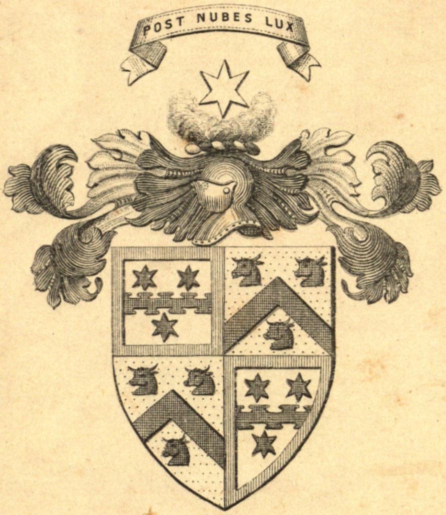
Ralph M. Waters
M. D.



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1st Ed. See pages 507, 513 & 533 for testimonials
of Robert Southey (age 25), S.T. Coleridge (age 27) and
~~the article~~ by Thomas Beddoes.

See p 556 Surgical [illegible]

# RESEARCH I.

CONCERNING THE ANALYSIS

OF

NITRIC ACID AND NITROUS GAS.

AND

THE PRODUCTION OF

# NITROUS OXIDE.

This leaf should
follow p. XVI

DAVY, Sir HUMPHREY, bart., one of the most eminent among modern chemists, was born at Penzance, in Cornwall, in 1778. He was intended for the medical profession, and placed with an apothecary for the necessary initiation; but he gave himself up to the study of chemistry, and, with the consent of his master, quitted him in his 15th year, in order to prepare for graduating as a physician at Edinburgh. Indefatigable in the pursuit of his favourite science, his progress in it was most rapid; his friends encouraged the bent of his genius, and he was induced to suspend his design of going to Edinburgh, and to accept the superintendence of a Pneumatic Institution at Bristol. While there he published his "Chemical and Philosophical Researches," the fame of which immediately obtained him the professorship of chemistry at the Royal Institution, where his popularity as a lecturer was unbounded. In 1802 he became professor to the Board of Agriculture; in 1818 he was created a baronet; and in 1820 he was elected president of the Royal Society; and a series of scientific discoveries and professional honours flowed on without interruption till his death, which took place at Geneva, in 1829. The invention of the safety-lamp, the discovery of the metallic bases of the alkalies and earths, and of the principles of electro-chemistry, and numerous other discoveries and inventions not less important, attest his skill and industry, and give him an imperishable fame. Besides his separate works of a scientific character, he was the author of numerous papers in the Philosophical Transactions; and when, during his illness, he was disposed to divert his mind with lighter studies, he wrote "Salmonia, or Days of Fly-fishing," and "Consolations in Travel."

# MERCURIAL AIR-HOLDER and BREATHING MACHINE.

Fig. 2.

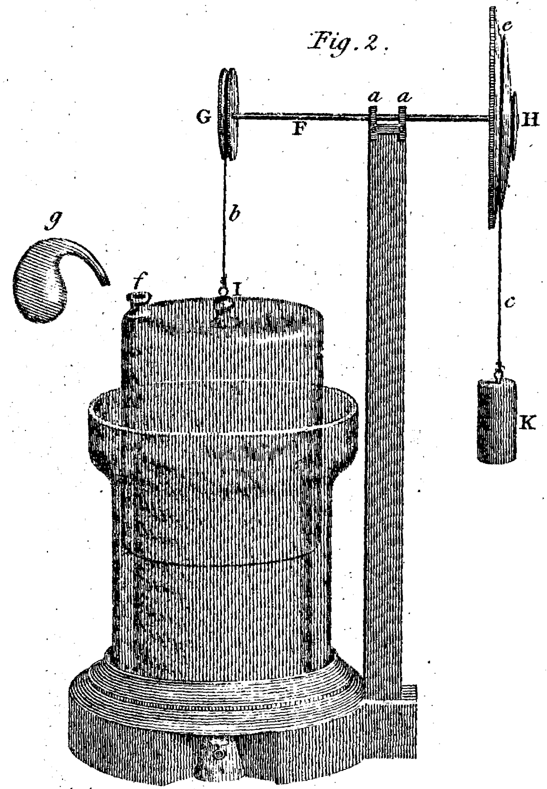



Fig. 3.

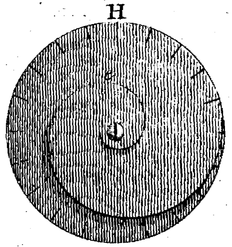
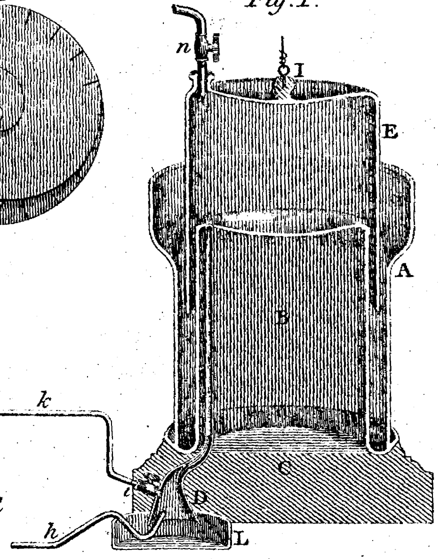



Fig. 1.

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# RESEARCHES,

# CHEMICAL AND PHILOSOPHICAL;

CHIEFLY CONCERNING

# NITROUS OXIDE,

OR

# DEPHLOGISTICATED NITROUS AIR,

AND ITS

# RESPIRATION.

By HUMPHRY DAVY,

SUPERINTENDENT OF THE MEDICAL PNEUMATIC INSTITUTION.

LONDON:

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1800.

# CONTENTS.

INTRODUCTION, - - - - - - xi.

## RESEARCH I.

*Into the analysis of* NITRIC ACID *and* NITROUS GAS, *and the production of* NITROUS OXIDE.

### DIVISION I.

EXPERIMENTS *and* OBSERVATIONS *on the composition of* NITRIC ACID, *and on its combinations with* WATER *and* NITROUS GAS.

1. Preliminaries - - - - - - 1
2. Production of aëriform Nitrous Acid - - 3
3. Specific gravity of Gases - - - 6
4. Experiment on the formation of Nitrous Acid - 11
5. Conclusions - - - - - - 17
6. Experiments on the combination of Nitrous Gas with Nitric Acid - - - - - 17
7. Additional Experiments - - - 23
8. Conclusions - - - - - 29
9. Mr. THOMSON's Theory of the difference between Nitric and Nitrous Acid - - - 30
10. Composition of the different Nitrous Acids - 36
11. Combination of Nitric Acid with Water - 38
12. Of Nitrous Vapor - - - - 42
13. Comparison of the results with those of Cavendish and Lavoisier - - - - 43

## DIVISION II.

EXPERIMENTS and OBSERVATIONS on the compofition of AMMONIAC and on its combinations with WATER and NITRIC ACID.

1. Analyfis of Ammoniac - - - 56
2. Specific gravity of Ammoniac - - 62
3. Of the quantities of true Ammoniac in Ammoniacal Solutions - - - - 65
4. Compofition of Nitrate of Ammoniac - 71
5. Decompofition of Carbonate of Ammoniac, by Nitrous Acid - - - - - 75
5. Decompofition of Sulphate of Ammoniac by Nitre 77
6. Non-exiftence of Ammoniacal Nitrites - 79
7. Sources of error in Analyfis - - 80
8. Lofs in Solutions of Nitrate of Ammoniac during evaporation - - - - 83

## DIVISION III.

DECOMPOSITION of NITRATE of AMMONIAC—Preparation of RESPIRABLE NITROUS OXIDE.

1. Of the heat required for the decompofition of Nitrate of Ammoniac - - - - 84
2. Decompofition of Nitrate of Ammoniac—Production of refpirable Nitrous Oxide—its properties - 86
3. Of the Gas remaining after the abforption of Nitrous Oxide by Water - - - - 89
4. Specific Gravity of Nitrous Oxide - - 94
5. Analyfis of Nitrous Oxide - - - 95
6. Minute examination of the decompofition of Nitrate of Ammoniac - - - - 101
7. Of the heat produced during the decompofition of Nitrate of Ammoniac - - - 108

8. Decompofition of Nitrate of Ammoniac at high temperatures - - - - - 109
9. Speculations on the decompofitions of Nitrate of Ammoniac - - - - - 113
10. Of the preparation of Nitrous Oxide for experiments on refpiration - - - - 117

## DIVISION IV.

EXPERIMENTS and OBSERVATIONS on the compofition of NITROUS GAS, and on its abforption by different bodies.

1. Preliminaries - - - - 122
2. Analyfis of Nitrous Gas by Charcoal - 126
3. Analyfis of Nitrous Gas by Pyrophorus - 132
4. Additional obfervations on the compofition of Nitrous Gas - - - - - 134
5. Abforption of Nitrous Gas by Water - 140
6. Abforption of Nitrous Gas by Water of different kinds 147
7. Abforption of Nitrous Gas by folution of pale green Sulphate of Iron - - - 152
8. Abforption of Nitrous Gas by folution of green muriate of Iron - - - - - 179
9. By Solution of Nitrate of Iron - - 187
10. By other metallic Solutions - -
11 Action of fulphurated Hydrogene on folution of green fulphate of iron impregnated with Nitrous Gas. 191
12. Additional Obfervations - - - 193

## DIVISION V.

EXPERIMENTS and OBSERVATIONS on the production of NITROUS OXIDE from NITROUS GAS and NITRIC ACID in different modes.

1. Preliminaries - - - - 197
2. Converfion of Nitrous Gas into Nitrous Oxide by alkaline fulphites - - - - 199

3. By Muriate of Tin - - - - - 202
4. By Sulphurated Hydrogene - - - 203
5. Decompofition of Nitrous Gas by Nafcent Hydrogene - - - - - - 206
6. Mifcellaneous Obfervations - - - - 209
7. Recapitulation - - - - - 211
8. Production of Nitrous Oxide from Metallic Solutions 213
9. Additional Obfervations relating to the production of Nitrous Oxide - - - - 219
10 Decompofition of Aqua regia by platina, and evolution of a gas analogous to oxygenated muriatic acid, and nitrogene - - - - 222
11. Action of the electric fpark on a mixture of Nitrogene and Nitrous gas - - - 229
12. General remarks on the production of Nitrous Oxide 231

## RESEARCH II.

*Into the combinations of* NITROUS OXIDE, *and its decompofition.*

### DIVISION I.

EXPERIMENTS and OBSERVATIONS on the combinations of NITROUS OXIDE.

1. Combination of Water with Nitrous Oxide - 235
2. ——— of Nitrous Oxide with fluid inflammable bodies. - - - - - 240
3. Action of fluid Acids on Nitrous Oxide - 244
4. —— of Saline Solutions - - 245
5. —— of Gafes - - - - 248
6. Action of aëriform Nitrous Oxide on the alkalies—Hiftory of the difcovery of the combinations of Nitrous Oxide, with the alkalies - 254
7. Combination of Nitrous Oxide with Potafh - 262

8. Combination of Nitrous Oxide with Soda - 268
9. ——— ——— ——— with Ammoniac 269
10. Probability of forming compounds of Nitrous Oxide and the alkaline earths - - 273
11 Additional Obfervations - - 274
12 The properties of Nitrous oxide refemble thofe of Acids 276

## DIVISION II.

Decompofition of NITROUS OXIDE by combuftible Bodies.

1 Preliminaries - - - - 278
2 Converfion of Nitrous Oxide into Nitrous Acid and a gas analogous to Atmofpheric Air by ignition 279
3 Decompofition of Nitrous Oxide by Hydrogene 286
4 ——— ——— ——— by Phofphorus 293
5 ——— ——— by Phofphorated Hydrogene 300
6 ——— ——— by Sulphur - 303
7 ——— ——— by Sulphurated Hydrogene 306
8 ——— ——— by Charcoal - 311
9 ——— ——— by Hydrocarbonate - 313
10 Combuftion of Iron in Nitrous Oxide - 316
11 ——— of Pyrophorus - - 318
12 ——— of the Taper - - 319
13 ——— of different Compound Bodies - 321
14 General Conclufions relating to the decompofition of Nitrous Oxide, and to its analyfis - 322
15 Obfervations on the combinations of Oxygene and Nitrogene - - - 325

# RESEARCH III.

*Relating to the* RESPIRATION *of* NITROUS OXIDE *and* OTHER GASES.

## DIVISION I.

EXPERIMENTS and OBSERVATIONS on the effects produced upon Animals by the respiration of NITROUS OXIDE.

1 Preliminaries - - - - 333
2 On the respiration of Nitrous Oxide by warm-blooded Animals - - - - - 336
3. Effects of the respiration of Nitrous Oxide upon Animals, as compared with those produced by their immersion in Hydrogene and Water - 343
4. Of the changes effected in the organisation of warm blooded Animals, by the respiration of Nitrous Oxide - - - - 347
5. Of the respiration of mixtures of Nitrous Oxide and other Gases, by warm-blooded Animals - 358
6. Recapitulation of facts relating to the respiration of Nitrous Oxide, by warm-blooded Animals - 360
7. Of the respiration of Nitrous Oxide, by amphibious Animals - - - - 362
8. Effects of Solution of Nitrous Oxide on Fishes - 366
9. Effects of Nitrous Oxide on Insects - - 370

## DIVISION II.

Of the changes effected in NITROUS OXIDE and other Gases, by the Respiration of Animals.

1. Preliminaries - - - - 373
2. Absorption of Nitrous Oxide by Venous Blood 374
3. Of the changes effected in Nitrous Oxide by Respiration . . . . 388
4. Respiration of Hydrogene - - 400
5. Additional Observations and Experiments on the Respiration of Nitrous Oxide - - 411

6. Of the Refpiration of Atmofpheric Air - 429
7. Refpiration of Oxygene - - - 439
8. Obfervations on the changes effected in the blood by Atmofpheric Air and Oxygene - - 445
9. Obfervations on the Refpiration of Nitrous Oxide 449

## RESEARCH IV.

*Relating to the* EFFECTS *produced by the* RESPIRATION *of* NITROUS OXIDE *upon different* INDIVIDUALS.

### DIVISION I.

HISTORY of the Difcovery.—EFFECTS produced by the Refpiration of different GASES.

1. Refpirability of Nitrous Oxide - - 456
2. Effects of Nitrous Oxide - - - 458
3. General Effects of Nitrous Oxide on the Health 464
4. Refpiration of Hydrogene - - - 466
5. —— of Nitrogene - - - 467
6. Effects of Hydrocarbonate - - 468
7. — of Carbonic Acid - - 472
8. — of Oxygene - - - 473
9. — of Nitrous Gas - - - 475
10. Moft extenfive action of Nitrous Oxide produces no debility - - - - 485

### DIVISION II.

DETAILS of the Effects produced by the Refpiration of NITROUS OXIDE upon different Individuals, furnifhed by Themfelves.

1 Detail of Mr. J. W. Tobin - - 497
2 — of Mr. W. Clayfield - - 502
3 Letter from Dr. Kinglake - - 503
4 Detail of Mr. Southey - - - 507

5 Letter from Dr. Roget - - - 509
6 Letter from Mr. James Thomson - - 512
7 Detail of Mr. Coleridge - - - 516
8 — of Mr. Wedgwood - - - 518
9 — of Mr. G. Burnet - - - 520
10 — of Mr. T. Pople - - - 521
11 — of Mr. Hammick - - - 522
12 — of Dr. Blake - - - - 524
13 — of Mr. Wansey - - - 525
14 — of Mr. Rickman - - - 526
15 — of Mr. Lovell Edgworth - - 527
16 — of Mr. G. Bedford - - - 528
17 — of Miss Ryland - - - 530
18 Letter from Mr. M. M. Coates - - 530

DIVISION III.

Abstracts from additional Details—Observations on the effects of Nitrous Oxide, by Dr. Beddoes—Conclusion.

1 Abstracts from additional details - - 533
2 Of the effects of Nitrous Oxide on delicate females 537
3 Observations on the effects of Nitrous Oxide by Dr. Beddoes. - - - - - 541
4 Conclusion - - - - - 548

APPENDIX.

No. I. Of the effects of Nitrous Oxide on Vegetables 561
No. II. Table of the Weight and Composition of the combinations of Nitrogene - - - 566
No. III. Additional Observations - - - 567
No. IV. Description of a Mercurial Airholder, and Breathing Machine, by Mr. W. Clayfield.
No. V. Proposals for the Preservation of Accidental Observations in Medicine. By Dr. Beddoes.

# INTRODUCTION.

IN consequence of the discovery of the respirability and extraordinary effects of nitrous oxide, or the dephlogisticated nitrous gas of Dr. Priestley, made in April 1799, in a manner to be particularly described hereafter, * I was induced to carry on the following investigation concerning its composition, properties, combinations, and mode of operation on living beings.

In the course of this investigation, I have met with many difficulties; some arising from the novel and obscure nature of the subject, and

* A short account of this discovery has been given in Dr. Beddoes's Notice of some Observations made at the Pneumatic Institution, and in Mr. Nicholson's Phil. Journal for May and December 1799.

others from a want of coincidence in the obſervations of different experimentaliſts on the properties and mode of production of the gas. By extending my reſearches to the different ſubſtances connected with nitrous oxide; nitrous acid, nitrous gas and ammoniac; and by multiplying the compariſons of facts, I have ſucceeded in removing the greater number of thoſe difficulties, and have been enabled to give a tolerably clear hiſtory of the combinations of oxygene and nitrogene.

By employing both analyſis and ſyntheſis whenever theſe methods were equally applicable, and comparing experiments made under different circumſtances, I have endeavoured to guard againſt ſources of error; but I cannot flatter myſelf that I have altogether avoided them. The phyſical ſciences are almoſt wholly dependant on the minute obſervation and compariſon of properties of things not immediately obvious to the ſenſes: and from the difficulty of diſcovering every poſſible mode of examination, and from the modification of perceptions by the ſtate of feeling, it appears nearly impoſſible that all the relations of a ſeries of phænomena can be diſcovered by a ſingle inveſtigation, particularly when theſe relations are complicated, and many of the agents unknown. Fortunately for the active and progreſſive nature of the human mind, even experimental reſearch is only a method of approximation to truth.

In the arrangement of facts, I have been guided as much as poſſible by obvious and ſimple analogies only. Hence I have ſeldom entered into theoretical diſcuſſions, particularly concerning light, heat, and other agents, which are known only by iſolated effects.

Early experience has taught me the folly of haſty generaliſation. We are ignorant of the laws of corpuſcular motion; and an immenſe maſs of minute obſervations concerning the more complicated chemical changes muſt be collected, probably before we ſhall be able to aſcertain even whether we are capable of diſcovering them. Chemiſtry in its preſent ſtate, is ſimply a partial history of phænomena, consisting of many series more or less extensive of accurately connected facts.

With the most important of these series, the arrangement of the combinations of oxygene or the antiphlogistic theory discovered by Lavoisier, the chemical details in this work are capable of being connected.

In the present state of science, it will be unnecessary to enter into discussions concerning the importance of investigations relating to the properties of physiological agents, and the changes effected in them during their operation. By means of such investigations, we arrive nearer towards that point from which we shall be able to view what is within the reach of discovery, and what must for ever remain unknown to us, in the phænomena of organic life. They are of immediate utility, by enabling us to extend our analogies so as to investigate the properties of untried substances, with greater accuracy and probability of success.

The firſt Reſearch in this work chiefly relates to the production of nitrous oxide and the analyſis of nitrous gas and nitrous acid. In this there is little that can be properly called mine; and if by repeating the experiments of other chemiſts, I have ſometimes been able to make more minute obſervations concerning phænomena, and to draw different concluſions, it is wholly owing to the uſe I have made of the inſtruments of inveſtigation diſcovered by the illuſtrious fathers of chemical philoſophy,* and ſo ſucceſsfully applied by them to the diſcovery of truth.

In the ſecond Reſearch the combinations and compoſition of nitrous oxide are inveſtigated, and an account given of its decompoſition by moſt of the combuſtible bodies.

The third Reſearch contains obſervations on the action of nitrous oxide upon animals, and

* Cavendiſh, Prieſtley, Black, Lavoiſier, Scheele, Kirwan, Guyton, Berthollet, &c.

an inveſtigation of the changes effected in it by reſpiration.

In the fourth Reſearch the hiſtory of the reſpirability and extraordinary effects of nitrous oxide is given, with details of experiments on its powers made by different individuals.

I cannot cloſe this introduction, without acknowledging my obligations to Dr. Beddoes. In the conception of many of the following experiments, I have been aided by his converſation and advice. They were executed in an Inſtitution which owes its exiſtence to his benevolent and philoſophic exertions.

*Dowry-Square, Hotwells, Briſtol.*

*June 25th,* 1800.

# RESEARCH I.

## INTO THE PRODUCTION AND ANALYSIS OF NITROUS OXIDE, AND THE AERIFORM FLUIDS RELATED TO IT.

## DIVISION I.

*EXPERIMENTS and OBSERVATIONS on the composition of* NITRIC ACID, *and on its combinations with* WATER *and* NITROUS GAS.

I. THOUGH ſince the commencement of Pneumatic Chemiſtry, no ſubſtance has been more the ſubject of experiment than Nitrous Acid; yet ſtill the greateſt uncertainty exiſts with regard to the quantities of the principles entering into its compoſition.

In comparing the experiments of the illuſtrious Cavendiſh on the ſyntheſis of nitrous acid, with thoſe of Lavoiſier on the decompoſition of nitre by charcoal, we find a much greater difference in the reſults than can be accounted for by ſuppoſing the acid formed, and that decompoſed, of different degrees of oxygenation.

In the moſt accurate experiment of Cavendiſh, when the nitrous acid appeared to be in a ſtate of deoxygenation, 1 of nitrogene combined with about 2,346 of oxygene.* In an earlier experiment, when the acid was probably fully oxygenated, the nitrogene employed was to the oxygene nearly as 1 to 2,92.†

Lavoiſier, from his experiments on the decompoſition of nitre, and combination of nitrous gas and oxygene, concludes, that the perfectly oxygenated, or what he calls nitric acid, is compoſed of nearly 1 nitrogene, with 3,9 of oxygene; and the acid in the laſt ſtate of deoxygenation, or nitrous acid, of about 3 oxygene with 1 nitrogene.‡

* Phil. Tranſ. v. 78, p. 270. † Phil. Tranſ. v. 75, p. 381.

‡ Elem. Kerr's Tranſ. page 76, and 216, and Mem. des Sav. Etrang. tom. 7, page 629.

Great as the difference is between the eſtimations of theſe philoſophers, we find differences ſtill greater in the accounts of the quantities of nitrous gas neceſſary to ſaturate a given quantity of oxygene, as laid down by very accurate experimentaliſts. On the one hand, Prieſtley found 1 of oxygene condenſed by 2 of nitrous gas, and Lavoiſier by 1 $\frac{7}{8}$. On the other, Ingenhouz, Scherer, and De la Metherie, ſtate the quantity neceſſary to be from 3 to 5.* Humbolt, who has lately inveſtigated Eudiometry with great ingenuity, conſiders the mean quantity of nitrous gas neceſſary to ſaturate 1 of oxygene, as about 2,55.†

II. To reconcile theſe different reſults is impoſſible, and the immediate connection of the ſubject with the production of nitrous oxide, as well as its general importance, obliged me to ſearch for means of accurately deter-

* Ingenhouz ſur les Vegetaux, pag. 205. De la Metherie. Eſſai ſur differens Airs, pag. 252.

† Annales de Chimie, tome 28, p. 168.

mining the compoſition of nitrous acid in its different degrees of oxygenation.

The firſt deſideratum was to aſcertain the nature and compoſition of a fluid acid, which by being deprived of, or combined with nitrous gas, might become a ſtandard of compariſon for all other acids.

To obtain this acid I ſhould have preferred the immediate combination of oxygene and nitrogene over water by the electric ſpark, had it been poſſible to obtain in this way by a common apparatus ſufficient for extenſive examination; but on carefully peruſing the laborious experiments of Cavendiſh, I gave up all thoughts of attempting it.

My firſt experiments were made on the decompoſition of nitre, formed from a known quantity of pale nitrous acid of known ſpecific gravity, by phoſphorus, tin, and charcoal: but in thoſe proceſſes, unaſcertainable quantities of nitrous acid, with exceſs of nitrous gas, always eſcaped undecompounded, and from the non-coincidence of reſults, where different quantities of combuſtible ſubſtances were employed, I had reaſons for believing that water was generally decompoſed.

Before theſe experiments were attempted, I had analized nitrous gas and nitrous oxide, in a manner to be particularly deſcribed hereafter; ſo that a knowledge of the quantities of nitrous gas and oxygene entering into the compoſition of any acid, enabled me to determine the proportions of nitrogene and oxygene it contained. In conſequence of which I attempted to combine together oxygene and nitrous gas, in ſuch a manner as to abſorb the nitrous acid formed by water, in an apparatus by which the quantities of the gaſes employed, and the increaſe of weight of the water, might be aſcertained; but this proceſs likewiſe failed. It was impoſſible to procure the gaſes perfectly free from nitrogene, and during their combination, this nitrogene made to paſs into a pneumatic apparatus communicating with a veſſel containing the water carried over with it, much nitrous acid vapor, of different compoſition from the acid abſorbed.

After many unſucceſsful trials, Dr. Prieſtley's experiments on nitrous vapor * induced me to ſuppoſe that oxygene and nitrous gas, made to combine out of the contact of bodies having affinity for oxygene, would remain permanently aëriform, and on throwing them ſeparately into an exhauſted glaſs balloon, I found that this was actually the caſe; increaſe of temperature was produced, and orange colored nitrous acid gas formed, which after remaining for many days in the globe, at a temperature below 56°, did not in the ſlighteſt degree condenſe.

This fact afforded me the means not only of forming a ſtandard acid, but likewiſe of aſcertaining the ſpecific gravity of nitrous acid in its aëriform ſtate.

III. Previous to the experiment, for the purpoſe of correcting incidental errors, I was induced to aſcertain the ſpecific gravity of the

* Experiments and Obſervations, Vol. iii. laſt edition, page 105, &c.

gafes employed, particularly as I was unacquainted with any procefs by which the weight of nitrous gas had been accurately determined. Mr. Kirwan's eftimation, which is generally adopted, being founded upon the comparifon of the lofs of weight of a folution of copper in dilute nitrous acid, with the quantity of gas produced.*

The inftruments that I made ufe of for containing and meafuring my gafes, were two mercurial airholders graduated to the cubic inch of Everard, and furnifhed with ftop cocks.†

* When copper is diffolved in dilute nitrous acid, certain quantities of nitrogene are generally produced, likewife the nitrous gas carries off in folution fome nitrous acid.

† This airholder, confidered as a pneumatic inftrument, is of greater importance, and capable of a more extenfive application than any other. It was invented by Mr. W. Clayfield, and in its form is analogous to Mr. Watt's hydraulic bellows, confifting of a glafs bell playing under the preffure of the atmofphere, in a fpace between two cylinders filled with mercury. A particular account of it will be given in the appendix.

They were weighed in a glaſs globe, of the capacity of 108 cubic inches, which with the ſmall glaſs ſtop-cock affixed to it, was equal, when filled with atmoſpheric air, to 1755 grains. The balance that I employed, when loaded with a pound, turned with leſs than one eighth of a grain.

Into a mercurial airholder, of the capacity of 200 cubic inches, 160 cubic inches of nitrous gas were thrown from a ſolution of mercury in nitrous acid.

70 meaſures of this were agitated for ſome minutes in a ſolution of ſulphate of iron,* till the diminution was complete. The nitrogene remaining hardly filled a meaſure; and if we ſuppoſe with Humbolt † that a very ſmall portion of it was abſorbed with the nitrous gas, the whole quantity it contained may be eſtimated at 0,0142, or $\frac{1}{70}$.

75 cubic inches received from the airholder

* This abſorption will be hereafter particularly treated of.

† Annales de Chimie. Tome xviii. page 139.

into an exhausted balloon, increased it in weight 25,5 grains; thermometer being 56°, and barometer 30,9. And allowing for the small quantity of nitrogene in the gas, 100 cubic inches of it will weigh 34,3 grains.

One hundred and thirty cubic inches of oxygene were procured from oxide of manganese and sulphuric acid, by heat, and received in another mercurial airholder.

10 measures of it, mingled with 26 of the nitrous gas, gave, after the residuum was exposed to solution of sulphate of iron, rather more than one measure. Hence we may conclude that it contained about 0,1 nitrogene.

60 cubic inches of it weighed 20,75 grains; and accounting for the nitrogene contained in these, 100 grains of pure oxygene will weigh 35,09 grains.

Atmospherical air was decomposed by nitrous gas in excess; and the residuum washed with solution of sulphate of iron till the Nitrogene remained pure; 87 cubic inches of it weighed 26,5 grains, thermometer being 48°, barometer 30,1; 100 will consequently weigh 30,45.

90 cubic inches of the air of the laboratory not deprived of its carbonic acid, weighed 28,75 grains; thermometer 53, barometer 30: 100 cubic inches will consequently weigh 31,9.* 16 measures of this air, with 16 nitrous gas, of known composition, diminished to 19. Hence it contained about ,26 oxygene.§

In comparing my results with those of Lavoisier and Kirwan, the estimation of the weights of nitrogene and oxygene is very little different, the corrections for temperature and pressure being made, from that of those celebrated philosophers. The first makes oxygene to weigh † 34,21, and nitrogene 30,064 per cent; and the last, oxygene 34, ‡ and nitrogene 30,5.

* A table of the specific gravities of these gases, and other gases, hereafter to be mentioned, reduced to a barometrical and thermometrical standard, will be given in the appendix.

§ 40 measures, exposed to solution of potash, gave an absorption of not quite a quarter of a measure: hence it contained an inconsiderable quantity of carbonic acid.

† Traité Elementaire.

‡ Essai sur le phlogistique, page 30.

The ſpecific gravity of nitrous gas, according to Kirwan, is to that of common air as 1194 to 1000. Hence it ſhould weigh about 37 grains per cent. This difference from my eſtimation is not nearly ſo great as I expected to have found it.§

IV.* The thermometer in the laboratory ſtanding at 55°, and the barometer at 30,1, I now proceeded to my experiment. The oxygene that I employed was of the ſame compoſition as that which I had previouſly weighed. The nitrous gas contained ,0166 nitrogene.

For the purpoſe of combining the gaſes, a glaſs balloon was procured, of the capacity of 148 cubic inches, with a glaſs ſtop-cock adapted to it, having its upper orifice tubulated and graduated for the purpoſe of containing and meaſuring a fluid. The whole weight of this globe and its appendages, when filled with common air, was 2066,5 grains.

§ The diminution of the ſpecific gravity of the gas from the quantity of nitrogene evolved in his experiment, probably deſtroyed, in ſome meaſure, the ſource of error from the nitrous acid carried over.

* Experiment I.

It was partially exhaufted by the air-pump, and loft in weight juft 32 grains. From whence we may conclude that about 15 grains of air remained in it.

In this ftate of exhauftion it was immediately cemented to the ftop-cock of the mercurial airholder, and the communication being made with great caution, 82 cubic inches of nitrous gas rufhed into the globe, on the outfide of which a flight increafe of temperature was perceived, while the gafes on the infide appeared of a deep orange.

Before the common temperature was reftored, the communication was ftopped, and the globe removed. The increafe of weight was 29,25 grains; whence it appeared that 1,14 grains of common air, part of which had been contained in the ftop-cocks, had entered with the nitrous gas.

Whilft it was cooling, from the accidental loofening of the ftopper of the cock, 3 grains more of common air entered.*

* That no greater contraction took place depended on the folution of the nitrous acid formed in the nitrous gas; a phænomenon to be explained hereafter.

The communication was now made between the globe and the mercurial airholder containing oxygene. 64 cubic inches were ſlowly preſſed in, when the outſide of the globe became warmer, and the color on the inſide changed to a very dark orange. As it cooled, 6 cubic inches more ſlowly entered; but no new increaſe of temperature, or change of color took place.

The globe being now completely cold, was ſtopped, removed, and weighed; it had gained 24,5 grains, from whence it appears that 0,4 grains of common air contained in the ſtop-cocks, had entered with the oxygene.*

To abſorb the nitrous acid gas, 41 grains of water were introduced by the tube of the ſtop-cock, which though cloſed as rapidly as poſſible, muſt have ſuffered nearly ,5 grains of air to

* I judged it expedient always to aſcertain the quantity of air in the ſtop-cocks by weight, as it was impoſſible to join them ſo as to have always an equal capacity. The upper tubes of the two ſtop-cocks not joined, contained nearly an inch and half.

enter at the ſame time, as the increaſe of weight was 41,5 grains. The dark orange of the globe diminiſhed rapidly; it became warm at the bottom, and moiſt on the ſides. After a few minutes the color had almoſt wholly diſappeared.

