick-silver by
Chalk and by Lime.**

I WEIGHED separately, in two vessels, eight ounces, fifteen grains of the above solution, each of which portions, according to the preceding experiment, ought to contain two ounces of nitrous acid, and two ounces of quick-silver. On the other side, I prepared six drachms, thirty-six grains of chalk, and four drachms, thirty-six grains of flaked lime. It has been seen, Chapter I. Experiments I. and IV. that these were the proportions necessary to saturate two ounces of nitrous acid. I put the chalk into one of the vessels, and the lime into the other.

An effervescence attended the precipitation by the chalk, but without any heat; the mercury precipitated in a light yellow powder; at the same time the chalk was dissolved in the nitrous acid.

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THE precipitation by the lime was effected without effervescence, but with heat; the mercury was precipitated in a brownish powder. When the precipitates were well subsided, I decanted off the liquor from them, and carefully edulcorated them. After which I caused them to be dried in a heat nearly equal to that in which mercury boils.

THE precipitate by the chalk weighed two ounces, two drachms, forty-five grains; that by the lime weighed two ounces, one drachm, forty-five grains. It was of a deep grey earthy appearance.

EXPERIMENT III.
**THE SOLUTION OF IRON BY THE NITROUS
ACID.**

SIXTEEN ounces of nitrous acid, the same as employed in the preceding Experiments, were placed in a matraff, and some iron filings gradually added; the effervescence was brisk, attended with great heat, red vapours, and a very

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rapid discharge of elastic fluid : the quantity of iron, necessary to attain the point of saturation, was two ounces four drachms ; after which the loss of weight was found to be four drachms nineteen grains. As the solution was turbid, I added as much distilled water as made the whole weight of the solution to be exactly six pounds.

EXPERIMENT IV.

THE PRECIPITATION OF IRON, DISSOLVED IN
NITROUS ACID, BY CHALK AND BY LIME.

I TOOK two portions, each weighing twelve ounces of the above solution, and containing two ounces of nitrous acid, and two drachms, thirty-six grains of iron filings. I placed them in two separate vessels ; to one were added six drachms, thirty-six grains of chalk, and to the other four drachms, thirty-six grains of flaked lime, being the quantities necessary to saturate the acid,

THE precipitation was effected by the chalk with effervescence and tumefaction ; that by the lime,

lime, without either effervescence or heat. Each precipitate was a yellow-brown rust of iron. They were washed in several parcels of distilled water, and then dried in a sand heat something superior to that used in the last experiment.

THE precipitate by the chalk when dried, was a greyish rust of iron, inclining even to white by veins; it weighed 6 drachms, 35 grains; that by the lime was rather yellower, and weighed 4 drachms, 69 grains.

THE results of these experiments are, 1st, That iron and mercury dissolved in the nitrous acid, acquire in general a remarkable increase of weight, whether they be precipitated by chalk or by lime. 2dly, That this increase is greater in respect to iron than to mercury. 3dly, That one reason for thinking that the elastic fluid contributes to this augmentation is, that it is constantly greater when an earth is employed saturated with elastic fluid, such as chalk, than when an earth is used which has been deprived of it, as lime. 4thly, That it is probable that the increase of weight which is experienced in the pre-

cipitation by lime, although not so great as that by chalk, proceeds, in part, from a portion of elastic fluid which probably remains united to the lime, and which could not be separated by the calcination. Experiment VI. Chapter I. confirms this opinion; we there actually see, that flaked lime still contains some portion of elastic fluid.

To these experiments, which seem to lead to a belief that the augmentation of weight in the metallic precipitates is partly due to a portion of elastic fluid which is united to them, one very strong argument may be joined; which is, that if, instead of precipitating by an earth, the precipitation be made by another metal, as shewn in columns 2 and 3 of Geoffroy's table of affinities, the dissolved metal, instead of being precipitated in form of a calx, reappears, on the contrary, in a metallic form, and is no heavier than it was, previous to its solution: It is very probable that this circumstance depends on the metal not finding, in its precipitation, any body from which it can attract the elastic fluid.

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I SHALL employ myself more particularly on this subject, at some future period.

CHAPTER V.

ON THE EXISTENCE OF ELASTIC FIXABLE FLUID, IN THE METALLIC CALCES.

ALLOWING that the experiments, related in the last chapter, may not have completely proved the possibility of the union of elastic fluid with metallic substances, they at least afforded such indications of it, as were sufficient to engage me to pursue the subject with particular attention. I began, from this time, to suspect, that the air of the atmosphere, or an elastic fluid contained in the air, was capable, in a great many circumstances, of being fixed and combined with metals; that to the addition of this substance, the phenomena of calcination

were

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were owing, as likewise the augmentation in weight of the metals converted into calces, and, perhaps, several other phenomena of which philosophers have, as yet, given us no satisfactory explanation. These conjectures also acquired a very great degree of probability, in my opinion, from the following reflections.

1st. THE calcination of metals cannot take place, in vessels closely stopped and exhausted of air.

2dly. IT is proportionably the more readily performed, as a greater extent of surface in the metal is exposed to the air.

3dly. IT is a fact known to all the metallurgists, and observed by all those who have been conversant in the operations of assaying metals, that in every reduction, there is an effervescence at the moment the metallic substance passes from the state of a calx to that of a metal; now, an effervescence is commonly no more than a separation of elastic fluid; the calx therefore contains an elastic fluid, under a fixed form, which recovers

recovers its elasticity, at the instant of reduction.

HOWEVER probable these conjectures might appear to me, it was by experiment, alone, they could be either confirmed or refuted; I accordingly made a succession of different trials, several of which were not successful, and the detail of which I think I ought to spare the reader, till I finally attain the establishment of the following facts.

EXPERIMENT I.

THE REDUCTION OF MINIUM IN AN APPARATUS PROPER TO MEASURE THE QUANTITY OF ELASTIC FLUID DISCHARGED OR ABSORBED.

B. C. D. E. fig. 8th. represents a cistern or other vessel of delf or glass, into which is inverted a crystal receiver F. G. H: in the middle of the cistern at K. a small column of crystal I. K. is erected

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erected formed into a cup at the top, and which is to be secured at the bottom with some green wax*. On this column is placed a cupel of porcelain or any other very refractory matter. A glass syphon or bent tube M. N. fig. 9th. is to be passed under the edges of the receiver, and the cistern B. C. D. E. is to be filled with water. The water then is made to rise to such a height as is judged necessary, in the receiver F. G. H. by sucking the air through the aperture N. of the syphon M. N. and then, with the funnel, with a curved neck, represented fig. 3d. a covering of oil is introduced into the receiver: the oil rises to the surface and prevents the elastic fluid, which is separated in the operation, from having immediate contact with the water or being absorbed by it.

IN the capsule A fig. 8th, were placed two drachms of minium, mixed with twelve grains of baker's fuel, which had been previously reduced

* THESE kind of columns may be procured at most of the china shops; they are used in deserts to support the fruit.

to powder, and calcined for several hours, by a strong fire in a close vessel. The height, to which the water, had been raised, G H, was marked with a narrow slip of paper, and the apparatus, thus disposed was carried to the focus of one of Tchirnausen's large burning glasses, belonging to M. le Comte de la Tour d' Auvergne: this lens was at that time placed at the Louvre, for the purpose of other experiments made by Messrs. Macquer, Brisson, Cadet, and myself jointly, of which one part is already communicated to the Academy of Sciences.

ALMOST at the very instant that the cupel A was presented to the focus, the reduction was accomplished and the lead reappeared in little round pieces or very small shot: at the same time a yellowish vapour arose, which adhered to the arch of the receiver, and seemed, to me, to be a calx of lead which had been volatilized by the violent heat. When I thought the reduction was finished, I withdrew the apparatus from the focus, and placed it on the same stand, and exactly in the same place in which it was before the operation; and when the vessels were perfectly cooled,

cooled, and had recovered the temperature which they had previous to the reduction, I observed the height of the water, and discovered, by the sinking of the surface, that a separation had been made of about fourteen cubic inches of elastic fluid.

THE quantity of lead obtained by this reduction was about $\frac{1}{2}$ of a cubic inch, whence it follows that the volume of elastic fluid separated, was equal to four hundred and forty eight times the volume of the lead reduced; there were still found at the bottom of the cupel some portions of minium unreduced. This experiment was frequently repeated and with different proportions; those which I have here mentioned constantly succeeded best: when too much charcoal was used, the reduction was made with difficulty at the bottom of the vessel; the charcoal on the contrary was burned on its surface, and there resulted errors so considerable as to prevent any confidence in the events.

THOUGH this experiment was sufficiently decisive, I still remained dissatisfied; first, because
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the focus of the burning glass being very narrow. I was not able to operate but on small quantities. Secondly, because the heat was so great in the vicinity of the focus, that it was impossible for me to employ receivers of less than five or six inches in diameter; and even these were much heated, and some of them were broken; from hence it happened that the small number of cubic inches separated during the reduction, being divided in a space so extended in surface, the differences were little perceptible. Thirdly, because the volume of air, contained in the receiver, being very considerable, the least difference in the temperature might occasion sensible errors. And fourthly, because the oil also, which covered the surface of the water, being exposed to so considerable a degree of heat, might discharge some portions of elastic fluid.

THESE different considerations obliged me to have recourse to an apparatus represented in fig. 10, the first idea of which came from Dr. Hales; it has been since corrected by the late Mr. Rouelle, and I have also made some occasional additions and alterations in it.

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THE retort A. fig. 10. is fitted at G. G. to the receiver G. H. which may be, according to the nature of the operation, either of tin, plated iron, or glass; the receiver has an aperture at b. which is lengthened in a tube b. I. about $2\frac{1}{2}$ feet in length. V. V. F. F. is a large bucket of wood, or rather of metal, perforated at K. K. in which the receiver G. H. is placed; and all the parts are secured with either mastick or solder, according as it may be glass or metal. Lastly, the whole is covered with a large glass receiver n. N. O. O. which should be perforated with a small hole at n. This receiver is supported by a pedestal, composed of four small pillars, kept at a convenient distance, by means of metal bands. These pillars are notched at the top, to receive the edges of the receiver.

WHEN we use this apparatus, the substances on which we intend to operate are placed in the retort A. It is luted closely at G. G. to the receiver G. H. with fat lute, of rather a thick consistence. This operation ought to be conducted with the greatest attention, and the lute must by no means be omitted, as it is extremely essential, that

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that not the least particle of air should be introduced through the junctures. The lute is to be covered with a moistened bladder, which is to be secured by thread passed several times round, pretty tight. It is not unuseful to remark, that before the thread be passed over the lute, it is necessary that the bladder be first firmly tied both above and below the juncture; to prevent the lute from spreading further than is necessary, and escaping from the pressure of the thread.

WHEN the vessels are thus luted, the cistern V. V. F. F. is to be filled with water, which is to be pumped up into the receiver by suction at the hole $n.$ and raised to whatever height is desired; care must be taken to fill up the cistern in proportion.

THE suction is not so easily performed as may be imagined; it becomes even extremely troublesome when the height of the water approaches 28 or 30 inches. This difficulty appeared to me so important, that it was necessary to remedy it; and I accomplished it by applying to this apparatus the little pump represented fig. 1. I introduced

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troduced under the recipient *n. N. o. o.* fig. 10, a tin syphon or tube E. B. C. D. represented separately fig. 11. Its extremity D. is proportioned so as exactly to fit it into the tube S. S. fig. 10, which is furnished with a cock R. and whose other end is adapted at S. X. to the tube X. L. of the pump P. When the junctures D. S. and S. X. have been exactly secured with the fat lute or green wax covered with a hog's bladder, moistened and tied round with strong thread, the cock R. is to be opened, the piston Z. put in action, and the air, contained in the receiver *n. N. o. o.* pumped out, and thus we are able to raise the water conveniently to the necessary height.

THE operations performed, with the assistance of this apparatus, were on the calx of lead, the reduction of which is so easy that I did not foresee that any difficulty would occur in the execution of it. I had very much, however, to encounter, from my embarrassment in the choice of retorts; those of glass are so liable to be acted on by the calxes of lead, that they lose their form, and flux before the reduction be made. Those of earth resist them better, but almost all

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of them have small imperceptible holes through which the air penetrates, so that one can scarcely ever be easy about the success of the experiments.

THESE difficulties interrupted me for a long time; and it was not till I was able to procure iron retorts that I began to operate commodiously. As the same obstacles which have obstructed me, may also occur to those who would repeat my operations; I shall give some description of the fabrication of the retorts which I used.

A PIECE of the strongest iron plate that can be procured is to be forged into the shape of a cap A. A. B. fig. 12. to form the bottom of the retort; three parcels are afterwards to be made from the same plate A. A. C. C., C. C. D. D., D. D. E. the edges of which must fit into each other very exactly; the lateral juncture of each ferrel is to be carefully soldered with copper, and the ferrels are to be united to each other and to the cap A. A. B. with the same folder. The only difficulty in this matter is with the folder

X 2 which

which is reserved for the last, as it must be done on the outside, but a dextrous workman will easily accomplish it, and I have not been much disappointed in this respect. These retorts may be made sufficiently red-hot without melting the folder; only this precaution is necessary, that when we use metallic bodies which are capable of attacking the copper, and of uniting with it, we fill only the lower part A. A. B. of the retort, below the folder. The same retort may be used frequently, and needs not be laid aside till the iron be burnt and reduced to scales. However attentive the workman may be, yet it is possible that some small imperceptible holes may remain in the folder, through which the air may be introduced; the method of discovering them is to pour a small quantity of water into the retort, and to shake it about till the inner surface be totally moistened, on blowing through the aperture E. the hole, if there be one, is announced by a small bubble of water which is perceived and points out the flaw.

However tedious these preliminaries may seem, they will be easily judged indispensably necessary for

for understanding the following experiments; and I chose to begin with them that I might less interrupt the attention of my reader.

EXPERIMENT II.

TO REDUCE LEAD BY THE FIRE OF FURNACES, IN AN APPARATUS PROPER FOR MEASURING THE QUANTITY OF ELASTIC FLUID SEPARATED.

IN the iron retort A. fig. 10th. were placed six ounces of minium, and six drachms of powdered charcoal, passed through a hair sieve. It will soon be seen that this quantity of charcoal is much more than sufficient to produce the reduction; but one circumstance makes this proportion necessary when iron retorts are used; for, then, the lead, after its reduction, remains in small shot which are mixed with the powdered charcoal, and are easily taken out of the retort; whereas, on the contrary, when only just the necessary quantity of charcoal is used, the lead forms into a mass, and if it be melted again, in

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order to get it out, there is danger that some part of it may unite with the solder, or some little of it may adhere to the retort. These inconveniences are avoided by using an over proportion of charcoal.

I LUTED accurately, in the manner above related, the retort A. to the receiver G.H. The water was raised to Y.Y. and a covering of oil was introduced on the surface of it. When every thing was thus disposed, I left the apparatus in the same state till the following day that I might be sure the air did not penetrate on either side. I then marked the length of the water at Y.Y. with a slip of paper, and lighted some charcoal in my furnace.

IN proportion as the vessels were heated, the air which they contained was rarefied, and the water descended accordingly; but this effect had its limits, and after some time, the rarefaction abated, and the water continued a while nearly stationary. When the fire came to be so much raised as to make the bottom of the retort red-hot; the water began suddenly to descend, almost

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most visibly, at the rate of 14 or 15 cubic inches in the minute; at length the separation abated, and when it was entirely ceased, I put out the fire and suffered the vessels to cool perfectly. The air contained in the receiver *n. N. o. e.* presently condensed as it cooled, and the water rose again. When it was quite fixed, I marked, with a slip of paper the place at which it stopped; and I left the vessels in the same state for 48 hours, without any sensible difference ensuing in the height of the water; the thermometer in the laboratory was, at this time, at 15 degrees and the barometer at 28 inches $1\frac{1}{2}$ line.

NOTHING now remained but to determine the quantity of cubic inches contained between the two slips of paper, and this I did in two different ways. 1st. By determining from exact measure and calculation the solid contents of the cylinder. 2dly. By filling the intermediate space, between the two slips of paper, with water, and then determining the weight and volume of this water. The two methods afforded me exactly the same results, and the quantity of elastic fluid separated, was found, from both, to be 560 cu-

bic inches. The quantity of lead resulting from this reduction was about $\frac{1}{4}$ of a cubic inch; whence it follows, that calx of lead contains a quantity of elastic fluid 747 times the bulk of the metal which was used to form it. When the vessels were unluted, I shook the retort, and poured out the lead; it was in grains, mixed with a considerable quantity of powdered charcoal. Having examined it carefully, I could not find any portion of the minium unreduced. The weight of the residuum was 5 ounces 7 drachms 66 grains. The experiment was frequently repeated, and the circumstances were always exactly the same.

THE weight of the ingredients employed in this experiment was, before the reduction, six ounces six drachms; after that process it was no more than five ounces, seven drachms and sixty six grains; and therefore the loss of weight was six drachms and six grains: but the quantity of elastic vapour separated was only five hundred and sixty cubic inches,
and

and an equal bulk of atmospheric air should only have weighed, that day, three drachms and forty one grains. It is true that there is every reason to believe that the elastic fluid from metallic reductions, which is the same as that from effervescence, as I shall demonstrate in the sequel, is heavier than atmospheric air: it has also been seen (Chapter I.) that its gravity may be estimated at $\frac{575}{1000}$ the cubic inch; but even from this reckoning, 560 cubic inches of elastic fluid should only weigh four drachms, thirty-four grains, and there would still be a deficiency in weight of one drachm, forty-four grains.

SOME drops of phlegm which I constantly found in the receiver GH, fig. 10th, in all the reductions of the calces of lead which I have made, induced me to suspect, that, independant of the elastic fixable fluid, there existed a portion of water in the minium; that it was separated during the reduction, and that this was probably the cause of the loss of weight which I had observed; but as the receiver GH, fig. 10th, was

too

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too small to condense the vapours sufficiently, it was thought proper to repeat the experiment with a common apparatus for distillation, employing a larger receiver.

EXPERIMENT III.

TO DETERMINE THE QUANTITY OF WATER
WHICH IS SEPARATED IN THE REDUCTION
OF MINIUM BY POWDERED CHARCOAL.

THE same quantities of minium and charcoal were used in this experiment as in the last: the receiver had a small hole drilled into it, which I was obliged to leave open during the operation. The elastic fluid was separated with a hissing noise, and at the beginning of the reduction some little water passed into the receiver. The weight of this water did not exceed twenty-four grains; it was an insipid phlegm, not seeming to differ from distilled water.

THOUGH the result of this experiment only yielded twenty-four grains of phlegm, it is however

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however probable that more was discharged, and that one part of it was carried off by the current of elastic fluid, and dissipated in vapours through the perforation in the receiver. Again it is probable that the elastic fluid disengaged from minium may be something heavier than that from effervescence, and it is very probable that to one of these two causes we may attribute the deficiency in weight remarked in Experiment II.

I PROPOSED, in order to clear up this point, immediately to determine the relative gravity of the different elastic fluids discharged from different substances, and to compare them with the air of the atmosphere : but the different apparatuses, necessary to complete this object, not having been procured in time, I thought it would not be right to defer, on that account, the publication of this work ; besides, I shall have occasion, more than once, to recur to this point.

THE quantity of powdered charcoal, employed in Experiment II. was six drachms ; the quantity of elastic fluid obtained in the reduction did

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did not exceed four drachms, or four and a half at the most. The weight of the elastic fluid separated was then much less than that of the charcoal employed; and it may be objected that the quantity of elastic fluid, which was discharged, might as well come from the charcoal as from the metallic calx. To obviate this objection, I made the following experiment.

EXPERIMENT IV.

TO SEPARATE FROM THE LEAD THE PORTION OF CHARCOAL WHICH REMAINS AFTER THE REDUCTION.

THE residuum of Experiment II. was placed in an iron ladle; we may recollect that it was composed of granulated lead and powdered charcoal, and its weight was five ounces, seven drachms, sixty-six grains. As soon as the charcoal powder began to heat, it lighted and gradually consumed, after which there only remained a lump of lead, and a little calx of the same metal which had formed anew during the burning

ing of the charcoal. The whole of the lead together weighed nearly five ounces, three drachms, twelve grains; I say nearly, because if the operation be not quite finished, a small portion of charcoal remains unburned; on the other hand again, if it be carried too far, one part of the lead is recalcined and augments the weight. This circumstance occasions an uncertainty in the result of about twelve grains; and it is by repeating the experiment several times, and adhering to the lesser weight, that I have fixed it such as it is here.

IT appears from this experiment, 1st, That the relative weight of lead to that of minium is five ounces, three drachms, twelve grains, to six ounces, viz. that with 100 pounds of lead, 111 pounds 10 ounces of minium may be made, or, which is still the same thing, that 100 pounds of minium contain 89 pounds 9 ounces of lead. 2dly, That the 5 ounces 7 drachms 66 grains, remaining in the retort, Experiment II. after the reduction, was composed of 5 ounces 3 drachms 12 grains of lead, and of 4 drachms 54 grains of charcoal. So that only 1 drachm 18 grains of

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of charcoal, had been actually employed in the reduction: but the quantity of elastic fluid discharged in Experiment II. placing it at the lowest, weighed at least $3\frac{1}{2}$ drachms; it could not therefore be supplied by $1\frac{1}{4}$ drachm of charcoal, and consequently it must necessarily be at the expence of the minium, that the greater part of the elastic fluid was furnished.

NOTWITHSTANDING the conclusiveness of this experiment, I was not yet satisfied, and I thought it requisite to attend particularly to the examination whether charcoal alone would not yield, in the same degree of heat, an elastic fluid similar to that which I had obtained in the reduction of minium. This is the object of the following experiment.

E X P E-

EXPERIMENT V.

TO CALCINE POWDERED CHARCOAL ALONE, IN
A STRONG FIRE, AND IN AN APPARATUS
PROPER FOR MEASURING THE QUANTITY OF
ELASTIC FLUID SEPARATED FROM IT.

I CAUSED a new gun-barrel, well cleaned within, to be bent, and the touch hole and breech stopped, and each covered with a piece of iron, soldered hot to it, that I might be certain that the external air had no admission into it. Two drachms of the same bake-house fuel powdered (Experiments I. and II.) were then introduced, and it was adapted to the apparatus of fig. 10th. in which, on this occasion, I was obliged to make some trifling changes, which is unnecessary to relate: I then luted all the juncitures, very exactly, in the usual manner; the water in the receiver zNOO was raised and covered with a thin bed of oil; and when I was certain that the air could not penetrate either way, I marked the height of the water yy . I then

then made a brisk fire round the gun-barrel, and kept it for an hour in a white heat.

THE air was rarefied at first as usual, and the surface of the water sunk proportionably; but after the fire was extinguished it reascended by degrees, and when the gun-barrel was quite cold, it returned nearly to the point from which it began to move. The product of air was only thirteen cubic inches, which in two days was reduced to eight inches. The powdered charcoal being weighed, at the conclusion of this experiment, had lost but six grains, and it is even probable, that some of it still adhered to the gun-barrel.

THE fire, in this process, was infinitely stronger, and continued for a much longer time than is necessary for the reduction of the calx of lead, yet the product of air was very small; and therefore the air obtained in Experiments I. and II. was not merely the effect of the calcination of the charcoal, but, on the contrary, was produced by the reduction.

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I HAVE before observed, that I had made use of a new gun-barrel which had been well cleaned on the inside, in this experiment, and this circumstance is worthy of remark, for the phenomena are very different, when a gun-barrel is employed which is rusty within. In that case we procure a little water, and a greater production of elastic fluid in proportion to the rustiness of the barrel ; but it is evident from the last experiment, that these products belong to the calx of the iron which is reduced, not to the charcoal. It has sometimes happened that I have obtained from 80 to 100 cubic inches of elastic fluid with a very rusty gun-barrel, the first time it has been used, I here only mention this experiment, reserving the different accounts relative to it to a future opportunity.

It might perhaps be suspected, that the gun-barrel which I employed in Experiment V. though new and well cleaned, might yet contain some rust, and that the separation of the eight inches of elastic fluid, which I observed, might be owing to this circumstance ; but I was convinced to the contrary, by repeating this exper-

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ment with the same barrel and fresh charcoal. It is plain, that if the elastic fluid had been produced, in the former experiment, by the reduction of the iron of the barrel, that separation could not take place in the latter. However, the quantity of elastic fluid, this last time, proved to be twelve inches at least, which was something greater than at first; from whence it seems demonstrated that the separation is to be placed to the account of the charcoal.

THE diminution of weight, in this experiment, was eight grains.

E X P E R I M E N T VI.

**THE REDUCTION OF MINIUM IN A
GUN-BARREL.**

FOUR ounces of minium were mixed with the same charcoal which was so strongly calcined in the preceding experiment, and the whole put into the same gun-barrel which had been used in the two former calcinations. It was then adapted to

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to the apparatus, fig. 10th. and all was disposed in the same manner as in Experiment V. after which the fire was kindled.

WHEN the gun-barrel began to be obscurely red, the discharge of elastic fluid was so very rapid, that the water descended, perceptibly, in the receiver n NOO. fig. 10th. The discharge being finished, I continued to raise the fire, but the water did not fall sensibly lower. When the vessels were grown cold, I measured the quantity of elastic fluid which was separated, and found it to be 360 inches, or in the proportion of 90 inches for each ounce of minium. It has been seen above, Experiment III. that six ounces of minium had yielded a discharge of elastic fluid of 560 cubic inches, which is something more than 93 inches for each ounce; from whence it is evident that the results of these two experiments agree almost perfectly. As in the operation of which I have here given an account, the charcoal was strongly calcined a second time, before it was mixed with the minium, the results of this experiment should seem to deserve more reliance than those of Experiment III.

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IT appears to be proved from these experiments, that it is by no means the charcoal alone that produces the discharge of elastic fluid, observed in Experiments I. and II. neither is it the minium alone, since after Dr. Hales's experiments (see page 25) it affords but a very small portion of air; the greater part of elastic fluid which is detached, arises from the union of the powdered charcoal with the minium. This last observation leads us insensibly to very important observations on the use of charcoal, and such kind of substances in general, in the reduction of metals. Do they serve, as the disciples of Mr. Stahl think, to restore to the metal the phlogiston which it has lost? Or rather do these substances enter into the composition of the elastic fluid? This is a point which, in my opinion, the present state of our knowledge does not permit us to decide.

If it were permitted me to indulge in conjectures, I should say, that some experiments, which are not sufficiently complete to submit to public inspection, induce me to believe, that every elastic fluid results from the combination of

of some solid or fluid body with the inflammable principle, or perhaps even with the matter of pure fire, and that on this combination the state of elasticity depends. I should add that the substance fixed in metallic calces, and which augments their weight, would not be, properly speaking, on this hypothesis, an elastic fluid, but the fixed part of an elastic fluid, which has been deprived of its inflammable principle. The principal action of charcoal, and all other substances of that nature employed in reductions, would then be, to restore the phlogiston, or matter of fire, to the fixed elastic fluid, and with it the elasticity which depends on it.

HOWEVER different this opinion may seem to be from that of M. Stalh, it yet perhaps is not incompatible with it. It is possible that the addition of charcoal in the reduction of metals may answer two purposes at once; 1st, That of restoring to the metal the inflammable principle which it has lost: 2dly, That of restoring to the fixable elastic fluid in the metallic calx, the principle which constitutes its elasticity. But I repeat it again that it is with great caution, that

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an opinion on so delicate, so difficult a subject
should be hazarded; a subject which is very
nearly connected with one still more obscure,
I mean the nature of the elements themselves,
or at least of what we regard as elements. Time
and experiment alone can settle our opinions on
these points.

C H A P T E R VI,

OF THE COMBINATION OF ELASTIC FLUID WITH METALLIC SUBSTAN- CES BY CALCINATION.

I HAVE hitherto proved the existence of an elastic fixable fluid in the metallic calces, only by the separation which takes place in the moment of reduction. Though the experiments I have related might appear, in this respect, of such a nature as not to leave any doubt; it must nevertheless

less be confessed; that conviction in physics is only produced in as much as we arrive at the same point by different roads.

I THEREFORE intend to shew in the course of this chapter, that as whenever a metallic calx passes from that state to that of a metal there is a *discharge* of elastic fluid, so also whenever a metal passes from its metallic state to that of a calx there is an *absorption* of the same fluid, and that the calcination is nearly proportionable to this absorption.

EXPERIMENT I.

THE CALCINATION OF LEAD, BY A BURNING GLASS, UNDER A GLASS RECEIVER INVERTED IN WATER.