To aſcertain the quantity of aëriform fluid abſorbed, the globe was again attached to the mercurial air apparatus, containing 140 cubic inches of common air. When the communication was made, 51 cubic inches ruſhed in, and it gained in weight 16,5 grains.

A quantity of fluid equal to 54 grains was now taken out of the globe. On examination it proved to be ſlightly tinged with green, and occupied a ſpace equal to that filled by 41,5 grains of water. Its ſpecific gravity was conſequently 1,301.

To aſcertain if any unabſorbed aëriform nitrous acid remained in the globe, 13 grains of ſolution of ammonia were introduced in the ſame manner as the water, and after ſome minutes, when the white vapor had condenſed, the communication was again made with the mercurial airholder containing common air. A minute quantity entered, which could not be eſtimated at more than three fourths of an inch, and the globe was increaſed in weight about 13,25 grains.*

Common air was now thrown into the globe till the reſidual gaſes of the experiment were judged to be diſplaced; it weighed 2106,5 grains, that is, 40 grains more than it had weighed when filled with common air before the experiment.†

* That is, by the ſolution of ammonia, and air.

† The following is an account of the increaſe and diminution of weight of the globe, as it was noted in the journal.

| | |
|---|---|
| Globe filled with common air - gr. | 2066,5 |
| After exhauſtion - - - | 2034,5 |
| After introduction of nitrous gas, 82 cubic inches - - | 2064,25 |
| After the accidental admiſſion of common air - - - | 2067,25 |
| After the admiſſion of oxygene - | 2091,75 |
| —— —— 41 grains of water | 2133,25 |
| —— —— 51 cubic inches of air | 2149,75 |
| Taken out 54 grains of ſolution - | 2095,75 |
| Introduced 13 grains of ammoniacal ſolution - - - | 2109,25 |
| After introduction of common air - | 2106,5 |

And if from thoſe 40 grains we take 13 for the ſolution of ammonia introduced, the remainder, 27, will be the quantity of ſolution of nitrous acid in water remaining in the globe, which added to 54, equals 81 grains, the whole quantity formed; but if from this be taken 41 grains, the quantity of water, the remainder 40 grains, will be the quantity of nitrous acid gas abſorbed in the ſolution.

To find the abſolute quantity of nitrous acid formed, we muſt find the ſpecific gravity of that abſorbed; but as during, and after its abſorption, 17 grains of air, equal to 53,2 cubic inches entered, it evidently filled ſuch a ſpace. 53,2 cubic inches of it conſequently weigh 40 grains, and 100 cubic inches 75,17 grains. Then ,75 cubic inches weigh ,56 grains, and this added to 40, makes 40,56 grains, equal to 57,0 cubic inches, the whole quantity of aëriform nitrous acid produced.

But the quantity of nitrous gas entering into this, allowing for the nitrogene it contained, is

27,6 grains, equal to about 80,5 cubic inches; and the oxygene is 40,56 — 27,6 = to 12,96 grains, or 36,9 cubic inches.

V. There could exist in this experiment no circumstance connected with inaccuracy, except the impossibility of very minutely determining the quantities of common air which entered with the gases from the stop-cocks. But if errors have arisen from this source, they must be very inconsiderable; as will appear from a calculation of the specific gravity of the nitrous acid gas, founded on the volume of the gases that entered the globe.

| | |
|---|---|
| The air that remained in the globe after exhaustion was 15 grains = | 47* cub. in. |
| The nitrous gas introduced was | 82 |
| Common air - - - | 13 |
| Oxygene - - - - | 70 |
| Common air - - - . | 1 |

* Decimals are omitted, because the excess of the two first numbers is exactly corrected by the deficiency of the last.

| | |
|---|---|
| Whole quantity of air thrown into the globe | 213 |
| From which ſubtract its capacity | 148 |
| The remainder is | 65 |

And this remainder taken from 80,5 nitrous gas + 36,9 oxygene, leaves 52,4 cubic inches, which is the ſpace occupied by the nitrous acid gas, and which differs from 53,95 only by 1,55 cubic inches.

I ought to have obſerved, that before this concluſive experiment, two ſimilar ones had been made. In comparing the reſults of one of them, performed with the aſſiſtance of my friend, Mr. Joseph Priestley, Dr. Priestley's eldeſt ſon, and chiefly detailed by him in the journal, I find a coincidence greater than could be even well expected, where the proceſſes are ſo complex. According to that experiment, 41,5 grains of nitrous acid gas fill a ſpace equal to 53 cubic inches, and are compoſed of nearly 29 nitrous gas, and 12,5 oxygene.

We may then conclude, First, that 100 cubic inches of nitrous acid, such as exists in the * aëriform state saturated with oxygene, at temperature 55°, and atmospheric pressure 30,1 weigh 75,17 grains.

Secondly, that 100 grains of it are composed of 68,06 nitrous gas, and 31,94 oxygene. Or assuming what will be hereafter proved, that 100 parts of nitrous gas consist of 55,95 oxygene, and 44,05 nitrogene, of 29,9 nitrogene, and 70,1 oxygene; or taking away decimals, of 30 of the one to 70 of the other.

Thirdly, that 100 grains of pale green solution of nitrous acid in water, of specific gravity 1,301, is composed of 50,62 water, and 49,38 acid of the above composition.

VI. Having thus ascertained the composition of a standard acid, my next object was to obtain it in a more condensed state, as it was otherwise impossible to saturate it to its full

* As is evident from the superabundant quantity of oxygene thrown into the globe.

extent with nitrous gas. But this I could effect in no other way than by comparing mixtures of known quantities of water, and acids of different specific gravities and colors, with the acid of 1,301.

For the purpose of combining my acids with water, I made use of a cylinder about 8 inches long, and ,3 inches in diameter, accurately graduated to grain measures, and furnished with a very tight stopper.

The concentrated acid was first slowly poured into it, and the water gradually added till the required specific gravity was produced;* the cylinder being closed and agitated after each addition, so as to produce combination without any liberation of elastic fluid.

After making a number of experiments with

* The weight of the acid poured into the cylinder being known, its specific gravity was known from the space it occupied in the phial. The weight of water being likewise known, the specific gravity of the solution, when the common temperature was produced, was given by the condensation.

acids of different colors in this advantageous way, I at length found that 90 grains of a deep yellow acid, of ſpecific gravity 1,5, became, when mingled at 40° with 77,5 grains of water, of ſpecific gravity 1,302, and of a light green tinge, as nearly as poſſible reſembling that of the ſtandard acid.

Suppoſing, then, that theſe acids contain nearly the ſame relative proportions of oxygene and nitrogene, 100 grains of the deep yellow acid of 1,5, are compoſed of 91,9 grains true nitrous acid,† and 8,1 grains of water.

To aſcertain the difference between the compoſition of this acid, and that of the pale, or nitric acid, of the ſame ſpecific gravity, I inſerted 150 grains of it into a ſmall cylindrical mattraſs of the capacity of ,5 cubic inches, accurately graduated to grain meaſures, and connected by a curved

† That is, ſuch as it exiſts in the aëriform ſtate at 55°. From the ſtrong affinity of nitrous acid for water, we may ſuppoſe that this acid gas contains a larger proportion of it than the other gaſes.

tube with the water apparatus. After heat had been applied to the bottom of the mattrafs for a few minutes, the color of the fluid gradually changed to a deep red, whilft the globules of gas formed at the bottom of the acid, were almoft wholly abforbed in paffing through it. In a fhort time deep red vapour began to fill the tube, and being condenfed by the water in the apparatus, was converted into a bright green fluid, at the fame time that minute globules of gas were given out. As the heat applied became more intenfe, a very fingular phænomenon prefented itfelf; the condenfed vapor, increafed in quantity, at length filled the curvature of the tube, and when expelled, formed itfelf into dark green fpherules, which funk to the bottom of the water, refted for a moment, and then refolved themfelves into nitrous gas.*

When the acid was become completely pale, it was fuffered to cool, and weighed. It had loft near 15 grains, and was of fpecific gravity

* This appearance will be explained hereafter.

1,491. 2 cubic inches and quarter of nitrous gas only were collected.

From this experiment evidently no conclusions could be drawn, as the nitrous gas had carried over with it much nitrous acid (in the form of what Dr. Priestley calls nitrous vapor) and was partially dissolved with it in the water.†

To ascertain, then, the difference between the pale and yellow acids, I was obliged to make use of synthesis, compared with analysis, carried on in a different mode, by means of the following apparatus.

VII. To the stop-cock of the upper cylinder of the mercurial airholder, a capillary tube was adapted, bent so as to be capable of introduction into an orifice in the stopper of a graduated phial similar to that employed for mingling acids with water, and sufficiently long to reach the bottom. With another orifice in the stopper of the phial was connected a similar tube cur-

† This phænomenon will be particularly explained hereafter.

ved, for the purpoſe of containing a fluid; and of increaſed diameter at the extremity.*

50 cubic inches of pure nitrous gas † were thrown into the mercurial apparatus. The graduated phial, containing 90 grains of nitric acid, of ſpecific gravity 1,5, was placed on the top of the airholding cylinder, and made to communicate with it by means of the ſtop-cock and firſt tube. Into the ſecond tube a ſmall quantity of ſolution of potaſh was placed. When all the junctures were carefully cemented, by preſſing on the air-holder, the nitrous gas was ſlowly paſſed into the phial, and abſorbed by the nitrous acid it contained; whilſt the ſmall quantities of nitrogene evolved, ſlowly drove forward the ſolution in the curved tube; from the height of which, as compared with that of the mercury in the conducting tube, the preſſure on the air in the cylinder was known.

* The outline only of this apparatus is given here, as far as was neceſſary to make the experiment intelligible; a detailed account of it, and of its general application, will be given in the appendix.

† That is, from nitrous acid and mercury.

In proportion as the nitrous gas was abſorbed, the phial became warm, and the acid changed color; it firſt became ſtraw-colored, then pale yellow, and when about $7\frac{1}{2}$ cubic inches had been combined with it, bright yellow. It had gained in weight nearly 3 grains, and was become of ſpecific gravity 1,496.

This experiment afforded me an approximation to the real difference between nitric and yellow nitrous acid; and learning from it that nitric acid was diminiſhed in ſpecific gravity by combination with nitrous gas, I procured a pale acid of ſpecific gravity 1,504.* After this acid had been combined in the ſame manner as before, with about 8 cubic inches of nitrous gas,§ it became nearly of ſpecific gravity 1,5, and had gained in weight about 3 grains.

* A pale acid of 1.52, by being converted into yellow acid, became nearly of ſpecific gravity 15,1.

§ It is impoſſible to aſcertain the quantity of gas abſorbed to more than a quarter of a cubic inch, as the firſt portions of nitrous gas thrown into the graduated cylinder are combined with the oxygene of the common air in it, to form nitrous acid, and hence the ſlight exceſs of weight.

Aſſuming the accuracy of this experiment as a foundation for calculation, I endeavoured in the ſame manner to aſcertain the differences in the compoſition of the orange-colored acids, and the acids containing ſtill larger proportions of nitrous gas.

93 grains of the bright yellow acid of 1,5 became, when 6 cubic inches of gas had been paſſed through it, orange colored and fuming, whilſt the undiſſolved gas increaſed in quantity ſo much as to render it impoſſible to confine it by the ſolution of potaſh. When 9 cubic inches had paſſed through, it became dark orange. It had gained in weight 2,75 grains, and was become of ſpecific gravity 1,48 nearly. Hence it was evident that much nitrous gas had paſſed through it undiſſolved. 25 cubic inches more of nitrous gas were now ſlowly ſent through it: it firſt became of a light olive, then of a dark olive, then of a muddy green, then of a bright green, and laſtly of a blue green. After its aſſumption of this color, the gas appeared to paſs through it unaltered, and large globules

of fluid, of a darker green than the reft, remained at the bottom of the cylinder, and when agitated, did not combine with it. The increafe of weight was only 1 grain, and the acid was of fpecific gravity 1,474 nearly.

In this experiment it was evident that the unabforbed nitrous gas had carried over with it a confiderable quantity of nitrous acid. I endeavoured to correct the errors refulting from this circumftance, by connecting the curved tube firft with a fmall water apparatus, and afterwards with a mercurial apparatus; but when the water apparatus was ufed, the greater part of the unabforbed gas was diffolved with the nitrous acid it held in folution, by the water; and when mercury was employed, the nitrous acid that came over was decompofed, and the quantity of nitrous gas evolved, in confequence increafed.

As it was poffible that a fmall deficiency of weight might arife from the red vapor given out during the proceffes of weighing and examining the acid in the laft experiment, 35 cubic inches of nitrous gas were very ſlowly paſſed through 90 grains of pale nitrous acid, of ſpecific gravity 1,5: it became of ſimilar appearance to that juſt deſcribed, had gained in weight 6,75 grains, and was become of ſpecific gravity 1,475.

Theſe experiments did not afford approximations ſufficiently accurate towards the compoſition of deoxygenated acids, containing more nitrous gas than the dark orange colored. To obtain them, a ſolution conſiſting of 94,25 grains of blue green, or perfectly nitrated acid, (if we may be allowed to employ the term), of ſpecific gravity 1,475, was inſerted into a graduated phial, and connected by a curved tube, with the mercurial airholder; in the conductor of which a ſmall quantity of water was inſerted to abſorb the nitrous acid which might be carried over by the gas. Heat was ſlowly applied to the phial, and nitrous gas given out with great rapidity. When 4 cubic inches were collected, the acid became dark olive, when 9 dark red, when 13 bright orange, and when 18 pale. It had loſt 31 grains, and when completely cool, was of ſpecific gravity 1,502 nearly. The water in the apparatus was tinged of a light blue; from whence we may conclude that ſome of the nitrous gas was abſorbed by it with the nitrous acid: but it will be hereafter proved that the orange colored acid is the moſt nitrated acid capable of combining undecompounded with water, and that the color it communicates to a large quantity of water, is light blue. If then we take 6,1 grains, the quantity of gas collected, from 31 the loſs, the remainder is 24,9, which reaſoning from the ſynthetical experiment, may be ſuppoſed to contain nearly 3 cubic inches of nitrous gas. Conſequently, 94,25 grains of dark green acid, of ſpecific gravity 1,475, are compoſed of nearly 21 cubic inches, or 7,2 grains of nitrous gas, and 87,05 grains of pale nitrous acid, of 1,504.

VIII. Comparing the different ſynthetical and analytical experiments, we may conclude with tolerable accuracy, that 92,75 grains of bright yellow, or ſtandard acid of 1,5, are compoſed of 2,75 grains of nitrous gas, and 90 grains of nitric acid of 1,504; but 92,75 grains of ſtandard acid contain 85,23 grains of nitrous acid, compoſed of about 27,23 of oxygene, and 58, nitrous gas: now from 58, take 2,75, and the remainder 55,25, is the quantity of nitrous gas contained in 90 grains of nitric acid of 1,504; conſequently, 100 grains of it are compoſed of 8,45 water, and 91,55 true acid, containing 61,32 nitrous gas, and 30,23 oxygene; or 27,01 nitrogene, and 64,54 oxygene: and the nitrogene in nitric acid, is to the oxygene as 1 to 2,389.

IX. My ingenious friend, Mr. James Thomson, has communicated to me ſome obſervations relating to the compoſition of nitrous acid (that is, the orange-colored acid), from which he draws a concluſion which is, in my opinion, countenanced by all the facts we are in poſſeſſion of, namely, "that it ought "not to be conſidered as a diſtinct and leſs "oxygenated ſtate of acid, but ſimply as nitric "or pale acid, holding in ſolution, that is, "looſely combined with, nitrous gas."*

It is impoſſible to call any ſubſtance a ſimple acid that is incapable of entering undecompounded into combination with the alkalies, &c; but it will appear hereafter that the ſalts called in the

* In a letter to me, dated Oct. 28, 1799, after giving an account of ſome experiments on the phlogiſtication of nitric acid by heat and light, he ſays, "It was from an "attentive examination of the manner in which the nitric "acid was phlogiſticated in theſe experiments, that I was "confirmed in the ſuſpicion I had long before entertained, "of the real difference between the *nitrous* and *nitric* acids. "It is not enough to ſhew that in the *nitrous* acid, (that is, "the nitric holding nitrous gas in ſolution), the proportion "of oxygene in the whole compound is leſs than that enter- "ing into the compoſition of the nitric acid, and that it is "therefore leſs oxygenated. By the ſame mode of reaſoning "we might prove that water, by abſorbing carbonic acid "gas, became leſs oxygenated, which is abſurd. Should "any one attempt to prove (which will be neceſſary to ſub- "ſtantiate the generally received doctrine) that the oxygene "of the nitrous gas combines with the oxygene of the acid, "and the nitrogene, in like manner, ſo that the reſulting acid, "when nitrous gas is abſorbed by nitric acid, is a binary "combination of oxygene and nitrogene, he would find it "ſomewhat more difficult than he at firſt imagined; it ap- "pears to me impoſſible. It is much more conſonant with

new nomenclature *nitrites*, cannot be directly formed. If, indeed, it could be proved, that the heat produced by the combination of nitrous acid with salifiable bases, was the only cause of the partial decomposition of it, and that when this process was effected in such a way as to prevent increase of temperature, no nitrous gas was liberated, the common

" experiment to suppose that nitrous acid is nothing more " than nitric acid holding nitrous gas in solution, which " might in conformity to the principles of the French " nomenclature, be called nitrate of nitrogene. The difficulty, " and in some cases the impossibility, of forming nitrites, " arises from the weak affinity which nitrous gas has for " nitric acid, compared with that of other substances; and " the decomposition of nitrous acid) that is, nitrate of " nitrogene) by an alkaline or metallic substance, is perfectly " analogous to the decomposition of any other nitrate, the " nitrous gas being displaced by the superior affinity of the " alkali for the acid.

" Agreeable to this theory, the salts denominated " *nitrites* are in fact triple salts, or ternary combinations of " nitric acid, nitrous gas, and salifiable bases."

This theory is perfectly new to me. Other Chemists to whom I have mentioned it, have likewise considered it as new. Yet in a subsequent letter Mr. Thomson mentions that he had been told of the belief of a similar opinion among the French Chemists.

theory might have ſome foundation ; but though dilute phlogiſticated nitrous acid combines * with alkaline ſolutions without decompoſition, yet no exceſs of nitrous gas is found in the ſolid ſalt : it is either diſengaged in proportion as the water is evaporated, or it abſorbs oxygene from the atmoſphere, and becomes nitric acid.

In proportion as the nitrous acids contain more nitrous gas, ſo in proportion do they more readily give it out. From the blue green acid it is liberated ſlowly at the temperature of 50°, and from the green likewiſe on agitation. The orange-coloured and yellow acids do not require a heat above 200° to free them of their nitrous gas ; and all the

* In ſome experiments made on the nitrites of potaſh, and of ammoniac, before I was well acquainted with the compoſition of nitric acid, I found that a light olive-colored acid of 1,28, was capable of being ſaturated by weak ſolutions of potaſh and ammoniac, without loſing any nitrous gas ; but after the evaporation of the neutraliſed ſolution, at very low temperatures, the ſalts in all their properties reſembled *nitrates*.

colored acids, when expoſed to the atmoſphere abſorb oxygene, and become by degrees pale.

If the nitrous vapour, i. e. ſuch as is diſengaged during the *denitration* of the colored acids, was capable of combining with the alkalies, it might be ſuppoſed a diſtinct acid, and called nitrous acid; and the acids of different colors might be conſidered ſimply as compounds of this acid with nitric acid; but it appears to be nothing more than a ſolution of nitric acid in nitrous gas, incapable of condenſation, undecompounded, and when decompounded and condenſed, conſtituting the dark green acid, which is immiſcible with water,† and uncombinable with the alkalies.‡

It ſeems therefore reaſonable, till we are in poſſeſſion of new lights on the ſubject, to conſider, with Mr. Thomſon, the deoxygenated or nitrous acids ſimply as ſolutious of nitrous gas

† As is evident from the curious appearance of the dark green ſpherules, repulſive both to water, and light green acid.

‡ That is, undecompounded.

in nitric acid, and as analogous to the ſolutions of nitrous gas in the ſulphuric and marine acids, &c. and the ſalts called nitrites, ternary combinations, ſimilar to the triple compounds compoſed of ſulphuric acid, metallic oxides, and nitrous gas.*

Suppoſing the truth of theſe principles according to the logic of the French nomenclature, there is no acid to which the term nitrous acid *ought* to be applied; but as it has been uſed to ſignify the acids holding in ſolution nitrous gas, it is perhaps better ſtill to apply it to thoſe ſubſtances, than to invent for them new names. A nomenclature, accurately expreſſing their conſtituent parts, would be too complex, and like all other nomenclatures founded upon theory, liable to perpetual alterations. Their compoſition is known from their ſpecific gravity and their colors; hence it is better to denote it by thoſe phyſical properties: thus orange nitrous acid, of ſpecific gravity 1,480, will ſignify a ſolution of nitrous

* The exiſtence of theſe bodies will be hereafter proved.

gas in nitric acid, in which the nitric acid is to the nitrous gas, nearly as 87 to 5, and to the water as 11 to 1.

X. The eſtimation of the compoſition of the yellow and orange colored nitrous acids given in the following table, may be conſidered as tolerably accurate, being deduced from the ſynthetical experiments in the ſixth ſection, compared with the analytical ones. But as in the ſynthetical experiment, when the acid became green, it was impoſſible to aſcertain the quantity of nitrous gas that paſſed through it unabſorbed, and as in the analyſis the quantity of nitrous gas diſſolved by the water at different periods of the experiment could not be aſcertained, the accounts of the compoſition of the green acids mnſt be conſidered only as very imperfect approximations to truth.

# TABLE I.

*Containing Approximations to the quantities of NITRIC ACID, NITROUS GAS, and WATER in NITROUS ACIDS, of different colors and specific gravities.*

| 100 Parts | | Specific gra. | | Nitric Acid | Water | Nitrous gas. |
|---|---|---|---|---|---|---|
| Sol. Nitric Acid | of | 1,504 | contain | 91,55 | 8,45 | — — |
| Yellow Nitrous‡ | | 1,502 | | 90,5 | 8,3 | 1,2 |
| Bright Yellow | | 1,500 | | 88,94 | 8,10 | 2,96 |
| Dark Orange | | 1,480 | | 86,84 | 7,6 | 5,56 |
| Light Olive ‡ | | 1,479 | | 86,00 | 7,55 | 6,45 |
| Dark Olive ‡ | | 1,478 | | 85,4 | 7,5 | 7,1 |
| Bright Green ‡ | | 1,476 | | 84,8 | 7,44 | 7,76 |
| Blue Green* | | 1,475 | | 84,6 | 7,4 | 8,00 |

* The blue green acid is not homogeneal in its compofition, it is compofed of the blue green fpherules and the bright green acid. The blue green fpherules are of greater fpecific gravity than the dark green acid, probably becaufe they contain little or no water.

‡ The compofition of the acids thus marked, is given from calculations.

## TABLE II.

*Binary Proportions of OXYGENE and NITROGENE in NITRIC and NITROUS ACIDS.**

| 100 Parts. | contain | Oxygene | Nitrogene | Proportions. Nitrogene. Unity. | Nitrogene | Oxygene |
|---|---|---|---|---|---|---|
| Nitric Acid | | 70,50 | 29,50 | | 1 | 2,389 |
| Bright yellow Nitrous | | 70,10 | 29,90 | | 1 | 2,344 |
| Orange coloured | | 69,63 | 30,37 | | 1 | 2,292 |
| Dark Green | | 69,08 | 30,92 | | 1 | 2,230 |

XI. I have before mentioned that dilute nitric acids are incapable of diſſolving ſo much nitrous gas in proportion to their quantities of true acid, as concentrated ones. During their abſorption of it, they go through ſimilar changes of color; 330 grains of nitric acid, of ſpecific gravity 1,36, after 50 cubic inches of gas had been paſſed through it, became blue green, and

* Nitrous gas contains 44,05 Nitrogene, and 55,95 Oxygene, as has been ſaid before.

of ſpecific gravity 1,351. It had gained in weight but 3 grains; and when the nitrous gas was driven from it by heat into a water apparatus, but 7 cubic inches were collected.*

From the diminution of ſpecific gravity of nitric acid by combination with nitrous gas, and from the ſmaller attraction of nitric acid for nitrous gas, in proportion as it is diluted, it is probable that the nitrated acids, in their combinations with water, do not contract ſo much as † nitric acids of the ſame ſpecific gravities. The affinities reſulting from the ſmall attraction of nitrous gas for water, and its greater attraction for nitric acid, muſt be ſuch as to leſſen the affinity of nitric acid and water for each other.

Hence it would require an infinite number of experiments to aſcertain the real quantities of acid, nitrous gas, and water, contained in the

* A great portion of it, of courſe, diſſolved in the water with the nitrous acid carried over.

† Their changes of volume, correſponding to changes of temperature, moſt probably, are likewiſe different.

different diluted nitrous acids; and after these quantities were determined, they would probably have no important connection with the chemical arrangement. As yet, our instruments of experiment are not sufficiently exact to afford us the means of ascertaining the ratio in which the attraction of nitric acid* for water diminishes in its progress towards saturation.

The estimations in the following table, of the real quantities of nitric acid in solutions of different specific gravities, were deduced from experiments made in the manner described in section VI, except that the phial employed was longer, narrower, and graduated to half grains. The temperature, at the time of combination, was from 40° to 46°.

* Probably in the ratio of the square of the quantity of water united to it.

# TABLE III.

*Of the Quantities of True NITRIC ACID in solutions of different SPECIFIC GRAVITIES.*

| 100 Parts Nitric Acid of ſpecfic gravity | | True Acid* | Water |
|---|---|---|---|
| 1,5040 | contain | 91,55 | 8,45 |
| 1,4475 | | 80,39 | 19,61 |
| 1,4285 | | 71,65 | 28,35 |
| 1,3906 | | 62,96 | 37,04 |
| 1,3551 | | 56,88 | 43,12 |
| 1,3186 | | 52,03 | 47,97 |
| 1,3042 | | 49,04 | 50,96 |
| 1,2831 | | 46,03 | 53,97 |
| 1,2090 | | 45,27 | 54,73 |

* The quantities of Oxygene and Nitrogene in any ſolution, may be thus found —— Let A = the true acid, X the oxygene, and Y the nitrogene,

Then $X = \frac{238\ A}{239}$ and $Y = \frac{A}{239}$

XII. The blue green ſpherules mentioned in ſection V. produced by the condenſation of nitrous vapor, and by the combination of nitric acid with nitrous gas, may be conſidered as ſaturated ſolutions of nitrous gas in nitric acid.* The combinations of nitric acid and nitrous gas containing a larger proportion of nitrous gas, are incapable of exiſting in the fluid ſtate at common temperatures; and, as appears from the firſt experiment, an increaſe of volume take place during their formation. They conſequently ought to be looked upon as ſolutions of nitric acid in nitrous gas, identical with the nitrous vapor of Prieſtley.

From the reſearches of this great diſcoverer, we learn that nitrous vapor is decompoſable, both by water and mercury. Hence it is almoſt impoſſible accurately to aſcertain its compoſition. In one of his experiments,‡ when more than 130 grains of ſtrong nitrous acid were expoſed

‡ Experiments and Obſervations; laſt edition, vol. 1, page 384.

for two days to nearly 247 cubic inches of nitrous gas, over water: about half of the acid was diſſolved, and depoſited with the gas in the water.§

XIII. In comparing the reſults of my fundamental experiment on the compoſition of nitrous acid, with thoſe of Cavendiſh, the great coincidence between them gave me very high ſatisfaction, as affording additional proofs of accuracy. If the acid formed in the laſt experiment of this illuſtrious philoſopher be ſuppoſed analogous to the light green acid formed in my firſt experiment, our eſtimations will be almoſt identical.

Lavoiſier's account of the compoſition of the nitric and nitrous acids, has been generally adopted. According to his eſtimation, theſe ſubſtances contain a much larger quantity of oxygene than I have aſſigned to them.

§ Nitrous gas, holding in ſolution nitrous acid, is more readily abſorbed by water than when in its pure form, from being preſented to it in a more condenſed ſtate in the green acid, formed by the contact of water and nitrous vapor.

The fundamental experiments of this great philosopher were made at an early period of pneumatic chemiſtry,* on the decompoſition of nitre by charcoal; and he conſidered the nitrogene evolved, and the oxygene of the carbonic acid produced in this proceſs, as the component parts of the nitric acid contained in the nitre.

I have before mentioned the liberation of nitrous acid, in the decompoſition of nitre by combuſtible bodies; and I had reaſons for ſuſpecting that this circumſtance was not the only ſource of inaccuracy.

That my ſuſpicions were well founded, will appear from the following experiments:

EXPERIMENT *a.* I introduced into a ſtrong glaſs tube, 3 inches long, and nearly ,3 wide, a mixture of 10 grains of pulveriſed, well burnt charcoal, and 60 grains of nitre. It was fired by means of touch-paper, and the tube inſtantly plunged under a jar filled with

* Mem. des Savans Etrangers, v. xi. 226. Vide Kirwan ſur le phlogiſtique pag. 110.

dry mercury. A quantity of gas, clouded with denſe white vapor was collected. When this vapor was precipitated, ſo that the ſurface of the mercury could be ſeen, it appeared white, as if acted on by nitrous acid. On introducing a little oxygene into the jar, copious red fumes appeared.

EXP. *b*. A ſimilar mixture was fired* under the jar, the top of the mercury being covered with a ſmall quantity of red cabbage juice, rendered green by an alkali. This juice, examined when the vapor was precipitated, was become red, and on introducing to it a little carbonate of potaſh, a ſlight effervеſcence took place.

EXP. *c*. Five grains of charcoal, and 20 of nitre, were now fired in the ſame manner as before, the mercury being covered with a ſtratum of water. After the precipitation of the vapor

* In this experiment, as well as in the laſt, ſome of the mixture was thrown into the jar undecompounded.

on the introduction of oxygene, no red fumes were perceived.

EXP. *d.* 30 grains of nitre, 5 of charcoal, and five of silicious earth,* were now mingled and fired. The gas received under mercury was composed of 18 carbonic acid, and nearly 12 nitrogene.† A little muriatic acid was poured on the residuum in the tube; a slight effervescence took place.

EXP. *e.* The top of the mercury in the jar was now covered with a little diluted muriatic acid, and a small glass tube filled with a mixture of 3 grains of charcoal, and 20 nitre. After the deflagration, the tube itself with the residuum it contained, were thrown into the jar. The carbonic acid was quickly detached from them by the muriatic acid, and the whole quan-

* To detach the potash from the carbonic acid.

† This nitrogene contained a little nitrous gas, as it gave red fumes when exposed to the air. The free nitrous acid was decomposed by the mercury, as it was not covered with water.

tity of gas generated in the proceſs, obtained; it meaſured 15 cubic inches.

4 cubic inches of it expoſed to ſolution of potaſh, diminiſhed to 1 $\frac{4}{10}$; 7 of the remainder, with 8 of oxygene, gave only 12.

EXP. *f.* 60 grains of nitre, and 9 of charcoal were fired, the top of the mercury in the jar being covered with water. After the deflagration, the tube that had contained them was introduced, and the carbonic acid contained by the carbonate of potaſh, diſengaged by muriatic acid. 30 meaſures of the gaſes evolved were expoſed to cauſtic potaſh; 20 exactly were abſorbed, the 10 remaining, with 10 of oxygene, diminiſhed to 17.

EXP. *g.* A mixture of nitre and charcoal were deflagrated over a little water, in the mercurial jar: after the precipitation of the vapor, the water was abſorbed by filtrating paper. This filtrating paper, heated in a ſolution of potaſh, gave a faint ſmell of ammoniac.

EXP. *h.* Water impregnated with the vapor produced in the deflagration, was heated with quicklime, and presented separately to three persons accustomed to chemical odors. Two of them instantly recognised the ammoniacal smell, the other could not ascertain it. Paper reddened with cabbage juice was quickly turned green by the vapor.

These experiments are sufficient to shew that the decomposition of nitre by charcoal is a very complex process, and that the intense degree of heat produced may effect changes in the substances employed, which we are unable to estimate.