THREE drachms of sheet lead were placed in the apparatus represented fig. 8th. and exposed to the focus of one of Tchirnausen's large burning glasses of 33 inches diameter, the same as

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was mentioned above. The focus of this lens was contracted and made thinner by means of a second which had been added at a suitable distance. A piece of hard stone, such as is used to pave the streets of Paris, served to support the lead; it was hollowed in the middle to prevent the metal from slipping off before it was fused.

The lead flowed at the instant it was presented to the focus; soon after, a whitish vapour began to rise which collected on the sides of the receiver, and formed a yellowish sediment. At the same time there was formed on the surface of the lead a thin stratum of calx, which as the calcination proceeded, became of a yellow masicot colour. These different effects were produced during the first five minutes; after which, having continued to keep the lead exactly in the focus, I beheld with surprise, that the calcination did not proceed. I persisted in the pursuit of this experiment for half an hour, without perceiving that the stratum of calx which had formed on the lead, was in the least increased. It may be imagined, that the air contained in the receiver

receiver must be much heated, and that from its rarefaction it must have lowered the surface of the water G. H. but in proportion as the vessels cooled, it re-ascended, and when the whole apparatus was returned to the same degree of temperature as before the operation, a diminution of about seven cubic inches was perceived in the volume of the air.

THE lead, having been withdrawn, was found as malleable as before the operation, except the little coat of calx adhering to it, which was exceedingly small. It had lost nearly half a grain of its weight, but it was evident from inspecting the yellow flowers which lined the dome of the receiver, that this diminution proceeded from the evaporation, and that, by adding their weight to that of the lead, there had been an augmentation of several grains.

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EXPERIMENT II.

THE CALCINATION OF TIN.

TWO drachms of tin were exposed to the focus of the same lens and under the same apparatus. The calcination was still more difficult than that of lead ; the metal was covered with a small but exceedingly thin coat of calx ; and it fumed a little. The operation was continued for twenty minutes, and yet I did not perceive that the calcination had made any progress. When the vessels had recovered the temperature at which they were before the experiment, only a very small diminution was found in the volume of air. The tin having been weighed again, was, augmented about the eighth of a grain ; it also continued as malleable as before the operation, and had but an extremely thin covering of calx on its surface.

EXPE-

EXPERIMENT III.

THE CALCINATION OF A MIXTURE OF LEAD
AND TIN.

I WAS inclined to try whether the calcination of a mixture of tin and lead would not be more easily performed; and therefore composed an alloy of equal parts of lead and tin, and exposed two drachms of it to the focus of the burning glass; the receiver was, at most, but half as large as that of the first experiment of this chapter, and was only $5\frac{1}{2}$ inches in diameter.

THE mixture fused immediately; many white fumes arose, part of which adhered to the top of the receiver, and part were deposited on the surface of the oil. The operation was continued for 20 minutes; after which the calcination appeared much more advanced than in the former experiments, and there were appearances like vegetation on the surface. The vessels being cooled, a diminution was found in the bulk
of

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of the air, from 5 to 6 cubic inches. The receiver contained a great quantity of flowers, and the button of tin and lead was diminished 4 grains, which loss seemed to be recovered with increase in the portion which was sublimed. Though the calcination was rather farther advanced in this than in the preceding experiments, yet the greater part of the mixture was still malleable, and in its metallic state.

THE preceding experiments, though they confirm those of Chapter V., left me, notwithstanding, yet dissatisfied; 1st, Because the surface of the oil, confined in the receiver, being exposed to so considerable a degree of heat, might possibly produce air during the calcination, or else might absorb it: 2dly, Because the heat of the focus being too violent, it volatilized the lead and the tin, in proportion as they were calcined, in so much that I could not obtain any certain result of the increase of weight in these metals. I endeavoured to remedy both these inconveniences in the following experiment.

EXPE-

EXPERIMENT IV.

THE CALCINATION OF LEAD UNDER A GLASS
VESSEL INVERTED IN QUICK-SILVER.

I MADE use of an apparatus nearly similar to that represented by fig. 8th. it differed however from it; 1st, in having, instead of the bucket or cistern B. D. C. E. a strong earthen glazed pan: 2dly. In that, instead of filling it with water, I poured into it 80 pounds of quick-silver: and, 3dly, That instead of the receiver F. G. H. I substituted a glass cucurbit with a flat bottom. The object of this last alteration was to have a vessel of the same capacity as the receiver, but whose aperture should be narrower, that less mercury might be used. These dispositions being made, I placed on the column I. K. a stone crucible containing 3 drachms of lead: the crucible was above an inch in diameter, and about four lines in depth: it was flat at the bottom, that the metal might present a larger surface to the sun's rays. I afterwards covered the whole with the

glas

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glass cucurbit which served me instead of a receiver; the mercury was raised by the syphon, L. M. as high as G. H. and the point parallel to its surface was carefully marked with a slip of paper which went almost round the vessel; and lastly, I presented the whole apparatus to the great burning lens, observing that the lead was a full inch from the true focus, and that the heat was not much greater than that necessary to melt it.

At the very instant that the lead melted, though it was taken out of the centre of a large piece, though it was bright on every side, and had not the least appearance of foulness, a pellicle immediately formed on its surface. In the progress of the calcination this pellicle became of a yellow massicot colour, and wrinkled on the side towards the meridian; after which, at the end of ten or twelve minutes, the calcination stopped, and no farther effect was observed; only it happened that at those instants when the heat was a little stronger, the yellow pellicle fused in some places, and formed a yellowish glass; from the portions, thus vitrefied, fumes arose

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arose plentifully, which tarnished the top of the cucurbit. I opposed, as much as possible, the evaporation, by removing the lead farther and farther from the true focus of the lens.

The lead was thus exposed to the action of the large burning glass for one hour, forty-five minutes; but as, during this period, the sun was, at times, obscured by small clouds, we cannot reckon on more than an hour and fifteen minutes of real effect.

The operation being finished and the vessels perfectly cooled, the surface of the mercury was found to have ascended two lines and half above its former level. The diameter of the cucurbit was, in this place, $4\frac{8}{10}$ inches, which makes the quantity of air absorbed to be $3\frac{1}{2}$ cubic inches. The lead having been carefully separated from the crucible, weighed 3 drachms $1\frac{3}{4}$ grain: I estimated the yellowish vapours adhering to the sides of the cucurbit at about $\frac{3}{4}$ of a grain; the total increase of weight in the calcination had been, then, $2\frac{1}{4}$ grains, viz. $\frac{2}{3}$ of a grain for each inch of air. It results, therefore, that

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that the quantity absorbed is exactly proportionate to the augmentation in weight of the metallic calx.

THE void space of the cucurbit, or, in other words, the volume of air in which the calcination was made, was 75 cubic inches; so that the absorption was precisely a twentieth.

E X P E R I M E N T V.

THE EFFECT OF AIR IN WHICH LEAD HAS BEEN CALCINED, ON BURNING BODIES.

I CALCINED, as in the last experiment, and in the same apparatus, three drachms of lead. The operation being finished, turning the cucurbit FGH. fig. 8th. briskly, I placed it with its mouth uppermost, and immediately introduced a wax candle. It burnt tolerably well just at first, but it began insensibly to languish, and it was extinguished in about a minute.

EXPE-

EXPERIMENT VI.

THE EFFECT OF AIR, IN WHICH METALS HAVE
BEEN CALCINED, ON LIME-WATER.

THE process was conducted in the same manner in this as in the preceding experiment, with this difference only, that instead of introducing a candle into the cucurbit, I poured some lime-water into it. Its mouth was then stopped, and I shook it strongly. The lime-water acquired a slight, but scarcely perceptible turbid appearance, and no precipitation ensued.

IT is evident from these two experiments, that air in which metals have been calcined, is not, by any means, in the same state as that separated by effervescence, and by metallic reductions.

EXPERIMENT VII.

THE CALCINATION OF IRON BY MEANS OF
MOISTURE.

FOUR ounces of iron filings were put into a glass vessel, and moistened with a little distilled water, and the whole covered with a glass receiver, the vacant space of which was about 200 cubic inches. No sensible effect was produced for some days; the finest particles of the iron swam upon the surface of the water without being reduced to rust, the remainder was at the bottom. At the end of eight days, a small quantity of rust was formed, and the volume of air was diminished six or eight inches, in fifteen days fifteen inches, in a month thirty-six inches, and lastly, in two months the diminution amounted to about fifty inches; at this period the absorption ceased to proceed, for at the end of seven months, the apparatus continued in the same state, and the absorption had not, in the least degree, increased.

IT

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It appears from these experiments: 1st, That the calcination of metals is not near so easily performed, when they are confined in a portion of air contained in a glass receiver, as in the open air.

2dly. THAT this calcination is also limited; viz. that when a certain portion of metal has been reduced to a calx in a given quantity of air, it is impossible to carry on the calcination farther in the same air:

3dly. THAT in proportion as the calcination proceeds, there is a diminution in the volume of air, and this diminution is nearly answerable to the augmentation of weight in the metal:

4thly. THAT by comparing these facts with those contained in the preceding chapter, it seems proved, that an elastic fluid is combined with, and becomes fixed in metals during their calcination, and that to this fixation their augmentation in weight is to be attributed:

5thly. THAT several circumstances would seem to

lead to a belief, that the whole of the air which we breathe is not adapted to be fixed, and enter into combination with metallic calces; but that there exists in the atmosphere, an elastic fluid of a particular kind which is mixed with the air, and that it is at the instant when the quantity of this fluid contained under the receiver is consumed, that the calcination can no longer take place. The experiments which I shall relate in Chapter IX. will give, at least, some degree of probability to this opinion.

THE experiments of which I have given an account, would appear also to lead to the two following consequences: 1st, That the calcination of metals cannot take place in vessels closely stopped, or, at least, that it can only be in proportion to the quantity of fixable air which is confined in them: 2dly, That in case the calcination could proceed in vessels closely stopped and exhausted of air, it should then be without increase of weight, and consequently with circumstances very different from those observed in calcination performed in air.

THE

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THE train of experiments which Messieurs Darcet and Rouelle have announced in a memoir inserted in the Journal de Medicine for the month of January last, on the calcination of metals in porcelain vessels exactly stopped, will, without doubt, throw great light on this subject. Perhaps this calcination may be no more than a simple privation of Phlogiston in the sense which Stahl meant. However this may be; the learned cannot but expect with much impatience the publication of these experiments, and the reputation which these two chemists have so justly acquired, sufficiently answer for the accuracy to be expected in them.*

CHAP.

* I HAD not the least knowledge of Dr. Priestley's experiments, when I was employed in those related in this chapter. He has observed with me and before me, as has been seen in the first part of this work, that a diminution in the volume of air was produced during the calcination of metals: this diminution in some experiments, was equal to $\frac{1}{2}$, and even to $\frac{1}{4}$ of the bulk of air which he had employed. Although I have made use of a lens, the strongest of any known, I have not been able to carry it to above $\frac{1}{8}$ in

CHAPTER VII.

EXPERIMENTS ON ELASTIC FLUID
DISENGAGED FROM EFFERVESCENT
MIXTURES, AND FROM METALLIC
REDUCTIONS.

HAVING shewn that elastic fluid is separated very plentifully in the reduction of minium, it remains that I should give some experiments on the nature of that fluid, and especially that

in the dry way. This circumstance induced me to suspect that the elastic, fixable, fluid which is diffused in the air is, perhaps, more abundant in it at one time and in one place than another; that it is mixed, in greater proportion, with the atmospheric air in inhabited places; in our laboratories, &c. than in fields, gardens, and in places in general where the air is perpetually renewed. Further, Dr. Priestley is persuaded that the diminution in the quantity of air which he observed, proceeded from a superabundance of Phlogiston which was supplied to it by the calcination

that I should prove its perfect identity with that disengaged by effervescence: but previous to entering on the detail of experiments, which I shall adduce in proof of it, it may be necessary to proceed here to some preliminary descriptions.

AN APPARATUS PROPER FOR OBTAINING THE
ELASTIC FLUID FROM EFFERVESCENT MIX-
TURES AS PURE AS POSSIBLE, WITHOUT MAK-
ING USE OF BLADDERS.

THIS apparatus is represented fig. 13th. ACB is a bottle containing about two pints, tubulated of the metal, and he does not seem to have suspected² that the calcination itself was an absorption, a fixation of elastic fluid.

² AT the time Mr. Lavoisier wrote these observations, he had only seen Dr. Priestley's memoir published in the Philosophical Transactions; these two great philosophers seem to have made the discovery of the cause of the increase in the weight of metallic calces, nearly about the same time. They have both, since their respective publications, been employed in the farther pursuit of these researches, and I hope to be enabled, to add, by way of appendix to this translation, an account of their discoveries. Mr. Lavoisier has already published a memoir on this subject, in a periodical work, of which M. l' Abbé Rozier is the editor, entitled Observations sur la Physique, sur l' Histoire Naturelle et sur les Arts; and I have the pleasure to find that a second volume of Dr. Priestley's Experiments and Observations on Air, containing the most important discoveries on that subject, is in the press, and in great forwardness. T. H.

tubulated at E, the same of which a description has been given above, fig. 4th. As much chalk, in gross powder, is to be put into it as will fill it up to about a third, or at most one half of its capacity, and then the funnel G is to be luted to it in the same manner as in fig. 5th. and 7th.

AGAIN, the bottle O is to be filled with pure water, inverted into an earthen bucket, VVFF, also full of water, and placed on a wooden stand or trivet, perforated in the middle, and weighted with lead to prevent it from floating. The communication between the bottles A and O is to be procured by means of the two bent tubes, EI and TXLM.

SS is a tube which is fitted by friction with great exactness to two other tubes IE and TX. The tube SS has a cock at R, which may be opened or shut at pleasure.

WHEN all the junctures are exactly secured with the fat lute covered with moistened bladders, as much dilute vitriolic acid is to be introduced into the bottle A, by means of the funnel G,

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G, as is sufficient to produce a quantity of elastic fluid, at least, capable of filling the vacant space in the vessels, and of driving off the common air which they contain. This being done, the mouth of the funnel is to be stopped, with the cork P, fig. 5th. and it must be filled with dilute vitriolic acid; after which, by means of the small rod, OP, which is fastened to the cork P, the necessary quantity of vitriolic acid is permitted to enter into the bottle A. At the same time we must not forget to open the cock R.

As fast as the elastic fluid is separated from the chalk in the bottle A, it passes into the bottle O and drives out the water in proportion. It is necessary in some experiments to introduce, into the bottle O, a thin covering of oil, which swimming on the surface of the water, may prevent the elastic fluid from coming into actual contact with it.