The products, instead of being simply carbonic acid, and nitrogene, are carbonic acid, nitrogene, nitrous acid, probably ammonia, and sometimes nitrous gas. The nitrous acid is disengaged from the base by the intense heat. Concerning the formation of the ammonia, it is useless to reason till we have obtained unequivocal testimonies of its existence; it may be produced either by the decomposition of the water contained in the nitre, by the combination of its oxygene with the charcoal, and of its nascent hydrogene with the nitrogene of the nitric acid ; or from ſome unknown decompoſition of the potaſh.

As neither Lavoiſier nor Berthollet found nitrous gas produced in the decompoſition of nitre by charcoal, when a water apparatus was employed ; and as it was not uniformly evolved in my experiments, the moſt probable ſuppoſition is, that it ariſes from the decompoſition of a portion of the free nitrous acid intenſely heated, by the mercury.

In none of my experiments was the whole of the nitre and charcoal decompoſed, ſome of it was uniformly thrown with the gaſes into the mercurial apparatus. The nitrogene evolved, as far as I could aſcertain by the common teſts, was mingled with no inflammable gas.

If we conſider experiment *f* as accurate, with regard to the relative quantities of carbonic acid and nitrogene produced, they are to each other nearly as 20 to 8 ; that is, allowing 2 for the nitrous gas, and conſequently, reaſoning in the ſame manner as Lavoiſier, concerning the compoſition of nitric acid, it ſhould be compoſed of 1 nitrogene to 3,38 oxygene. But though the quantity of oxygene in this eſtimation is far ſhort of that given in his, yet ſtill it is too much. From whatever ſource the errors ariſe, whether from the evolution of phlogiſticated nitrous acid, or the decompoſition of water, or the production of nitrous gas, they all tend to increaſe the proportion of the carbonic acid to the nitrogene.

I am unacquainted with any experiment from which accurate opinions concerning the different relative proportions of oxygene and nitrogene in the nitric and nitrous acids could be deduced. Lavoiſier's calculation is founded on his fundamental experiment, and on the combination of nitrous gas and oxygene.

Dr. Prieſtley's experiment mentioned in ſection 12, on the abſorption of nitrous gas by nitrous acid, from which Kirwan* deduces the compoſition of the differently colored nitrous acids, was made over water, by which, as is

* Eſſay on phlogiſton.

evident from a minute examination of the facts†, the greater portion of the nitrous gas employed was absorbed.

XIV. The opinions heretofore adopted respecting the quantities of real or true acid in solutions of nitrous acid of different specific gravities, have been founded on experiments made on the nitro-neutral salts, the most accu-

† Dr. Priestley says, "Having filled a phial containing "exactly the quantity of four pennyweights of water, with "strong, pale, yellow spirit of nitre, with its mouth quite "close to the top of a large receiver standing in water, I "carefully drew out almost all the common air, and then "filled it with nitrous air; and as this was absorbed, I kept "putting in more and more, till in less than two days it "had completely absorbed 130 ounce measures. Presently "after this process began, the surface of the acid assumed "a deep orange color, and when 20 or 30 ounce measures "of air were absorbed, it became green at the top: this "green descended lower and lower, till it reached the "bottom of the phial. Towards the end of the process, "the evaporation was perceived to be very great, and when "I took it out, the quantity was found to have diminished "to one half. Also it had become, by means of this pro-"cess, and the evaporation together, exceeding weak, and "was rather blue than green."

*Experiments and Observations*, vol. 1, p. 384. Last edition.

rate of which are thoſe of Kirwan, Bergman, and Wenzel. The great difference in the reſults of theſe celebrated men, proves the difficulty of the inveſtigation, and the exiſtence of ſources of error.* Kirwan deduces the compoſition of the ſolutions of nitrous acid in water, from an experiment on the formation of nitrated ſoda. In this experiment, 36,05 grains of ſoda were ſaturated by 145 grains of nitrous acid, of ſpecific gravity 1,2754. By a teſt experiment, he found the quantity of ſalt formed to be 85,142 grains.† Hence he concludes that 100 parts of nitrous acid, of ſpecific gravity 1,5543, contain 73,54 of the ſtrongeſt, or moſt concentrated acid.

Suppoſing his eſtimation perfectly true, 100 parts of the aëriform acid of 55° would be compoſed of 74,54 of his real acid, and 25,46 water. In examining, however, one of his later

* See Mr. Keir's excellent obſervations on this ſubject. Chem. Dict. Art. Acid.

† Iriſh Tranſactions, vol. 4, p. 34.

experiments,* we ſhall find reaſons for concluding, that the acid in nitrated ſoda cannot contain much leſs water than the aëriform acid. A ſolution of carbonated ſoda, containing 125 grains of real alkali, was ſaturated by 306,2 grains of nitrous acid, of ſpecific gravity 1,416. The evaporation was carried on in a temperature not exceeding 120°, and the reſiduum expoſed to a heat of 400° for ſix hours, at the end of which time it weighed 308 grains. Now according to my eſtimation, 306 grains of nitric acid, of 1,416, ſhould contain 215 true acid; and we can hardly ſuppoſe, but that during the evaporation and conſequent long expoſure to heat, ſome of the nitrated ſoda was loſt with the water.

Bergman eſtimates the quantity of water in this ſalt at 25, and the acid at 43 per cent; but his real acid was not ſo concentrated as Kirwan's, conſequently the nitric acid in nitrated ſoda ſhould contain more water than my true acid.

* Addit. Obſ. pag. 74.

Wenzel, from an experiment on the compofition of nitrated foda, concludes that it contains 37,48 of alkali, and 62,52 of nitrous acid; and 1000 of this acid, from Kirwan's calculation, contain 812,6 of his real acid; confequently, 100 parts of my aëriform acid fhould contain 93,28 of Wenzel's acid, and 6,72 of water.

I faturated with potafh 54 grains of folution of nitric acid, of fpecific gravity 1,301. Evaporated at about 212°, it produced 66 grains of nitre. This nitre expofed to a higher temperature, and kept in fufion for fome time, was reduced to 60 grains.

Now from the table, 54 of 1,301, fhould contain 26,5 of true acid. But according to Kirwan's eftimation, 100 parts of dry nitre contain 44* of his real acid, with 4 water; confequently 60 fhould contain 26,4.

Again, 90 grains of acid, of fpecific gravity 1,504, faturated with potafh, and treated in

* Additional Obfervations, page 70.

the ſame manner, gave 173 grains of dry nitre. Conſequently, 100 parts of it ſhould contain 47,3 grains of true acid.

Now Lavoiſier † allows about 51 of dry acid to 100 grains of nitre, and Wenzel 52.

From Berthollet's‡ experiments, 100 grains of nitre, in their decompoſition by heat, give out nearly 49 grains of gas.§

Hence it appears that the aëriform acid, that is, the true acid of my table, contains rather leſs water than the acid ſuppoſed to exiſt in nitre.

† Elements, pag. 103, Kerr's Tranſlation.

‡ Mem. Acad. 1787.

§ As well as oxygene and nitrogene, Mr. Watt's experiments prove that much phlogiſticated nitrous acid is produced.

# DIVISION II.

*EXPERIMENTS and OBSERVATIONS on the composition of AMMONIAC and on its combinations with WATER and NITRIC ACID.*

## *I. Analysis of AMMONIAC or VOLATILE ALKALI.*

THE formation and decompoſition of volatile alkali in many proceſſes, was obſerved by Prieſtley, Scheele, Bergman, Kirwan, and Higgins; but to Berthollet we owe the diſcovery of its conſtituent parts, and their proportions to each other. Theſe proportions this excellent philoſopher deduced from an experiment on the decompoſition of aëriform ammoniac by the electric ſpark:* a proceſs in which no apparent ſource of error exiſts.

* Journal de Phyſique. 1786. Tom. 2, pag. 176.

Since, however, his eſtimations have been made, the proportions of oxygene and hydrogene in water have been more accurately determined. This circumſtance, as well as the conviction of the impoſſibility of too minutely ſcrutinizing facts, fundamental to a great maſs of reaſoning, induced me to make the following experiments.

A porcelain tube was provided, open at both ends, and well glazed inſide and outſide, its diameter being about ,5 inches. To one end of this, a glaſs tube was affixed, curved for the purpoſe of communicating with the water apparatus. With the other end a glaſs retort was accurately connected, containing a mixture of perfectly cauſtic ſlacked lime, and muriate of ammoniac.

The water in the apparatus for receiving the gaſes had been previouſly boiled, to expel the air it might contain, and during the experiment was yet warm.

When the tube had been reddened in a furnace adapted to the purpoſe, the flame of a ſpirit lamp was applied to the bottom of the retort. A great quantity of gas was collected in the water apparatus; of this the firſt portions were rejected, and the laſt transferred to the mercurial trough.

A ſmall quantity examined, did not at all diminiſh with nitrous gas, and burnt with a lambent white flame, in contact with common air.

$2\frac{3}{4}$ of this gas, equal to 110 grain meaſures, were fired with 2, equal to 80, of oxygene, in a detonating tube, by the electric ſpark. They were reduced to $2\frac{1}{4}$, or 90. On introducing to the remainder a ſolution of ſtrontian,* it became ſlightly clouded on the top, and an abſorption of ſome grain meaſures took place.

It was evident, then, that in this experiment, charcoal * had been ſomehow preſent in the

* Though the tube had never been uſed, and was apparently clean and dry on the inſide, it muſt have contained ſomething in the form of duſt, capable of furniſhing either hydro-carbonate, or charcoal.

tube; which being diſſolved by the naſcent hydrogene, had rendered it ſlightly carbonated, and in conſequence made the reſults inconcluſive.

A tube of thick green glaſs carefully made clean, was now employed, incloſed in the porcelain tube. Every other precaution was taken to prevent the exiſtence of ſources of error, and the experiment conducted as before.

140 grain meaſures of the gas produced, fired with 120 of oxygene, left, in two experiments, nearly 110. Solution of ſtrontian placed in contact with the reſiduum, did not become clouded, and no abſorption was perceived.

Now 150 meaſures of gas were deſtroyed, and if we take Lavoiſier's and Meuſnier's eſtimation of the compoſition of water, and ſuppoſe the weight of oxygene to be 35 grains, and that of hydrogene 2,6 the hundred cubic inches; the oxygene employed will be to the hydrogene as 243 to 576. Put $x$ for the oxygene, and $y$ for the hydrogene.

Then $x + y = 150$

$$x : y :: 243 : 576$$

$$x = \frac{243\,y}{576}$$

$$819\,y = 86400$$

$$y = 105 \qquad x = 45$$

And $140 - 105 = 35$

Confequently, the nitrogene in ammoniac is to the hydrogene as 35 : 105 in volume: and 13,3 grains of ammoniac are compofed of 10,6 nitrogene, (fuppofing that 100 cubic inches weigh 30,45 grains) and 2,7 hydrogene.

According to Berthollet, the weight of the nitrogene in ammoniac is to that of the hydrogene as 121 to 29.* The difference between this eftimation and mine is fo fmall as to be almoft unworthy of notice, and arifes moft probably from the flight difference between the accounts of Lavoifier and Monge, of the compofition of water, and the different weights affigned to the gafes employed.

* Journal de Phyfique, 1786, t. 2, 177.

We may then conclude, that 100 grains of ammoniac are compofed of about 80 nitrogene, and 20 hydrogene.

The decompofition of ammoniac by heat, as well as by the electric fpark, was firft difcovered by Prieftley. In an experiment† when aëriform ammoniac was fent through a heated tube from a cauftic folution of ammoniac in water, this great difcoverer obferved that an inflammable gas was produced, though in no great quantity, and that a fluid blackened by matter, probably carbonaceous, likewife came over.

In my experiments the whole of the ammoniac appeared to be decompofed; the quantity of gas generated was immenfe, and not clouded, as is ufually the cafe with gafes generated at high temperatures. It is poffible, that the larger quantity of water carried over in his experiment, by its ftrong attraction for ammoniac in the aëriform ftate, might have, in fome meafure, retarded the decompofition. It is how-

* Phil. Tranf. vol. 79, page 294.

ever, more probable to ſuppoſe, that a fiſſure exiſted in the earthen tube he employed, through which a certain quantity of gas eſcaped, and coaly matter entered.

Prieſtley found that the metallic oxides when ſtrongly heated, decompoſed ammoniac, the metal being revivified and water and nitrogene produced.* The eſtimations of the compoſition of ammoniac that may be deduced from his experiments on the oxide of lead, differ very little from thoſe already detailed.

## II. *Specific gravity of Ammoniac.*

From the great ſolubility of ammoniac in water, it is difficult to aſcertain its ſpecific gravity in the ſame manner as that of a gas combinable to no great extent with that fluid. It is impoſſible to prevent the exiſtence of a

* Vol. 2, page 398.

ſmall quantity of ſolution of ammoniac in the mercurial airholder,† or apparatus containing the gas; and during the diminution of the preſſure of the atmoſphere on this ſolution,‡ a certain quantity of gas is liberated from it, and hence a ſource of error.

To aſcertain, then, the weight of ammoniac, I employed an apparatus ſimilar to that uſed for the abſorption of nitrous gas by nitric acid.

50 cubic inches of gas were collected in the mercurial airholder, from the decompoſition of muriate of ammoniac by lime; thermometer being 58°, and barometer 29,6.

100 grains of diluted ſulphuric acid were introduced into the ſmall graduated cylinder, which after being carefully weighed, was made

† Ammoniac generated at a temperature above that of the atmoſphere, always depoſits ammoniacal ſolution during its reduction to the common temperature.

‡ By the introduction of aëriform ammoniac into the exhauſted globe.

to communicate with the airholder, the curved tube containing a ſmall quantity of water. The gas was ſlowly paſſed into the fluid, and the globules wholly abſorbed before they reached the top; much increaſe of temperature being conſequent. When the abſorption was compleat, the phial was increaſed in weight exactly 9 grains.

This experiment was repeated three times. The difference of weight, which was probably connected with alterations of temperature and preſſure, never amounted to more than one ſixth of a grain.

We may then conclude, that at temperature 58°, and atmoſpheric preſſure 29,6, 100 cubic inches of ammoniac weigh 18 grains.

According to Kirwan, 100 cubic inches of alkaline air * weigh 18,16 grains; barometer $30_{0}$, thermometer 61. The difference between theſe eſtimations, the corrections for temperature and preſſure being made, is trifling.

* Additional Obſervations, page 107.

III. *Of the quantities of true Ammoniac in Aqueous Ammoniacal Solutions, of different specific gravities.*

To aſcertain the quantities of ammoniac, ſuch as exiſts in the aëriform ſtate, ſaturated with moiſture, in ſolutions of different ſpecific gravities, I employed the apparatus for abſorption ſo often mentioned. Thermometer being 52°, the mercurial airholder was filled with ammoniacal gas, and the graduated phial, containing 50 grains of pure water, connected with it. During the abſorption of the gas, the phial became warm. When about 30 cubic inches had been paſſed through, it was ſuffered to cool, and weighed: it had gained 5,25 grains, and the fluid filled a ſpace equal to that occupied by 57* grains of water.

* It is neceſſary in theſe experiments, that the greateſt care be obſerved in the introduction and extraction of the capillary tube. If it is introduced dry, there will be a ſource of error from the moiſture adhering to it when taken out. I therefore always wetted it before its introduction, and took care that no more fluid adhered to it after the experiment, than before.

Confequently, 100 grains of folution of ammoniac in water of fpecific gravity ,9684 contain 9,502 grains of ammoniac.

The apparatus being adjufted as before, 50 grains of pure water were now perfectly faturated with ammoniac. They gained in weight 17 grains, and when perfectly cool, filled a fpace equal to 74 of water Confequently 100 grains of aqueous ammonial folution of fpecific gravity ,9054 contain 25,37 grains of ammoniac.

The two folutions were mingled together; but no alteration of temperature took place. Confequently the refulting fpecific gravity might have been found by calculation.

On mingling a large quantity of cauftic folution of ammoniac with $\frac{1}{4}$ of its weight of water, of exactly the fame temperature, no alteration of it was perceptible by a fenfible thermometer.—Hence the two experiments* being affumed as

* Previous to thofe experiments, I had made a number of others on the combination of ammoniac with water.—My defign was, to afcertain the diminution of fpecific

data, the intermediate estimations in the following table, were found by calculation.

gravity for every three grains of ammoniac absorbed; but this I found impossible. The capillary tube, when taken out of the phial, always carried with it a minute portion of the solution, which partially evaporated before it could be again introduced; and thus the sources of error increased in proportion to the number of examinations.

# TABLE IV.

*Of approximations to the quantities of AMMONIAC, such as exists in the aëriform state, saturated with water at 52°, in AQUEOUS AMMONIACAL SOLUTIONS of different specific gravities.*

| 100 Specific gra. | | Ammoniac | Water. |
|---|---|---|---|
| 9054 | contain | 25,37 | 74,63 |
| 9166 | | 22,07 | 77,93 |
| 9255 | | 19,54 | 80,46 |
| 9326 | | 17,52 | 82,48 |
| 9385 | | 5,88 | 84,12 |
| 9435 | | 14,53 | 85,47 |
| 9476 | | 13,46 | 86,54 |
| 9513 | | 12,40 | 87,60 |
| 9545 | | 11,56 | 88,44 |
| 9573 | | 10,82 | 89,18 |
| 9597 | | 10,17 | 89,83 |
| 9619 | | 9,60 | 90,40 |
| 9684 | | 9,50 | 90,5 |
| 9639 | | 9,09 | 90,91 |
| 9713 | | 7,17 | 92,83 |

* As yet no mode has been difcovered for obtaining gafes in a ftate of abfolute drynefs; confequently we are ignorant of the different quantities of water they hold in folution at different temperatures. As far as we are acquainted with the combinations of ammoniac, there is no ftate in which it exifts fo free from moifture, as when aëriform, at low temperatures.

That no confiderable fource of error exifted in the two experiments, is evident from the trifling difference between the eftimations of the quantities of real ammoniac, in the folution of ,9684, as found in the firft experiment, and as given by calculation from the laft.

The quantity of ammoniac in a folution of fpecific gravity not in the table, may be thus determined—Find the difference between the two fpecific gravities neareft to it in the table; *d*, and the difference between their quantities of alkali, *b*; likewife the difference between the given fpecific gravity and that neareft to it, *c*.

then $d : b :: c : x$ and $x = \frac{b\,c}{d}$

Which, added to the quantity of the lower fpecific gravity, is the alkali fought.

The differences in fpecific gravity of the folutions of ammoniac at temperatures between 40° and 65° * are fo trifling as to be hardly

* The expanfion from increafe of temperature is probably great in proportion to the quantity of ammoniac in the folution.

afcertainable, by our imperfect inftruments, and confequently are unworthy of notice.

It is poffible at very low temperatures to obtain ammoniacal folutions of lefs fpecific gravity than ,9, but they are incapable of being kept for any length of time under the common preffure of the atmofphere.

IV. *Combinations of Ammoniac with Nitric Acid. Compofition of Nitrate of Ammoniac, &c.*

200 grains of ammoniacal folution, of fpecific gravity ,9056, were faturated by 385,5 grains of nitric acid, of fpecific gravity 1,306. The combination was effected in a long phial, the nitrous acid added very flowly, and the phial clofed after every addition, to prevent any evaporation in confequence of the great increafe of temperature.† The fpecific gravity of the folution, when reduced to the common temperature, was 1,15. Evaporated at a heat of

† From the combination.

212°,‡ it gave 254 grains of ſalt of fibrous cryſtalization. This ſalt was diſſolved in 331 grains of water; the ſpecific gravity of the ſolution was 1,148 nearly.

Hence it was evident that ſome of the ſalt had been loſt during the evaporation.

To find the quantity loſt, fibrous nitrate of ammoniac was diſſolved in ſmall quantities in the ſolution, the ſpecific gravity of which was examined after every addition of 3 grains. When 16 grains had been added to it, it became of 1,15.

Conſequently, the ſolution compoſed of 200 grains of ammoniacal, and of 385,5 of nitric acid ſolution, contained 262 grains of ſalt of fibrous cryſtalization, and of this ſalt 8 grains were loſt during the evaporation.

But the alkali in 200 grains of ammoniacal ſolution of ,9056 = 50,5 grains. And the true nitric acid in 385,5 grains of ſolution of 1,306 = 190 grains.

‡ I had before proved that at this temperature the ſalt neither decompoſed nor ſublimed.

Then 262 — 240,5 = 21,5, the quantity of water.

And 262 grains of fibrous cryſtalized nitrate of ammoniac, contain 190 grains true acid, 50,5 ammoniac, and 21,5 water. And 100 parts contain 72,5 acid, 19,3 ammoniac, and 8,2 water.

In proportion as the temperature employed for the evaporation of nitro-ammoniacal ſolutions, is above or below 212°, ſo in proportion does the ſalt produced contain more or leſs water than the fibrous nitrate. But whatever may have been the temperature of evaporation, the acid and alkali appear always to be in the ſame proportions to each other.

Of the ſalts containing different quantities of water, two varieties muſt be particularly noticed. The priſmatic nitrate of ammoniac, produced at the common temperatures of the atmoſphere, and containing its full quantity of water of cryſtaliſation; and the compact nitrate of ammoniac, either amorphous, or compoſed of delicately needled cryſtals, formed at 300°, and containing but little more water than exifts in nitric acid and ammoniac.

To difcover the compofition of the prifmatic nitrate of ammoniac, 200 grains of fibrous falt were diffolved in the fmalleft poffible quantity of water, and evaporated in a temperature not exceeding 70°. The greater part of the falt was compofed of perfectly formed tetrahædral prifms, terminated by tetrahædral pyramids. It had gained in weight about 8,5 grains.

Confequently 100 grains of prifmatic nitrate of ammoniac may be fuppofed to contain 69,5 acid, 18,4 ammoniac, and 12,1 water.

To afcertain the compofition of the compact nitrate of ammoniac, I expofed in a deep porcelain cup, 400 grains of the fibrous falt, in a temperature below 300°. It quickly became fluid, and flowly gave out its water without any ebullition, or liberation of gas. When it was become perfectly dry, it had loft 33 grains. I fufpected, that in this experiment fome of the falt had been carried off with the water; to determine this, I introduced into a fmall glafs retort, 460 grains of fibrous ſalt; it was kept at a heat below 320°, in communication with a mercurial apparatus, in a regulated air-furnace, till it was perfectly dry: it had loſt 23 grains. No gas, except the common air of the retort came over, and the fluid collected had but a faint taſte of nitrate of ammoniac.

Though in this experiment I had removed all the fluid retained in the neck of the retort, ſtill a few drops remained in the head, and on the ſides, which I could not obtain. It was of importance to me to be accurately acquainted with the compoſition of the compact ſalt, and for that reaſon I compared theſe analytical experiments with a ſynthetical one.

I ſaturated 200 grains of ſolution of ammoniac, of ,9056 with acid, aſcertained the ſpecific gravity of the ſolution, evaporated it at 212°, and fuſed and dried it at about 300°—260°. It gave 246 grains of ſalt, and a ſolution made of the ſame ſpecific gravity as that evaporated, indicated a loſs of 9 grains. Conſequently, 255 grains of this ſalt contain 50.5

grains alkali, 190 grains acid, and 14,5 grains water.

We may then conclude, that 100 parts of compact nitrate of ammoniac contain 74,5 acid, 19,8 alkali, and 5,7 water.

### V. *Decompoſition of Carbonate of Ammoniac by Nitric Acid.*

In my firſt experiments on the production of nitrate of ammoniac, I endeavoured to aſcertain its compoſition by decompounding carbonate of ammoniac by nitric acid; and in making for this purpoſe, the analyſis of carbonate of ammoniac, I diſcovered that there exiſted many varieties of this *ſalt*, containing very different proportions of carbonic acid, alkali, and water; the carbonic acid and water being ſuperabundant in it, in proportion as the temperature of its formation was low, and the alkali in proportion as it was high: and not only that a different ſalt was formed at every different temperature, but likewiſe that the difference in them was fo great, that the carbonate of ammoniac formed at 300° contained more than 50 per cent alkali, whilft that produced at 60° contained only 20.*

I found 210 grains of carbonate of ammoniac, which from comparifon with other falts previoufly analifed, I fufpected to contain about 20 or 21 per cent alkali, faturated by 200 grains of nitric acid of 1,504. But though the carbonate was diffolved in much water, ftill, from the fmell of the carbonic acid generated, I fufpect that a fmall portion of the nitric acid was diffolved, and carried off by it. The folution, evaporated at about 200°, and afterwards expofed to a temperature below 300°, gave 232 grains of compact falt. But reafoning from the quantity of acid in 200 grains of nitric acid of 1,504, it ought to have given 245. Confequently 13 were loft by

* A particular account of the experiments from which thefe facts were deduced, was printed in September, and will appear in the firft volume of the *Refearches.*

evaporation; and this loſs agrees with that in the other experiments.

V. *Decompoſition of Sulphate of Ammoniac by Nitre.*

As a cheap mode of obtaining nitrate of ammoniac, Dr. BEDDOES propoſed to decompoſe nitre by ſulphate of ammoniac, which is a well known article of commerce. From ſyntheſis of ſulphate of ammoniac, compared with analyſis made in Auguſt 1799,* I concluded that 100 grains of priſmatic ſalt were compoſed of about 18 grains ammoniac, 44 acid, and 38 water; and ſuppoſing 100 grains of nitre to contain 50 acid, 100 grains of ſulphate of ammoniac will require for their decompoſition 134 grains of nitre, and form 90,9 grains of compact nitrate of ammoniac.

* And which will be publiſhed, with an account of its perfect decompoſition at a high temperature, in the *Reſearches.*

To aſcertain if the ſulphate of potaſh and nitrate of ammoniac could be eaſily ſeparated, I added to a heated ſaturated ſolution of ſulphate of ammoniac, pulveriſed nitre, till the decompoſition was complete. After this decompoſition, the ſolution contained a ſlight exceſs of ſulphuric acid, which was combined with lime, and the whole ſet to evaporate at a temperature below 250°. As ſoon as the ſulphate of potaſh began to cryſtaliſe, the ſolution was ſuffered to cool, and then poured off from the cryſtaliſed ſalt, which appeared to contain no nitrate of ammoniac. After a ſecond evaporation and cryſtaliſation, almoſt the whole of the ſulphate appeared to be depoſited, and the ſolution of nitrate of ammoniac was obtained nearly pure: it was evaporated at 212°, and gave fibrous cryſtals.

### VI. *Non-exiſtence of Ammoniacal Nitrites.*

I attempted in different modes to combine *nitrous* acids with ammoniac, ſo as to form the ſalts which have been ſuppoſed to exiſt, and called *nitrites* of ammoniac; but without ſucceſs.

I firſt decompoſed a ſolution of carbonate of ammoniac by dilute olive colored acid; but in this proceſs, though no heat was generated, yet all the nitrous gas appeared to be liberated with the carbonic acid.* I then combined a ſmall quantity of nitrous gas, with a ſolution of nitrate of ammoniac. But after evaporating this ſolution at 70°—80°, I could not detect the exiſtence of nitrous gas in the ſolid ſalt; it was given out during the evaporation and cryſtaliſation, and formed into nitrous acid by the oxygene of the atmoſphere. I likewiſe heated nitrate of ammoniac to different degrees, and partially decompoſed it, to aſcertain if in any caſe the acid was phlogiſticated by heat: but in no experiment could I detect the exiſtence

* When nitrous gas exiſts in neutro-ſaline ſolutions, they are always colored more or leſs intenſely, from yellow to olive, in proportion to the quantity combined with them.

of *nitrous* acid in the heated ſalt, when it had been previouſly perfectly neutraliſed.

When nitrate of ammoniac, indeed, with exceſs of nitric acid, is expoſed to heat, the ſuperabundant nitric acid becomes phlogiſticated, and is then liberated from the ſalt, which remains neutral.*

We may therefore conclude that nitrous gas has little or no affinity for ſolid nitrate of ammoniac, and that no ſubſtance exiſts to which the name *nitrite* of ammoniac can with propriety be applied.

## VII. *Of the ſources of error in Analyſis.*

To compare my ſyntheſis of nitrate of ammoniac with analyſis, I endeavoured to ſeparate the ammoniac and nitric acid from each other, without decompoſition. But in going through the analytical proceſs, I ſoon diſcovered that

* Hence a nitrate of ammoniac with exceſs of acid, when expoſed to heat, firſt becomes yellow, and then white.

it was impoſſible to make it accurate, without many collateral laborious experiments on the quantities of ammoniac ſoluble in water at different temperatures.

At a temperature above 212°, I decompoſed, by cauſtic ſlacked lime, 50 grains of compact nitrate of ammoniac in a retort communicating with the mercurial airholder, the moiſture in which had been previouſly ſaturated with ammoniac. 22 cubic inches of gas were collected at 38°, and from the loſs of weight of the retort, it appeared that 13 grains of ſolution of ammoniac in water, had been depoſited by the gas.

Now evidently, this ſolution muſt have contained much more alkali in proportion to its water than that of 55°, otherwiſe the quantity of ammoniac in 50 grains of ſalt would hardly equal 8 grains.*

* The accounts given by different chemiſts of the compoſition of nitrate of ammoniac, are extremely diſcordant; they have been chiefly deduced from decompoſitions of carbonate of ammoniac (the varieties of which have been

VIII. *Of the loss of Solutions of Nitrate of Ammoniac during evaporation.*

The most concentrated solution of nitrate of ammoniac capable of existing at 60°, is of specific gravity 1,304, and contains 33 water, and 67 fibrous salt, per cent. When this solution is evaporated at temperatures between 60° and 100, the salt is increased in weight by the addition of water of crystalisation, and no portion of it is lost.

During the evaporation of solutions of specific gravity 1,146 and 1,15, at temperatures below 120°, I have never detected any loss of salt. When the temperature of evaporation is 212°, the loss is generally from 3 to 4 grains per cent; and when from 230° to the standard of their ebullition, from 4 to 6 grains.

heretofore unknown) by nitrous acids of unknown degrees of nitration. Hence they are particularly erroneous with regard to the alkaline part. Wenzel supposes it to be 32 per cent, and Kirwan 24. *Addit. Observ.* pag. 120.

In proportion as ſolutions are more diluted, their loſs in evaporation at equal temperatures is greater.

## DIVISION III.

*Decomposition of NITRATE of AMMONIAC: preparation of RESPIRABLE NITROUS OXIDE; its ANALYSIS.*

### I. *Of the heat required for the decomposition of NITRATE of AMMONIAC.*

THE decompofition of nitrate of ammoniac has been fuppofed by Cornette* to take place at temperatures below 212°, and its fublimation at 234°.

Kirwan, from the non-coincidence in the accounts of its compofition, has imagined that it is partially decompofable, even by a heat of 80°.†

To afcertain the changes effected by increafe of temperature in this falt, a glafs retort was provided, tubulated for the purpofe of introducing

* Mem. Par. 1783. See Irifh Tranf. vol. 4.

† Addit. Obf. pag. 120.

the bulb of a thermometer. After it had been made to communicate with the mercurial air-holder, and placed in a furnace, the heat of which could be eafily regulated, the thermometer was introduced, and the retort filled with the falt, and carefully luted; fo that the appearances produced by different temperatures could be accurately obferved, and the products evolved obtained.

From a number of experiments made in this manner on different falts, the following conclufions were drawn.

1ft. Compact, or dry nitrate of ammoniac, undergoes little or no change at temperatures below 260°.

2dly. At temperatures between 275° and 300°, it flowly fublimes, without decompofition, or without becoming fluid.

3dly. At 320° it becomes fluid, decompofes, and ftill flowly fublimes; it neither affuming, or continuing in, the fluid ftate, without decompofition.

4thly. At temperatures between 340° and 480°, it decompofes rapidly.

5thly. The prifmatic and fibrous nitrates of ammoniac become fluid at temperatures below 300°, and undergo ebullition at temperatures between 360° and 400°, without decompofition.

6thly. They are capable of being heated to 430° without decompofition, or fublimation, till a certain quantity of their water is evaporated.

7thly. At temperatures above 450° they undergo decompofition, without previoufly lofing their water of cryftalifation.

II. *Decompofition of Nitrate of Ammoniac; production of refpirable Nitrous Oxide; its properties.*

200 grains of compact nitrate of ammoniac were introduced into a glafs retort, and decompofed flowly by the heat of a fpirit lamp. The firft portions of the gas that came over were rejected, and the laft received in jars containing

mercury. No luminous appearance was perceived in the retort during the procefs, and almoft the whole of the falt was refolved into fluid and gas. The fluid had a faint acid tafte, and contained fome undecompounded nitrate. The gas collected exhibited the following properties.—

*a.* A candle burnt in it with a brilliant flame, and crackling noife. Before its extinction, the white inner flame became furrounded with an exterior blue one.

*b.* Phofphorus introduced into it in a ftate of inflammation, burnt with infinitely greater vividnefs than before.

*c.* Sulphur introduced into it when burning with a feeble blue flame, was inftantly extinguifhed; but when in a ftate of active inflammation (that is, forming fulphuric acid) it burnt with a beautiful and vivid rofe-colored flame.

*d.* Inflamed charcoal, deprived of hydrogene, introduced into it, burnt with much greater vividnefs than in the atmofphere.

*e.* To ſome fine twiſted iron wire a ſmall piece of cork was affixed: this was inflamed, and the whole introduced into a jar of the air. The iron burned with great vividneſs, and threw out bright ſparks as in oxygene.

*f.* 30 meaſures of it expoſed to water previouſly boiled, was rapidly abſorbed; when the diminution was complete, rather more than a meaſure remained.

*g.* Pure water ſaturated with it, gave it out again on ebullition, and the gas thus produced retained all its former properties.

*h.* It was abſorbed by red cabbage juice; but no alteration of color took place.

*i.* Its taſte was diſtinctly ſweet, and its odor ſlight, but agreeable.

*j.* It underwent no diminution when mingled with oxygene or nitrous gas.

Such were the obvious properties of the Nitrous Oxide, or the gas produced by the decompoſition of nitrate of ammoniac in a temperature not exceeding 440°. Other properties of it will be hereafter demonſtrated, and its affinities fully inveſtigated.

### III. *Of the gas remaining after the abſorption of Nitrous Oxide by Water.*

In expoſing nitrous oxide at different times to rain or ſpring water, and water that had been lately boiled, I found that the gas remaining after the abſorption was always leaſt when boiled water was employed, though from the mode of production of the nitrous oxide, I had reaſon to believe that its compoſition was generally the ſame.

This circumſtance induced me to ſuppoſe that ſome of the reſiduum might be gas previouſly contained in the water, and liberated from it in conſequence of the ſtronger affinity of that fluid for nitrous oxide. But the greater part of it, I conjectured to conſiſt of nitrogene produced in conſequence of a complete decompoſition of part of the acid, by the hydrogene. It was in endeavoring to aſcertain the relative purity of nitrous oxide produced at different periods of the procefs of the decompofition of nitrate of ammoniac, that I difcovered the true reafon of the appearance of refidual gas.

I decompofed fome pure nitrate of ammoniac in a fmall glafs retort; and after fuffering the firft portions to efcape with the common air, I caught the remainder in three feparate veffels ftanding in the fame trough, filled with water that had been long boiled, and which at the time of the experiment was fo warm that I could fcarcely bear my hands in it. The different quantities collected gave the fame intenfe brilliancy to the flame of a taper.

26 meafures of each of them were feparately inferted into 3 graduated cylinders, of nearly the fame capacity, over the fame boiled water. As the water cooled, the gas was abforbed by agitation. When the diminution was complete, the refiduum in each cylinder filled, as nearly as poffible, the fame fpace; about two thirds of a meafure.

To each of the refiduums I added two meafures of nitrous gas; they gave copious red vapor, and after the condenfation filled a fpace rather lefs than two meafures.

Hence the refidual gas contained more oxygene than common air.

I now introduced 26 meafures of gas from one of the veffels into a cylinder filled with unboiled fpring water of the fame kind.* After the abforption was complete, near two meafures remained. Thefe added to two meafures of nitrous air, diminifhed to 2,5 nearly.

Thefe experiments induced me to believe that the refidual gas was not produced in the decompofition of nitrate of ammoniac, but that it was wholly liberated from the water.

To afcertain this point with precifion, I diftilled a fmall quantity of the fame kind of water, which had been near an hour in ebullition, into a graduated cylinder containing mercury. To this I introduced about one third

* Two meafures of air difpelled from this water by boiling, mingled with 2 of nitrous gas, diminifhed to 2,4 nearly.

of its bulk, i. e. 12 meaſures of nitrous oxide, which had been carefully generated in the mercurial apparatus. After the abſorption, a ſmall globule of gas only remained, which could hardly have equalled one fourth of a meaſure. On admitting to this globule a minute quantity of nitrous gas, an evident diminution took place.

Though this experiment proved that in proportion as the water was free from air, the reſiduum was leſs, and though there was no reaſon to ſuppoſe that the ebullition and diſtillation had freed the water from the whole of the air it had held in ſolution, ſtill I conſidered a deciſive experiment wanting to determine whether nitrous oxide was the only gas produced in the ſlow decompoſition of nitrate of ammoniac, or whether a minute quantity of oxygene was not likewiſe evolved.

I received the middle part of the product of a decompoſition of nitrate of ammoniac, under a cylinder filled with dry mercury, and introduced to it ſome ſtrong ſolution of ammoniac. After the white cloud produced by the combination of the ammoniacal vapor with the nitric acid suspended in the nitrous oxide, had been completely precipitated, I introduced a small quantity of nitrous gas. No white vapor was produced.

Now if any gas combinable with nitrous gas had existed in the cylinder, the quantity of nitrous acid produced, however small, would have been rendered perceptible by the ammoniacal fumes; for when a minute globule of common air was admitted into the cylinder, white clouds were instantly perceptible.

It seems therefore reasonable to conclude,

1. That the residual gas of nitrous oxide, is air previously contained in the water, (which in no case can be perfectly freed from it by ebullition), and liberated by the stronger attraction of that fluid for nitrous oxide.

2. That nitrate of ammoniac, at temperatures below 440°, is decompounded into pure nitrous oxide, and fluid.

3. That in ascertaining the purity of nitrous oxide from its absorption by water, corrections ought to be made for the quantity of gas dif-

pelled from the water. This quantity in common water diſtilled under mercury being about $\frac{1}{50}$; in water ſimply boiled, and uſed when hot, about $\frac{1}{36}$; and in common ſpring water, $1\frac{1}{12}$.

## IV. *Specific gravity of Nitrous Oxide.*

To underſtand accurately the changes taking place during the decompoſition of nitrate of ammoniac, we muſt be acquainted with the ſpecific gravity and compoſition of nitrous oxide.

90 cubic inches of it, containing about $\frac{1}{35}$ common air, introduced from the mercurial airholder into an exhauſted globe, increaſed it in weight 44,75 grains; thermometer being 51°, and atmoſpheric preſſure 30,7.

106 cubic inches, of ſimilar compoſition, weighed in like manner, gave at the ſame temperature and preſſure nearly 52,25 grains; and in another experiment, when the thermometer was 41°, 53 grains.

So that accounting for the ſmall quantity of common air contained in the gaſes weighed, we may conclude, that 100 cubic inches of pure nitrous oxide weigh 50,1 grains at temperature 50°, and atmoſpheric preſſure 37.

I was a little ſurpriſed at this great ſpecific gravity, particularly as I had expected, from Dr. Prieſtley's obſervations, to find it leſs heavy than atmoſpherical air. This philoſopher ſuppoſed, from ſome appearances produced by the mixture of it with aëriform ammoniac, that it was even of leſs ſpecific gravity than that gas.*

### V. *Analyſis of Nitrous Oxide.*

The nitrous oxide may be analiſed, either by charcoal or hydrogene; during the combuſtion of other bodies in it, ſmall portions of nitrous acid are generally formed, as will be fully explained hereafter.

The gas that I employed was generated from

* Experiments and Obſervations, vol. 2, pag. 89. Laſt Edition.

compact nitrate of ammoniac, and was in its highest state of purity, as it left a residuum of 38 only, when absorbed by boiled water.

10 cubic inches of it were inserted into a jar graduated to ,1 cubic inches, containing dry mercury. Through this mercury a piece of charcoal which had been deprived of its hydrogene by long exposure to heat, weighing about a grain, was introduced, while yet warm: No perceptible absorption of the gas took place.*

Thermometer being 46°, the focus of a lens was thrown on the charcoal, which instantly took fire, and burnt vividly for about a minute, the gas being increased in volume. After the vivid combustion had ceased, the focus was again thrown on the charcoal; it continued to burn for near ten minutes, when the process stopped.

The gas, when the original pressure and temperature were restored, filled a space equal to 12,5 cubic inches.

* A minute quantity, however, must have been absorbed, and given out again when the charcoal was heated.

On introducing to it a ſmall quantity of ſtrong ſolution of ammoniac*, white vapor was inſtantly perceived, and after a ſhort time the reduction was to about 10,1 cubic inches; ſo that apparently, 2,4 cubic inches of carbonic acid had been formed. The 10,1 cubic inches of gas remaining were expoſed to water which had been long in ebullition, and which was introduced whilſt boiling, under mercury. After the abſorption of the nitrous oxide by the water, the gas remaining was equal to 5,3.

But on combining a cubic inch of pure nitrous oxide with ſome of the ſame water, which had been received under mercury in a ſeparate veſſel, nearly $\frac{1}{22}$ remained. Conſequently we may conclude, that 5,1 of a gas unabſorbable by water, was produced in the combuſtion.

This gas extinguiſhed flame, gave no diminution with oxygene, and the ſlighteſt poſſible

* Strong ſolution of ammoniac has no attraction for nitrous oxide.

with nitrous gas. When an electric fpark was paffed through it, mingled with oxygene; no inflammation, or *perceptible* diminution took place.† We may confequently conclude that it was nitrogene, mingled with a minute portion of common air, expelled from the water.

The charcoal was diminifhed in bulk to one half nearly, but the lofs of weight could not be afcertained, as its pores were filled with mercury.

Now 5 cubic inches of nitrous oxide were abforbed by the water, confequently 5 were decompounded by the charcoal; and thefe produced 5,1 cubic inches of nitrogene; and by giving their oxygene to the charcoal, apparently 2,4 of carbonic acid.

But 5 cubic inches of nitrous oxide weigh 2,5 grains, and 5,1 cubic inches of nitrogene 1,55; then 2,5 — 1,55 = ,95.

So that reafoning from the relative fpecific

† The gas was examined by thofe tefts in order to prove, that no water had been decompofed.

gravities of nitrogene and nitrous oxide, 2,5 grains of the laſt are compoſed of 1,55 nitrogene, and ,95 oxygene.

But from many experiments made on the ſpecific gravity of carbonic acid, in Auguſt, 1799, I concluded that 100 cubic inches of it weighed 47,5 grains, thermometer being 60,1°, and barometer 29,5. Conſequently, making the neceſſary corrections, 2,4 cubic inches of it weigh 1,14 grains; and on Lavoiſier's and Guyton's * eſtimation of its compoſition, theſe 1,13 grains contain 8,2 of oxygene.

So that, drawing concluſions from the quantity of carbonic acid formed in this experiment, 2,5 grains of nitrous oxide will be compoſed of ,82 oxygene, and 1,68 nitrogene.

The difference between theſe eſtimations is conſiderable, and yet not more than might have been expected, if we conſider the probable ſources of error in the experiment.

* See the curious paper of this excellent philoſopher, on the combuſtion of the diamond, in which he proves that charcoal is, in fact, oxide of diamond. Annales de Chimie. xxxi.

1. It is likely that variable minute quantities of hydrogene remain combined with charcoal, even after it has been long expofed to a red heat.

2. It is probable that the nitrogene and carbonic acid produced were capable of diffolving more water than that held in folution by the nitrous oxide; and if fo, they were more condenfed than if faturated with moifture, and hence the quantity of carbonic acid under-rated.

We may confequently fuppofe the eftimation founded on the quantity of nitrogene evolved, moft correct; and making a fmall allowance for the difference, conclude, that 100 grains of nitrous oxide are compofed of about 37 oxygene, and 63 nitrogene; exifting in a much more condenfed ftate than when in their fimple forms.

The tolerable accuracy of this ftatement will be hereafter demonftrated by a number of experiments on the combuftion of different bodies in nitrous oxide, detailed in Refearch II.

VI. *Minute examination of the decompoſition of Nitrate of Ammoniac.*

Into a retort weighing 413,75 grains, and of the capacity of 7,5 cubic inches, 100 grains of pulveriſed compact nitrate of ammoniac were introduced. To the neck of this retort was adapted a recipient, weighing 711 grains, tubulated for the purpoſe of communicating with the mercurial airholder, and of the capacity of 8,3 cubic inches.

Temperature being 50°, and atmoſpheric preſſure 30,6, the recipient was inſerted into a veſſel of cold water, and made to communicate with the airholder. The heat of a ſpirit lamp was then ſlowly applied to the retort: the ſalt quickly began to decompoſe, and to liquify. The temperature was ſo regulated, as to keep up an equable and ſlow decompoſition.

During this decompoſition, no luminous appearance was perceived in the retort; the gas that came into the airholder was very little clouded, and much water condenſed in the receiver.

After the proceſs was finiſhed, the communication between the mercurial airholder and the recipient was preſerved till the common temperature was reſtored to the retort.

The volume of the gas in the cylinder was 85,5 cubic inches. The abſolute quantity of nitrous oxide in thoſe 85,5 cubic inches, it was difficult to aſcertain with great nicety, on account of the common air previouſly contained in the veſſels.

45 meaſures of it, expoſed to well boiled water, diminiſhed by agitation to 8 meaſures. So that reaſoning from the quantity of air, which ſhould have been expelled from the water by the nitrous oxide, we may conclude that the 85,5 cubic inches were nearly pure.

The retort now weighed 419,25 grains, conſequently 5,5 grains of ſalt remained in it. This ſalt was chiefly collected about the lower part of the neck, and contained rather more water than the compact nitrate, as in some places it was crystalised.

The recipient with the fluid it contained, weighed 759 grains. It had consequently gained in weight 48 grains.

Now the 85,5 cubic inches of nitrous oxide produced, weigh about 42,5 grains; and this added to 48 and 5,5, = 96 grains; so that about 4 grains of salt and fluid were lost, probably by being carried over and deposited by the gas.*

As much of the fluid as could be taken out of the recipient, weighed 46 grains, and held in solution much nitrate of ammoniac with superabundance of acid. This acid required for its saturation, 3 $\frac{1}{8}$ of carbonate of ammoniac (containing, as well as I could guess), about 20 per cent alkali.

The whole solution evaporated, gave 18 grains of compact nitrate of ammoniac. But

* This was actually the case; for on examining the conducting tube the day after the experiment, some minute crystals of prismatic nitrate of ammoniac were perceived in it.

reafoning from the quantity of carbonate of ammoniac employed, the free nitric acid was equal to 2,75 grains, and this muft have formed 3,56 grains of falt. Confequently the falt pre-exifting in the folution was about 14,44 grains.

But befides the fluid taken out of the recipient, 2 grains remained in it: let us fuppofe this, and the 4 grains loft, to contain 2 of falt, and ,6 of free acid.

Then the undecompounded

| | | |
|---|---|---|
| falt is 5,5 + 14,4 + 2 | = | 21,9 |
| The free acid 2,75 + ,6 | = | 3,35 |
| Gas - - - | | 42,5 |
| Water - - - | | 32,25 |
| | | 100 |

Now about 78,1 grains of falt were decompounded, and formed into 42.5 grains of gas, 3,35 grains acid, and 32,25 grains water.

But there is every reafon to fuppofe, that in this procefs, when the hydrogene of the ammoniac combines with a portion of the oxygene of the nitric acid to form water, and the nitrogene enters into union with the nitrogene and remaining oxygene of the nitric acid, to form nitrous oxide; that water pre-exifting in nitric acid and ammoniac, fuch as they exifted in the aëriform ftate, is depofited with the water produced by the new arrangement, and not wholly combined with the nitrous oxide formed. Hence it is impoffible to determine with great exactitude, the quantity of water which was abfolutely formed in this experiment.

78,1 grains of falt are compofed of 15,4 alkali, 58 acid, and 4,7 water.

And reafoning from the different affinities of water for nitric acid, ammoniac, and nitrous oxide, it is probable that ammoniac, in its decompofition, divides its water in fuch a ratio, between the nitrogene furnifhed to the nitrous oxide, and the hydrogene entering into union with the oxygene of the nitric acid, as to enable us to affume, that the hydrogene requires for its faturation nearly the fame quantity of oxygene as when in the aëriform ftate; or that it certainly cannot require lefs.

But 15,4 alkali contain 3,08 hydrogene, and 12,32 nitrogene;* and 3,08 hydrogene require 17,4 of oxygene to form 20,48 of water.

Now 32,5 grains of water exifted before the experiment; 4,7 grains of water were contained by the falt decompofed, and 32,5 — 4,7 = 27,8: and 27,8 — 20,48, the quantity generated, = 7,52, the quantity exifting in the nitric acid.

But the nitric acid decompofed is 58g — 3,35 = to 54,7; and 54,7 — 7,5 = 47,2, which entered into new combinations. Thefe 47,2 confift of 33,2 oxygene, and 14, nitrogene. And 33,2 — 17,4, the quantity employed to form the water, = 15,8, which combined with 14,, nitrogene of the nitric acid, and 12,32 of that of the ammoniac, to form 42,12 of nitrous oxide. And on this eftimation, 100 parts of nitrous oxide would contain 37,6 oxygene, and 62,4 nitrogene; a computation much nearer the refults of the analyfis than could

* Owing part of their weight to an unknown quantity of water.

have been expected, particularly as ſo many unavoidable ſources of error exiſted in the proceſs.

The experiment that I have detailed is the moſt accurate of four, made on the ſame quantity of ſalt. The others were carried on at rather higher temperatures, in conſequence of which, more water and ſalt were ſublimed with the gas.

To Berthollet, we owe the diſcovery of the products evolved during the ſlow decompoſition of nitrate of ammoniac; but as this philoſopher in his examination of this proceſs, chiefly deſigned to prove the exiſtence of hydrogene in ammoniac, he did not aſcertain the quantity of gas produced, or minutely examine its properties; from two of them, its abſorption by water and its capability of ſupporting the vivid combuſtion of a taper, he inferred its identity with the dephlogiſticated nitrous gas of Prieſtley, and concluded that it was nitrous gas with exceſs of pure air.*

* Mem. de Paris. 1785, and Journal de Phyſique, 1786, page 175.

VII. *Of the heat produced during the decomposition of nitrate of ammoniac.*

To aſcertain whether the temperature of nitrate of ammoniac was increaſed or diminiſhed after it had been raiſed to the point eſſential to its decompoſition, during the evolution of nitrous oxide and water; that is, in common language, whether heat was generated or abſorbed in the proceſs; I introduced a thermometer into about 1500 grains of fibrous nitrate of ammoniac, rendered liquid in a deep porcelain cup. During the whole of the evaporation, the temperature was about 380°, the fire being carefully regulated.

As ſoon as the decompoſition took place, the thermometer began to riſe; in leſs than a quarter of a minute it was 410°, in two minutes it was 460°.

The cup was removed from the fire; the decompoſition ſtill went on rapidly, and for about a minute the thermometer was ſtationary. It then gradually and flowly fell; in three minutes it was 440°, in five minutes 420°, in feven minutes 405°, in nine minutes 360°, and in thirteen minutes 307°, when the decompofition had nearly ceafed, and the falt began to folidify.

From this experiment, it is evident that an increafe of temperature is produced by the decompofition of nitrate of ammoniac: though the capacity of water and nitrous oxide for heat, fuppofing the truth of the common doctrine, and reafoning from analogy, muft be confiderably greater than that of the falt.

VIII. *Of the decompofition of Nitrate of Ammoniac at high temperatures, and production of Nitrous gas, Nitrogene, Nitrous Acid, and Water.*

At an early period of my inveftigation relating to the nitrous oxide, I difcovered that when a heat above 600° was applied to nitrate of ammoniac, fo that a vivid luminous appearance was produced in the retort, certain portions of nitrous gas, and nitrogene, were evolved with the nitrous oxide. But I was for ſome time ignorant of the preciſe nature of this decompoſition, and doubtful with regard to the poſſibility of effecting it in ſuch a manner as to prevent the production of nitrous oxide altogether.

I firſt attempted to decompoſe nitrate of ammoniac at high temperatures, by introducing it into a well coated green glaſs retort, having a wide neck, communicating with the pneumatic apparatus, and ſtrongly heated in an air-furnace. But though in this proceſs a detonation always took place, and much light was produced, yet ſtill the greater portion of the gas generated was nitrous oxide; the nitrous gas and nitrogene never amounting to more than one third of the whole.

After breaking many retorts by exploſions, without gaining any accurate reſults, I employed a porcelain tube, curved ſo as to be capable of introduction into the pneumatic apparatus, and cloſed at one end.

The cloſed end was heated red, nitrate of ammoniac introduced into it, and all the latter portions of gas produced in the explofion, received in the pneumatic apparatus, filled with warm water.

Three explofions were required to fill a jar of the capacity of 20 cubic inches. The gas produced in the firft, when it came over, was tranfparent and dark orange, fimilar in its appearance to the nitrous acid gas produced in the firft experiment; but it fpeedily became white and clouded, whilft a flight diminution of volume took place.

When the fecond portion was generated and mingled with the clouded gas, it again became tranfparent and yellow for a fhort time, and then affumed the fame appearance as before.

The water in the trough, after this experiment, had an acid tafte, and quickly reddened cabbage juice rendered green by an alkali.

6 cubic inches of the gas produced were expofed to boiled water, but little or no abforption took place. Hence, evidently, it contained no nitrous oxide.

They were then expofed to folution of fulphate of iron: the folution quickly became dark colored, and an abforption of 1,6 took place on agitation.*

The gas remaining inftantly extinguifhed the taper, and was confequently nitrogene.

This experiment was repeated, with nearly the fame refults.

We may then conclude, that at high temperatures, nitrate of ammoniac is wholly refolved into water, nitrous acid, nitrous gas, and nitrogene; whilft a vivid luminous appearance is produced.

The tranfparency and orange color produced in the gas that had been clouded, by new portions of it, doubtlefs arofe from the folution of the nitric acid and water forming the cloud, in the heated nitrous vapor produced, fo as to conftitute an aëriform triple compound; whilft the cloudinefs and abforption fubfequent were pro-

* The abforption of nitrous gas by fulphate of iron, &c. will be treated of in the next divifion.

duced by the diminifhed temperature, which deftroyed the ternary combination, and feparated the nitrous acid and water from the nitrous gas,

From the rapidity with which the deflagration of nitrate of ammoniac proceeds, and from the immenfe quantity of light produced, it is reafonable to fuppofe that a very great increafe of temperature takes place. The tube in which the decompofition has been effected, is always ignited after the procefs.

IX. *Speculations on the decompofitions of Nitrate of Ammoniac.*

All the phænomena of chemiftry concur in proving, that the affinity of one body, A, for another, B, is not deftroyed by its combination with a third, C, but only modified; either by condenfation, or expanfion, or by the attraction of C for B.

On this principle, the attraction of compound bodies for each other muft be revolved into the reciprocal attractions of their conftituents, and confequently the changes produced in them by variations of temperature explained, from the alterations produced in the attractions of thofe conftituents.

Thus in nitrate of ammoniac, four affinities may be fuppofed to exift:

1. That of hydrogene for nitrogene, producing ammoniac.

2. That of oxygene for nitrous gas, producing nitric acid.

3. That of the hydrogene of ammoniac for the oxygene of nitric acid.

4. That of the nitrogene of ammoniac for the nitrous gas of nitric acid.

At temperatures below 300°, the falt, from the equilibrium between thefe affinities, preferves its exiftence.

Now when its temperature is raifed to 400°, the attractions of hydrogene for nitrogene,* and

* As is evident from the decompofition of ammoniac by heat.

of nitrous gas for oxygene,‡ are diminished; whilst the attraction of hydrogene for oxygene† is increased; and perhaps that of nitrogene for nitrous gas.

Hence the former equilibrium of affinity is destroyed, and a new one produced.

The hydrogene of the ammoniac combines with the oxygene of the nitric acid to generate water; and the nitrogene of the ammoniac enters into combination with the nitrous gas to form nitrous oxide: and the water and nitrous oxide produced, most probably exist in binary combination in the aëriform state, at the temperature of the decomposition.

But when a heat above 800° is applied to nitrate of ammoniac, the attractions of nitrogene and hydrogene for each other, and of

‡ Nitric acid is phlogisticated by heat, as appears from Dr. Priestley's experiments. Vol. 3, p. 26.

† As is evident from the increase of temperature required for the formation of water.

oxygene for nitrous gas,* are ſtill more diminiſhed; whilſt that of nitrogene for nitrous gas is deſtroyed, and that of hydrogene for oxygene increaſed to a great extent: likewiſe a new attraction takes place; that of nitrous gas for nitric acid, to form nitrous vapor.† Hence a new arrangement of principles is rapidly produced; the nitrogene of ammoniac

* For ammoniac and nitrous oxide are both decompoſed at the red heat, and oxygene given out from nitric acid when it is paſſed through a heated tube.

† Whenever nitrous acid is produced at high temperatures, it is always highly phlogiſticated, provided it has not been long in contact with oxygene. When Dr. Prieſtley paſſed nitric acid through a tube heated red, he procured much oxygene, and phlogiſticated acid; and the water in the apparatus employed was fully impregnated with nitrous air. Hence it would appear, that heat diminiſhes the attraction between oxygene and nitrous gas, and increaſes the affinity of nitrous gas for nitrous acid. Mr. James Thomson, whoſe theory of the Nitrous Acid I have already mentioned, from ſome experiments on the phlogiſtication of Nitric Acid by heat, which he has communicated to me, concludes with great juſtneſs, that a portion of the acid is always completely decompoſed in this proceſs: the oxygene liberated, and the nitrous gas combined with the remaining acid.

having no affinity for any of the ſingle principles at this temperature, enters into no binary compound: the oxygene of the nitric acid forms water with the hydrogene, and the nitrous gas combines with the nitric acid to form nitrous vapor. All theſe ſubſtances moſt probably exiſt in combination at the temperature of their production; and at a lower temperature, aſſume the forms of nitrous acid, nitrous gas, nitrogene, and water.

I have avoided entering into any diſcuſſions concerning the light and heat produced in this proceſs; becauſe theſe phænomena cannot be reaſoned upon as iſolated facts, and their relation to general theory will be treated of hereafter.

X. *On the preparation of Nitrous Oxide for experiments on Reſpiration.*

When compact nitrate of ammoniac is ſlowly decompoſed, the nitrous oxide produced is almoſt immediately fit for reſpiration; but as one part of the ſalt begins to decompoſe before the other is rendered fluid, a confiderable lofs is produced by fublimation.

For the production of large quantities of nitrous oxide, fibrous nitrate of ammoniac fhould be employed. This falt undergoes no decompofition till the greater part of its water is evaporated, and in confequence at the commencement of that procefs, is uniformly heated.

The gas produced from fibrous nitrate, muft be fuffered to reft at leaft for an hour after its generation. At the end of this time it is generally fit for refpiration. If examined before, it will be found to contain more or lefs of a white vapor, which has a difagreeable acidulous tafte, and ftrongly irritates the fauces and lungs. This vapor, moft probably, confifts of acid nitrate of ammoniac and water, which were diffolved by the gas at the temperature of its production, and afterwards flowly precipitated.

It is found in lefs quantity when compact nitrate is employed, becaufe more falt is fublimed in this procefs, which being rapidly precipitated, carries with it the acid and water.

Whatever falt is employed, the laft portions of gas produced, generally contain lefs vapor, and may in confequence be refpired fooner than the firft.

The nitrate of ammoniac fhould never be decompofed in a metallic veffel,* nor the gas produced fuffered to come in contact with any metallic furface; for in this cafe the free nitric acid will be decompofed, and in confequence, a certain quantity of nitrous gas produced.

The apparatus that has been generally employed in the medical pneumatic inftitution, for the production of nitrous oxide, confifts

1. Of a glafs retort, of the capacity of two or three quarts, orificed at the top, and furnifhed with a ground ftopper.

2. Of a glafs tube, conical for the purpofe of receiving the neck of the retort; about ,4 inches wide in the narroweft part, 4 feet long, curved at the extremity, fo as to be capable of

* Except it be gold or platina.

introduction into an airholder, and inclosed by tin plate to preserve it from injury.

3. Of airholders of Mr. Watt's invention, filled with water saturated with nitrous oxide.

4. Of a common air-furnace, provided with dampers for the regulation of the heat.

The retort, after the insertion of the salt, is connected with the tube, carefully luted, and exposed to the heat of the furnace, on a convenient stand. The temperature is never suffered to be above 500°. After the decomposition has proceeded for about a minute, so that the gas evolved from the tube enlarges the flame of a taper, the curved end is inserted into the airholder, and the nitrous oxide preserved.

The water thrown out of the airholders in consequence of the introduction of the gas, is preserved in a vessel adapted for the purpose, and employed to fill them again; for if common water was to be employed in every experiment, a great loss of gas would be produced from absorption.

A pound of fibrous nitrate of ammoniac, decompofed at a heat not above 500°, produces nearly 5 cubic feet of gas; whilft from a pound of compact nitrate of ammoniac, rarely more than 4,25 cubic feet can be collected.

For the production of nitrous oxide in quantities not exceeding 20 quarts, a mode ftill more fimple than that I have juft defcribed may be employed. The falt may be decompofed by the heat of an argands lamp, or a common fire, in a tubulated glafs retort, of 20 or 30 cubic inches in capacity, furnifhed with a long neck, curved at the extremity; and the gas received in fmall airholders.

Thus, if the pleafurable effects, or medical properties of the nitrous oxide, fhould ever make it an article of general requeft, it may be procured with much lefs time, labor, and expence,* than moft of the luxuries, or even neceffaries, of life.

* A pound of nitrate of ammoniac cofts about 5s. 10d. This pound, properly decompofed, produces rather more than 34 moderate dofes of air; fo that the expence of a dofe is about 2d. What fluid ftimulus can be procured at fo cheap a rate?

## DIVISION IV.

*EXPERIMENTS and OBSERVATIONS on the COMPOSITION of NITROUS GAS, and on its ABSORPTION by different bodies.*

### I. *Preliminaries.*

IN my account of the compofition of nitric acid, in Divifion I. I gave an eftimation of the quantities of oxygene and nitrogene combined in nitrous gas: I fhall now detail the experiments on which that eftimation is founded.

At an early period of my refearches relating to nitrous oxide, from the obfervation of the phænomena taking place during the production of this fubftance, I had concluded, that the common opinion with regard to the compofition of nitrous gas, was very diftant from the truth. I had indeed analyfed nitrous gas, by converting it into nitrous oxide, before I attempted to aſcertain its compoſition by immediately ſeparating the conſtituent principles from each other: and my firſt hopes of the poſſibility of effecting this, were derived from Dr. Prieſtley's experiments on the combuſtion of pyrophorus in nitrous gas, and on the changes effected in it, by heated iron and charcoal.

This great philoſopher found, that pyrophorus placed in contact with nitrous gas, burnt with great vividneſs, whilſt the gas was diminiſhed in volume to about one half, which generally conſiſted of nitrogene and nitrous oxide.* He likewiſe found, iron heated by a lens in nitrous gas, increaſed in weight, whilſt the gas was diminſhed about $\frac{1}{2}$, and converted into nitrogene.*

He heated common charcoal, and charcoal of copper,‡ in nitrous gas by a lens. When

* Experiments and Obſervations, vol. ii. pag. 50. Laſt Edition.

‡ That is, charcoal produced by the decompoſition of ſpirits of wine. Vol. 11. pag. 39.

common charcoal was employed, the gas was neither increaſed or diminiſhed in bulk, but wholly converted into nitrogene; when charcoal of copper was uſed, the volume was a little increaſed, and the gas remaining conſiſted of $\frac{5}{7}$ nitrogene, and $\frac{2}{7}$ carbonic acid.

In his experiments on the iron and pyrophyrus, the nitrous gas was evidently decompoſed. From the great quantity of nitrogene produced in thoſe on the charcoal, it ſeems likely that both the common charcoal,* and the charcoal of copper employed contained atmoſpherical air, which being diſpelled by the heat of the lens,

* Dr. Prieſtley ſays, "having heated iron in nitrous air, "I proceeded to heat in the ſame air, a piece of charcoal "not long after it had been ſubjected to a ſtrong heat covered "with ſand. The ſun not ſhining immediately, after the "charcoal was introduced into the veſſel of air, through the "mercury by which it was confined, part of the air was "abſorbed; but on heating the charcoal, the quantity was "increaſed. Having continued the progreſs as long as I "thought neceſſary, I examined the air and found it to be "about as much as the original quantity of nitrous air; "but it was all phlogiſticated air extinguiſhing a candle "and having no mixture of fixed air in it."—Experiments and Obſervations, Vol. II, page 39.

was decompofed by the nitrous gas: indeed, till I made the following experiment, I fufpected that the carbonic acid produced, when the charcoal of copper was employed, arofe from a decompofition of the nitrous acid, formed in this way.