The

THE MANNER OF PRESERVING THE ELASTIC
FLUID IN BOTTLES, FOR ANY LENGTH OF
TIME WE PLEASE.

WHEN all the water has been expelled from the bottle O, fig. 13th. by the elastic fluid, and there only remains a thin coat of oil in the neck, the extremity M of the syphon TXLM is to be withdrawn, the bottle corked under water, and afterwards removed wherever is thought proper. The elastic fluid may be preserved a very long time in this state. However, when it is to be kept from one season to another, and obliged to undergo the changes of heat and cold, it is necessary to take some farther precautions; as this air is susceptible of condensation by cold like that of the atmosphere, the external air, when the weather is very cold, presses on the cork, and it is difficult to prevent it, in time, from entering into the bottle and mixing with the elastic fluid contained in it. It is easy to avoid this mixture of the two airs, by plunging the bottles of elastic fluid with their necks downwards either into a dish or jar of water, as may be

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be seen represented fig. 14th. In experiments where we are not apprehensive of the small loss of elastic fluid caused by the absorption of the water, we dispense with the omission of the bed of oil in the neck of the bottle; this precaution may even become injurious if we intend to keep the elastic fluid for a very considerable time; for the oil being liable to ferment and be corrupted, it might produce particular phenomena. It is, then, necessary to leave a very small quantity of water in the neck of the bottle, instead of the coat of oil.

THE METHOD OF REMOVING THE ELASTIC FLUID FROM ONE VESSEL TO ANOTHER.

LET the recipient π NOO, fig. 10th. be supposed to contain a certain quantity of elastic fluid, which there is occasion to transfer into a jar, bottle, or any other vessel: a communication is to be formed by means of the bent tube EBCD and the tube SS furnished with its cock, between the inside of the receiver π NOO and the body of the pump P, as also, by means of

the

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the tube SS and that marked *trim*, between the pump P and the vessel Q, which should be exactly filled with water; lastly, the piston Z of the pump P is to be set in action, and every time the piston is raised, the air of the ambient NOO passes into the body of the pump P; it is then driven on, and obliged to pass into the vessel Q from which it displaces the water in proportion. If the vessel made use of be a bottle, it may be corked under the water, and the elastic fluid preserved, in the manner which has been before described.

A DESCRIPTION OF AN APPARATUS FOR TRANSMITTING AN ELASTIC FLUID THROUGH ANY KIND OF LIQUOR, AND AFTERWARDS PRESERVING IT FOR EXAMINATION.

THIS apparatus represented fig. 15th. differs in nothing from that of the last experiment except in the bottles p', p'', p''', which are placed between the pump PP and the bucket *snff*. These bottles are entirely similar to that of fig. 4. They are to be filled with lime-water, or any

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any other liquor through which the operator chuses to transmit the elastic fluid. A communication is to be made from the pump $P\bar{P}$ to the first bottle, by means of the bent tube $m\bar{p}$ represented separately fig. 16th. Lastly, when by the action of the piston Z , the elastic fluid has passed into the body of the pump P , and it is again filled, it is necessarily obliged to pass into the tube $m'\bar{p}'$, and to bubble over into the liquor contained in the bottle p' ; the pressure of the succeeding streams of air forces it to continue its passage, and to rise over successively into each of the bottles p'', p''' , and so on into as many as may be thought proper, till, in fine, all the air, which could not be absorbed by the liquor, passes into the bottle or jar Q , through the tube $t\bar{x}\bar{l}m$ fig. 17th.

THE different instruments which I have described, changed and modified in various manners, have been sufficient for almost all the experiments which I have been obliged to make on the elastic fluid discharged from bodies. Indeed those relative to Dr. Priestley's nitrous and inflammable airs should be here excepted, which have not as yet been the objects of my examination,

ation, and which require particular precautions. I thought it necessary to begin with these descriptions, that there might be no occasion to return to them in the course of this Chapter, nor any necessity to interrupt the recital of my experiments.

EXPERIMENT I.

THE EFFECT OF ELASTIC FLUID DISCHARGED
FROM CHALK ON ANIMALS.

A QUANTITY of elastic fluid was separated from chalk by the vitriolic acid, and transmitted by means of the apparatus fig. 13th. into the jar Q fig. 15th. represented separately fig. 18th. The jar was corked under water with a large cork exactly fitted to it; after which I turned it up, pulled out the cork and immediately introduced a young sparrow.

IT had scarcely reached the bottom of the jar before it fell on its side in convulsions; and having withdrawn it in a quarter of a minute, it
was

was expiring, and I could not possibly restore it to life by any method.

THE same experiment being repeated on a rat, it perished with the same circumstances, and nearly in the same space of time. Its sides were shrunk, and had a kind of convulsive motion as if it had endeavoured to inspire the air without being able to accomplish it.

EXPERIMENT II.

THE EFFECT OF ELASTIC FLUID, DISENGAGED FROM METALLIC CALCES, ON ANIMALS.

THE same jar Q was filled with elastic fluid disengaged in the reduction of minium, and a sparrow, a mouse and a rat were successively introduced into it. They died almost instantly, as in elastic fluid discharged by effervescence, and their death was attended by similar circumstances.

THESE experiments seem to discover one of the principal causes of the almost immediate death

death of animals in the elastic fluid of effervescent mixtures and metallic reductions. Without knowing precisely what is the use of respiration to animals, we at least know that this function is so essential to their existence, that they must very soon perish if their lungs be not inflated almost every moment by the elastic fluid which composes our atmosphere; but it may easily be conceived that the elastic fluid from effervescence, or that from metallic reductions, is not by any means proper to perform that office of the animal œconomy, and cannot inflate the lungs of animals like the air which we commonly breathe. We have actually seen above, that this fluid is absorbed with great facility by water and most other liquors, that it fixes itself with them and suddenly loses its elasticity: hence it necessarily results that the interior part of the lungs being composed of moist membranes, and even of vessels through which watery vapours continually transude; the elastic fluid cannot arrive there without suddenly losing its elasticity: indeed it is even probable that the elastic fixable fluid does not reach the last ramifications of the lungs, but is fixed before it come there. The action of the lungs

lungs then must be suspended by the deficiency of elastic fluid, they must collapse and become flaccid; and this is, in fact, what is observed in the dissection of animals which have perished in this way. Almost the same effect would be experienced in a pair of bellows, the inside of which was moistened with water, and its action attempted to be supported with elastic fixable fluid.

EXPERIMENT III.

THE EFFECT OF ELASTIC FLUID SEPARATED
FROM EFFERVESCENT MIXTURES ON BURNING
BODIES AND ON FLAME.

A LONG, narrow jar, fig. 19th. was filled with elastic fluid discharged from chalk, and I plunged into it a lighted wax taper or candle, fig. 20th. suspended by means of an iron wire.

SCARCELY was it arrived at the mouth of the jar, but it was instantly extinguished; the burnt part of the wick was even become black. It

A a

sometimes

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sometimes happened that I could light the same candle ten or twelve times, and extinguish it as often in the same jar; so true it is that a considerable time is necessary for the elastic fixable fluid to be mixed with the air of the atmosphere: only it is to be observed that every time the candle is extinguished anew, it is necessary to immerse it lower than the preceding time, which seems to prove that the union of elastic fluid with atmospheric air is only made on the surface, and one stratum after another, nearly in the manner in which solution is performed.

A RED hot piece of charcoal immersed in the same air, became black in it immediately, as if it had been plunged into water.

EXPERIMENT IV.

THE EFFECT OF ELASTIC FLUID DISCHARGED
FROM METALLIC CALCES ON BURNING BO-
DIES AND ON FLAME.

THE same experiment was repeated, making use of elastic fluid discharged from minium instead

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stead of that from chalk; the effects were precisely the same, nor did I perceive the least difference.

EXPERIMENT V.

TO TRANSMIT ELASTIC FLUID, DISENGAGED FROM AN EFFERVESCENT MIXTURE, THROUGH LIME-WATER, AND TO OBSERVE THE QUANTITY ABSORBED BY IT.

I FILLED a bottle, whose capacity was $206\frac{1}{2}$ cubic inches, with elastic fluid separated from chalk by vitriolic acid; it was placed with its mouth downwards in a bucket full of water, VV, fig. 15th. and the whole was disposed in the manner described at the beginning of this Chapter. The jar Q was 69 cubic inches in capacity, it was exactly filled with water, and the three bottles p, p", p", contained $7\frac{1}{2}$ pints of lime-water. When every thing was thus prepared, and all the junctures were exactly closed with fat lute, I opened the cocks R r and worked the piston Z of the pump P.

A a 2

As

As soon as the air came over into the three bottles p , p'' , p''' , and at the first stroke, the first bottle began to assume a cloudy appearance; the same happened to the second towards the end of the second stroke, and to the third on the fourth. I was obliged to make fifteen strokes and a half of the piston to fill the jar Q with elastic fluid.

THE capacity of the pump is $12\frac{1}{2}$ inches, from whence it follows that the quantity of elastic fluid which I had caused to pass into the lime-water was 188 inches, the air which had separated from the lime-water was 69 inches, the quantity then which was combined with the lime was 119 or about two-thirds.

It is proper to observe that this experiment does not give very exactly the portion of elastic fluid, capable of being absorbed by the lime; in fact, a portion of air contained in the vacant part of the bottles p , p'' , p''' , passes into the jar Q , and is replaced by elastic fluid; and consequently the quantity of elastic fluid absorbed appears less than it really is. It is, moreover, probable

probable that $7\frac{1}{2}$ pints of lime-water would not be sufficient to deprive the elastic fluid of all the portion capable of being fixed, and that some small part still penetrates into the jar Q. It is doubtless for these various reasons that the elastic fluid was only reduced $\frac{2}{7}$ in this experiment, whereas Dr. Priestley was able to reduce it $\frac{4}{7}$.

EXPERIMENT VI.

THE EFFECT OF ELASTIC FLUID FROM EFFERVESCENT MIXTURES ON ANIMALS, AFTER IT HAS BEEN DEPRIVED OF ITS FIXABLE PART BY LIME.

WHEN the water in the jar Q, fig. 15th. was wholly displaced by the elastic fluid which had passed through lime-water, I was curious to try what effects it would produce on animals, and I accordingly withdrew the jar from the water, after having corked it, as has been before described, and I introduced a young sparrow into it; it did not appear to suffer very sensibly at first, but in about half a minute its respiration

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tion seemed difficult, it opened its bill, and in about a minute it fell on its side almost motionless. It was left in this state fully half a minute longer, after which it was taken out and exposed to a current of fresh air. For the first moments it had no motion except of its eyes, and, in a small degree also, of its bill, but in less than a minute it came to itself and began to hop and fly.

E X P E R I M E N T VII.

THE EFFECT OF THE SAME FLUID ON FLAME.

A SMALL portion of elastic fluid from chalk which remained in the bottle A, fig. 15th. was passed through the same lime-water, and afterwards collected in a small jar: a small taper, which was immersed in it in the manner represented fig. 19th. and 20th. was extinguished in an instant.

THE lime-water, which had served for these experiments,

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experiments, and was contained in the bottles p' , p'' , p''' , was found to be entirely deprived of its alkaline taste. The lime which was precipitated made a brisk and long continued effervescence with acids, and after all the trials to which it was exposed, I could not find that it differed in any respect from chalk.

EXPERIMENT VIII.

TO MAKE ELASTIC FLUID, SEPARATED FROM
A METALLIC CALX BY REDUCTION, PASS
THROUGH LIME-WATER; TO OBSERVE THE
QUANTITY ABSORBED BY IT, AND THE EF-
FECT OF THE RESIDUUM ON ANIMALS AND
ON FLAME.

INSTEAD of the bottle A, fig. 15th. I made use of a large receiver $\#NOO$, fig. 10th. into which I transferred a mixture of 560 cubic inches of elastic fluid separated from a metallic calx, and 80 cubic inches of common air. It would doubtless have been preferable to have used elastic fluid pure and unmixed, but the ap-

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paratus described above, fig. 10th. did not permit me to obtain it so, because some common air always necessarily remained in the empty space of the retort A, and in the tubulated receiver GH. The large syphon EBCD was adapted in the same manner as in the former experiment to the pump PP, fig. 10th. and 11th. and the elastic fluid was made to pass through four bottles, each of which contained two pints ten ounces of lime-water; the jar Q, the capacity of which was 66 inches, was then disposed to receive the air which could not be absorbed by the lime-water.

AT the first stroke of the piston, the lime-water contained in the first bottle, began to lose its transparency, and grew sensibly turbid on the second.

THE water in the second bottle began to be cloudy at the third stroke of the piston, that in the third on the fourth stroke, and that of the fourth on the sixth.

IT was requisite to pump 135 cubic inches of elastic

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elastic fluid, to displace all the water contained in the receiver Q and to fill it with air; hence it appears that 135 cubic inches had been reduced to 66 inches in passing through the lime-water, and therefore that 69 inches of air had been combined and fixed either with the lime or with the water.

A RAT having been put into this air, continued sufficiently easy at first. It then appeared to suffer, and was violently agitated; at length, in three or four minutes, it fell down in a kind of stupor, and continued without motion as if dead. Having withdrawn it, it began, in a few minutes, to shew some signs of life; it afterwards recovered gradually, and presently became as lively as before the operation,

A WAX candle lighted and plunged into the same air was immediately extinguished.

THE water in the two first bottles p', p'', at the end of this operation, had already formed a very considerable sediment; that in the third and fourth was already very turbid; but one might easily

easily judge that all the lime, which was dissolved there, was not yet precipitated. I therefore endeavoured to transmit again more elastic fluid through the same water into the jar Q; the quantity of air necessary to fill it was found to be 120 inches, and consequently, this second time, no more than 54 inches were absorbed, or precisely $\frac{45}{100}$.

I FILLED the same jar Q a third time in like manner, and the quantity of elastic fluid absorbed by the lime, in this operation, was but 48 inches, viz. $\frac{48}{100}$.

THE same rat, having been put into this air, seemed to feel much greater inconvenience from it; in less than a minute it fell on its side; I withdrew it, but it was dead, and it was no longer possible to restore it to life.

THE same jar was again filled a fourth time in the same manner; 44 inches only were now absorbed, viz. exactly $\frac{44}{100}$ of the quantity employed. A mouse being placed in this air, perished in the third part of a minute.