I introduced a piece of well-burnt charcoal, which could hardly have weighed the eighth of a grain, whilft red hot, under a cylinder filled with mercury, and admitted to it half a cubic inch of nitrous gas. A flight abforption took place.

The fun being very bright, I kept the charcoal in the focus of a fmall lens for near a quarter of an hour. At the end of this time the gas occupied a fpace nearly as before the experiment, and a very minute portion of the charcoal had been confumed. On introducing into the cylinder a fmall quantity of folution of ftrontian, a white precipitation was perceived, and the gas flowly diminifhed to about three tenths of a cubic inch. To thefe three tenths a little common air was admitted, when very ſlight red fumes were perceived.

This experiment convinced me, that the attraction of charcoal for the oxygene of nitrous gas, at high temperatures, was ſufficiently ſtrong to effect a ſlow decompoſition of it.

To be more accurately acquainted with this decompoſition, and to learn the quantities of carbonic acid and nitrogene produced from a known quantity of nitrous gas, I proceeded in the following manner.

### II. *Analyſis of Nitrous Gas by Charcoal.*

A quantity of nitrous gas was procured in a water apparatus, from the decompoſition of nitrous acid by mercury. A portion of it was transferred to the mercurial trough. After the mercury and the jar had been dried, by bibulous paper, 40 meaſures of this portion were agitated in a ſolution of ſulphate of iron. The gas remaining after the abſorption was complete, filled about a meaſure and half; ſo that the nitrous gas contained nearly $\frac{1}{26}$ nitrogene.

Thermometer being 53°, a ſmall piece of well burnt charcoal, the weight of which could hardly have equalled a quarter of a grain, was introduced ignited, into a ſmall cylinder filled with mercury, graduated to ,10 grain meaſures; to this, 16 meaſures, equal to 160 grain m. of nitrous gas, were admitted. An abſorption of about one meaſure and half took place. When the focus of a lens was thrown on the charcoal, a ſlight increaſe of the gas was produced, from the emiſſion of that which had been abſorbed.

After the proceſs had been carried on for about a half an hour, the charcoal evidently began to fume, and to conſume very ſlowly, though no alteration in the volume of the gas was obſerved.

The ſun not conſtantly ſhining, the progreſs of the experiment was now and then ſtopped: but taking the whole time, the focus could not have been applied to it for leſs than four hours. When the proceſs was finiſhed, the gas was increaſed in bulk nearly three quarters of a meaſure.

A drop of water was introduced into the cylinder, by means of a ſmall glaſs tube, on the ſuppoſition that the carbonic acid, and nitrogene, might be capable of holding in ſolution, more water than that contained in the nitrous gas decompoſed; but no alteration of volume took place.

When 20 grain meaſures of ſolution of pale green* ſulphate of iron were introduced into the cylinder, they became rather yellower than before, but not dark at the edges, as is always the caſe when nitrous gas is preſent. On agitation, a diminution of nearly half a meaſure was produced, doubtleſs from the abſorption of ſome of the carbonic acid by the ſolution.

A ſmall quantity of cauſtic potaſh, much more than was ſufficient to decompoſe the ſulphate of iron, was now introduced. A rapid diminution took place, and the gas remaining

* That is, ſulphate of iron containing oxide of iron, in the firſt degree of oxygenation.

filled about 8 meaſures. This gas was agitated for ſome time over water, but no abſorption took place. Two meaſures of it were then transferred into a detonating cylinder with two meaſures of oxygene. The electric ſpark was paſſed through them, but no diminution was produced. Hence it was nitrogene, mingled with no aſcertainable quantity of hydrogene: conſequently little or no water could have been decompoſed in the proceſs.

Now ſuppoſing, for the greater eaſe of calculation, each of the meaſures employed, cubic inches.

16 of nitrous gas — $\frac{1}{26}$ = 15,4 were decompoſed, and theſe weigh, making the neceſſary corrections, 5,2; but 7,4 nitrogene were produced, and theſe weigh about 2,2. So that reaſoning from the relative ſpecific gravities of nitrous gas and nitrogene, 5,2 grains of nitrous gas will be compoſed of 3 oxygene, and 2,2 nitrogene.

But 8,7 of carbonic acid were produced, which weigh 41 grains, and conſiſt of 2,9 oxygene, and 1,2 charcoal.* Consequently, drawing conclusions from the quantity of carbonic acid formed, 5,2 grains of nitrous gas will consist of 2,9 oxygene, and 2,3 nitrogene.

The difference in these estimations is much less than could have been expected; and taking the mean proportions, it would be inferred from them, that 100 grains of nitrous gas, contain 56,5 oxygene, and 43,5 nitrogene.

I repeated this experiment with results not very different, except that the increase of volume was rather greater, and that more unabsorbable gas remained; which probably depended on the decomposition of a minute quantity of water, that had adhered to the charcoal in passing through the mercury.

As nitrous gas is decomposable into nitrous acid, and nitrogene, by the electric spark; it occurred to me, that a certain quantity of nitrous acid might have been possibly produced, in the experiments on the decomposition of nitrous gas, by the intensely ignited charcoal.

* That is, carbon, or oxide of diamond.

To aſcertain this circumſtance, I introduced into 12 meaſures of nitrous gas, a ſmall piece of charcoal which had been juſt reddened. The ſun being very bright, the focus of the lens was kept on it for rather more than an hour and quarter. In the middle of the proceſs it began to fume and to ſparkle, as if in combuſtion. In three quarters of an hour, the gas was increaſed rather more than half a meaſure; but no alteration of volume took place afterwards.

The mercury was not white on the top as is uſually the caſe when nitrous acid is produced. On introducing into the cylinder a little pale green ſulphate of iron, and then adding pruſiate of potaſh, a white precipitate only was produced. Now, if the minuteſt quantity of nitric acid had been formed, it would have been decompoſed by the pale green oxide of iron, and hence, a viſible quantity of pruſſian blue* produced, as will be fully explained hereafter.

* That is, blue pruſſiate of iron.

## III. *Analyſis of Nitrous Gas by Pyrophorus.*

I placed ſome newly made pyrophorus, about as much as would fill a quarter of a cubic inch, into a jar filled with dry mercury, and introduced to it, four cubic inches of nitrous gas, procured from mercury and nitric acid. It inſtantly took fire and burnt with great vividneſs for ſome moments.

After the combuſtion had ceaſed, the gas was diminiſhed about three quarters of a cubic inch. The remainder was not examined; for the diminution appeared to go on for ſome time, after; in an half hour, when it was compleat, it was to 2 cubic inches. A taper, introduced into theſe, burnt with an enlarged flame, blue at the edges; from whence it appeared, that they were compoſed of nitrogene and nitrous oxide.

I now introduced about half a cubic inch of pyrophorus to two cubic inches of nitrous gas; the combuſtion took place, and the gas was rapidly diminiſhed to one half; and on ſuffering it to remain five minutes to one-third nearly; which extinguiſhed flame.

Suſpecting that this great diminution was owing to the abſorption of ſome of the nitrogene formed, by the charcoal of the pyrophorus, I carefully made a quantity of pyrophorus; employing more than two-thirds of alumn, to one-third of ſugar.

To rather more than half of a cubic inch of this, two cubic inches of nitrous gas, which contained about $\frac{1}{40}$ nitrogene, were admitted. After the combuſtion, the gas remaining, *apparently* filled a ſpace equal to 1,2 cubic inches; but, as on account of the burnt pyrophyrus in the jar, it was impoſſible to aſcertain the volume with nicety, it was carefully and wholly transferred into another jar. It filled a ſpace equal to 1,15 cubic inches nearly.

When water was admitted to this gas no abſorption took place. It underwent no diminution with nitrous gas, and a taper plunged into it was inſtantly extinguiſhed. We may conſequently conclude that it was nitrogene.

Now 2 cubic inches of nitrous gas weigh ,686 grains, and 1,1 of nitrogene — ,05, the quantity previously contained in the gas = to 1,05, 3,19. Hence ,686 of nitrous gas would be composed of ,367 oxygene, and ,319 nitrogene; and 100 grains would contain 53,4 oxygene, and 46,6 nitrogene.

IV. *Additional observations on the combustion of bodies in Nitrous Gas, and on its Composition.*

Though phosphorus may be fused, and even sublimed, in nitrous gas, without producing the slightest luminous appearance,* yet when

* No luminous appearance is produced when phosphorus is introduced into *pure* nitrous gas. It has been often observed, that phosphorus is luminous in nitrous gas, that has not been long in contact with water after its production. This phænomenon, I suspect, depends either on the decomposition of the nitric acid held in solution by the nitrous gas; or on the combination of the phosphorus with oxygene loosely adhering to the binary aëriform compound of nitric acid and nitrous gas. I have not yet examined if nitrous gas can be converted into nitrous oxide by long exposure to heated phosphorus: it appears, however, very probable.

it is introduced into it in a ſtate of active inflammation, it burns with almoſt as much vividneſs as in oxygene.* Hence it is evident, that at the heat of ignition, phoſphorus is capable of attracting the oxygene from the nitrogene of nitrous gas.

I attempted to analiſe nitrous gas, by introducing into a known quantity of it, confined by mercury, phoſphorus, in a veſſel containing a minute quantity of oxygene.† The phoſphorus was inflamed with an ignited iron wire, by which, at the moment of the combuſtion, the veſſel containing it was raiſed from the mercury into the nitrous gas. But after making in this way, five of ſix unſucceſsful experiments, I deſiſted. When the communication between the veſſels was made before the oxygene was nearly combined with the phoſphorus, nitrous

* Perhaps this fact has been noticed before; I have not, however, met with it in any chemical work.

† This mode of inflaming bodies in gaſes, not capable of ſupporting combuſtion at low temperatures, will be particularly deſcribed hereafter.

acid was formed, which inſtantly deſtroyed the combuſtion; when, on the contrary, the phoſphorus was ſuffered to conſume almoſt the whole of the oxygene, it was not ſufficiently ignited when introduced, to decompoſe the nitrous gas.

In one experiment, indeed, the phoſphorus burnt for a moment in the nitrous gas; the diminution however was ſlight, and not more than $\frac{1}{4}$ of it was decompoſed.

Sulphur, introduced in a ſtate of vivid inflammation, into nitrous gas, was inſtantly extinguiſhed.

I paſſed a ſtrong electric ſhock through equal parts of hydrogene and nitrous gas, confined by mercury in a detonating tube; but no inflammation, or perceptible diminution, was produced.

19,2 grain meaſures of hydrogene were fired by the electric ſhock, with 10 of nitrous oxide, and 6 of nitrous gas; the diminution was to 17; and pale green ſulphate of iron admitted to the reſiduum, was not diſcolored. Conſequently the nitrous gas was decompoſed by the hydrogene, and as will be hereafter more clearly underſtood, nearly as much nitrogene furniſhed by it, as would have been produced from half the quantity of nitrous oxide.

Suſpecting that phoſphorated hydrogene might inflame with nitrous gas, I paſſed the electric ſpark through 1 meaſure of phoſphorated hydrogene, and 4 of nitrous gas; but no diminution was perceptible. I likewiſe paſſed the electric ſpark through 1 of nitrous gas, with 2 of phoſphorated hydrogene, without inflammation.

Perhaps if I had tried many other different proportions of the gaſes, I ſhould have at laſt diſcovered one, in which they would have inflamed; for, as will be ſeen hereafter, nitrous oxide cannot be decompoſed by the compound combuſtible gaſes, except definite quantities are employed.

From Dr. Prieſtley's experiments on iron and pyrophorus, and from the experiments I have detailed, on charcoal, phoſphorus, and hydrogene, it appears that at certain temperatures, nitrous gas is decomposable by most of the combustible bodies: even the extinction of sulphur, when introduced into it in a state of inflammation, depends perhaps, on the smaller quantity of heat produced by the combustion of this body, than that of most others.

The analysis of nitrous gas by charcoal, as affording data for determining immediately the quantities of oxygene and nitrogene, ought to be considered as most accurate; and correcting it by mean calculations derived from the decomposition of nitrous gas by pyrophorus and hydrogene, and its conversion into nitrous oxide, a process to be described hereafter, we may conclude, that 100 grains of nitrous gas are composed of 55,95 oxygene, and 44,05 nitrogene; or taking away decimals, of 56 oxygene, and 44 nitrogene.

This estimation will agree very well with the mean proportions that would be given from Dr. Priestley's experiments on the decomposition of nitrous gas by iron; but as he never ascertained the purity of his nitrous gas,* and probably employed different kinds in different experiments, it is impoffible to fix on any one, from which accurate conclufions can be drawn.

Lavoifier's eftimation of the quantities of oxygene and nitrogene entering into the compofition of nitrous gas, has been generally adopted. He fuppofes 64 parts of nitrous gas to be compofed of 43½ of oxygene, and 20½ of nitrogene.†

The difference between this account and mine is very great indeed; but I have already, in Divifion 1ft, pointed out fources of error in the experiments of this great man, on the decompofition of nitre by charcoal; which experiments were fundamental, both to his accounts of the conftitution of nitrous acid, and nitrous gas.

* Elements Englifh Tranf. edit. i. pag. 216.

† Experiments and Obfervations, Vol. II. pag. 40, 2d. Ed.

## V. *Of the absorption of Nitrous Gas by Water.*

Amongst the properties of nitrous gas noticed by its great discoverer, is that of absorbability by water.

In exposing nitrous air to distilled water, Dr. Priestley found a diminution of the volume of gas, nearly equal to one tenth of the bulk of the water; and by boiling the water thus impregnated, he procured again a certain portion of the nitrous gas.

Humbolt, in his paper on eudiometry, mentions the diminution of nitrous gas by water. This diminution, he supposes to arise from the decomposition of a portion of the nitrous gas, by the water, and the consequent formation of nitrate of ammoniac.*

* He says, " On a observé, (depuis qu'on travaille sur le " pureté de l'air) que le gaz nitreux, secoué avec l'eau, en " souffre une diminution de volume. Quelques physiciens " attribuent ce changement à une vraie absorption, à une " dissolution du gaz nitreux dans l'eau; d'autres à l'air con- " tenu dans les interstices de tous les fluides. Le cit. " Vanbreda, à Delft, a fait des recherches très-exactes sur " l'influence des eaux de pluie et de puit, sur les nombres " eudiométriques; et les belles expériences du cit. Hassen-

I confefs, that even before the following experiments were made, I was but little inclined to adopt this opinion : the fmall diminution of nitrous gas by water, and the uniform limits of this diminution, rendered it extremely improbable.

*a.* To afcertain the quantity of nitrous gas

" fratz, fur l'abondance d'oxygène, contenue dans les eaux " de neige et de pluie, font fuppofer que l'air des interftices " de l'eau joue un róle important dans l'abforption du gaz " nitreux. En comparant ces effets avec les phénomènes " obfervé dans la decompofition du fulfate de fer, nous fup- " posâmes, le cit. Taffaert et moi, que le fimple contact du " gaz nitreux avec l'eau diftillée pourroit bien caufer une " décompofition de ce dernier. Nous examinâmes foign- " eufement une petite quantité d'eau diftillée, fecouée avec " beaucoup de gas nitreux trés-pur, et nous trouvâmes, " au moyen de la terre calcaire, et l'acide muriatique, qu'il " s'y forme du *nitrate d'ammoniaque.* L'eau fe décompofe " en cette opération, par un double affinité de l'oxygene " pour le gaz nitreux, et de l'hydrogène pour l'azote; il fe " forme de l'acide nitrique et de l'*ammoniaque*; et, quoique " la quantité du dernier paroiffe trop petite pour en évaluer " exactment la quantité, fon exiftence cependant fe mani- " fefte, (à ne pas fans douter) par le dégagement des va- " peurs, qui blanchiffent dans la proximité de l'acide mu- " riatique. Voilá un fait bien frappant que la compofition " d'une fubftance alcaline par le contact d'une acide, et de " l'eau.

Annales de Chimie, t. xxviii. pag. 153.

absorbable by pure water, and the limits of absorption, I introduced into a glass retort about 5 ounces of water, which had been previously boiled for some hours. The neck of the retort was inverted in mercury, and the water made to boil. After a third of it had been distilled, so that no air could possibly remain in the retort, the remainder was driven over, and condensed in an inverted jar filled with mercury. To three cubic inches of this water,* confined in a cylinder graduated to ,05 cubic inches, 5 cubic inches of nitrous gas, containing nearly one thirtieth nitrogene, were introduced.

After agitation for near an hour, rather more than $\frac{4}{20}$ of a cubic inch appeared to be absorbed; but though the process was continued for near two hours longer, no further diminution took place.

The remaining gas was introduced into a tube graduated to ,02 cubic inches. It measured $\frac{14}{50}$; hence $\frac{11}{50}$ had been absorbed.

* Which was certainly as free from air as it ever can be obtained.

Consequently, 100 cubic inches of pure water are capable of absorbing 11,8 of nitrous gas. In the water thus impregnated with nitrous gas I could distinguish no peculiar taste;* it did not at all alter the color of blue cabbage juice.

*b.* To determine if the absorption of nitrous gas was owing to a decomposition of it by the water, as Humbolt has supposed, or to a simple solution; I procured some nitrous gas from nitrous acid and mercury, containing about one seventieth nitrogene. ,5 cubic inches of it, mingled with 25, of oxygene, from sulphuric acid and manganese left a residuum of ,03. 5 cubic inches more were introduced to 3 of water, procured in the same manner as in the last experiment, in the same cylinder.

* Dr. Priestley found distilled water, saturated with nitrous air, to acquire an astringent taste and pungent smell. In some unboiled impregnated pump water, I once thought that I perceived a subacid taste; but it was extremely slight, and probably owing to nitrous acid formed by the union of the oxygene of the common air in the water, with some of the nitrous gas.

After the diminution was complete, the cylinder was transferred in a ſmall veſſel containing mercury, into a water bath, and nearly covered by the water.

As the bath was heated, ſmall globules of gas were given out from the impregnated water; and when it began to boil, the production of gas was ſtill more rapid. After an hour's ebullition, the volume of heated gas was equal to 1,4 cubic inches nearly.

The cylinder was now taken out of the bath, and quickly rendered cool by being placed in a water apparatus. At the common temperature the gas occupied, as nearly as poſſible, the ſpace of ,5 cubic inches: theſe ,5 mingled with ,25 of oxygene, of the ſame kind as that employed before, left a reſiduum nearly equal to ,03.

From this experiment, which was repeated with nearly the ſame reſults, it is evident,

1, That nitrous gas is not decompoſable by pure water.

2, That the diminution of volume of nitrous gas placed in contact with water, is owing to a ſimple ſolution of it in that fluid.

3, That at the temperature of 212°, nitrous gas is incapable of remaining in combination with water.

Humbolt's opinion relating to the decompofition of nitrous gas by water, is founded upon the difengagement of vapor from diftilled water impregnated with nitrous gas, by means of lime, which became white in the proximity of the muriatic acid. But this is a very imperfect, and fallacious teft, of the prefence of ammoniac. I have this day, April 2, 1800, heated 4 cubic inches of diftilled water, impregnated with nitrous gas, with cauftic lime; the vapor certainly became a little whiter when held over a veffel containing muriatic acid; but the vapor of diftilled water produced precifely the fame appearance,* which was owing, moft likely, to

* As carbonic acid and ammoniac are both products of animalifation, is it not probable that our common waters particularly thofe in, and near towns and cities, contain carbonate of ammoniac? If fo, this falt will always exift in them after diftillation. In the experiments on carbonate of ammoniac, to which I have often alluded, I found, in diftilling a folution of this falt in water, that before half of

the combination of the acid with the aqueous vapor. Indeed, when I added a particle of nitrate of ammoniac, which might have equalled one twentieth of a grain, to the lime and impregnated water, the increaſed whiteneſs of the vapor was but barely perceptible, though this quantity of nitrate of ammoniac is much more conſiderable than that which could have been formed, even ſuppoſing the nitrous gas decompoſed.

VI. *Of the abſorption of Nitrous Gas by Water of different kinds.*

In agitating nitrous gas over ſpring water, the diminution rarely amounts to more than one thirtieth, the volume of water being taken as unity. I at firſt ſuſpected that this great dif-

the water had paſſed into the recipient, the carbonate of ammoniac had ſublimed; ſo that the diſtilled ſolution was much ſtronger than before, whilſt the water remaining in the retort was taſteleſs. Will this ſuppoſition at all explain Humbolt's miſtake?

ference in the quantity of gas abſorbed by ſpring water, and pure water, depended on carbonic acid contained in the laſt, diminiſhing the attraction of it for nitrous gas: but by long boiling a quantity of ſpring water confined by mercury, I obtained from it about one twentieth of its bulk of air, which gave nearly the ſame diminution with nitrous gas, as atmoſpheric air.

This fact induced me to refer the difference of diminution to the decompoſition of the atmoſpheric air held in ſolution by the water, the oxygene of which I ſuppoſed to be converted into nitric acid, by the nitrous gas, whilſt the nitrogene was liberated; and hence the increaſed reſiduum.

*a.* I expoſed to pure water, that is, water procured by diſtillation under mercury, nitrous gas, containing a known quantity of nitrogene. After the abſorption was complete, I found the ſame quantity of nitrogene in the reſiduum, as was contained in a volume of gas equal to the whole quantity employed.

*b.* Spring water boiled for ſome hours, and ſuffered to cool under mercury, abſorbed a quantity of nitrous gas equal to one thirteenth of its bulk; which is not much leſs than that abſorbed by pure water.

*c.* I expoſed to ſpring water, 10 meaſures of nitrous gas; the compoſition of which had been accurately aſcertained; the diminution was one twenty-eighth, the volume of water being taken as unity. On placing the reſiduum in contact with ſolution of ſulphate of iron, the nitrogene remaining was nearly one-twentieth more than had been contained by the gas before its expoſure to water.

*d.* Diſtilled water was ſaturated with common air, by being agitated for ſome time in the atmoſphere. Nitrous gas placed in contact with this water, underwent a diminution of $\frac{1}{18}$; the volume of water being unity. The gas remaining after the abſorption contained about one twenty-ſeventh nitrogene more than before.

*e.* Nitrous gas expoſed to water combined with about one fourth of its volume of carbonic acid, diminiſhed to $\frac{1}{32}$* nearly. The remainder contained little or no ſuperabundant nitrogene.

From theſe obſervations it appears, that the different degrees of diminution of nitrous gas by different kinds of water, may depend upon various cauſes.

1. Leſs nitrous gas will be abſorbed by water holding in ſolution earthy ſalts, than by pure water; and in this caſe the diminution of the attraction of water for nitrous gas will probably be in the ratio of the quantities of ſalt combined with it. *a. b.*

2. The apparent diminution of nitrous gas in water, holding in ſolution atmoſpheric air, will be leſs than in pure water, though the abſolute diminution will be greater; for the ſame portion will be abſorbed, whilſt another portion is combined with the oxygene of the atmoſpheric air contained in the water; and from the diſengagement of the

* The water ſtill being unity.

nitrogene of this air, arifes an increafed refiduum. *c. d.*

3. Probably in waters containing nitrogene, hydrogene, and other gafes, abforbable only to a flight extent, the apparent diminution will be lefs, on account of the difengagement of thofe gafes from the water, by the ftronger affinity of nitrous gas for that fluid.

4. In water containing carbonic acid, and probably fome other acid gafes, the diminution will be fmall in proportion to the quantity of gas contained in the water: the affinity of this fluid for nitrous gas being diminifhed by its greater affinity for the fubftance combined with it. *e.*

The different diminution of nitrous gas when agitated in different kinds of water, has been long obferved by experimenters on the conftituent parts of the atmofphere, and various folutions have been given of the phænomenon; the moft fingular is that of Humbolt.* He fuppofes

* He fays "100 parties de gaz nitreux, (à o.14 d'azote) fe- "couées avec l'eau diftillée, récemment cuite, diminuent en

that the apparent diminution of nitrous gas is less in spring water than distilled water, on account of the decomposition of the carbonate of lime contained in the spring water, by the nitrous acid formed from the contact of nitrous gas with the water; the carbonic acid disengaged from this decomposition increasing the residuum.

This opinion may be confuted without even reference to my observations. It is, indeed,

" volume de 0.11, ou 0.12. Ce même gaz, en contact avec
" l'eau de puits, ne perd que 0.02. La cause de cette dif-
" férence de 0.9, ou 0.10, ne doit pas être attribuée ni à
" l'impurité de l'air atmosphérique, contenu dans les inter-
" stices de l'eau, ni à la décomposition de cette eau même.
" Elle n'est qu'apparente; car l'acide nitrique, qui se forme
" par le contact du gaz nitreux avec l'eau de puits, en dé-
" compose le carbonate de chaux. Il se dégage de l'acide
" carbonique, qui, en augmentant le volume du residu, rend
" l'absorption du gaz nitreux moins sensible. Pour déter-
" miner la quantité de cet acide carbonique, je lavai le
" résidu avec de l'eau de chaux. Dans un grand nombre
" d'expériences, le volume diminua de 0.09, ou 0,07. Il
" faut en concluire que l'eau de puits absorbe réellement
" 9 + 2, ou 7 + 2 parties de gas nitreux, c'est-à-dire, à
" peu-près la même quantité que l'eau distillée."

Annales de Chimie, xxviii. pag. 154.

altogether unworthy of a philoſopher, generally acute and ingenious. He ſeems to have forgotten that carbonic acid is abſorbable by water.

### VII. *Of the abſorption of Nitrous Gas, by ſolution of pale green Sulphate of Iron.*

*a.* The diſcovery of the exact difference between the ſulphates of iron, is owing to Prouſt.* According to the ingenious reſearches of this chemiſt, there exiſt two varieties of ſulphate of iron, the green and the red. The oxide in the green ſulphate contains $\frac{27}{100}$ oxygene. This ſalt, when pure, is inſoluble in ſpirit of wine; its ſolution in water is of a pale green color; it is not altered by the gallic acid, and affords a white precipitate with alkaline pruſſiates.

The red ſulphate of iron is ſoluble in alcohol and uncryſtalizable; its oxide contains $\frac{48}{100}$ oxygene. It forms a black precipitate with the gallic acid, and with the alkaline pruſſiates, a blue one.

* Nicholſon's Phil. Jour. No. 1, p. 453.

The common ſulphates of iron generally conſiſt of combinations of theſe two varieties in different proportions.

The green ſulphate may be converted into the red by oxygenated muriatic acid or nitric acid. The common ſulphate may be converted into green ſulphate, by agitation in contact with ſulphurated hydrogene.

The green ſulphate has a ſtrong affinity for oxygene, it attracts it from the atmoſphere, from oxygenated marine acid, and nitric acid. The alkalies precipitate from it a pale green oxide, which if expoſed to the atmoſphere, rapidly becomes yellow red.

The red ſulphate of iron has no affinity for oxygene, and when decompoſed by the alkalies, gives a red precipitate, which undergoes no alteration when expoſed to the atmoſphere.*

*b*. The abſorption of nitrous gas by a ſolution of ſulphate of iron, was long ago diſcovered by

* I have been able to make theſe obſervations on the ſulphates of iron, moſt of them after Prouſt.

Prieſtley. During this abſorption, he remarked a change of color in the ſolution, analogous to that produced by the mixture of it with nitric acid.

This chemical fact has been lately applied by Humbolt, to the diſcovery of the nitrogene generally mingled with nitrous gas.

Vauquelin and Humbolt have publiſhed a memoir, on the cauſes of the abſorption* of nitrous gas by ſolution of ſulphate of iron. They ſaturated an ounce and half of ſulphate of iron in ſolution, with 180 cubic inches of nitrous gas.

Thus impregnated it ſtrongly reddened tincture of turnſoyle; when mingled with ſulphuric acid, gave nitric acid vapor; and ſaturated with potaſh, ammoniacal vapor.

By analyſis, it produced as much ammoniac as that contained in 4 grains of ammoniacal muriate, and a quantity of nitric acid equal to that exiſting in 17 grains of nitre. Hence they

* Annales de chimie, vol. xxviii. pag. 182.

concluded, that the nitrous gas and a portion of the water of the ſolution, had mutually decompoſed each other; the oxygene of the water combining with the oxygene and a portion of the nitrogene of nitrous gas to form nitric acid; and its hydrogene uniting with the remaining nitrogene, to generate ammoniac.

They have taken no notice of the nature of the ſulphate of iron employed, which was moſt probably the common or mixed ſulphate; nor of the attraction of the oxide of iron in this ſubſtance for oxygene.

*c*. Before I was acquainted with the obſervations of Prouſt, the common facts relating to the oxygenation of vitriol of iron induced me to ſuppoſe, that the attraction of this ſubſtance for oxygene was in ſome way connected with the proceſs of abſorption. The compariſon of the experiments of Humbolt and Vauquelin, with the obſervations of Prouſt, enabled me to diſcover the true nature of the proceſs.

I procured a ſolution of red ſulphate of iron, by paſſing oxygenated muriatic acid through a ſolution of common ſulphate of iron, till it gave only a red precipitate, when mingled with cauſtic potaſh. To nitrous gas confined by mercury, a ſmall quantity of this ſolution was introduced. On agitation, its color altered to muddy green; but the abſorption that took place was extremely trifling: in half an hour it did not amount to ,2, the volume of the ſolution being unity, when it had nearly regained the yellow color.

I now obtained a ſolution of green ſulphate of iron, by diſſolving iron filings in diluted ſulphuric acid. The ſolution was agitated in contact with ſulphurated hydrogene, and afterwards boiled; when it gave a white precipitate with pruſſiate of potaſh.

A ſmall quantity of this ſolution agitated in nitrous gas, quickly became of an olive brown, and the gas was diminiſhed with great rapidity; in two minutes, a quantity equal to four times the volume of the ſolution, had been abſorbed.

Theſe facts convinced me that the ſolubility of nitrous gas in common ſulphate of iron, chiefly depended upon the pale green ſulphate contained by it; and that the attraction of one of the conſtituents of this ſubſtance, the green oxide of iron, for oxygene, was one of the cauſes of the phænomenon.

*d.* Green ſulphate of iron rapidly decompoſes nitric acid. It was conſequently difficult to conceive how any affinities exiſting between nitrous gas, water, and green ſulphate of iron, could produce the nitric acid found in the experiments of Vauquelin and Humbolt.

To aſcertain if the preſence of a great quantity of water deſtroyed the power of green ſulphate of iron to decompoſe nitric acid, I introduced into a cubic inch of ſolution of green ſulphate of iron, two drops of concentrated nitric acid.

The ſolution aſſumed a very light olive color; pruſſiate of potaſh mingled with a little of it, gave a dark green precipitate. Hence the nitric acid had been evidently decompoſed. As no nitrous gas was given out, which is always the caſe when nitric acid is poured on cryſtaliſed ſulphate of iron, I ſuſpected that a compleat decompoſition of the acid had taken place; but when the ſolution was heated, a few minute globules of gas were liberated, and it gradually became ſlightly clouded.

Having often remarked that no precipitation is ever produced during the converſion of green ſulphate of iron into red, by oxygenated muriatic acid, or concentrated nitric acid, I could refer the cloudineſs to no other cauſe than to the formation of ammoniac.

To aſcertain if this ſubſtance had been produced, a quantity of ſlacked cauſtic lime was thrown into the ſolution. On the application of heat, the ammoniacal ſmell was diſtinctly perceptible, and the vapor held over orange nitrous acid, gave denſe white fumes.

*e*. When I conſidered this fact of the decompoſition of nitric acid and water by the ſolution of green ſulphate of iron, and the change of color effected in it by the abſorption of nitrous gas, exactly analogous to that produced by the decompoſition of nitric acid; I was induced to believe that the nitric acid found in the analyſis of Vauquelin and Humbolt, had been formed by the combination of ſome of the nitrous gas thrown into the ſolution with the oxygene of the atmoſphere: and that the abſorbability of nitrous gas, by ſolution of green ſulphate of iron, was owing to a decompoſition produced by the combination of its oxygene with the green oxide of iron, and of its nitrogene with the hydrogene diſengaged from water, decompounded at the ſame time.

To aſcertain this, I procured a quantity of nitrous gas: it was ſuffered to remain in contact with water for ſome hours after its production. Transferred to the mercurial apparatus, it gave no white vapor when placed in contact with ſolution of ammoniac; and conſequently held no nitric acid in ſolution.

Into a graduated jar filled with mercury, a cubic inch of concentrated ſolution of pure green ſulphate of iron was introduced, and 7 cubic inches of nitrous gas admitted to it. The ſolution immediately became dark olive at the edges, and on agitation this color was diffused through it. In 3 minutes, when near $5\frac{3}{4}$ cubic inches had been absorbed, the diminution ceased. The solution was now of a bright olive brown, and transparent at the edges. After it had rested for a quarter of an hour, no farther absorption was observed; the color was the same, and no precipitation could be perceived. A little of it was thrown into a small glass tube, under the mercury, and examined in the atmosphere. Its taste was rather more astringent than that of solution of green sulphate; it did not at all alter the color of red cabbage juice. When a little of it was poured on the mercury, it soon lost its color, its taste became acid, and it quickly reddened cabbage juice, even rendered green by an alkali.

To the solution remaining in the mercurial jar, a small quantity of prussiate of potash was introduced, to ascertain if any red sulphate of iron had been formed; but instead of the production of either a blue, or a white precipitate, the whole of the solution became opaque, and chocolate colored.

Surprifed at this appearance, I was at firft induced to fuppofe, that the ammoniac formed by the nitrogene of the nitrous gas and the hydrogene of the water, had been fufficient to precipitate from the fulphuric acid, the red oxide of iron produced, and that the color of the mixture was owing to this precipitation. To diffolve any uncombined oxide that might exift in the folution, I added a very minute quantity of diluted fulphuric acid; but little alteration of color was produced. Hence, evidently, no red oxide had been formed.

This unexpected refult obliged me to theorife a fecond time, by fuppofing that nitrate of ammoniac had been produced, which by combining with the white pruffiate of iron, generated a new combination. But on mingling together green fulphate of iron, pruffiate of potafh, and nitrate of ammoniac in the atmofphere, the mixture remained perfectly white.

To afcertain if any nitric acid exifted, combined with any of the bafes, in the impregnated folution, I introduced into it an equal bulk of diluted ſulphuric acid: it became rather paler; but no green or blue tinge was produced.

That the pruſſic acid had not been decompoſed, was evident from the bright green produced, when leſs than a grain of dilute nitric acid was admitted into the ſolution.

*f.* From theſe experiments it was evident, that no red ſulphate of iron, or nitric acid, and conſequently no ammoniac, had been produced after the abſorption of nitrous gas by green ſulphate of iron. And when I compared them with the obſervations of Prieſtley, who had expelled by heat a minute quantity of nitrous gas from an impregnated ſolution of common ſulphate of iron, and who found common air phlogiſticated by ſtanding in contact with it, I began to ſuſpect that nitrous gas was ſimply diſſolved in the ſolution, without undergoing decompoſition.

*g.* To determine more accurately the nature of the proceſs, I introduced into a mercurial cylinder 410 grains of ſolution of green ſulphate of iron, occupying a ſpace nearly equal to a cubic inch and quarter; it was ſaturated with nitrous gas, by abſorbing 8 cubic inches. This ſaturated ſolution exhibited the ſame appearance as the laſt; and after remaining near an hour untouched, had evidently depoſited no oxide of iron, nor gained any acid properties.

Into a ſmall mattraſs filled with mercury, having a tight ſtopper with a curved tube adapted to it, the greater part of this ſolution was introduced; judging from the capacity of the mattraſs, about 50 grains of it might have been loſt. To prevent common air from coming in contact with the ſolution, the ſtopper was introduced into the mattraſs under the mercury; the curved tube connected with a graduated cylinder filled with that ſubſtance; and the mattraſs brought over the ſide of the mercurial trough. But in ſpite of theſe precautions a large globule of common air got into the top of the mattraſs, from the curvature of the tube. When the heat of a ſpirit lamp was applied to the ſolution, it gave out gas with great rapidity, and gradually loſt its color. When 5 cubic inches were collected it became perfectly pale green, whilst a yellow red precipitate was deposited on the bottom of the mattrass.

On pouring a little of the clear solution into prussiate of potash, it gave only white prussiate of iron.

But on introducing a particle of sulphuric acid into the solution, sufficient to dissolve some of the red precipitate, and then pouring a little of it into a solution of prussiate of potash, it gave a fine blue prussiate of iron.

Hence the red precipitate was evidently red yellow oxide of iron.

I now examined the gas, suspecting that it was nitrous oxide. On mingling a little of it with atmospheric air, it gave red vapor, and diminished. Solution of sulphate of iron introduced to the remainder, almost wholly absorbed it: the small residual globule of nitrogene could not equal one thirtieth of a cubic inch.

Consequently it was nitrous gas, nearly pure.

Caustic potash was now introduced into the solution, till all the oxide of iron was precipitated. The solution, when heated, gave a ſtrong ſmell of ammoniac, and denſe white fumes when held over muriatic acid. It was kept at the heat of ebullition till the evaporation had been nearly compleated. Sulphuric acid poured upon the reſiduum gave no yellow fumes, or nitric acid vapor in any way perceptible; even when heated and made to boil, there was no indication of the production of any vapor, except that of the ſulphuric acid.

*h.* This experiment, compared with the others, ſeemed almoſt to prove, that nitrous gas combined with ſolution of pale green ſulphate of iron, at the common temperature, without decompoſition; and that when the impregnated ſolution was heated, the greater portion of gas was diſengaged, whilſt the remainder was decompounded by the green oxide of iron; which attracted at the ſame time oxygene from the water and the nitrous gas; whilſt their other conſtituent principles, hydrogene and nitrogene, entered into union as ammoniac.

Whilſt, however, I was reaſoning upon this ſingular chemical change, as affording preſumptive proofs in favor of the exertion of ſimple affinities by the conſtituent parts of compound ſubſtances, a doubt concerning the decompoſition of the nitrous gas occurred to me. As near as I could gueſs at the quantity of nitrous gas contained by the impregnated ſolution, at leaſt $\frac{3}{4}$ of it muſt have been expelled undecompounded.

More than a quarter of a cubic inch of common air had been preſent in the mattraſs: the oxygene of this common air muſt have combined with the nitrous gas, to form nitric acid. Might not this nitric acid have been decompoſed, and furniſhed oxygene to the red oxide of iron, and nitrogene to the ſmall quantity of ammoniac found in the ſolution, as in *d*?

*i*. I now introduced to a ſolution of green ſulphate confined by mercury, nitrous gas, perfectly free from nitric acid. When the ſolution was ſaturated, a portion of it was introduced into a ſmall mattraſs filled with dry mercury, in the mercurial trough. The curved tube was cloſed by a ſmall cork at the top, and filled with nitrous gas; it was then adapted to the mattrafs, which was raifed from the trough, and the folution thus effectually preferved from the contact of the atmofphere.

When the heat of a fpirit lamp was applied to the mattrafs, it began to give out gas with great rapidity. After fome time the folution loft its dark color, and became turbid. When the production of nitrous gas had ceafed, it was fuffered to cool. A copious red precipitate had fallen down; which, examined by the fame tefts as in the laft experiment, proved to be red oxide of iron.

The folution treated with lime, as before, gave ammoniac; but with fulphuric acid, not the flighteft indications of nitric acid.

*k*. Having thus procured full evidence of the decompofition of nitrous gas in the heated folution, in order to gain a more accurate acquaintance with the affinities exerted, I endeavoured to afcertain the quantity of nitrous gas decompofed by a given folution, under known circumftances.

Into a cylinder of the capacity of 20 cubic inches, inverted in mercury, 1150 grains of ſolution of green ſulphate of iron, of ſpecific gravity 1.4, were introduced. Nitrous gas was admitted to it, and after ſome time 21 cubic inches were abſorbed.

The impregnated ſolution was thrown into a mattraſs, in the ſame manner as in the laſt experiment, and the ſame precautions taken to preſerve it from the contact of atmoſpheric air. A quantity was loſt during the proceſs of tranſferring, which, reaſoning from the ſpace occupied in the mattraſs by the remaining portion, as determined by experiment afterwards, muſt have amounted nearly to 240 grains.

The curved tube from the mattraſs was now made to communicate with the mercurial air-holder. By the application of heat 12,5 cubic inches of nitrous gas were collected, after the common temperature had been reſtored to the mattraſs; which was ſuffered to remain in communication with the conducting tube.

The ſolution was now pale green, that is, of its natural color, and a confiderable quantity of red oxide of iron had been depofited.

Solid cauftic potafh was introduced into it, till all the green oxide of iron had been precipitated, and till the folution rendered green, red cabbage juice.

A tube was now accurately connected with the mattrafs, bent, and introduced into a fmall quantity of diluted fulphuric acid. Nearly half of the fluid in it was flowly diftilled into the fulphuric acid, by the heat of a fpirit lamp. The impregnated acid evaporated at a heat above 212°, and gave a fmall quantity of cryftalifed falt, which barely amounted to two grains and quarter: it had every property of fulphate of ammoniac. Sulphuric acid in excefs was poured on the refiduum, and the whole diftilled by a heat not exceeding 300°, into a fmall quantity of water. This water, after the procefs, tafted ftrongly of fulphuric acid; it had no peculiar odor. Tin thrown into it when heated, was not perceptibly oxydated; mingled with ftrontitic lime water, it gave a copious white precipitate, and after the precipitation became almoſt taſteleſs. Hence it evidently contained no nitric acid.

The 12,5 cubic inches of undecompounded gas that came over were examined; and accounting for the ſmall quantity of common air previouſly contained in the airholder, muſt have been almoſt pure.

*l.* Now ſuppoſing 927 grains of the impregnated ſolution (including the weight of the nitrous gas), to have been operated upon, this muſt have contained about 16,7 cubic inches of nitrous gas. But 12,5 cubic inches eſcaped undecompounded: hence 4,2 were decompoſed; and theſe weigh 1,44 grains, and are compoſed of ,8 oxygene, and ,64 nitrogene.*

Conſequently, the nitrous gas muſt have furniſhed ,8 of oxygene to the green oxide of iron.

But ,64 of nitrogene require ,15 of hydrogene to form ,79 of ammoniac :† conſequently 1 of

* Diviſion IV. Section 5.

† Diviſion II. Section 1.

water was decompounded, and this furniſhed ,85 of oxygene to the green oxide of iron.

The green oxide of iron contains $\frac{27}{100}$ oxygene; the red $\frac{48}{100}$. But the whole quantity of oxygene ſupplied from the water and nitrous gas is $8 + 85 = 1,65$; and calculating on the difference of the compoſition of the red and green oxide of iron, 5,7 grains of red oxide muſt have been depoſited, and conſequently theſe would ſaturate as much acid as ,79 grains of ammoniac, or 4,1 grains of green oxide of iron.*

And ſuppoſing the ammoniac in ſulphate of ammoniac to be to the acid as 1 is to 3,† 3.2 grains of ſulphate of ammoniac muſt have been formed, containing about 2,4 grains acid; and then 6,5 grains of green ſulphate of iron muſt have been decompoſed.

Hence we gain the following equation:

* No precipitation takes place during the converſion of ſolution of green ſulphate into red; and the acid appears ſaturated.

† Diviſion II, Section 6.

6,5 green ſ. = 2,41 ſul. acid + 4,1 gr. ox. iron.
+
1,44 nit. gas = ,64 nitrogene + ,8 oxygene.
+
1 water = ,85 oxygene + ,15 hydrogene.
equal
3,2 ſul. am. = 2,41 ſ. acid + ,64 nit. + ,15 hyd.
+
5,7 r. ox. iron = 4,1 gr. ox. iron + 1,6 oxyg.

Though the eſtimation of the quantities in this equation muſt not be conſidered as ſtrictly accurate, on account of the degree of uncertainty that remains concerning the exact numerical expreſſion of the quantities of the conſtituents of water, ammoniac, and the other compound bodies employed; yet as founded on a ſimple quantity, that is, the nitrous gas decompoſed, it cannot be very diſtant from the truth.

The ſulphate of ammoniac given by experiment, is conſiderably leſs than that which was really produced; much of it was probably carried off during the evaporation of the ſuperabundant acid.

The conclusions that may be drawn from this experiment, afford a striking instance of the importance of the application of the science of quantity to the chemical changes: for the data being one chemical fact, the decomposition of a given quantity of nitrous gas by known agents; the composition of nitrous gas, of water, ammoniac, the oxides of iron, and sulphate of ammoniac; we are able not only to determine the quantities of the simple constituents that have entered into new arrangements, but likewise the composition of two compound bodies, the green and red sulphates of iron.*

*m.* Though from the experiments in *e* it appeared that no decomposition of nitrous gas had been produced during or even after its absorption by solution of sulphate of iron at the common temperature; yet a suspicion that it might take place slowly, and that

* According to the estimation in the equation, 6.5 of dry green sulphate of iron contain 4.1 green oxide of iron, and 2.4 of Kirwan's real sulphuric acid; and 8.1 red sulphate of iron, contain 2.4 acid, and 5.7 red oxide of iron.

indications of it might be given by depoſition, induced me to examine minutely two impregnated ſolutions, one of which had been at reſt, confined by mercury, for 19 hours, and the other for 27. In neither of them could I diſcover any depoſition, or alteration of color, which might denote a change.

Two cubic inches of oxygene were admitted to half a cubic inch of one of theſe ſolutions. The oxygene was ſlowly abſorbed, and the ſolution gradually loſt its color.

To aſcertain if during the converſion of the nitrous gas held in ſolution by ſulphate of iron, into nitric acid, by the oxygene of the atmoſphere at the common temperature, any water was decompoſed; I ſuffered an impregnated ſolution, weighing nearly two ounces, to remain in contact with the atmoſphere at 57°—62°, till it was become perfectly pale. It then had a ſtrong acid taſte, effervеſced with carbonate of potaſh, and gave a blue precipitate with pruſſiate of potaſh.—It was ſaturated with quicklime, and heated: ſlight indications of the presence of ammoniac were perceived.

As in this experiment the nitric acid had been most probably decomposed by the green oxide of iron, as in *f*, I sent oxygenated muriatic acid through an impregnated solution, till all the green oxide of iron was converted into red, and all the nitrous gas into nitric acid.

This solution saturated with potash, and heated, gave no ammoniacal smell.

From these experiments we may conclude,

1st. That solution of red sulphate of iron has little or no affinity for nitrous gas*; and that solution of common sulphate absorbs nitrous gas only in proportion as it contains green sulphate.

2dly. That solutions of green sulphate of iron dissolve nitrous gas in quantities proportionable to their concentration, without effecting

* The muddy green color produced in a solution of red sulphate of iron agitated in nitrous gas, depended upon impurities in the mercury. I have since found, that when the solution is completely oxygenated, the diminution is barely perceptible.

any decompofition of it at common temperatures. And the folubility of nitrous gas in folution of green fulphate, may be fuppofed to depend on an equilibrium of affinity, produced by the following fimple attractions:

1. That of green oxide of iron for the oxygene of nitrous gas and water.

2. That of the hydrogene of the water for the nitrogene of the nitrous gas.

3. That of the principles of the fulphuric acid, for nitrogene and hydrogene.

3dly. That at high temperatures, that is, from 200° to 300°, the equilibrium of affinity producing the binary combination between nitrous gas and folution of green fulphate of iron is deftroyed; the attraction of the green oxide of iron for oxygene being increafed; whilft probably that of nitrogene for hydrogene is diminifhed.

Hence the nitrous gas is either liberated,* in

* Perhaps the liberation of nitrous gas from the folution

consequence of the affinity between oxygene and hydrogene, and oxygene and nitrogene not following the same ratio of alteration on increased temperature; or decomposed, because at a certain temperature the green oxide exerts such affinities upon water and nitrous gas, as to attract oxygene from both of them to form red oxide; whilst the still existing affinity between the hydrogene of the one, and the nitrogene of the other, disposes them to combine to form ammoniac.

4thly. That the change of color produced by introducing nitric acid to solution of common sulphate of iron, exactly analogous to that occasioned in it by impregnation with nitrous gas, is owing to the decomposition of the acid, by the combination of its oxygene with the green

takes place at a lower temperature than its decomposition. I have always observed that the quantity of yellow precipitate is greater when the solution is rapidly made to boil. Were it possible to heat it to a certain temperature at once, probably a compleat decomposition would take place.

oxide of iron, and of its nitrous gas with the ſolution.

5thly. That nitrous gas in combination with ſolution of green ſulphate of iron, is capable of exerting a ſtrong affinity upon free or looſely combined oxygene, and of uniting with it to form nitric acid.

*n.* The products obtained from a ſolution of ſulphate of iron ſaturated with nitrous gas, by Vauquelin and Humbolt, and their conſequent miſtake with regard to the nature of the proceſs of abſorption,* muſt have ariſen from expoſure of their impregnated ſolution to the atmoſphere.

Indeed, from the acidity of it, on examination, from the ſmall portion of ammoniac, and the large quantity of nitric acid obtained, it appears moſt probable that the whole of the nitrous gas employed was converted into nitric acid, by combining with atmoſpheric oxygene; for no nitric acid could have been obtained in

* Annales de Chimie. T. 38, pag. 187.

the mode in which they operated, unless the green oxide of iron in the solution had been previously converted into red.

VIII. *On the absorption of Nitrous Gas by solution of green Muriate of Iron.*

*a.* The analogy between the affinities of the constituents of the muriate and sulphate of iron, induced me to conjecture that they possessed similar powers of absorbing nitrous gas; and I soon found that this was actually the case; for on agitating half a cubic inch of solution of muriated iron, procured by dissolving iron filings in muriatic acid, in nitrous gas, the gas was absorbed with great rapidity, whilst the solution assumed a deep and bright brown tinge.

*b.* Proust,* who as I have before mentioned, supposes the existence of two oxides of iron only,

* Annales de Chimie, xxiii. pag. 85; or Nicholson's Phil. Journal vol. i. pag. 45.

one containing $\frac{27}{100}$ oxygene, the other $\frac{48}{100}$, has aſſumed, that the muriatic acid, and moſt other acids as well as the ſulphuric, are capable of combining with theſe oxides, and of forming with each of them a diſtinct ſalt. He has, however, detailed no experiments on the muriates of iron.

As theſe ſalts are ſtill more diſtinct from each other in their properties than the ſulphates, and as theſe properties are connected with the phænomenon of the abſorption and decompoſition of nitrous gas, I ſhall detail the obſervations I have been able to make upon them.

*c*. When iron filings have been diſſolved in pure muriatic acid, and the ſolution preſerved from the contact of air, it is of a pale green color, and gives a white precipitate with alkaline pruſſiates. The alkalies throw down from it a light green oxide of iron.

When evaporated, it gives cryſtals almoſt white, which are extremely ſoluble in water; but inſoluble in alcohol.

The ſolution of green muriate of iron has a great affinity for oxygene, and attracts it from the atmosphere, from nitric acid, and probably from oxygenated muriatic acid.

When red oxide of iron is dissolved in muriatic acid, or when nitric acid is decomposed by solution of green muriate of iron; the red muriate of iron is produced. The solution of this salt is of a deep brown red, its odor is peculiar, and its taste, even in a very diluted state, highly astringent. It acts upon animal and vegetable matters in a manner somewhat analogous to the oxygenated muriatic acid, rendering them yellowish white, or yellow.*

Sulphuric acid poured upon it, produces a smell resembling that of oxygenated muriatic acid. Evaporated at a low temperature, it gives an uncrystalisable dark orange colored salt, which is soluble in alcohol, and when decomposed by the alkalies, gives a red precipitate. With prussiate of potash it gives prussian blue.

* Probably by giving them oxygene; whereas the green muriate and sulphate blacken animal substances; most likely by abstracting from them oxygene.

The common muriate of iron confifts of different proportions of thefe two falts. It may be converted into red muriate by concentrated nitric acid, or into green by fulphurated hydrogene.

*d.* To afcertain if folution of red muriate of iron was capable of abforbing nitrous gas, I introduced into a jar filled with mercury, a cubic inch of nitrous gas, and admitted to it nearly half a cubic inch of folution of red muriate of iron. No difcoloration took place. By much agitation, however, an abforption of nearly ,2 was produced, and the folution became of a muddy green. But this change of color, and probably the abforption, was in confequence of the oxydation of either the mercury, or fome imperfect metals combined with it, by the oxygene of the red muriate. For I afterwards found, that precifely the fame change of color was produced when a folution was agitated over mercury.

*e* I introduced to a cubic inch of concentrated folution of green muriate of iron, 7 cubic inches of nitrous gas, free from nitric acid; the ſolution inſtantly became colored at the edges, and on agitation abſorbed the gas with much greater rapidity than even ſulphate of iron; in a minute, only a quarter of a cubic inch remained.

The ſolution appeared of a very dark brown, but evidently no precipitation had taken place in it, and the edges, when viewed againſt the light, were tranſparent and puce colored.

Five cubic inches more of nitrous gas were now diſſolved in the ſolution. The intenſity of the color increaſed, and after an hour no depoſition had taken place. A little of it was then examined in the atmoſphere; it had a much more aſtringent taſte than the unimpregnated ſolution, and effected no change in red cabbage juice. When pruſſiate of potaſh was introduced into it, its color changed to olive brown. A few drops of the ſolution, that had accidentally fallen on the mercury, ſoon became colorleſs, and then effervеſced with carbonate of potaſh, and taſted ſtrongly acid.

The remainder of the impregnated ſolution, which muſt have nearly equalled ,75 cubic inches, was introduced into a mattraſs, having a ſtopper and curved tube, as in the experiments on the ſolution of ſulphate of iron; great care being taken to preſerve it from the contact of air.

The mattraſs was heated by a ſpirit lamp, the curved tube being in communication with a mercurial cylinder. Near 8 cubic inches of nitrous gas were collected, when the ſolution became of a muddy yellow. It was ſuffered to cool, and examined. A ſmall quantity of yellow precipitate covered the bottom of the mattraſs; the fluid was pellucid, and light green. A little of it thrown on pruſſiate of potaſh, gave a white precipitate, colored by ſtreaks of light blue. When the yellow precipitate was partly diſſolved by ſulphuric acid, a drop of the ſolution, mingled with pruſſiate of potaſh, gave a deep blue green.

Hence, evidently, the precipitate was red oxide of iron.

Cauſtic potaſh in exceſs was introduced into the remainder of the ſolution, and it was heated. It gave an evident ſmell of ammoniac, and denſe white fumes, when held over ſtrong phlogiſticated nitrous acid.

When half of it was evaporated, ſulphuric acid in exceſs was poured on the remainder; muriatic acid was liberated, not perceptibly combined with any nitric acid.

*f*. In an experiment that I made to aſcertain the quantity of nitrous gas capable of combining with ſolution of green muriate of iron; I found that ,75 cubic inches of ſaturated ſolution abſorbed about 18 of nitrous gas, which is nearly double the quantity combinable with an equal portion of the ſtrongeſt ſolution of ſulphate of iron. A part of this impregnated ſolution, heated ſlowly, gave out more gas in proportion to the quantity it contained, than the laſt, and conſequently produced leſs precipitate; ſo that I am inclined to ſuppoſe it probable, that at a certain temperature, all the diſſolved nitrous gas may be diſpelled from a ſolution.

From theſe experiments we may conclude,

1ſt. That the ſolution of green muriate of iron abſorbs nitrous gas in conſequence of nearly the ſame affinities as ſolution of green ſulphate of iron; its capability of abſorbing larger quantities depending moſt probably on its greater concentration (that is, on the greater ſolubility of the muriate of iron), and perhaps, in ſome meaſure, on a new combining affinity, that of muriatic acid for oxygene.

2dly. That at certain temperatures nitrous gas is either liberated from ſolution of green muriate, or decompoſed, by the combination of its oxygene with green oxide of iron, and of its nitrogene with hydrogene, produced by water decompounded by the oxide at the ſame time.

IX. *Abſorption of Nitrous Gas by Solution of Nitrate of Iron.*

*a.* As well as two ſulphates and two muriates of iron, there exist two nitrates.* When concentrated nitric acid is made to act upon iron, nitrous gas is disengaged with great rapidity, and with great increase of temperature: the solution assumes a yellowish tinge, and as the process goes on, a yellow red oxide is precipitated.

Nitrate of iron made in this way, gives a bright blue mingled with prussiate of potash, and decomposed by the alkalies, a red precipitate. Its solution has little or no affinity for nitrous gas.

*b.* When very dilute nitric acid, that is, such as of specific gravity 1,16, is made to oxydate iron, without the assistance of heat, the solution gives out no gas for some time, and becomes dark olive brown: when neutralised it gives, decomposed by the alkalies, a light green precipitate; and mingled with prussiate of potash, pale green prussiate of iron.

* The existence of green nitrate was not suspected by Proust.

It owes its color to the nitrous gas it holds in ſolution. By expoſure to the atmoſphere it becomes pale, the nitrous gas combined with it being converted into nitric acid.

It is then capable of abſorbing nitrous gas, and conſiſts of pale nitrate of iron, mingled with red nitrate.

I have not yet obtained a nitrate of iron giving only a white precipitate with pruſſiate of pot-aſh, that is, ſuch as contains *only* oxide of iron at its minimum of oxydation; for when pure green oxide of iron is diſſolved by very dilute nitric acid, a ſmall quantity of the acid is generally decompoſed, which is likewiſe the caſe in the decompoſition of nitre by green ſulphate of iron. The ſolutions of nitrate of iron, how-ever, procured in both of theſe modes, abſorb nitrous gas with rapidity, and by ſulphurated hydrogene might probably be converted into pale nitrate.

As it is impoſſible to obtain concentrated ſolutions of pale nitrate of iron, chiefly containing green oxide, its powers of abſorbing nitrous gas cannot be compared with the muriatic and fulphuric folutions, unlefs they are made of nearly the fame fpecific gravity.

Nitrous gas is difengaged by heat from the impregnated folution of nitrate of iron, at the fame time that much red oxide of iron is precipitated. Whether any nitrous gas is decompofed, I have not yet afcertained; for when unimpregnated pale nitrate of iron is heated, a part of the acid, and of the water of the folution, is decompofed by the green oxide of iron;* and in confequence ammoniac, and red nitrate of iron formed, whilft red oxide is precipitated.

X. *Abforption of Nitrous Gas by other Metallic Solutions.*

*a.* White pruffiate of iron in contact with water abforbs nitrous gas to a great extent, and

* In this procefs nitrous oxide is fometimes given out, as will be feen hereafter.

becomes dark chocolate.*

*b.* Concentrated ſolution of ſulphate of tin, *probably* at its minimum of oxydation, abſorbs one eighth of its bulk of nitrous gas, and becomes brown, without depoſition.

*c.* Solution of ſulphate of zinc‡ abſorbs about one tenth of its volume of nitrous gas, and becomes green.

*d.* Solution of muriate of zinc‡ abſorbs nearly the ſame quantity, and becomes orange brown.

*e.* Theſe are all the metallic ſubſtances on which I have experimented. It is more than probable that there exiſt others poſſeſſing ſimilar powers of abſorbing nitrous gas.

Whenever the metals capable of decompoſing water exiſt in ſolutions at their minimum of oxydation, the affinities exerted by them on

* Hence we learn why no nitrous gas is diſengaged when impregnated ſolution of ſulphate of iron is decompoſed by pruſſiate of potaſh, as in Div. IV. Sec. vii.

‡ In both of theſe ſolutions the metal is at its minimum of oxydation. The abſorption of a ſmall quantity of nitrous gas by white vitriol was obſerved by Prieſtley.

nitrous gas and water, will be ſuch as to produce combination. The powers of metallic ſolutions to combine with nitrous gas at common temperatures, as well as to decompoſe it at higher temperatures, will probably be in the ratio of the affinity of the metallic oxides they contain, for oxygene.

XI. *The action of Sulphurated Hydrogene on ſolution of Green Sulphate of Iron, impregnated with Nitrous Gas.*

*a.* In an experiment on the abſorption of nitrous gas by ſolution of green ſulphate of iron, I introduced an unboiled ſolution of common ſulphate, deprived of red oxide of iron by ſulphurated hydrogene, into a jar filled with nitrous gas; the abſorption took place as uſual, and nearly ſix of gas entered into combination, the volume of the ſolution being unity. On applying heat to a part of this impregnated ſolution, the whole of the nitrous gas it contained (as nearly as I could gueſs), was expelled undecompounded, and no yellow precipitate produced. Pruſſiate of potaſh poured into it gave only white pruſſiate of iron; and when it was heated with lime, no ammoniacal ſmell was perceptible.

I could refer this phænomenon to no other cauſe than to the exiſtence of a ſmall quantity of ſulphurated hydrogene in the ſolution. That this was the real cauſe I found from the following experiment.

*b*. One part of a ſolution of green ſulphate of iron, formed by the agitation of common ſulphate of iron in contact with ſulphurated hydrogene, was boiled for ſome minutes to expel the ſmall quantity of gas retained by it undecompounded. It had then no peculiar ſmell, and gave a white pruſſiate of iron with pruſſiate of potaſh; the other part had a faint odor of ſulphurated hydrogene, and gave a dirty white precipitate with pruſſiate of potaſh. Nearly equal quantities of each were ſaturated with nitrous gas, and heated. The unboiled impregnated ſolution gave out all its nitrous gas undecompounded; whilſt in the boiled ſolution it was partly decompoſed, yellow precipitate and ammoniac being formed.

*c.* This ſingular phænomenon of the power of a minute quantity of ſulphurated hydrogene, in preventing the decompoſition of nitrous gas and water, by green oxide of iron, will moſt probably take place in other impregnated ſolutions. It ſeems to depend on the ſtrong affinity of the hydrogene of ſulphurated hydrogene for oxygene.

## XII. *Additional Obſervations.*

*a.* For ſeparating nitrous gas from gaſes abſorbable to no great extent by water; a well boiled ſolution of green muriate of iron ſhould be employed. Nitrous gas agitated in this is rapidly abſorbed, and it has no affinity for, or action on, nitrogene, hydrogene, or hydrocarbonate.

*b.* Nitrous gas carefully obtained from mercury and nitric acid, when received under mercury, or boiled water, and abſorbed by ſolution of green muriate, or ſulphate of iron, rarely leaves a reſiduum of $\frac{1}{200}$ of its volume: preſerved over common water, and abſorbed, the remainder is generally from $\frac{1}{40}$ to $\frac{1}{90}$, from the nitrogene diſengaged by the decompoſition of the common air contained in the water.

*c.* The nitrous gas carefully obtained from the decompoſition of nitric acid of 1.26, by copper, I have hardly ever found to contain more than from $\frac{1}{80}$ to $\frac{1}{50}$ nitrogene, when received through common water: when boiled water is employed, the reſiduum is nearly the ſame as that of nitrous gas obtained from mercury.

*d.* Conſequently the gas from thoſe two ſolutions may be uſed in common. It is more than probable, that the ſmall quantities of nitrogene generally mingled with nitrous gas from copper and mercury, ariſe either from the common air of the veſſels in which it was produced, or that of the water over which it was received. There is no reaſon for ſuppoſing that it is generated by a complete decompoſition of a portion of the acid.*

*e*. Whenever nitrous oxide is mingled with nitrous gas and nitrogene, it muſt be ſeparated by well boiled water; and after the corrections are made for the quantity of air diſengaged from the water, the nitrous gas abſorbed by the muriatic ſolution.

* Humbolt, who is the firſt philoſopher that has applied the ſolution of ſulphate of iron to aſcertain the purity of nitrous gas, aſſerts that he uniformly found nitrous gas obtained from ſolution of copper in nitrous acid, to contain from ſix tenths to one tenth nitrogene.

Annales de Chimie, vol. xxviii. pag. 147.

# DIVISION V.

*EXPERIMENTS and OBSERVATIONS on the production of NITROUS OXIDE from NITROUS GAS and NITRIC ACID, in different modes.*

## I. *Preliminaries.*

*a.* The opinions of Prieſtley* and Kirwan,† relating to the cauſes of the converſion of nitrous gas into nitrous oxide, were founded on the theory of phlogiſton. The firſt of theſe philoſophers obtained nitrous oxide by placing nitrous gas in contact with moiſtened iron filings, or the alkaline ſulphures. The laſt by expoſing it to ſulphurated hydrogene.

The Dutch chemiſts,‡ the lateſt experi-

* Vol. ii. pag. 55. † Phil. Tranſ. vol. lxxvi. pag. 133.