THE

THE quantity of elastic fluid, necessary to fill the jar Q the first time, was 135 cubic inches; but it must be remembered that this elastic fluid contained $\frac{1}{7}$ of common air; the 135 cubic inches were therefore composed of 115 $\frac{1}{7}$ inches of elastic fluid separated from the calx of lead, and of 19 $\frac{1}{7}$ inches of common air: but, on the other side, common atmospherical air is not capable of uniting suddenly with lime-water, like the elastic fluid arising from effervescence and reductions; therefore the 19 $\frac{1}{7}$ inches of common air, after having bubbled through the lime-water, should pass, without diminution, into the jar Q. It is evident from this calculation, that in reality 135 inches of elastic fluid were not reduced to 66 inches, but 115 $\frac{1}{7}$ were reduced to 46 $\frac{1}{7}$. The lime-water, therefore, absorbed $\frac{1}{7}$ of the quantity of elastic fluid which was employed.

WHEN this calculation is applied to the second, third and fourth times of filling the jar, it will be found that the quantity of elastic fluid used for the second was 103 inches; that it was reduced to 49; from whence it follows that the quantity

quantity absorbed by the lime-water was 54 cubic inches or $\frac{54}{100}$. That for the third the quantity of elastic fluid employed was 98 inches, which was reduced to 50, so that the quantity absorbed was 48 inches or something less than a half; and lastly, that for the fourth, the quantity of elastic fluid expended was 94, and was reduced to 50 inches, viz. the quantity absorbed by the lime was 44 inches or the $\frac{44}{100}$.

ONE remarkable circumstance, which I have taken notice of above, is that the water in the bottles p, p'', p''' , which became very turbid at the beginning of these different operations, and had deposited all the lime which it contained in a state of solution, towards the end of them grew gradually transparent. The reason of this phenomenon depends on the elastic fluid with which the water was impregnated, and by the assistance of which it became capable of dissolving the calcareous earth. Some observations on this solution will be found in the next Chapter.

EXPE.

EXPERIMENT IX.

THE EFFECT OF A VERY GREAT DEGREE OF COLD ON ELASTIC FLUID FROM EFFERVESCENCE.

FIG. 21st. represents an apparatus which I judged necessary for this experiment. A is a bottle filled with elastic fluid separated from chalk by vitriolic acid; the tube EBCD is exactly luted to it with the fat lute covered with a bladder, and it is adapted at its extremity D to the tube SS furnished with its cock R. Every thing being thus prepared, the bottle A was placed in a bucket which I filled with a mixture of broken ice and sea salt.

REFLECTING afterwards on this experiment, I considered that its principal end was to contract the elastic fluid and condense it as much as possible; that, however, as the air in the bottle A had no communication with the external air, my object would not be accomplished. Indeed, whatever

whatever degree of cold I had applied to it, in this apparatus, its volume would always have remained equal to the capacity of the bottle. From these considerations I perceived that it was indispensably necessary, in order to procure any advantage from this experiment, to lute to the other extremity of the tube *SS*, a syphon *TXLM* which might communicate with the inside of the inverted bottle *O* filled with elastic fluid also discharged from chalk. I then opened the cock *R*. It is evident that by means of the communication formed between the bottles *A* and *O*, the elastic fluid could not be condensed by cold in the first, but a portion of that contained in the second must pass to fill up the vacuum; by which means the condensation might be made with all possible freedom.

THE air of the laboratory was at $10\frac{1}{2}$ degrees above the freezing point. When I began this experiment, the degree of cold was at about 15 degrees below freezing. I continued to support it at this temperature for five hours, without the elastic fluid being more diminished than common air would have been. Having at this time removed

moved the ice which surrounded the bottle, I found it covered internally with a white efflorescence, which was nothing but the moisture of the air which was condensed by the cold and had formed a kind of hoar frost.

It was afterwards desirable to try whether the cold had changed the nature of the elastic fluid, and whether it had become more similar to that of the atmosphere, as the Count de Saluces had asserted. (See Part I. page 47.) For this purpose I replaced the bottle A in an earthen bucket VV filled with water, fig. 15th. the elastic fluid was then drawn off by means of the pump PP, and made to pass through three bottles p' , p'' , p''' , filled with lime-water.

At the first stroke of the piston the liquor began to become cloudy, and afterwards became turbid in the same manner as if the elastic fluid had not been exposed to the action of cold. I also tried the effect of this fluid on animals; they perished in it in a few seconds, and it instantly extinguished flame.

IT

It appears from the experiments contained in this Chapter, 1st. That there is a perfect resemblance between the elastic fluid disengaged in the reduction of minium, and that separated from effervescing mixtures; and that they both produce the same phenomena on lime-water, on calcareous earth, on burning bodies and on animals.

2dly. THAT both these fluids are composed, 1st. Of a fixable part capable of being combined with water, &c. 2dly. Of another part, much more difficult to fix; capable of supporting, in a certain degree, the lives of animals, and in its nature much resembling the air of the atmosphere.

3dly. That this portion of common air is rather more considerable in the elastic fluid disengaged in metallic reductions, than in that detached from chalk.

4thly. THAT it seems certain that the noxious property of this fluid resides in its fixable part, because it is less fatal to animals in proportion as it

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it is farther deprived of this part, as is proved by Experiment VIII.

5thly. THAT nothing as yet enables us to decide whether the fixable part of elastic fluid from effervescent mixtures and reductions, be a substance essentially different from air, or whether it be air itself to which something has been added, or from which something has been subtracted, and that prudence demands us to suspend our judgment, at present, on this subject *.

* THIS point has been much elucidated by Dr. Priestley's experiments.

CHAPTER VIII.

OF SOME PROPERTIES OF WATER IMPREGNATED WITH ELASTIC FLUID SEPARATED FROM EFFERVESCENT MIXTURES OR METALLIC REDUCTIONS.

MESSIEURS Cavendish, Priestley, and Rouelle have communicated to the public some very interesting experiments on the solvent properties of water impregnated with fixed air, or, in other words, elastic fluid, separated from effervescent mixtures; they have demonstrated that this water has the property of dissolving calcareous earths, iron, zinc, iron ore, &c. I have had the curiosity to vary these experiments, to extend them, if possible, and I have endeavoured to form a three-fold union of fixed air, metals and acids, with a view of acquiring some ideas of the degree of affinity between these different substances.

To accomplish this design, I first impregnated a sufficient quantity of pure distilled water with elastic fluid separated from an effervescent mixture. For this purpose, I made use of the apparatus, fig. 7th.

SOME of this water was poured into glasses, in which I had previously placed solutions of iron, copper, and zinc, in the vitriolic acid, solutions of iron, copper, lead, and quick-silver, in the nitrous acid, of gold in aqua regia, and of corrosive sublimate: in whatever proportions these mixtures were tried, I was never able to produce a precipitation, and the liquors remained as transparent as they were before; nay, the solution of iron in the vitriolic acid, which was rather cloudy, became immediately clear by the mixture of water impregnated with elastic fluid.

I MIXED some of the same water with a solution of silver in nitrous acid; the liquor had a slightly cloudy appearance, but scarcely perceptible, and the most scrupulous attention was requisite to remark it. This circumstance might cause a suspicion, that chalk contains some particles of marine acid, which being combined in

it with a basis, is driven from it by the vitriolic acid, and passes over with the elastic fluid, and that uniting with the silver, in this experiment it forms a *luna cornea*; but supposing this suspicion to be well founded, that quantity of marine acid must be so inconsiderable, that one grain of spirit of salt diffused in two pints of water would produce a much greater effect.

THOUGH these experiments may not be quite complete, because I have not been able to extend them to all the metallic solutions; they should seem, however, to prove in general, that metallic substances have more affinity with the mineral acids, than with the elastic fixable fluid.

MR. HEY, some of whose experiments have been published by Dr. Priestley, has declared that fixed air did not at all change the blue colour of syrup of violets, and as this experiment has been since contested, I was curious to repeat it: some syrup of violets was accordingly diffused in water impregnated with elastic fluid, and its colour compared with that of the same syrup of violets diffused in distilled water. The colour was not sensibly changed; however regarding it

with the most nice attention, the syrup of violets mixed with water impregnated with fixed air, seemed to have something of a redder tinge; but the difference was so trifling, so imperceptible, that one might almost doubt it *.

AN experiment may be recollected which I have related in the first Chapter of the second part of this work. If water impregnated with elastic fluid be poured by degrees into saturated lime-water, the liquor presently becomes turbid, and the lime precipitates in the state of chalk; but if, after all the chalk has been precipitated, the addition of more water impregnated with fixed air be continued, all the chalk which had been precipitated will be gradually redissolved, and the liquor recover its former transparency.

IT has been also seen in the last Chapter, that
when

* DR. PRIESTLEY's very ingenious friend Mr. Bewley, of Great Massingham in Norfolk, seems to have determined this point. By means of fixed air, he has not only turned some of the blue juices, which are more delicate tests of acidity than syrup of violets, red, but has even neutralized alkaline salt. T. H.

when elastic fluid, separated either from effervescence, or metallic reduction, has been transmitted through lime-water, and all the alkaline earth has been thereby precipitated under the form of chalk, if we continue to throw in fresh streams of elastic fluid, the greater part of the precipitated earth is again dissolved, and the liquor recovers its transparency. The elastic fluid or fixed air being so common in the mineral kingdom, as may be judged from the mephitic or aerial waters and by many other phenomena in nature, the combination of this substance with calcareous earths should be frequently met with in waters; I therefore thought that it would be interesting to examine the effects which different kinds of tests would produce on this combination which is hitherto but little known.

To this purpose some distilled water was saturated with lime, and I threw a stream of air into it proceeding from a reduction of calx of lead; at first, as has been before remarked, the lime was precipitated; it was then redissolved, and I continued the operation in this manner till I imagined the water was as much loaded as it could be, with calcareous earth.

I Poured

WITH ELASTIC FLUID. 375

I POURED this water on solutions of iron and copper in the nitrous acid, the liquors neither became turbid nor formed any precipitation. The solution of silver in the same acid occasioned a slight, but scarcely perceptible cloud in the liquor, nearly such as I have remarked with the water impregnated with elastic fluid only.

THE case was different with solutions of copper, iron, and zinc in the vitriolic acid. The precipitation, it is true, did not instantly take place, but in a few seconds the liquor became turbid, and in a short time the precipitate collected and subsided to the bottom of the vessel.

A SOLUTION of lead in the nitrous acid afforded, immediately, a very plentiful white precipitation.

A SOLUTION of quicksilver in nitrous acid afforded a precipitate only by employing much water and little of the solution; this precipitate was of a pale yellow colour, but it gradually became grey with time.

A SOLUTION of gold in aqua regia gave no signs of precipitation,

I ALSO tried on this water the effects of fixed and volatile alkalis both in their caustic and mild state; they all occasioned a precipitation of the alkaline earth in the form of chalk; that is to say, they attracted to themselves the superabundant portion of elastic fluid which held it dissolved, but they could not deprive it of any more; we have seen, in fact, that elastic fluid has more affinity with alkaline earth than with alkaline salts.

THE same water being poured on syrup of violets, did not much affect its colour; however, a slight green tinge might be observed, which became more visible after some hours.

ALL these experiments have the same success, whether we employ elastic fluid separated from effervescent mixtures, or that from metallic reductions.

CHAPTER IX.

ON THE BURNING OF PHOSPHORUS, AND THE FORMATION OF ITS ACID.

EXPERIMENT I.

THE BURNING OF PHOSPHORUS UNDER A RECEIVER INVERTED IN WATER.

EIGHT grains of Kunkel's phosphorus were placed on a little agate cup which was put under a glass receiver inverted in water, and a thin covering of oil was introduced to the surface of the water by means of a crooked funnel: this apparatus is the same as that represented fig. 8. I then threw upon the phosphorus the focus of a glass lens of eight inches diameter.

THE phosphorus was soon fused, and then kindled,

kindled, yielding a beautiful flame; at the same time a great quantity of white vapours arose, which settled on the internal surface of the receiver and tarnished it. These vapours afterwards ran in *deliquium*, and formed drops of clear limpid liquor. At first, the water in the receiver rather fell, from the rarefaction occasioned by the heat; but it presently began to reascend sensibly, even during the burning, and when the vessels were grown cold it settled at one inch five lines above its first level.

THE internal diameter of this receiver was $4\frac{3}{5}$ inches; and consequently the absorption of air had been $19\frac{2}{3}$ inches. Having taken the cup from under the receiver, a yellow matter was found at the bottom which was nothing but the phosphorus half decomposed; I washed and dried it, after which it weighed between one and two grains, and therefore there had been only, in reality, between six and seven grains of phosphorus burnt, and the absorption of air had been about three inches for every grain of phosphorus.

THE part of the receiver above the water contained about 109 cubic inches. The absorption, then, of air was $\frac{2}{7}$, or, what is the same thing, between a fifth and sixth of the whole quantity of air contained under the receiver.

EXPERIMENT II.

THE BURNING OF PHOSPHORUS IN A RECEIVER INVERTED IN QUICKSILVER.

THIS experiment was repeated with the same receiver as in the last; I again employed eight grains of phosphorus; and all the circumstances were absolutely the same, with this difference only, that instead of inverting the receiver into a vessel of water with a covering of oil, it was inverted into a vessel full of quicksilver.

THE burning succeeded nearly as in the last experiment, with this difference, that the vapours which adhered to the receiver were more light and flocculent, much whiter, and did not run

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per deliquium. Independent of those attached to the receiver, the little cup was covered with them. The absorption of air was $16\frac{1}{2}$ cubic inches, viz. rather less than three inches for every grain of phosphorus. There also remained in the cup a small yellow residuum of half decomposed phosphorus.

EXPERIMENT III.

THE BURNING OF PHOSPHORUS OVER QUICK-SILVER, IN SMALLER QUANTITY THAN IN THE FORMER EXPERIMENTS.

I TRIED to burn a smaller quantity than eight grains of phosphorus under the same receiver and over quicksilver. The quantity of air absorbed was diminished in proportion to the diminution of the quantity of phosphorus, and it was constantly between $2\frac{1}{4}$ and $2\frac{1}{2}$ inches for each grain, deducting for the small portion of yellow residuum which remained after each burning.

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EXPERIMENT IV.

TO DETERMINE THE GREATEST QUANTITY OF PHOSPHORUS WHICH CAN BE BURNT IN A GIVEN QUANTITY OF AIR, AND WHAT ARE THE LIMITS OF THE ABSORPTION.

TWENTY-FOUR grains of phosphorus were placed on the agate cup in the same apparatus immersed in mercury.

THE phosphorus burned in the same manner at first as if the quantity had been but six or eight grains, excepting only that the inflammation was more rapid, more instantaneous, and that the rarefaction was greater; but presently, though a considerable quantity of phosphorus still remained unburnt, the combustion ceased, and I could not possibly renew it by the aid of the burning glass. I easily melted the phosphorus, made it bubble, and even sublimed it, but it no longer flamed. The quantity of air absorbed in this experiment was found to be about seventeen

seventeen or eighteen inches, and on comparing the remainder of the phosphorus with that which I had made use of, the quantity which was burnt was found to be no more than six or seven grains.

THESE experiments were very frequently repeated, and the results were always the same, except some difference in the quantities of air absorbed; it was never possible for me to carry this absorption farther than twenty or twenty-one inches, or nearly approaching to, but not entirely, $\frac{1}{3}$ of the whole volume, in a receiver whose capacity was 109 inches. Frequently, when the vessels had been suffered to cool for several hours, I endeavoured to restore the air under the receiver by lifting it up. As soon as the phosphorus came into contact with the fresh air, it immediately kindled again, and when I had covered it with another receiver of about the same size, six or eight grains more were burnt. The phosphorus was then extinguished without the possibility of being lighted again, by any other method than supplying it with fresh air.

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THESE experiments seem already to lead to a suspicion that atmospheric air, or some other elastic fluid contained in the air, is combined, during the combustion, with the vapours of the phosphorus. But there is a great difference between conjecture and proof, and it was essentially requisite that it should first be well established that a combination of any kind of substance was formed with phosphorus during its combustion. The following experiments appeared to me to be proper for furnishing that proof.

EXPERIMENT V.

TO DETERMINE, WITH AS MUCH PRECISION AS THE NATURE OF THE EXPERIMENT WILL ADMIT OF, THE AUGMENTATION IN WEIGHT OF THE ACID VAPOURS OF BURNING PHOSPHORUS.

EIGHT grains of phosphorus were put into a small glass cup, B, fig. 22. which was introduced into a wide mouthed bottle, P. The bottle was very exactly stopped with a cork, and

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and I weighed the whole to the exactness of half a grain. The bottle was then uncorked, and immediately placed under the crystal receiver ACG, which had been before made use of. The quicksilver was raised up to CG, and the phosphorus kindled by the burning glass.

THE phosphoric acid was sublimed in white flocculi, which were mostly attached to the interior sides of the bottle P, and to the cup B; one-fourth, at least, was separated on the outside of the bottle, and was deposited partly on the surface of the quicksilver, partly on the inner sides of the receiver, and on the exterior surface of the bottle.

WHEN the vessels were grown cold, the absorption was found to be from sixteen to seventeen cubic inches, and a small portion of yellow matter remained unburnt. I then lifted up the receiver A with proper precaution, and in less than four seconds, I recorked the bottle. It may easily be supposed that in so short a time the air contained in the bottle P could not have been renewed and replaced by the moist air, or
at

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at least that if such an effect could have taken place, it must have been only in a quantity nearly insensible.

THE outside of the bottle P having been very exactly wiped and cleaned, I put it into the scales, and found it to have received an increase of weight of six grains; viz. that instead of eight grains of phosphorus, which I had put into the bottle, there were now fourteen grains, either of concrete phosphoric acid or of phosphorus half decomposed: but it must be remembered that at least a fourth or three or four grains, was separated during the burning, on the outside of the bottle; and consequently that six or seven grains of phosphorus yield seventeen or eighteen grains of concrete phosphoric acid, or in other words, that six or seven grains of phosphorus absorb ten or twelve grains of some substance contained in the air which is confined under the receiver. This experiment leaves scope enough to preclude any reasonable doubt of the result, and all the arguments which could be adduced would, at most, tend only to reduce the increase

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of weight, from ten or twelve, to eight or ten grains.

THE quantity of air absorbed, was, at most, seventeen cubic inches combined with the phosphorus to form the phosphoric acid; this quantity communicated an increase in weight of from ten to twelve grains, and therefore every cubic inch of elastic fluid which was absorbed weighed about $\frac{1}{4}$ of a grain, i. e. nearly one-fourth more than the air which we breathe.

BUT if the matter attracted by the phosphorus, during its combustion, be the heavier part of the air, why may it not be water itself which that fluid holds dissolved, and is diffused so abundantly in the atmosphere in a kind of state of expansion? Without doubt, I reasoned with myself, water is necessary to the aliment of flame; in proportion as air contains it, it is proper to support combustion; but when deprived of it, combustion can no longer take place.

THIS sentiment was probable, and carried an air

air of truth adapted to seduce. I therefore resolved to submit it to the test of experiment, and the following was my method of reasoning. If this theory of the absorption of water be true, three things should result from it, 1st. That by restoring to the air confined under the receiver in which the phosphorus has been burnt, a quantity of water reduced to vapours proportionable to that which has been absorbed, the combustion, instead of ceasing, should be prolonged much farther. 2dly. That in this case there should be no farther diminution in the bulk of air in proportion as the phosphorus burns. 3dly. That by restoring to a quantity of air, in which phosphorus has been burnt, and consequently deprived of its water, and diminished about a fifth, a proportion of water reduced to vapours, an increase should be produced in its bulk equal to the diminution which it had suffered during the combustion. These reflections led me to the following experiments.

EXPERIMENT VI.

TO BURN PHOSPHORUS UNDER A RECEIVER
IMMERSED IN QUICKSILVER, MAINTAINING
AN ATMOSPHERE OF WATER REDUCED TO
VAPOURS UNDER THE SAME RECEIVER.

I PUT a sufficient quantity of mercury in a small earthen dish, and, on its surface, placed two small agate cups, in one of which were contained eight grains of phosphorus, and in the other about a drachm of water; these were covered with a crystal receiver, and the mercury was raised in it to a proper height.

THE focus of the burning lens was first thrown on the cup which contained the water: in a few minutes it became hot, and presently boiled and rose in vapours which condensed in drops and trickled down the sides of the receiver. When I was perfectly assured that a plentiful atmosphere of watery vapours existed under the receiver, I ceased to make the water boil, and
threw

threw the focus of the same lens on the phosphorus.

THE combustion was performed as usual; the same quantity of air was absorbed, nor did the experiment differ from all those made over quicksilver, except that the acid, instead of being in white flowers and in a concrete form, was deposited in drops on the sides of the receiver, in proportion to the quantity of water with which it had been supplied.

EXPERIMENT VII.

TO RESTORE MOISTURE TO AIR IN WHICH
PHOSPHORUS HAS BURNT.

THE same experiment was repeated, observing to burn the phosphorus first, and afterwards to make the water boil by means of the burning glass.

THE acid vapours were deposited on the sides
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of the receiver in white flowers, but less beautiful than in the fifth experiment; and, in some minutes, they deliquesced, occasioned by the humidity which the water, though cold, had furnished under the receiver. When the vessels were cooled, the absorption of air was found to be nearly equal to that of the preceding experiments. I then threw the focus of the lens on the water contained in the cup, and made it boil. The vapour was immediately diffused in the area of the receiver, and collected in drops along its sides; but the height of the quicksilver was neither increased nor diminished, so that the volume of air remained exactly the same.

EXPERIMENT VIII.

TO TRY WHETHER A GREATER QUANTITY OF PHOSPHORUS CAN BE BURNT IN A GIVEN QUANTITY OF AIR, BY THE AID OF WATER REDUCED TO VAPOURS.

I EMPLOYED, in this experiment, the same two agate cups as in the preceding ones; into one

one was put a little distilled water, and into the other eighteen grains of phosphorus. The water was made to boil by means of the burning glass, and I then kindled the phosphorus.

No more than seven or eight grains were burnt, after which the combustion ceased, and it was not possible to reanimate it by the help of the burning glass. The greater part of the unburnt phosphorus remained in the cup; some portions were sublimed on the inner sides of the receiver. The absorption of air was $18\frac{1}{2}$ inches, i. e. very nearly the same as in the other experiments.

IT appears evident, from these experiments, that the diminution in the volume of air observed during the burning of phosphorus, is not owing to the absorption of water which was contained in it; that the greater or smaller quantity of water introduced under the receiver, and combined with the enclosed air, makes no alteration in the phenomena; and that the whole difference which results from it is to have the acid either concrete or fluid. Not that I would deny that

the phosphoric acid, when forming, may attract from the air a portion of the moisture with which it is loaded; it is even very probable that this really happens; and it is, doubtless, on account of this moisture that the augmentation in weight, observed in Experiment V. was found to be rather greater than it ought to have been from the quantity of air absorbed; but it does not appear to be the less proved by every thing which has preceded, 1st. That the greater quantity of the substance absorbed by phosphorus during its burning, is something else than water. 2dly. That it is to the addition of this substance that the phosphoric acid owes its increase in weight. 3dly. That to the subtraction of it, the diminution in the bulk of air in which phosphorus has burned is to be attributed. A concluding experiment which I would introduce by some preliminary reflections, will I hope carry these truths to demonstration.

I suppose a bottle or some other vessel with a narrow neck to be exactly filled with distilled water, in such a manner that it would not be possible to add a single drop more without its running

running over the brim. If afterwards some phosphoric acid be introduced into this bottle, or any other acid in a state of absolute concentration, that is, absolutely deprived of water, it is clear that one of these two things must happen; either that the acid must be lodged between the particles of water and be combined with it without increasing its bulk; or rather, which is more probable, that in mixing with the water, it will separate the parts, and a greater volume than that of the water will result from the mixture; there would then be a quantity of fluid greater than what the bottle could contain, and this excess would trickle over its brim.

SUPPOSING the quantity of acid introduced to be unknown, it will not be difficult to determine it in the first case; it is only necessary to weigh the bottle, and the increase of weight which it has acquired will be equal to the weight of the acid which has been added.

THE circumstances will differ in the second case; for to have the quantity of acid which has been introduced into the bottle, we ought to add

add to the increase of weight which it has acquired, the weight of the fluid which may have run over its brim: but it will always be certain, and we may regard it as demonstrated that in both cases the quantity of acid added, if it be not greater, is at least equal to the augmentation in weight which the bottle has acquired. These reflections will naturally apply to the following experiment.

EXPERIMENT IX.

AN EXAMINATION OF THE RELATIVE WEIGHT
OF PHOSPHORIC ACID WITH DISTILLED WA-
TER, AND THE CONSEQUENCES DEDUCIBLE
FROM THENCE.

IN the middle of a large glazed earthen dish, I placed a small agate saucer, and covered the whole with a great glass receiver, but in such a manner that the margin of the dish should extend farther than that of the receiver. Both vessels had been previously moistened with some distilled

distilled water. The apparatus having been thus disposed, I put in the agate saucer two or three grains of phosphorus, and inflamed it by means of the blade of a knife moderately heated, which I passed under the receiver and touched the phosphorus with it. As soon as the inflammation began, a very thick column of white vapours arose from the phosphorus, and was diffused in the receiver; but it was remarkable that though the receiver was merely placed on the dish, and did not even touch exactly in all its points, the vapour which circulated within it, instead of being driven out by the rarefaction occasioned by the heat, seemed on the contrary to be pushed inwards by the currents of external air which were introduced under the receiver. This circumstance however did not prevent a small quantity of vapours from escaping at some other moments.

An hour was necessary to condense the whole of the vapours contained under the receiver; after which I recommenced the same operation, only taking care to moisten the receiver again either with distilled water, or the same water which

which had been already employed, and which became more and more acid.

IT is proper to observe that at the end of each combustion, there always remained, in the bottom of the saucer, some portions of yellow matter, of which I have before spoken, and which consist of half decomposed phosphorus; I took particular care to lay this aside. I continued to burn phosphorus in this manner to the amount of two drachms forty-two grains; when, having washed and dried the yellow matter which remained, its weight was thirty-two grains; the quantity of phosphorus therefore which had been burned, was in reality no more than two drachms ten grains.

THE liquor resulting from this operation was clear and limpid, without colour or smell, and had an acid taste like that of oil of vitriol diluted with a large quantity of water. It was evident that this liquor was only distilled water, into which a certain quantity of phosphoric acid had been introduced; and I was able to apply to it the reflections which preceded this experiment.

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I ACCORDINGLY took a phial capable of containing all the phosphoric acid which I had obtained, and having put the acid into it, as there still remained a small void space between the liquor and the neck of the bottle, I filled it up with distilled water, and tied a thread exactly level with the surface of the liquor. The bottle having been put into the scales, the weight of acid, exclusive of the bottle, was 6 ounces, 7 drachms, $69\frac{1}{2}$ grains.

THE bottle was afterwards emptied, and very carefully cleaned and then filled with distilled water up to the same mark. The weight of this water, allowance being made for that of the bottle, was found to be 6 ounces, 4 drachms, 42 grains, which makes the excess of weight in the acid above that of the distilled water, 3 drachms, $27\frac{1}{2}$ grains.

IT is clear from what has been said above, that an excess in weight of 3 drachms, $27\frac{1}{2}$ grains, shews at least that there existed that portion of acid in the liquor, even upon the most unfavourable suppositions; yet the quantity of phosphorus employed

employed was only 2 drachms, 10 grains; from whence it evidently follows that the phosphorus had attracted, during its combustion, at least 2 drachms, 17 grains of some kind of substance. This substance could not be water, because water could not have augmented the specific gravity of matter; it was therefore either air itself, or some other elastic fluid contained, in a certain proportion, in the air which we respire. This last experiment appears so demonstrative to me, that I do not foresee any objection that can be advanced against it.

CHAPTER X.

EXPERIMENTS ON COMBUSTION AND DETTONNATION IN VACUO.

If the combustion of phosphorus consist essentially, as the preceding experiments should seem to prove, in the absorption of air, or some other

other elastic fluid contained in the air, it should therefore result that the burning of phosphorus cannot be performed without air, and consequently cannot take place in the vacuum of an air pump, and I was curious to procure myself this addition of proof.

EXPERIMENT I.

TO TRY TO BURN PHOSPHORUS IN VACUO.

A LITTLE piece of phosphorus was placed under the receiver of an air pump, and as perfect a vacuum was made as the machine would admit of. I then threw the focus of a lens of 8 inches diameter on the phosphorus; it immediately fused, bubbled and acquired a colour rather of a deeper yellow than before; at length it sublimed, but it did not burn at all. Having admitted air into the receiver, and having tasted the watery vapours which adhered internally to its sides, I did not even find them sensibly acid; from whence it is plain there had been no combustion.