‡ Journal de Phyſique, tom. xliii. 323.

mentalifts on nitrous oxide, have fuppofed that the production of this fubftance depends upon the fimple abftraction of a portion of the oxygene of nitrous gas. They obtained nitrous oxide by expofing nitrous gas to muriate of tin, to copper in folution of ammoniac, and likewife by paffing it over heated fulphur.

The diminution of volume fuftained by nitrous gas during its converfion into nitrous oxide, has never been accurately afcertained; it has generally been fuppofed to be from two thirds to eight tenths.

*b*. Nitrous gas may be converted into nitrous oxide in two modes.

Firft, by the fimple abftraction of a portion of its oxygene, by bodies poffeffing a ftrong affinity for that principle, fuch as alkaline fulphites, muriate of tin, and dry fulphures.

Second, by the combination of a body with a portion both of its oxygene and nitrogene, fuch as hydrogene, when either in a nafcent form, or a peculiar ftate of combination.

c. Each of these modes will be distinctly treated of; and to prevent unnecessary repetitions, I shall give an account of the general manner in which the following experiments on the conversion of nitrous gas into nitrous oxide, have been conducted.

Nitrous gas, the purity of which has been accurately ascertained by solution of muriate of iron, is introduced into a graduated jar filled with dry mercury. If a fluid substance is designed for the conversion of the gas into nitrous oxide, it is heated, to expel any loosely combined air which might be liberated during the process; and then carefully introduced into the jar, by means of a small phial. After the process is finished, and the diminution accurately noted, the nitrous oxide formed is absorbed by pure water. If any nitrous gas remains, it is condensed by solution of muriate of iron; other residual gases are examined by the common tests. The quantity of nitrous oxide dissolved by the fluid is determined by a comparative experiment; and the corrections for temperature and preſſure being gueſſed at, the concluſions drawn.

If a ſolid ſubſtance is uſed, rather more nitrous gas than that deſigned for the converſion, is introduced into the jar. The ſubſtance is brought in contact with the gas, by being carried under the mercury; and as a little common air generally adheres to it, a ſmall portion of the nitrous gas is transferred into a graduated tube, after the inſertion, and its purity aſcertained. In other reſpects the proceſs is conducted as mentioned above.

II. *Of the converſion of Nitrous gas into Nitrous Oxide, by Alkaline Sulphites.*

The alkaline ſulphites, particularly the ſulphite of potaſh, convert nitrous gas into nitrous oxide, with much greater rapidity than any other bodies.

At temperature 46°, 16 cubic inches of nitrous gas were converted, in leſs than an hour, into 7,8 of nitrous oxide, by about 100 grains of pulverifed fulphite of potafh, containing its water of cryftalifation. No fenfible increafe of temperature was produced during the procefs, no water was decompofed, and the quantity of nitrogene remaining after the experiment, was exactly equal to that previoufly contained in the nitrous gas.

The nitrous oxide produced from nitrous gas by fulphite of potafh, has all the properties of that generated from the decompofition of nitrate of ammoniac. It gives, as will be feen hereafter, the fame products by analyfis. Phofphorus, the taper, fulphur, and charcoal, burn in it with vivid light. It is abforbable by water, and capable of expulfion from it unaltered, by heat.

Nitrous gas is converted into nitrous oxide by the alkaline fulphites with the fame readinefs, whether expofed to the light, or deprived of its influence.

The folid fulphites act upon nitrous gas much more readily than their concentrated folutions; they fhould however always be fuffered to retain their water of cryſtaliſation, or otherwiſe they attract moiſture from the gas, and render it drier, and in conſequence more condenſed than it would otherwiſe be. In caſe perfectly dry ſulphites are employed, the gas ſhould be always ſaturated with moiſture after the experiment, by introducing into the cylinder a drop of water.

The ſulphites, after expoſure to nitrous gas, are either found wholly, or partially, converted into ſulphates. Conſequently the converſion of nitrous gas into nitrous oxide by theſe bodies, ſimply depends on the abſtraction of a portion of its oxygene; the nitrogene and remaining oxygene aſſuming a more condenſed ſtate of exiſtence.

If we reaſon from the different ſpecific gravities of nitrous oxide and nitrous gas, as compared with the diminution of volume of nitrous gas, during its converſion into nitrous oxide, 100 parts of nitrous gas, ſuppoſing the former eſtimation of the compoſition of nitrous oxide given in Diviſion III, accurate, would conſiſt of 54 oxygene, and 46 nitrogene; which is not far from the true eſtimation. Or aſſuming the compoſition of nitrous gas, as given in Diviſion IV, it would appear from the diminution, that 100 parts of nitrous oxide conſiſted of 38 oxygene, and 62 nitrogene.

III. *Converſion of Nitrous Gas into Nitrous Oxide, by Muriate of Tin, and dry Sulphures.*

*a.* Nitrous gas expoſed to dry muriate of tin, is ſlowly converted into nitrous oxide: during this proceſs the apparent diminution is to about one half; but if the products are nicely examined, and the neceſſary corrections made, the real diminution of nitrous gas by muriate of tin, will be the ſame as by the ſulphites; that is, 100 parts of it will be converted into 48 of nitrous oxide.

During this converſion, no water is decompoſed, and no nitrogene evolved. Solution of muriate of tin converts nitrous gas into nitrous oxide; but with much leſs rapidity than the ſolid ſalt.

*b*. Nitrous gas expofed to dry and perfectly well made fulphures, particularly fuch as are produced from cryftalifed alumn* and charcoal not fufficiently inflammable to burn in the atmofphere, is converted into nitrous oxide by the fimple abftraction of a portion of its oxygene, and confequently undergoes a diminution of $\frac{52}{100}$.

It is probable, that all the bodies having ftrong affinity for oxygene will, at certain temperatures, convert nitrous gas into nitrous oxide. Prieftley, and the Dutch chemifts, effected the change by heated fulphur. Perhaps nitrous gas fent through a tube heated, but not ignited, with phofphorus, would be converted into nitrous oxide.

IV. *Decompofition of Nitrous Gas, by Sulphurated Hydrogene.*

*a*. When nitrous gas and fulphurated hy-

* That is, alumn containing fulphate of potafh.

drogene are mingled together, a decompoſition of them ſlowly takes place. The gaſes are diminiſhed, ſulphur depoſited, nitrous oxide formed, and ſigns of the production of ammoniac * and water perceived.

In this proceſs no ſulphuric, or ſulphureous acid is produced; conſequently none of the ſulphur is oxydated, and of courſe the changes depend upon the combination of the hydrogene of the ſulphurated hydrogene, with different portions of the oxygene and nitrogene of the nitrous gas, to form water and ammoniac, the remaining oxygene and nitrogene aſſuming the form of nitrous oxide.

This ſingular exertion of attractions by a ſimple body, appears highly improbable a priori, nor did I admit it, till the formation of ammoniac, and the non-oxygenation of the ſulphur, were made evident by many experiments.

In thoſe experiments, the diminution of the nitrous gas was not uniformly the ſame. It

* The production of ammoniac in this proceſs was obſerved by Kirwan and Auſtin.

varied from $\frac{11}{20}$ to $\frac{14}{20}$. In the moſt accurate of them, 5 cubic inches of nitrous gas were converted into 2.2 of nitrous oxide. Conſequently the quantity of ammoniac formed was ,047 grains.

In experiments on the converſion of nitrous gas into nitrous oxide, by ſulphurated hydrogene, the gaſes ſhould be rendered as dry as poſſible. The preſence of water conſiderably retards the decompoſition.

*b.* The ſulphures* diſſolved in water convert nitrous gas into nitrous oxide. This decompoſition is not, however, produced by the ſimple abſtraction of oxygene from the nitrous gas to form ſulphuric acid. It depends as well on the de-

* Solution of ſulphure of ſtrontian, or barytes, ſhould be uſed. During the converſion of nitrous gas into nitrous oxide by thoſe bodies, a thin film is depoſited on the ſurface of the ſolution. This film examined, is found to conſiſt of ſulphur and ſulphate. Poſſibly the nitrous gas is wholly decompoſed by the hydrogene of the ſulphurated hydrogene in the ſolution, whilſt the ſulphate is produced from water decompounded by the ſulphur to form more gas for the ſaturation of the hydro-ſulphure.

composition of the sulphurated hydrogene dissolved in the solution, or liberated from it. In this process sulphur is deposited on the surface of the fluid, sulphuric acid is formed, and the diminution, making the necessary corrections, is nearly the same as when free sulphurated hydrogene is employed.

It is extremely probable that sulphurated hydrogene, in combination with the alkalies, as well as with water, is capable of being slowly decomposed by nitrous gas.

V. *Decomposition of Nitrous Gas by Nascent Hydrogene.*

*a.* When nitrous gas, is exposed to wetted iron filings, a diminution of its volume slowly takes place; and after a certain time, it is found converted into nitrous oxide.

In this process ammoniac* is formed, and the iron partially oxydated.

* As was first observed by Priestley and Austin, and as I have proved by many experiments.

The water in contact with the iron is decomposed by the combination of its oxygene with that ſubſtance, and of its hydrogene with a portion of the oxygene and nitrogene of the nitrous gas, to form water and ammoniac.

That the iron is not oxydated at the expence of the oxygene of the nitrous gas, appears very probable from the analogy between this proceſs, and the mutual decompoſition of nitrous gas and ſulphurated hydrogene. Beſides, dry iron filings effect no change whatever in nitrous gas, at common temperatures.

I have generally found about 12 of nitrous gas converted into 5 of nitrous oxide in this proceſs; which is not very different from the diminution by ſulphurated hydrogene. It takes place equally well in light and darkneſs; but more rapidly in warm weather than in cold.

*b.* Nitrous gas expoſed to a large ſurface of zinc, in contact with water, is ſlowly converted into nitrous oxide; at the ſame time that ammoniac is generated, and white oxide of zinc formed. This proceſs appears to depend, like the laſt, upon the decompoſition of water by the affinities of part of the oxygene and nitrogene of nitrous gas, for its hydrogene, to form ammoniac and water; and by that of zinc for its oxygene. Zinc placed in contact with water, and confined by mercury,* decompoſes it at the common temperature. Zinc, when perfectly dry, does not in the ſlighteſt degree act upon nitrous gas.

I have not been able to determine exactly the diminution of volume of nitrous gas, during its converſion into nitrous oxide by zinc. In one experiment 20 meaſures of nitrous gas, containing about ,03 nitrogene, were diminiſhed to 9, after an expoſure of eight days to wetted zinc; but from an accident, I was not able to aſcertain the exact quantity of nitrous oxide formed.

*c*. It is probable that moſt of the imperfect metals will be found capable of oxydation, by the decompoſition of water, when its hydrogene is abſtracted by the oxygene and nitrogene of

* As I have found by experiment.

nitrous gas. I have this day (April 14, 1800), examined two portions of nitrous gas, one of which had been expofed to copper filings, and the other to powder of tin, for twenty-three days.

The gas that had been expofed to copper was diminifhed nearly two fifths. The taper burnt in it with an enlarged flame, blue at the edges. Hence it evidently contained nitrous oxide.

The nitrous gas in contact with tin had undergone a diminution of one fourth only, and did not fupport flame.

VI. *Mifcellaneous Obfervations on the converfion of Nitrous Gas into Nitrous Oxide.*

*a*. Dr. Prieftley found nitrous gas expofed to a mixture of iron filings and fulphur, with water, converted after a certain time, into nitrous oxide. Sulphurated hydrogene is always produced during the combination of iron and fulphur, when they are in contact with water; and by the hydrogene of this in the nafcent ſtate, the nitrous gas is moſt probably decompoſed.

*b*. Green oxide of iron moiſtened with water, expoſed to nitrous gas, ſlowly gains an orange tinge, whilſt the gas is diminiſhed. Moſt likely it is converted into nitrous oxide; but this I have not aſcertained.

*c* I expoſed nitrous gas, to the following bodies over mercury for many days, without any diminution, or apparent change in its properties. Alcohol, ſaccharine matter, hydro-carbonate, ſulphureous acid, and phoſphorus.

*d*. Cryſtaliſed ſulphate, and muriate of iron, abſorb a ſmall quantity of nitrous gas, and become dark colored on the outſide; but after this abſorption, (which probably depends on their water of cryſtaliſation,) has taken place, no change is effected in the gas remaining.

*e* The power of iron to decompoſe water being much increaſed by increaſe of temperature, nitrous gas is converted into nitrous oxide much more rapidly when placed in contact with a ſurface of heated iron, than when expoſed to it at common temperatures. During the decompofition of nitrous gas in this way, ammoniac * is formed.

*f.* The curious experiments of Rouppe,† on the abforption of gafes by charcoal, compared with the phænomena noticed in this Divifion, render it probable that hydrogene in a ftate of loofe combination with charcoal, will be found to convert nitrous gas into nitrous oxide.

VII. *Recapitulation of conclufions concerning the converfion of Nitrous Gas into Nitrous Oxide.*

*a.* Certain bodies having a ftrong affinity for oxygene, as the fulphites, dry fulphures, muriate of tin, &c. convert nitrous gas into nitrous oxide, by fimply attracting a portion of its oxygene; whilft the remaining oxygene

* As was obferved by Milner. Nitrous gas paffed over heated zinc, or tin, I doubt not will be found converted into nitrous oxide.

† Annales de Chimie. xxxii, p. 3.

enters into combination with the nitrogene, and they aſſume a more condenſed ſtate of exiſtence.

*b*. Nitrous gas is converted into nitrous oxide by hydrogene, in a peculiar ſtate of exiſtence, as in ſulphurated hydrogene; and that by a ſeries of very complex affinities. Both oxygene and nitrogene are attracted from the nitrous gas by the hydrogene, in ſuch proportions as to form water and ammoniac, whilſt the remaining oxygene and nitrogene * aſſume the form of nitrous oxide.

*c*. Nitrous gas placed in contact with bodies, ſuch as iron and zinc decompoſing water, is converted into nitrous oxide, at the ſame time that ammoniac is formed. It is difficult to aſcertain the exact rationale of this proceſs. For either the naſcent hydrogene produced by the decompoſition of the water by the metallic ſubſtance may combine with portions of both the

* The decompoſition and recompoſition of water, in this proceſs, are analogous to ſome of the phænomena obſerved by the ingenious Mrs. Fulhame.

oxygene and nitrogene of the nitrous gas; and thus by forming water and ammoniac, convert it into nitrous oxide. Or the metallic ſubſtance may attract at the ſame time oxygene from the water and nitrous gas, whilſt the naſcent hydrogene of the water ſeizes upon a portion of the nitrogene of the nitrous gas to form ammoniac.

The degree of diminution, and the analogy between this proceſs and the decompoſition of nitrous gas by ſulphurated hydrogene, render the firſt opinion moſt probable.

VIII. *The production of Nitrous Oxide during the oxydation of Tin, Zinc, and Iron, in Nitric Acid.*

*a.* Dr. Prieſtley diſcovered, that during the ſolution of tin, zinc, and iron, in nitric acid, certain portions of nitrous oxide were produced, mingled with quantities of nitrous gas, and nitrogene, varying in proportion as the acid employed was more or leſs concentrated.

It has long been known that ammoniac is formed during the ſolution of tin, zinc, and iron, in diluted nitric acid. Conſequently, in theſe proceſſes water is decompoſed.

I had deſigned to inveſtigate minutely theſe phænomena, ſo as to aſcertain the quantities of water and acid decompounded, and of the new products generated. But after going through ſome experiments on the oxydation of tin without gaining concluſive reſults, the labor, and ſacrifice of time they demanded, obliged me to deſiſt from purſuing the ſubject, till I had completed more important inveſtigations.

I ſhall detail the few obſervations which have occurred to me, relating to the production of nitrous oxide from metallic ſolutions.

*b.* When tin is diſſolved in concentrated nitric acid, ſuch as of 1.4, nitrous oxide is produced, mingled with generally more than twice its bulk of nitrous gas. In this proceſs but little free nitrogene is evolved, and the tin is chiefly precipitated in the form of a white powder. If the ſolution, after the generation of theſe products, is saturated with lime, and heated, the ammoniacal smell is distinct.

When nitric acid of specific gravity 1.24, is made to act upon tin; in the beginning of the process, nearly equal parts of nitrous gas and nitrous oxide are produced; as it advances, the proportion of nitrous oxide to the nitrous gas increases: the largest quantity of nitrous oxide that I have found in the gas procured from tin is $\frac{3}{4}$, the remainder being nitrous gas and nitrogene.

When tin is oxydated in an acid of less specific gravity than 1.09, the quantities of gas disengaged are very small, and consist of nitrogene, mingled with minute portions of nitrous oxide, and nitrous gas.

Whenever I have saturated solutions of tin in nitric acid of different specific gravities, with lime, and afterwards heated them, the ammoniacal smell has been uniformly perceptible, and generally most distinct when diluted acids have been employed.

*c*. When zinc is dissolved in nitric acid, whatever is its ſpecific gravity, certain quantities of nitrous oxide are produced.

Nitric acids of greater ſpecific gravity than 1.2, act upon zinc with great rapidity, and great increaſe of temperature. The gaſes diſengaged from theſe ſolutions conſiſt of nitrous gas, nitrous oxide, and nitrogene; the nitrous oxide rarely equals one third of the whole.

When nitric acid of 1,104 is made to diſſolve zinc, the gas obtained in the middle of the proceſs conſiſts chiefly of nitrous oxide. From ſuch a ſolution I obtained gas which gave a reſiduum of one ſixth only when abſorbed by water. The taper burnt in it with a brilliant flame, and ſulphur with a vivid roſe-colored light.

100 grains of granulated zinc, during their ſolution in 300 grains of nitric acid, of 1,43, diluted with 14 times its weight of water, produced 26 cubic inches of gas. Of this gas $\frac{7}{36}$ were nitrous, $\frac{17}{36}$ nitrous oxide, and the remainder nitrogene. The ſolution ſaturated with lime and heated, gave a diſtinct ſmell of ammoniac.

*d.* During the ſolution of iron in concentrated nitric acid, the gas given out is chiefly nitrous; it is however generally mingled with minute quantities of nitrous oxide. When very dilute nitric acids are made to act upon iron, by the aſſiſtance of heat, nitrous oxide is produced in conſiderable quantities, mingled with nitrous gas and nitrogene; the proportions of which are ſmaller as the proceſs advances.* The fluid remaining after the oxydation and ſolution of iron in nitric acid, always contains ammoniac.

*e.* As during the ſolution of tin, zinc, and iron, in nitric acid, the quantity of acid is diminiſhed in proportion as the proceſs advances, it is reaſonable to ſuppoſe that the relative quantities of the gaſes evolved are perpetually varying. In the beginning of a diſſolution, the nitrous gas

* From one of Dr. Prieſtley's experiments, it appears that hydrogene gas is ſometimes diſengaged during the ſolution of iron in very dilute nitric acid by heat. This phænomenon has never occurred to me.

generally predominates, in the middle nitrous oxide, and at the end nitrogene.

*f.* During the generation of nitrous gas, nitrous oxide, and ammoniac, from the decomposition of solution of nitric acid in water, by tin, zinc, and iron, very complex attractions must exist between the constituents of the substances employed. The acid and the water are decomposed at the same time, and in proportions different as the solution is more concentrated, by the combination of their oxygene with the metallic body.

The nitrous gas is produced by the combination of the metal with $\frac{32}{100}$ of the oxygene of the acid. The nitrous oxide is most probably generated by the decomposition of a portion of the nitrous gas disengaged, by the nascent hydrogene of the water decompounded; some of it may be possibly formed from a more complete decomposition of the acid.

The production of ammoniac may arise, probably from two causes; from the decomposition of the nitrous gas by the combination of the nascent hydrogene of the water, with portions of its oxygene and nitrogene at the same time; and from the union of hydrogene with nascent nitrogene liberated in consequence of a complete decomposition of part of the acid.

### IX. *Additional Observations on the production of Nitrous Oxide.*

*a.* When nitric acid is combined with muriatic acid, or sulphuric acid,* the quantities of nitrous oxide produced from its decomposition by tin, zinc, and iron, are rather increased than diminished. The nitrous oxide obtained from these solutions is, however, never sufficiently pure for physiological experiments. It is always mingled with either nitrous gas, nitrogene, or hydrogene, and sometimes with all of them.

*b.* From the solutions of bismuth, nickel,

* As was discovered by Priestley, and the Dutch Chemists.

lead, and copper, in diluted nitric acid, I have never obtained any perceptible quantity of nitrous oxide: the gas produced is nitrous, mingled with different portions of nitrogene. Antimony and mercury, during their ſolution in aqua regia, give out only nitrous gas.

Probably none of the metallic bodies, except thoſe that decompoſe water at temperatures below ignition, will generate nitrous oxide from nitric acid. On cobalt and manganeſe I have never had an opportunity of experimenting: manganeſe will probably produce nitrous oxide.

*c.* During the ſolution of vegetable matters* in nitric acid, by heat, very minute portions of nitrous oxide are ſometimes produced, always however mingled with large quantities of nitrous gas, and carbonic acid.

When nitric acid is decompounded by ether, fixed oils, volatile oils, or alcohol, towards the end of the proceſs ſmall quantities of nitrous oxide are produced, and

* Such as the leaves, bark, and wood, of trees.

fometimes fufficiently pure to fupport the flame of the taper.†

*d.* When green oxide of iron is diffolved in nitric acid, nitrous oxide is produced, mingled with nitrogene and nitrous gas.

*e* During the converfion of green fulphate, or green muriate of iron into red, by the decompofition of dilute nitric acid, nitrous oxide is formed, mingled with different proportions of nitrous gas and nitrogene.

*f.* When folution of green nitrate of iron is heated, a part of the acid is decompofed, red oxide is precipitated, red nitrate formed, and impure nitrous oxide evolved.

*g.* When iron is introduced into a folution of nitrate of copper, the copper is precipitated in its metallic ftate, whilft nitrous oxide, mingled with fmall portions of nitrogene, is produced.*

Both zinc and tin precipitate copper in its metallic form from folution in the nitric acid.

† As I have obferved after Prieftley.

* As was difcovered by Prieftley.

During theſe precipitations, certain quantities of nitrous oxide are generated, mingled however with larger quantities of nitrogene than that produced from decompoſition by iron. In all theſe proceſſes ammoniac is formed, and water conſequently decompoſed.

The decompoſition of water and nitric acid, during the precipitation of copper from ſolution of nitrate of copper, by tin, zinc, and iron, depends upon the ſtrong affinity of thoſe metals for oxygene, and their powers of combining with a larger quantity of it than copper.

X. *Decompoſition of Aqua Regia by Platina, and evolution of a Gas analogous to Oxygenated Muriatic Acid, and Nitrogene.*

*a.* De la Metherie, in his eſſay on different airs, has aſſerted that the gas produced by the ſolution of platina in nitro-muriatic acid, is identical with the dephlogiſticated nitrous gas of Prieſtley. He calls it nitrous gas with exceſs of pure air, and affirms that it diminiſhes, both with nitrous gas and common air.

*b.* I introduced into a veſſel containing 30 grains of platina, 2050 grains of aqua regia, compoſed of equal parts, by weight, of concentrated nitric acid of 1,43, and muriatic acid of 1,16. At the common temperature, that is, 49°, no action between the acid and platina appeared to take place. On the application of the heat of a ſpirit lamp, the ſolution gradually became yellow red, and gas was given out with rapidity. Some of this gas received in a jar filled with warm water, appeared of a bright yellow color. On agitation, the greater part of it was abſorbed by the water, and the remainder extinguiſhed flame. When it was received over mercury, it acted upon it with great rapidity, and formed on the ſurface a white cruſt.

As the proceſs of ſolution advanced, the color of the acid changed to dark red, at the ſame time that the production of gas was much increaſed; more than 40 cubic inches were ſoon collected in the water apparatus.

Different portions of the gas were examined; it exhibited the following properties:

1. Its color was orange red,* and its fmell exactly refembled that of oxygenated muriatic acid.

2. When agitated in boiled water, it was rapidly abforbed, leaving a refiduum of rather more than one twelfth.

3. The taper burnt in it with increafed brilliancy, the flame being long, and deep red at the edges.

4. Iron introduced into it ignited, burnt with a dull red light.

5. Green vegetables expofed to it were inftantly rendered white.

6. It underwent no diminution, mingled with atmofpheric air.

7. When mingled with nitrous gas, it gave denfe red vapor, and rapid diminution.

* This deep color depended, in fome meafure, upon the nitro-muriatic vapor fufpended in it. I have fince obferved that it is more intenfe in proportion as the heat employed for the production of the gas has been ftronger. The natural color of the peculiar gas is deep yellow.

*c.* From the exhibition of theſe properties, it was evident that the gas produced during the ſolution of platina in aqua regia, chiefly conſiſted of oxygenated muriatic acid, or of a gas highly analogous to it. It was, however, difficult to conceive how a body, by combining with a portion of the oxygene of nitro-muriatic acid, could produce from it oxygenated muriatic acid, apparently mingled with very ſmall portions of any other gas.

*d.* To aſcertain whether any permanent gas was produced during the ebullition of aqua regia, of the ſame compoſition as that uſed for the ſolution of the platina; I kept a large quantity of it boiling for ſome time, in communication with the water apparatus; the gas generated appeared to be wholly nitro-muriatic, and was abſorbed as faſt as produced, by the water.

*e.* To determine whether any nitrous oxide was mingled with the peculiar gas, as well as the nature and quantity of the unabſorbable gas, nitrous gas was gradually added to 21 cubic inches of the gas produced from a new ſolution, till the diminution was complete: the gas remaining equalled 2,3 cubic inches; it was unabſorbable by water, and extinguiſhed flame.

In another experiment, when the the laſt portions of gas from a ſolution were carefully received in water previouſly boiled, 12 cubic inches agitated in water left a reſiduum of 1.3; whilſt the ſame quantity decompoſed by nitrous gas, containing ,02 nitrogene, left about 1.5

Hence it appeared that the aëriform products of the ſolution conſiſted of the peculiar gas analogous to oxygenated muriatic acid, and of a ſmall quantity of nitrogene.

*f.* Conſequently a portion of the nitric acid of the aqua regia had been decompoſed; but if it had given oxygene both to the platina and muriatic acid, the quantity of nitrogene evolved ought to have been much more conſiderable.

*g.* To aſcertain if any water had been decompoſed, and the nitrogene condenſed in the ſolution by its hydrogene, to form ammoniac, I ſaturated a ſolution with lime, and heated it, but no ammoniacal ſmell was perceived.

*b*. To determine if any nitrogene had entered into chemical combination with muriatic acid and oxygene, ſo as to form an aëriform triple compound, analogous in its properties to oxygenated muriatic acid, I expoſed ſome of the gas to mercury, expecting that this ſubſtance, by combining with its oxygene, would effect a complete decompoſition; and this was actually the caſe: for the gas was at firſt rapidly diminiſhed, and the mercury became oxydated; its volume, however, ſoon increaſed; and the reſidual gas appeared to be nitrous, mingled with much nitrogene. The exact proportions of each, from an accident, I could not determine.

This experiment was inconcluſive, becauſe the nitro-muriatic acid ſuſpended in the peculiar gas, from which it can probably be never perfectly freed, acted in common with it upon the mercury, and produced nitrous gas: and this nitrous gas, at the moment of its production, formed nitrous acid by combining with the oxygene of the peculiar gas; and the nitrous acid generated * was again decompofed by the mercury; and hence nitrous gas evolved, and poffibly fome nitrogene.

*i.* Peculiar circumftances prevented me at this time from completely inveftigating the fubject. It remains doubtful whether the gas confifts fimply of highly oxygenated muriatic acid and nitrogene,† produced by the decom-

* The decompofition of aëriform nitrous acid by mercury, was firft noted by Prieftley; vol. iii. pag. 101. This decompofition I have often had occafion to obferve. In reading Humbolt's paper on eudiometry, Annales de Chimie, xxviii, pag. 150, I was not a little furprifed to find that he takes no notice of this fact. He feems to fuppofe that nitrous acid can remain aëriform, and even be condenfed, in contact with mercury, without alteration. He fays, "In mingling 100 parts of atmofpheric air with 100 of nitrous air, the air immediately became red, but all the acid produced remained aëriform; and after eighteen hours fome *drops* only of acid were formed, which *fwam* upon the mercury."

† Lavoifier has faid concerning aqua regia, "In folu-
"tions of metals in this acid, as in all other acids, the
"metals are firft oxydated, by attracting a part of the
"oxygene from the compound radical. This occafions the
"difengagement of a particular fpecies of gas not hitherto
"defcribed, which may be called nitro-muriatic gas. It

pofition of nitric acid from the coalefcing affinities of platina and muriatic acid for oxygene; or whether it is compofed of a *peculiar* gas, analogous to oxygenated muriatic acid, and nitrogene, generated from fome unknown affinities.†

## XI. *On the action of the Electric Spark on a mixture of Nitrogene and Nitrous Gas.*

Thinking it poffible that nitrous gas and

" has a very difagreeable fmell, and is fatal to animal life " when refpired; it attacks iron, and caufes it to ruft; it " is abforbed in confiderable quantities by water." Elem. Eng. 237.

† I have no doubt but that the gas procured from the folution of gold in aqua regia, is analogous to that produced from platina.

Some very uncommon circumftances are attendant on the folution of platina:

1ft. The immenfe quantity of acid required for the folution of a minute quantity of platina.

2d. The great quantity of gas produced during the folution of this minute quantity.

3d. The intenfe red color of the folution, and its perfectly acid properties after it ceafes to act upon the metal.

nitrogene might be made to combine by the action of the electric ſpark, ſo as to form nitrous oxide, I introduced 20 grain meaſures of each of them into a ſmall detonating tube, graduated to grains, ſtanding over mercury, and containing a very ſmall quantity of cabbage juice rendered green by an alkali. After electric ſparks had been paſſed through the gaſes for an hour and half, they were diminiſhed to about 32, and the cabbage juice was ſlightly reddened. On introducing about 10 meaſures of hydrogene, and paſſing the electric ſpark through the whole, no inflammation or diminution was perceptible. Hence the condenſation moſt probably aroſe whôlly from the formation of nitrous acid,* by the more intimate union of the oxygene of nitrous gas with ſome of its nitrogene, as in the experiments of Prieſtley.

As the naſcent nitrogene, in the decompo-

* For if nitrous oxide had been formed, it would have been decompoſed by the hydrogene.

fition of nitrate of ammoniac, combines with a portion of oxygene and nitrogene, to form nitrous oxide; it is probable that nitrous oxide may be produced during the paffage of nitrous gas and ammoniac through a heated tube.

## XII. *General Remarks.*

There are no reafons for fuppofing that nitrous oxide is formed in any of the proceffes of nature; and the nice equilibrium of affinity by which it is conftituted, forbids us to hope for the power of compofing it from its fimple principles. We muft be content to produce it, either directly or indirectly, from the decompofition of nitric acid. And as in the decompofition of nitrate of ammoniac, not only all the nitrogene of the nitric acid enters into the compofition of the nitrous oxide produced, but likewife that of the ammoniac, this procefs is by far the cheapeft, as well as the moft expeditious. A mode of producing ammoniac at little expence, has been propofed by Mr. Watt. Condenfed in the fulphuric acid, it can be eafily made to combine with nitric acid, from the decompofition of nitre by double affinity. And thus, if the hopes which the experiments at the end of thofe refearches induce us to indulge, do not prove fallacious, a fubftance which has been heretofore almoft exclufively appropriated to the deftruction of mankind, may become, in the hands of philofophy, a means of producing health and pleafurable fenfation.

# RESEARCH II.

INTO THE COMBINATIONS OF

# NITROUS OXIDE,

AND ITS

DECOMPOSITION

BY

# COMBUSTIBLE BODIES.

# RESEARCH II.

## DIVISION I.

### *EXPERIMENTS and OBSERVATIOUS on the COMBINATIONS of NITROUS OXIDE.*

#### I. *Combination of Water with Nitrous Oxide.*

*a.* THE difcoverer of nitrous oxide firft obferved its folubility in water; and it has fince been noticed by different experimentalifts.

Dr. Prieftley found that water diffolved about one half of its bulk of nitrous oxide, and that at the temperature of ebullition, this fubftance was incapable of remaining in combination with it.*

* Experiments and obfervations, vol. ii. pag. 81.