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EXPERIMENT II.

SULPHUR IN VACUO.

SULPHUR exposed in the vacuum of an air pump to the heat of a burning glass, sublimed like the phosphorus, nor was it possible to set it on fire.

EXPERIMENT III.

GUN-POWDER IN VACUO.

SOME gun-powder was placed under the receiver of an air pump, and as perfect a vacuum made as could possibly be effected. The focus of a burning glass being afterwards thrown up on the powder, it fused, the sulphur was sublimed to the top of the receiver, but it neither took fire nor exploded. I made use also in this experiment of a lens of eight inches diameter.

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HAVING introduced a small quantity of air under the receiver, about a twentieth part of what it was capable of containing, the explosion was easily made, with a noise nearly similar to that of the bursting of a thin bladder. The noise was proportionably less as the receiver was larger.

EXPERIMENT IV.

NITRE AND SULPHUR IN VACUO.

EQUAL parts of nitre and sulphur do not produce any kind of explosion in vacuo. The sulphur sublimes without burning in the same manner as if it were unmixed.

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CHAPTER XI.

OF AIR IN WHICH PHOSPHORUS HAS BEEN BURNT.

EXPERIMENT I.

THE EFFECT OF AIR, IN WHICH PHOSPHORUS HAS BEEN BURNT, ON ANIMALS.

A QUANTITY of air which had been diminished in its bulk by the burning of phosphorus, was transmitted into a jar by means of the pump PP, and by an apparatus nearly similar to that of fig. 10. I threw a bird into it and left it there a full half minute. I did not perceive that its respiration was more difficult than in common air, nor did it shew any signs of being injured. We may recollect on the contrary that an animal, of the same species, being put into fixed air, perished almost on the first inspiration.

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EXPERIMENT II.

THE EFFECT OF AIR, IN WHICH PHOSPHORUS HAS BEEN BURNT, ON LIGHTED CANDLES.

ANOTHER portion of the same air was thrown into a narrow jar, and a lighted wax candle plunged into it. It was instantly extinguished, as in elastic fluid from effervescences and reductions. The candle, having been several times lighted anew, it was constantly extinguished; I observed, however, that this experiment could not be so frequently repeated in this air, as in that obtained from effervescent mixtures and reductions; which induces me to believe that it is more easily and readily mixed with the air of the atmosphere *.

* THIS air was not pure fixed air, but common air in a state of decomposition, and from which a share of its fixable part had been absorbed by the phosphorus. T. H.

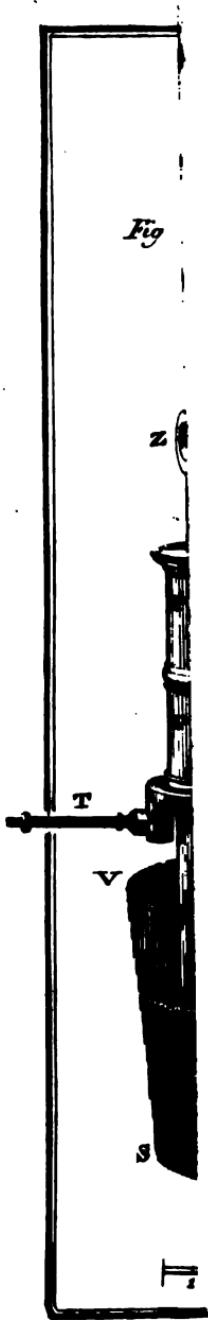
EXPERIMENT III.

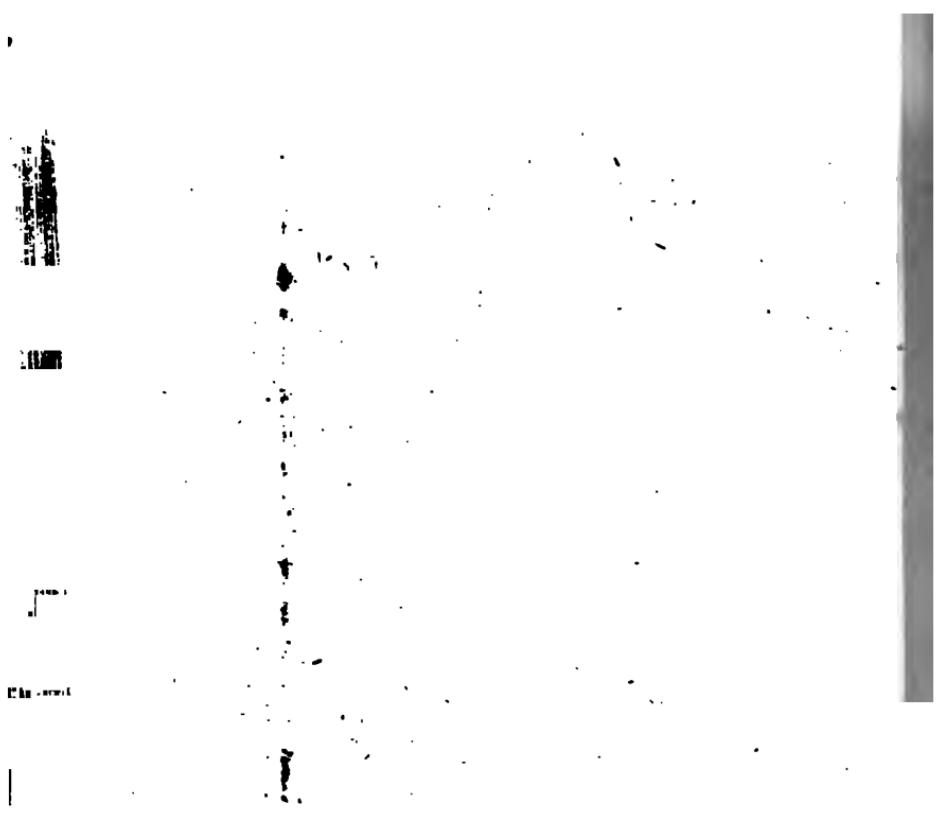
TO MIX A PORTION OF ELASTIC FLUID FROM AN EFFERVESCENT MIXTURE, WITH AIR IN WHICH PHOSPHORUS HAS BEEN BURNT.

I HAD the curiosity, relative to some views of which I shall give an account another time, to observe whether the mixture of one-third of elastic fluid from an effervescent mixture, would correct the air which had been employed for the burning of phosphorus, and restore to it the property of supporting flame. A narrow jar was filled with a mixture of them, and a candle introduced into it, but it was immediately extinguished.



Fig





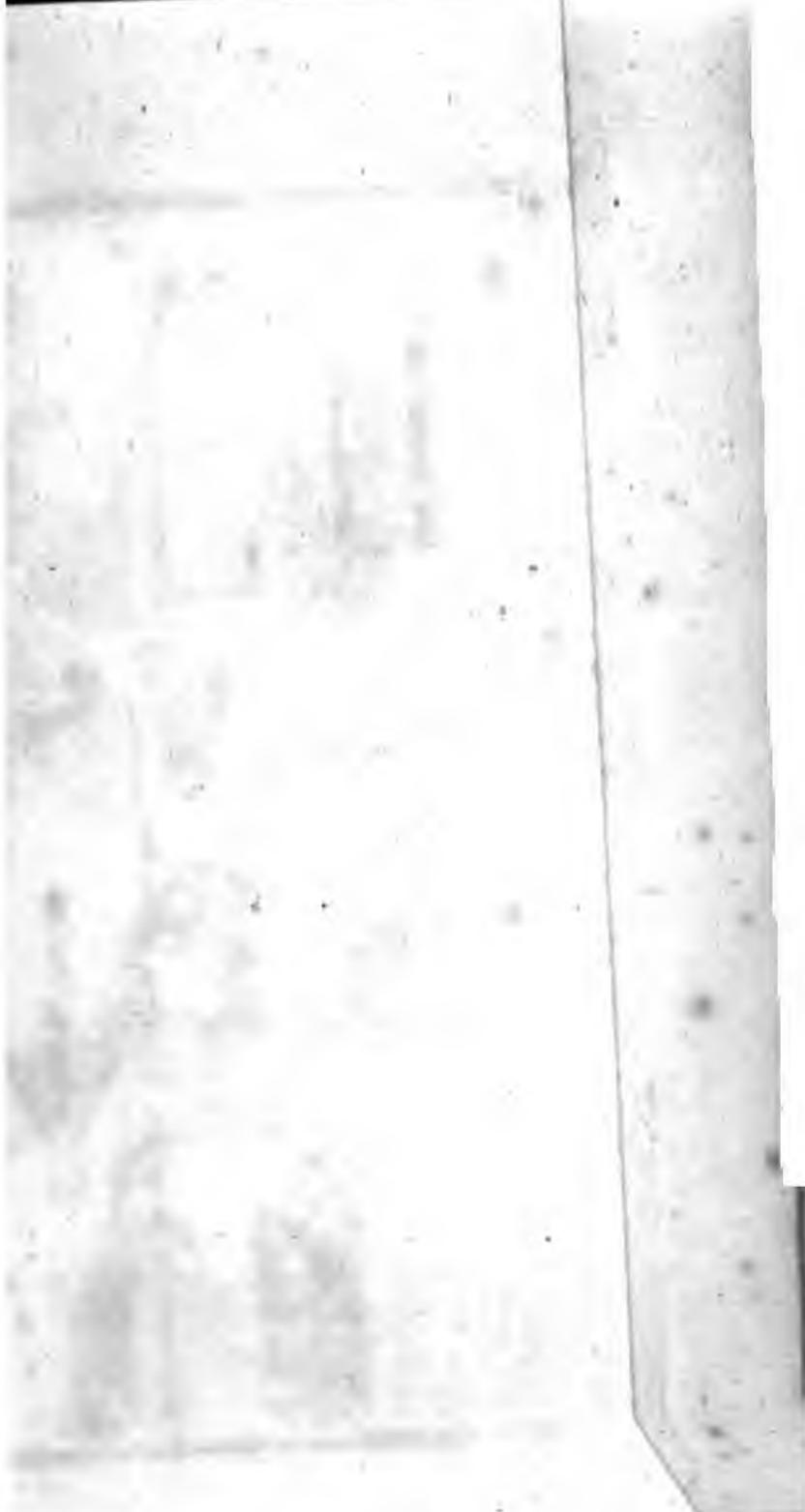
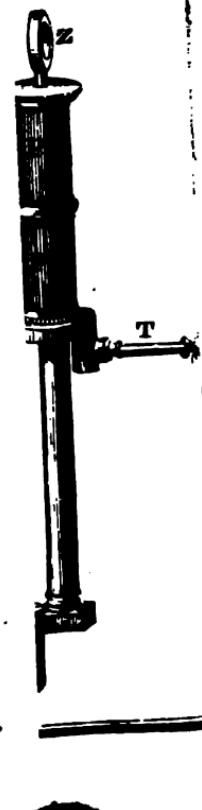




Fig. 11.





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P P E N D I X.

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NUMBER I.

A MEMOIR

ON THE NATURE OF THE PRINCIPLE
WHICH IS COMBINED WITH METALS DURING
THEIR CALCINATION, AND OCCASIONS AN
INCREASE IN THEIR WEIGHT *.

Read before the ROYAL ACADEMY, April 26th, by
M. LAVOISIER.

D O there exist different species of air?
Is it sufficient that a body be in a state
of

* THE first experiments relative to this memoir have been made above a year ago; those on the *mercurius precipitatus per se*, were first tried with a burning glass in the month of November 1774, and afterwards made with all the necessary precautions and care, in the laboratory of *Montigny*, in conjunction with M. de Trudaine on the 28th. of February, and the 1st. and 2d. of March of this year; and were finally repeated again the 31st. of last March, in presence of M. the Duke de la Rochefoucault, M. de Trudaine, M. de Montigny, M. Macquer, and M. Cadet.

of permanent expansibility * to constitute a species of air? And lastly, are the different kinds of air which nature affords us, or which we are able to form, substances distinct of themselves, or modifications of atmospheric air? Such are the principal questions which appertain to the plan which I proposed to myself to lay before the Academy: but the time devoted to our public meetings not permitting me to treat any question in its full extent, I shall confine myself to day to one particular case only, and limit myself to shew that the principle which is united to metals during their calcination, which increases their weight, and which constitutes them in the state of a calx, is neither one of the constituent parts of the air, nor a particular acid diffused in the atmosphere; that it is the air itself undivided, without alteration, without decomposition, to such a degree, that if after having been engaged in

* THIS word is at present generally received among philosophers and chemists, since a modern author has established the meaning of it, in a very elaborate article replete with views the most extensive and new, and which bear in every part the mark of genius. Vide Encyclopédie, Tome VI. p. 274, at the word *expansibilité*.

in this combination, it be set at liberty, it is separated more pure, more respirable, if I may be permitted to use the expression, than the air of the atmosphere, and is more proper to support flame and the combustion of bodies.

Most of the metallic calces are not reduced, that is to say, do not return to the state of a metal, without the immediate contact of charcoal, or some other substance which contains what is called phlogiston; the charcoal which is employed is wholly destroyed in this operation, when the quantity is well proportioned, and consequently the air, which is separated in metallic reductions effected by charcoal, is not a simple being, but is, in some degree, a combination of the elastic fluid separated from the metal, and of that discharged from the charcoal. Therefore because this fluid is obtained in the state of fixed air, we have no right to conclude that it existed in that state, in the metallic calx, previous to its combination with the charcoal.

THESE reflections made me sensible how essential it was to the unfolding of the mystery of the

the reduction of metallic calces, to direct all my experiments to those which are reducible without addition. The calx of iron afforded me this property; indeed, of all those, whether natural or artificial, which we have exposed to the focus of the great burning glass belonging to M. le Regent, or to that of M. de Trudaine, there was not one but what was thereby totally reduced.

- In consequence of this, I tried to reduce, by the assistance of a burning glass, several kinds of calces of iron, under large glass receivers inverted into mercury, and I became able to separate from them, by this means, a great quantity of air; but as at the same time this air was found to be mixed with the common air contained within the receiver, this circumstance threw a great uncertainty on my results; no proofs to which I submitted this air were perfectly conclusive, and it was impossible for me to be assured, whether the phenomena which I obtained depended on the common air, on that separated from the calx of iron, or on the combination of the two together. My design not having

having been completed by these experiments, I suppress the detail of them here; they will, besides, find their proper place in other memoirs.

As these difficulties depended on the very nature of iron, on the refractory quality of its calces, on the difficulty of reducing them without addition, I considered them as insurmountable, and I believed, from this time, that I ought to turn my attention to another kind of calx which would be more easily managed, and which had, like the calx of iron, the property of being reduced without addition. The *mercurius precipitatus per se*, which is nothing more than a calx of mercury, as some authors have already advanced, and as we shall still be more fully convinced by reading this memoir; the *mercurius precipitatus per se*, I say, appeared to me proper to fulfil completely the object which I had in view. Indeed it is at present known that it is reducible, without addition, in a very moderate degree of heat. Although I have frequently repeated the experiments which I am going to relate, I have not thought it proper to give, at this time, a detail of each of them in particular,

particular, for fear of swelling this memoir too much, and I have thrown together, in one recital, the circumstances which belong to several repetitions of the same experiment.

To assure myself at first, whether the mercurius precipitatus per se were a true metallic calx, whether it yielded the same results, the same species of air by reduction, I, in the first place, tried to reduce it by the common method, or, to use the received expression, by an addition of phlogiston.

I, ACCORDINGLY, mixed an ounce of this calx with forty-eight grains of powdered charcoal, and introduced the whole into a small glass retort, the utmost capacity of which did not exceed two cubic inches, and I placed it in a reverberatory furnace proportioned to its size. The neck of this retort was about a foot in length, and three or four lines in diameter; it had been bent in different places, in an enameller's lamp, and its extremity was so disposed, that it could be brought under a glass receiver, of sufficient dimensions, filled with water, and inverted

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inverted in a cistern filled with the same. This apparatus, simple as it is, is the more exact as it has neither folder nor lute, nor any passage through which air might be introduced or escape.

As soon as the fire was placed under the retort; and it received the first impressions of heat, the common air which it contained was dilated; and some little of it passed into the receiver; but considering the smallness of the void part of the retort, that air could not make any sensible error; and its quantity, reckoning it at the highest, could scarcely amount to a cubic inch. As soon as the retort began to be much heated, the air was separated with much rapidity, and mounted, through the water, into the receiver. The operation did not last above three quarters of an hour, and yet the fire had been kept low during that time. When the whole mercurial calx was reduced, and the air no longer continued to pass, the height was marked at which the water stood in the receiver, and the quantity of air separated was found to be sixty-four cubic inches, without reckoning the portion which must necessarily have

have been absorbed by the water in passing through it.

THIS air was immediately submitted to a great number of trials, the detail of which I am obliged to suppress, and the results were, 1st. that it was susceptible of being combined with water by agitation, and of communicating to it all the properties of the acidulous or aerial waters, such as those of Seltzer, Pouges, Bussang and Pyrmont, &c. 2dly. that animals plunged into it perished in a few seconds; 3dly. that candles, and all combustible bodies in general, were extinguished in it instantly; 4thly. that it precipitated lime-water; 5thly. that it very readily combined with alkalis, whether fixed or volatile; that it deprived them of their causticity, and gave them the property of crystallizing. All these qualities are precisely those of that species of air, known by the name of fixed or mephitic air, such as is obtained from all metallic calces by the addition of charcoal; such as is separated from effervescing and fermenting bodies, and it was therefore confirmed, that *mercurius precipitatus*

precipitatus per se belonged to the class of metallic calces.

THE only remaining inquiry was to examine this calx alone, to reduce it without addition, to see whether it yielded any air by that method; and supposing that it did yield it, to determine in what estate that air might be. For this purpose, in a retort, also, of two cubic inches in dimension, an ounce of *mercurius precipitatus per se* was placed, alone, the apparatus was disposed in the same manner as in the preceding experiment, and I so contrived that all the circumstances should be exactly similar. The reduction was accomplished this time with rather more difficulty than by the addition of charcoal; it required more heat, and the effect was not sensible till the retort began to be slightly red hot; the air was then separated gradually, and passed into the receiver, and, by keeping up the same degree of heat for two hours and half, the whole of the mercury was reduced.

THE operation being finished there were found, on one part, in the neck of the retort,
and

and in a glass vessel which was placed beneath the water under its beak, seven ounces eighteen grains of mercury, on the other hand, the quantity of air, which had passed into the receiver was found to be seventy-eight cubic inches; and therefore, supposing that the whole loss of weight ought to be attributed to the air, each cubic inch should weigh something less than two thirds of a grain, which does not differ much from the weight of common air.

HAVING thus fixed the first results, nothing now remained, but to submit the seventy-eight cubic inches of air which I had obtained to all the trials proper for determining its nature, and I discovered with much surprize, 1st. that it was not capable of combination with water by agitation; 2dly. that it did not precipitate lime-water; 3dly. that it did not unite with fixed or volatile alkalis; 4thly. that it did not, at all, diminish their caustic quality; 5thly. that it would serve again for the calcination of metals; 6thly. that it was diminished like common air, by addition of one-third of nitrous air; lastly, that it had none of the properties of fixed air:

far

far from being fatal, like it, to animals, it seemed, on the contrary, more proper for the purposes of respiration ; candles and burning bodies were not only *not* extinguished by it, but burned with an enlarged flame in a very remarkable manner ; the light they gave was much greater and clearer than in common air. All these circumstances fully convinced me that this air was not only common air, but that it was even more respirable, more combustible, and consequently more pure even than the air in which we live.

IT seems to be proved from hence, that the principle which combines with metals during their calcination, and which occasions the augmentation in their weight, is nothing but an exceedingly pure portion of the air which surrounds us, which we respire, and which passes, in this process, from a state of expansibility to that of solidity : if then it be obtained in the state of fixed air in all the metallic reductions when charcoal is employed, it is to the charcoal that this effect should be attributed ; and it is very probable that all the metallic calces would yield only common air if we could reduce them all

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without

without addition, in the same manner as the *mercurius precipitatus per se*.

EVERY thing which I have said of the air from metallic calces, may naturally be applied to that which is obtained from nitre by detonation; it is known from several experiments already published, most of which I have repeated, that the greater part of that air is in the state of fixed air, that it is mortal to animals who breathe it, that it has the property of precipitating lime-water, of uniting alone with lime and alkalis, of rendering them mild, and causing them to crystallize; but as, at the same time, the detonation of nitre does not take place without the addition of charcoal, or of some body which contains phlogiston, it is very probable that under this circumstance also the common air is converted into fixed air; from whence it should follow that the air combined in nitre, and which produces the terrible explosions of gun-powder, is common atmospherical air deprived of its expansibility.

As common air is changed into fixed air when combined

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combined with charcoal, it should seem natural to conclude that fixed air is merely a combination of common air and phlogiston. This is Dr. Priestley's opinion *, and it must be granted that it is not improbable; however, when one descends to a detail of facts, it is found so frequently contradicted, that I think it necessary to desire philosophers and chemists to suspend their judgment; I hope soon to be able to publish the motives of my doubts.

* DR. PRIESTLEY has, certainly, never delivered such an opinion as M. Lavoisier here ascribes to him. His doctrine is, that every diminution of common air is produced by phlogiston, and that this diminution is owing to the deposition of one of its constituent parts, viz. that principle which is commonly denominated fixed air. But he has never considered this fixed air as a compound, but rather as an elementary body. T. H.

NUMBER II.

AN ACCOUNT OF DR. PRIESTLEY'S
OPINION RELATIVE TO THE PRINCIPLE
WHICH IS COMBINED WITH METALS DU-
RING THEIR CALCINATION, AND OF HIS
DISCOVERY OF DEPHLOGISTICATED
AIR.

IT appears, from the preceding memoir, that M. Lavoisier not having obtained satisfactory results from his experiments on the calces of iron, had recourse to *mercurius calcinatus per se*, as being more easily reducible without addition. This substance afforded him not fixed, but, what he imagined to be, common air, in a state rather more pure than that which we usually breathe; and this circumstance led him to conclude, perhaps too hastily, that the air obtainable from all the metallic calces, if it were possible to reduce them without addition, would be of the same kind,

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kind, and that the fixed air which is produced from reductions in the common method, does not proceed from the calx but from the charcoal employed in the process.

DR. PRIESTLEY, however, differs in opinion from his ingenious fellow labourer on this point. He declares that several of the metallic calces yielded fixed air, by heat only, without any addition of charcoal, when exposed to the action of a large burning lens of twelve inches diameter and twenty inches focal distance.

THIS excellent philosopher also, from whom M. Lavoisier seems to have received the first idea* of extracting a particular kind of air from *mercurius calcinatus*, has extended his researches much farther into the nature of the air obtained from that calx. These inquiries have led him to discoveries and reflections, relative to the nature and composition of atmospheric air, of the highest importance to physics, and which may, perhaps, in time, prove productive of consequences the most beneficial to mankind. Having

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obtained

* SEE Priestley's Experiments and Observations, Vol. II.
page 36, and 320.

obtained air, from *mercurius calcinatus*, which was not absorbed by water, and in which a candle burned with an enlarged flame, he, at first, imagined it to be nothing more than air in the same state to which he had formerly brought nitrous air by exposing it to iron filings or liver of sulphur, and he little suspected that it was of that superior degree of purity which he has since proved it to possess. He afterwards procured air of a similar kind from common *red precipitate*, and sometimes, but not uniformly, even from red lead.

By a gradual train of experiments, Dr. Priestley, after having remained for several months without suspicion of the real properties of this air, discovered that not only candles burned in it with a more vigorous flame, but that animals lived in it, at least, three times as long as in common air, and that when they were withdrawn from the jar, the remaining air still continued purer than that of the atmosphere. On trying it by the test of nitrous air, he found it to be between four and five times as good as the air we breathe, viz. that whereas one measure

of

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of nitrous air is sufficient to produce the utmost possible diminution in two measures of common air, an equal quantity of this purer air required more than four measures to effect the same diminution. In the course of his experiments, he arrived at the power of procuring air even purer than this, and between five and six times as good as common air. This property of absorbing so large a proportion of nitrous air, he judged to depend on its being capable of taking more phlogiston from that body, as originally containing less of this principle, and he consequently denominated it *dephlogisticated air*.

THE circumstance of the *red precipitate*, or *mercurius corrosivus ruber*, which is produced from a solution of mercury in nitrous acid, yielding dephlogisticated air, induced Dr. Priestley to conclude that the properties of this air might depend on something being communicated to it by the nitrous acid, and that the *mercurius calcinatus* had, in the degree of heat to which it is exposed in its preparation, attracted something nitrous from the atmosphere. He was soon confirmed in this opinion, for endeavouring to ex-

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tract

tract air from some red lead which had been recently prepared and afforded it but in small quantity, he was desirous of bringing it to that state in which other red lead had yielded it plentifully; and concluding that the calx must imbibe some kind of acid from the air, he moistened three separate half ounces of the fresh prepared minium with each of the three mineral acids, and having dried, he afterwards exposed them to a sufficient degree of heat in a gun-barrel to which a suitable apparatus was affixed. From the parcels moistened with the vitriolic and marine acids no air was produced, but from that to which the nitrous acid had been added, much air was obtained, the greatest part of which was fixed air, though there was also a considerable portion of the dephlogisticated species. In this experiment the minium had been repeatedly moistened with nitrous acid and dried again, but when, afterwards, it was only once moistened with that acid, though less air was obtained, it proved to be almost all of the dephlogisticated kind, and was about five times as pure as common air.

He then proceeded to try different substances, and procured dephlogisticated air from every kind of earth, which is void of phlogiston, made into a paste, as above, with spirit of nitre. The metallic earths, in this state, and chalk, appeared to be the best adapted to this purpose. When these substances had yielded all the dephlogisticated air that could be extracted from them, they would again afford as much as at first, on being moistened with fresh spirit of nitre, and the process might be repeated till all the earthy matter be exhausted.

FROM hence Dr. Priestley was led to conclude "that atmospherical air, or the thing that we breathe, consists of the nitrous acid and earth, with so much phlogiston as is necessary to its elasticity; and likewise so much more as is required to bring it from its state of perfect purity to the mean condition in which we find it."

DEPHLOGISTICATED air does not only support the lives of animals confined in it, and the flame of burning bodies more powerfully than common air, but also conveys sounds with greater force.

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An explosion of two thirds of inflammable air mixed with rather more than one-third of dephlogisticated air, seemed to be forty times louder than when made with two thirds of atmospheric air.

DR. PRIESTLEY has, with great reason, formed very high expectations of the salutary and useful purposes to which this pure air may be applied, and seems to think that, in time, it may even become a fashionable article of luxury. "Hitherto," says he, "only two mice and myself have had the privilege of breathing it."

By these extracts which Doctor Priestley (with that friendly disposition and that desire of contributing to the improvement of science, which so remarkably distinguish him) has permitted me to make from his recent and very valuable publication, it appears that he thinks, contrary to the opinion of M. Lavoisier, as advanced in the last article, that the principle imbibed by metals during their calcination, and which constitutes the increase in their weight, is, in some instances, fixed air, and in others nitrous acid,

acid, attracted by them from the common air in which they are calcined.

IT also appears that this philosopher, instead of regarding atmospheric or common air (when free from the foreign matters which are always supposed to be dissolved, and intermixed with it) as a *simple elementary substance*, considers it as a mixture of several principles capable of decomposition; and that he has discovered a method of compounding common respirable air, by adding to dephlogisticated air such a quantity of phlogiston as reduces it to the state in which we generally find the atmosphere surrounding the planet which we inhabit.

IT has been seen, page 409, that M. Lavoisier having embraced the hypothesis of common air being converted into fixed air by combining with phlogiston, imagined it probable that in the detonation of nitre or of gun-powder, a conversion of the common into fixed air might result from the charcoal which is necessary to produce the explosion. From nitre, heated in a glass vessel, Dr. Priestley procured very pure dephlo-

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dephlogisticated air. A mixture of equal parts of brimstone and nitre yielded air which was highly nitrous, and as the produce of nitre and charcoal is nitrous air, he concludes that this kind of air is also produced in the explosion of gun-powder.

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1. What is the quantity of oil produced
in the Mississippi River basin? Is there
any way to estimate it?

3.

In Mississippi, the quantity of oil produced
is in communication with the rivers to which it
flows or is influenced by its communication with
the accumulation of time. And this time,
Mr. Craig Murphy told me, is
about one century from now, but on the
basis of 100 years.

Now, then, the calculated volume, the quantity of
oil that can be collected, is:

In Mississippi estimated as a percentage of land area
approximately 10%.

Therefore, in that same century there will be an
estimated 100 million barrels.

There's Murphy's oil, published in October 1955
which he stated this is the figure we are
using but we didn't understand it. But this

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