*b.* I introduced to 9 cubic inches of pure water, i. e. water diſtilled under mercury, 7 cubic inches of nitrous oxide, which had been obtained over mercury, from the decompoſition of nitrate of ammoniac, and in conſequence was perfectly pure. After they had remained together for 11 hours, temperature being 46°, during which time they were frequently agitated, the gas remaining was 2,3; conſequently 4,7 cubic inches had been abſorbed. And then, 100 cubic inches, = 25300 grains of water, will abſorb 54 cubic inches, = 27 grains, of nitrous oxide.

*c.* The taſte of water impregnated with nitrous oxide, is diſtinctly ſweetiſh; it is ſofter than common water, and, in my opinion, much more agreeable to the palate. It produces no alteration in vegetable blues, and effects no change of color in metallic ſolutions.

*d.* Thinking that water impregnated with nitrous oxide might probably produce ſome effects when taken into the ſtomach, by giving out its gas, I drank, in June, 1799, about

3 ounces of it, but without perceiving any effects.

A few days ago, considering this quantity as inadequate, I took at two draughts nearly a pint, fully saturated; and at this time Mr. Joseph Priestley drank the same quantity.

We neither of us perceived any remarkable effects.

Since that time I have drank near three pints of it in the course of a day. In this instance it appeared to act as a diuretic, and I imagined that it expedited digestion. As a matter of taste, I should always prefer it to common water.

*e.* Two cubic inches of pure water, that had been made to absorb about 1,1 cubic inches of nitrous oxide; when kept for some time in ebullition, and then rapidly cooled, produced nearly 1 of gas. Sulphur burnt in this gas with a vivid rose-colored flame.

In another experiment, in which the gas was expelled by heat from impregnated water, and absorbed again after much agitation on cooling; the refiduum was hardly perceptible, and moft likely depended upon fome gas which had adhered to the mercury, and was liberated during the ebullition. Hence it appears that nitrous gas is expelled unaltered from its aqueous folution by heat.

*f.* I have before mentioned, Divifion III, that nitrous oxide, during its combination with fpring water, expels the common air diffolved in it. This common air generally amounts to one fixteenth, the volume of the water being unity. A correction on account of this circumftance muft be made for the apparent deficiency of diminution, and for the common air mingled in confequence, with nitrous oxide during its abforption by common water.

*g.* Water impregnated with nitrous gas abforbed nitrous oxide; but the refidual gas was much greater than that of common water, and gave red fumes with atmofpheric air. Nitrous gas agitated for a long while over water highly impregnated with nitrous oxide, was not in the flighteft degree diminifhed, in one experiment indeed it was rather increafed; doubtlefs from the liberation of ſome nitrous oxide from the water by the agitation.

*h.* Nitrous oxide kept in contact with aqueous ſolution of ſulphurated hydrogene and often agitated, was not in the ſlighteſt degree diminiſhed.

Sulphurated hydrogene, introduced into a ſolution of nitrous oxide, was rapidly abſorbed, and as the proceſs advanced, the nitrous oxide was given out.

*i.* Water impregnated with carbonic acid, poſſeſſed no action upon nitrous oxide, and did not in the ſlighteſt degree abſorb it. When carbonic acid was introduced to an aqueous ſolution of nitrous oxide; the aëriform acid was abſorbed, and the nitrous oxide liberated.

*k.* From theſe obſervations it appears that nitrous oxide has leſs affinity for water, than even the weaker acids, ſulphurated hydrogene and carbonic acid; as indeed one might have conjectured a priori from its degree of ſolubility: likewiſe that it has a ſtronger attraction for water than the gaſes not poſſeſſed of acid or alkaline properties; it expelling from water nitrous gas, oxygene, and common air; probably hydro-carbonate, hydrogene, and nitrogene.

II. *Combinations of Nitrous Oxide with Fluid Inflammable Bodies.*

*a.* Vitriolic ether abſorbs nitrous oxide in much larger quantities than water.

A cubic inch of ether, at temperature 52°, combined with a cubic inch and ſeven tenths of nitrous oxide.

Ether thus impregnated was not at all altered in its appearance; its ſmell was preciſely the ſame, but the taſte appeared leſs pungent, and more agreeable. Nitrous oxide is liberated unaltered from ether at a very low temperature, that is, at about the boiling point of this fluid.

For expelling nitrous oxide from impregnated ether, and for aſcertaining in general the quantity of gaſes combined with fluids, I have lately made uſe of a very ſimple method, which it may not be amiſs to deſcribe.

The impregnated fluid is introduced into a ſmall thin tube, graduated to ,05 cubic inches, through mercury. The quantity of fluid ſhould never equal more than a fifth or ſixth of the capacity of the tube.

The lower part of the tube is adapted to an orifice in the ſhelf of the mercurial apparatus, ſo as to make an angle of about 40° with the ſurface of the mercury.

The flame of a ſmall ſpirit lamp is then applied to that part of the tube containing the fluid; and after the expulſion of the gas from it, the heat is raiſed ſo as to drive out the fluid through the orifice of the tube. Thus the liberated gas is preſerved in a ſtate proper for accurate examination.

Impregnated ether, during its combination with water, gives out the greater part of its nitrous oxide. During the liberation of nitrous oxide from ether, by its combination with water, a very curious phænomenon takes place.

If the water employed is colored, ſo that it may be ſeen in a ſtratum diſtinct from the impregnated ether, at the point of contact a number of small spherules of fluid will be perceived, apparently repulsive both to water and ether; these spherules become gradually covered with minute globules of gas, and as this gas is liberated from their surfaces, they gradually disappear.

*b.* Alcohol dissolves considerable quantities of nitrous oxide.

2 cubic inches of alcohol, at 52°, combined with 2,4 cubic inches of nitrous oxide. The alcohol thus impregnated had a taste rather sweeter than before, but in other physical properties was not perceptibly altered.

Nitrous oxide is incapable of remaining in combination with this fluid at the temperature of ebullition; it is liberated from it unaltered by heat.

Impregnated alcohol, during its combination with water, gives out the greater part of its combined nitrous oxide: on mingling the two fluids together, at the point of contact the alcohol becomes covered with an infinite number of small globules of gas, which continue to be generated during the whole of the combination, and in paffing through the fluid render it almoft opaque.

*c.* The effential oils abforb nitrous oxide to a greater extent than either alcohol or ether.

,5 cubic inches of oil of carui combined with 1,2 cubic inches of nitrous oxide at 51°. The color of the oil thus impregnated was rather paler than before.

Nitrous oxide is expelled unaltered from impregnated oil of carui, by heat.

1 of oil of turpentine abforbed nearly 2 of nitrous oxide, at 57°. Its properties were not fenfibly altered from this combination, and the gas was expelled from it undecompounded, by heat.

*d.* As well as the effential oils, the fixed oils diffolve nitrous oxide at low temperatures, whilft at high temperatures they do not remain in combination.

1 of olive oil abforbed, at 61°, 1,2 of nitrous oxide, but without undergoing any apparent phyfical change.

### III. *Action of Fluid Acids on Nitrous Oxide.*

*a.* Nitrous oxide expofed to concentrated fulphuric acid, undergoes no change, and fuffers no diminution, that may not be accounted for from the abftraction of a portion of its water by the acid.

*b.* Nitrous oxide is fcarcely at all foluble in nitrous acid, and expofed to that fubftance, undergoes no alteration.

*c.* Muriatic acid, of fpecific gravity 1,14 abforbs about a third of its bulk of nitrous oxide. It fuffers no apparent change in its properties from being thus impregnated, and the gas is again given out from it on the application of heat.

*d.* Acetic acid abforbs nearly one third of its bulk of nitrous oxide.

*e.* Aqua regia, that is, the nitro-muriatic acid, abforbs a very minute portion of nitrous oxide.

*f.* Nitrous oxide was expofed to a new compound acid, confifting of oxygenated muriatic acid, and fulphuric acid, which I difcovered in

July, 1799, and of which an account will be ſhortly publiſhed; but it was neither abſorbed or altered.

I have before mentioned that the aqueous ſolutions of ſulphurated hydrogene and carbonic acid, neither diſſolve or alter nitrous oxide.

IV. *Action of Saline Solutions, and other Subſtances, on Nitrous Oxide.*

*a.* Nitrous oxide expoſed to concentrated ſolution of green ſulphate of iron, at 58°, underwent no perceptible diminution; not even after it had been ſuffered to remain in contact with it for half an hour.

*b.* It underwent diminution of nearly ,2 when agitated in contact with a ſolution of red ſulphate of iron, the volume of the ſolution being unity.

*c.* Solution of green ſulphate of iron, fully impregnated with nitrous gas, did not in the ſlighteſt degree abſorb nitrous oxide, and appeared to have no action upon it.

*d.* Solution of green muriate of iron, whether impregnated with nitrous gas, or unimpregnated, has no affinity for, or action upon, nitrous oxide.

*e.* Solution of red muriate of iron in alcohol, abſorbed nearly one fifth of its bulk, of nitrous oxide.

*f.* Solution of pruſſiate of potaſh abſorbed nearly one third of its volume, of nitrous oxide, which was again expelled from it by heat.

*g.* Solution of nitrate of copper appeared to have no affinity for nitrous oxide.

*h.* Concentrated ſolution of nitrate of ammoniac, at 58°, abſorbed one eighth of its bulk of nitrous oxide.

*i.* Solutions of alkaline ſulphures abſorb nitrous oxide in quantities proportionable to the water they contain; it is expelled from them unaltered by heat. None of the hydroſulphures diſſolve more than half their bulk of nitrous oxide.

*k.* Concentrated ſolutions of the ſulphites poſſeſs little or no action on nitrous oxide;

diluted ſolutions abſorb it in ſmall quantities.

*l.* Concentrated ſolution of muriate of tin abſorbs about one eighth of nitrous oxide; more dilute ſolutions abſorb larger quantities.

From theſe obſervations we learn, that neutro-ſaline ſolutions in general, have very feeble attractions for nitrous oxide; and as ſolutions of green muriate, and ſulphate of iron, whether free from nitrous gas, or impregnated with it, poſſeſs no action upon nitrous oxide, nitrous gas may be ſeparated from this ſubſtance by thoſe ſolutions with greater facility than nitrous oxide can be ſeparated from nitrous gas, by water or alcohol.

Charcoal abſorbs nitrous oxide as well as all other gaſes; and it is diſengaged from it by heat.

I have as yet found no other ſolid body, not poſſeſſed of alkaline properties, capable of abſorbing nitrous oxide in any ſtate of exiſtence.

The bodies poſſeſſing the ſtrongeſt affinity for oxygene, the dry ſulphites, muriate of tin, the common ſulphures, white pruſſiate of potaſh, and green oxide of iron, do not in the ſlighteſt degree act on nitrous oxide at common temperatures.

V. *Action of different Gases on Nitrous Oxide.*

*a.* 12 meafures of muriatic acid gas were mingled with 7 meafuresof nitrous oxide at 56°. After remaining together for a minute, they filled a fpace equal to $19\frac{1}{2}$ meafures. When water was introduced to them, the muriatic acid was abforbed much more flowly than if it had been unmingled.

In another experiment, when the gafes were faturated with water, 9 meafures of each of them, when mingled and fuffered to remain in contact for a quarter of an hour, filled a fpace nearly equal to 19; and after the muriatic acid had been abforbed by potafh, the nitrous oxide remained unaltered in its properties.

From the expanfion, it appears moft probable that aëriform muriatic acid, and nitrous oxide, have a certain affinity for each other, and that they combine when mingled together; for in the last experiment, the increase of volume cannot be accounted for by supposing that nitrous oxide undergoes less change of volume than muriatic acid, by aëriform combination with water, and that the expansion depended upon the solution of some of its combined water by the muriatic acid. That muriatic acid and nitrous oxide have a slight affinity for each other, likewise appears from the absorption of nitrous oxide by aqueous solution of muriatic acid.

Thinking that nitrous oxide might attract muriatic acid from its solution in water, I exposed a minute quantity of fluid muriatic acid to nitrous oxide; but no alteration of volume took place in the gas.

*b.* 6 measures of nitrous oxide were mingled with 11 measures of sulphureous acid, saturated with water; after remaining at rest for six minutes, they filled a space nearly equal to 18 measures. Exposed to water, the sulphureous acid was absorbed, but not nearly so rapidly as when in a free ſtate. Sulphur burnt with a vivid flame in the reſidual nitrous oxide. 7 meaſures of ſulphureous acid were now mingled with 8 of nitrous oxide. They filled a ſpace nearly equal to 15¾, and no farther expanſion took place afterwards.

From theſe experiments it appears probable that ſulphureous acid, and nitrous oxide, have ſome affinity for each other.

*c*. 11 meaſures of carbonic acid were mingled with 8 of nitrous oxide; they filled a ſpace nearly equal to 19 meaſures. On expoſing the mixture to cauſtic potaſh, the carbonic acid was abſorbed, and the nitrous oxide remained pure. Hence it appears that carbonic acid and nitrous oxide do not combine with each other.

*d* Oxygenated muriatic acid, and nitrous oxide, were mingled in a water apparatus: there was a ſlight appearance of condenſation; but this was moſt probably owing to abſorption by the water; on agitation, the oxygenated muriatic acid was abſorbed, and the greater part of the nitrous oxide remained unaltered.

*e.* Sulphurated hydrogene and nitrous oxide, mingled together, neither expanded or contracted; expofed to folution of potafh, the acid* only was abforbed.

*f.* 10 meafures of nitrous gas were admitted to 12 of nitrous oxide at 59°. They filled a fpace equal to 22, and after remaining together for an hour, had undergone no change. Solution of muriate of iron abforbed the nitrous gas without affecting the nitrous oxide.

*g.* Nitrous oxide was fucceffively mingled with oxygene, atmofpheric air, hydro-carbonate, phofphorated hydrogene, hydrogene, and nitrogene, at 57°; it appeared to poffefs no action on any of them, and was feparated by water, the gafes remaining unaltered.

*h.* As nitrous oxide was foluble in ether, alcohol, and the other inflammable fluids, it was reafonable to fuppofe that its affinity for thofe bodies would enable them to unite with

* The experiments of Berthollet have clearly proved the perfect acidity of this fubftance.

it in the aëriform ſtate. At the ſuggeſtion of Dr. Beddoes I made the following experiment:

To 12 meaſures of nitrous oxide, at 54°, I introduced a ſingle drop of ether; the gas immediately began to expand, and in four minutes filled a ſpace equal to ſixteen meaſures and a quarter. When an inflamed taper was plunged into the gas thus holding ether in ſolution, a light blue flame ſlowly paſſed through it.

A conſiderable diminution of temperature is moſt probably produced, from the great expanſion of nitrous oxide during its combination with ether.

A drop of alcohol was admitted to 14 meaſures of nitrous oxide. In five minutes, the gas filled a ſpace equal to fifteen and a third; but no farther diminution took place afterwards.

A minute quantity of oil of turpentine was introduced to 14 meaſures of nitrous oxide; it filled, in 4 minutes, a ſpace rather leſs than 14; and no farther change took place afterwards. Moſt likely this contraction aroſe from the precipitation of the water diſſolved in the gas by the ftronger affinity of the oil for nitrous oxide. To afcertain with certainty if any oil had been diffolved by the gas, I introduced into it a fmall quantity of ammoniac. It immediately became flightly clouded, moft probably from the formation of foap, by the combination of the diffolved oil with the ammoniac.

From thefe experiments we learn, that when nitrous oxide is mingled with either carbonic acid, oxygene, common air, hydro-carbonate, fulphurated hydrogene, hydrogene, or nitrogene, they may be feparated from each other without making any allowance for contraction or expanfion: but if a mixture of either muriatic acid, or fulphureous acid gas, with nitrous oxide, is experimented upon; in the abforption of the acid by alkalies, the apparent volume of gas condenfed will be lefs than the real one, by a quantity equal to the fum of expanfion from combination. Confequently a correction muft be made on account of this circumftance.

Though alcohol, ether, effential oils, and the fluid inflammable bodies in general, diffolve nitrous oxide with much greater rapidity than water, yet as we are not perfectly acquainted with their action on unabsorbable gases, it is better to employ water for separating nitrous oxide from these substances; particularly as that fluid is more or less combined with all gases, and as we are acquainted with the extent of its action upon them.

By pursuing the subject of the solution of essential oils in gases, we may probably discover a mode of obtaining them in a state of absolute dryness. For if other gases as well as nitrous oxide, have a stronger affinity for oils than for water, water most probably will be precipitated from them during their solution of oils; and after their saturation with oil, it is likely that they are capable of being deprived of that substance by ammoniac.

### VI. *Action of aëriform Nitrous Oxide on the Alkalies. History of the discovery of the combinations of Nitrous Oxide with the Alkalies.*

*a*. When nitrous oxide in a free state is expoſed to the ſolid cauſtic alkalies and alkaline earths, at common temperatures, it is neither abſorbed nor acted upon; when it is placed in contact with ſolutions of them in water, a ſmall quantity is diſſolved; but this combination appears to depend on the water of the ſolution, for the gas can be expelled unaltered, at the temperature of ebullition.

*b.* Cauſtic potaſh was expoſed to nitrous oxide for 13 hours: the diminution was not to one fiftieth, and this ſlight condenſation moſt probably depended upon its combination with the water of the gas.

Concentrated ſolution of potaſh abſorbed a fourth of its bulk of nitrous oxide. When the impregnated ſolution was heated, globules of gas were given out from it rapidly; but the quantity collected was too ſmall to examine.

Soda, whether ſolid or in ſolution, exhibited exactly the ſame phænomena with nitrous oxide. The ſolution of ſoda abſorbed near a quarter of its bulk of gas.

*c.* 11 meaſures of ammoniacal gas were

mingled with 8 meaſures of nitrous oxide over dry mercury, both of the gaſes being ſaturated with water. No change of appearance was produced by the mixture, and they filled, after two minutes, a ſpace equal to 19. On the introduction of a little water, the ammoniac was abſorbed, and the nitrous oxide remained unaltered, for it was diſſolved by water as rapidly as if it had never been mingled with ammoniac.*

7 meaſures of nitrous oxide, expoſed to 6 meaſures of ſolution of ammoniac in water, was in an hour diminiſhed to $4\frac{1}{2}$ nearly. When the ſolution was heated over mercury, permanent gas was produced, which was unabſorbable by a minute quantity of water, and ſoluble in a large quantity; conſequently it was nitrous oxide.

* The Dutch chemiſts have aſſerted, that mixture with ammoniac prevents the abſorption of nitrous oxide by water, either wholly or partially. Journal de Phyſique, t. xliii. part ii. pag. 327. It is difficult to account for their miſtake.

*d.* Nitrous oxide was expoſed to dry cauſtic ſtrontian; it underwent a diminution of nearly one fortieth, which moſt likely was owing to the combination of the ſtrontian with its water.

11 meaſures of nitrous oxide were agitated in contact with 8 of ſtrontian lime water: nearly 4 meaſures were abſorbed. The impregnated ſolution expoſed to heat, rapidly gave out its gas; 3 meaſures were ſoon collected, which mingled with a ſmall quantity of hydrogene, and inflamed by the taper; gave a ſmart detonation.

*e.* Nitrous oxide expoſed to lime and argil, both wet and dry, was not in the ſlighteſt degree acted upon.

From theſe experiments it is evident that nitrous oxide in the aëriform ſtate cannot be combined either with the alkalies, or the alkaline earths. That a combination may be effected between nitrous oxide and theſe ſubſtances, it muſt be preſented to them, in the *naſcent ſtate.*

The ſalts compoſed of the alkalies and nitrous oxide, are not analogous to any other compound ſubſtances, being poſſeſſed of very ſingular properties. Before theſe properties are detailed, it may not be amiſs to give an account of the accidental way in which I diſcovered the mode of combination.

In December, 1799, deſigning to make a very delicate experiment, with a view to aſcertain if any water was decompoſed during the converſion of nitrous gas into nitrous oxide, by ſulphite of potaſh, I expoſed 200 grains of cryſtaliſed ſulphite of potaſh, containing great ſuperabundance of alkali, to 14 cubic inches of nitrous gas, containing one eighteenth nitrogene. The alkali was employed to preſerve any ammoniac that might be formed, in the free ſtate, as it would otherwiſe combine with ſulphureous acid.*

The volume of gas diminiſhed with great rapidity; in two hours and ten minutes it was

* Sulphureous acid ſaturates more potaſh than ſulphuric acid, ſo that moſt probably during the converſion of ſulphite of potaſh into ſulphate, portions of ſulphureous acid are diſengaged.

reduced to $6\frac{4}{5}$, which I confidered as the limit of diminution. Accidentally, however, fuffering it to remain for three hours longer, I was much furprifed by finding that not quite 12 cubic inches remained, which confifted of nitrous oxide, mingled with the nitrogene that exifted before the experiment.

In accounting theoretically for this phænomenon, different fuppofitions neceffarily prefented themfelves.

1ft, It was poffible, that though fulphite of potafh, and potafh, feparately poffeffed no action on free nitrous oxide, yet in combination they might exert fuch affinities upon it as either to abforb it, or make it enter into new combinations.

2dly. It was more probable that the cauftic potafh, though incapable of condenfing aëriform nitrous oxide, was yet poffeffed of a ftrong affinity for it when in the *nafcent ftate*, and that the nitrous oxide condenfed in the experiment had been combined in this ftate with the free alkali.

To afcertain if the compound of potafh and ſulphite of potaſh with ſulphate, was capable of acting upon nitrous oxide, I ſuffered a quantity of this ſubſtance to remain in contact with the gas for near a day: no change whatever took place.

To determine whether the diminution of nitrous oxide depended upon its abſorption in the naſcent ſtate, by the peculiar compound of potaſh and ſulphite of potaſh, or if it was ſimply owing to the alkali.

I mingled a ſolution of ſulphite of potaſh with cauſtic ſoda; the ſalt, after being evaporated at a low temperature, was expoſed to nitrous gas. The nitrous oxide formed was abſorbed, but in rather leſs quantities than when alkaline ſulphite of potaſh was employed.

Hence it was evident that the alkali was the agent that had condenſed the nitrous oxide in thoſe experiments, for ſoda is incapable of combining either with ſulphate, or ſulphite of potaſh.

To aſcertain whether any change in the conſtitution of the nitrous oxide had been produced by the condensation, I introduced a small quantity of sulphite of potash, with excess of alkali, that had absorbed nitrous oxide, into a long and thin cylindrical tube filled with mercury; and inclining it at an angle of 35° with the plane of the mercury, applied the heat of a spirit lamp to that part of the tube containing the salts; when the glass became very hot, gas was given out with rapidity; in less than a minute the tube was full. This gas was transfered into another tube, and examined; it proved to be nitrous oxide in its highest state of purity;* for a portion of it absorbed by common water, left no more than a residuum of $\frac{1}{15}$, and sulphur burnt in it with a vivid rose-colored flame.

Being now satisfied that the alkalies were capable of combining with nitrous oxide; to investigate with precision the nature of these new compounds, I proceeded in the following manner.

* Hence we learn that sulphite of potash, when strongly heated, does not decompose nitrous oxide, even in the *nascent state*.

VII. *Combination of Nitrous Oxide with Potash.*

*a*. Into a solution of sulphite of potash, which had been made by passing sulphureous acid gas from a mercurial airholder into caustic potash, dissolved in water, I introduced 17 grains of dry potash. The whole evaporated at a low temperature, gave 143 grains of salt. This salt was not *wholly* composed of sulphite of potash and potash; it contained as well, a minute quantity of carbonate, and sulphate of potash, formed during the evaporation.*

120 grains of it finely pulverised, and retaining the water of crystalisation, were exposed to 15 cubic inches of nitrous gas, over mercury. The nitrous gas diminished with great rapidity, and in three hours a cubic inch and nine tenths

* See the excellent memoir of Fourcroy and Vauquelin on the sulphureous acid, and its combinations. Annales de Chimie, ii, 54. Or Nicholson's Phil. Journal, vol. i, pag. 313.

only remained, which consisted of nearly one third nitrous oxide, and two thirds nitrogene that had pre-existed in the nitrous gas. The increase of weight of the salt could not be determined, as some of it was lost by adhering to the vessel in which the combination was effected, and to the mercury. It presented no distinct series of crystalisations, even when examined by the magnifier; rendered green vegetable blues, and its taste was very different from that of the remaining quantity of salt that had been exposed to the atmosphere. A portion of it strongly heated over mercury, gave out gas with great rapidity, which had all the properties of the purest nitrous oxide.

When water was poured upon some of it, no gas was given out, and the whole was equably and gradually dissolved. Alcohol, as well as ether, appeared incapable of dissolving any part of it.

When muriatic acid was introduced into it, confined by mercury, a rapid effervescence took place. Part of the gas disengaged was sulphureous acid, and carbonic acid; the remainder was nitrous oxide.

*b*. I made a number of experiments upon ſalts procured in the manner I have juſt deſcribed, with a view to obtain the compound of nitrous oxide and potaſh, free from admixture of other ſalts.

When the mixed ſalt was boiled in alcohol or ether, no part of it appeared to be diſſolved. Finding that little or no gas was given out during the ebullition of concentrated ſolutions of the mixed ſalts, I attempted to ſeparate the ſulphate, ſulphite, and carbonate of potaſh, from the combination of nitrous oxide and potaſh, by ſucceſſive evaporations and cryſtaliſations. But though in this way it was nearly freed from ſulphate of potaſh, yet the extreme and nearly equal ſolubility of the other ſalts, prevented me from completely ſeparating them from each other.

By expoſing, however, very finely pulveriſed ſulphite of potaſh, mingled with alkali, for a great length of time to nitrous gas, it was almoſt wholly converted into ſulphate; and after the ſeparation of this by ſolution, evaporation, and cryſtaliſation, at a low temperature, I obtained the new combination, mingled with very little carbonate of potaſh, and ſtill leſs of ſulphite.

The minute quantity of ſulphite chiefly appeared in very ſmall cryſtals; diſtinct from the maſs of ſalt, which poſſeſſed no regular cryſtaliſation, and was almoſt wholly compoſed of the new compound, intimately mingled with a little carbonate. The new compound, as nearly as as I could eſtimate from the quantity of nitrous oxide abſorbed, conſiſted of about 3 alkali, to 1 of nitrous oxide, by weight.

It exhibited the following properties:

1. Its taſte was cauſtic, and poſſeſſed of a pungency different from either potaſh or carbonate of potaſh.

2. It rendered vegetable blues green, which might poſſibly depend upon the carbonate of potaſh mixed with it.

3. Pulveriſed charcoal mingled with a few grains of it, and inflamed, burnt with ſlight

ſcintillations. Projected into zinc in a ſtate of ſuſion, a ſlight inflammation was produced.

4. When either ſulphuric, muriatic, or nitric acid was introduced to it under mercury, it gave out nitrous oxide, mingled with a little carbonic acid.

5. Thrown into a ſolution of ſulphurated hydrogene, gas was diſengaged from it, but in quantities too minute to be examined.

6. When carbonic acid was thrown into a ſolution of it in water, gas was diſengaged; on examination it proved to be nitrous oxide.

7. A concentrated ſolution of it kept in ebullition in a cylinder, confined by mercury, gave out a few globules of gas, which were too minute to be examined, and probably conſiſted of common air previouſly contained in the water.

*c.* In the experiments made to aſcertain theſe properties all the ſalt was expended, otherwiſe I ſhould have endeavoured to aſcertain what quantity of gas would have been liberated by heat from a given weight; and likewiſe what would have been the effects of admixture of it with oil. When some of the mixed salt was mingled with oil of turpentine, part of it was dissolved, and the fluid became white; but no gas was given out. On this coarse experiment, however, I cannot place much dependance. If the combination of nitrous oxide and potash is capable of combining with oil without decomposition, barytes and strontian* will probably separate the oil from it, and thus it may possibly be obtained in a state of purity.

In a rough experiment made on the conversion of nitrous gas into nitrous oxide, by concentrated solution of sulphite of potash with excess of alkali, very little of the nitrous oxide was absorbed. Hence it is probable that water lessens the affinity of potash for nascent nitrous oxide.

* Unless the sum of affinity of the potash, oil, nitrous oxide, and earths, should be such as to enable the nitrous oxide to combine with the earth, whilst the oil and alkali remained in combination, &c.

## VIII. *Combination of Nitrous Oxide with Soda.*

The union of nitrous oxide with ſoda is effected in the ſame manner as with potaſh. The alkali, mingled by ſolution and evaporation, with either ſulphite of ſoda, or of potaſh, is expoſed to nitrous gas; the nitrous oxide is condenſed by it at the moment of generation, and the combination effected.

As far as I have been able to obſerve, nitrous oxide is not abſorbed to ſo great an extent by ſoda, as potaſh.

I have not yet been able to obtain the combination of nitrous oxide with ſoda in its pure ſtate. To the attainment of this end, difficulties identical with thoſe noticed in the laſt ſection preſent themſelves. It is extremely difficult to procure the ſoda perfectly free from carbonic acid, and though by uſing ſulphite of potaſh the ſulphate formed is eaſily ſeparated, yet ſtill evaporation and cryſtaliſation will not diſengage the ſulphite and carbonate from the new compound.

The compound of ſoda and nitrous oxide, mingled with a little ſulphite and carbonate of ſoda, was rapidly ſoluble, both in warm and cold water, without effervefcence. Its ſolution, heated to ebullition, gave out no gas. The taſte of the ſolid ſalt was cauſtic, and more acrid than that of the mixture of carbonate and ſulphite of ſoda. When caſt upon zinc in fuſion, it burnt with a white flame. When heated to 400° or 500°, it gave out nitrous oxide with rapidity. Nitrous oxide was expelled from it by the ſulphuric, muriatic, and carbonic acids, *I believe*, by ſulphurated hydrogene.*

IX. *Combination of Nitrous Oxide with Ammoniac.*

I attempted to effect this combination by

* For when a little of the mixed ſalt was introduced into a ſolution of ſulphurated hydrogene, globules of gas were given out during the ſolution.

converting nitrous gas into nitrous oxide, by ſulphite of ammoniac, wetted with ſtrong ſolution of cauſtic ammoniac; but without ſucceſs; for the whole of the nitrous oxide produced remained in a free ſtate.

When I expoſed ſulphite of potaſh, mingled by ſolution and evaporation with highly alkaline carbonate of ammoniac,† to nitrous gas, the diminution was nearly one fourth more than if pure ſulphite of potaſh had been employed. Hence it appears moſt likely that ammoniac is capable of combination with nitrous oxide in the naſcent ſtate.

In the experiments on the converſion of nitrous gas into nitrous oxide, by naſcent hydrogene, and by ſulphurated hydrogene, Reſ. I. Diviſ. V. probably the water formed at the ſame

† Carbonate of ammoniac formed at a high temperature, containing near 60 per cent alkali, and capable of combining with ſmall quantities of acids without giving out its carbonic acid. Of this ſalt a particular account will be given in the experiments on the ammoniacal ſalts, which I have often mentioned in the courſe of this work.

time with the ammoniac and nitrous oxide, prevented them from entering into combination; *possibly* the peculiar compound was formed, but in quantities so minute as not to be distinguished from simple ammoniac;* for even the existence of ammoniac in these processes, is but barely perceptible.

If it should be proved by future experiments, that in the decomposition of nitrous gas by nascent hydrogene, a peculiar compound of nitrous oxide, water and ammoniac, is formed, it will afford proofs in favor of the doctrine of predisposing affinity;† for then this decom-

* It may not be amiss to mention some appearances taking place in the decomposition of nitrous gas by sulphurated hydrogene, though it is useless to theorise concerning them. The sulphur deposited is at first yellow; as the process proceeds, it becomes white, and in some instances I have suspected a diminution of it.

† Predisposing affinity, the existence of which at first consideration it is difficult to admit, may be easily accounted for by *supposing* the attractions of the simple principles of compound substances. And this doctrine will apply in all

pofition might be fuppofed to depend upon the difpofition of oxygene, hydrogene and nitrogene to affume the ftates of combination in which they might form a triple compound, of water, nitrous oxide, and ammoniac.

Nitrous oxide might probably be made to combine with ammoniac by expofing a mixture of nitrous gas and aëriform ammoniac, to the fulphites.

It is probable that nitrous oxide may be combined with ammoniac, by means of double affinity. Perhaps fulphate of ammoniac and the combination of potafh with nitrous oxide mingled together in folution, would be converted into fulphate of potafh and the compound of nitrous oxide, and ammoniac.

inftances where the conftitution of bodies is known. Predifpofing affinity ought not to be confidered as the affinity of *non-exifting* bodies for each other; but as the mutual affinity of their fimple principles, difpofing them to affume new arrangements.

### X. *Probability of forming Compounds of Nitrous Oxide and the Alkaline Earths.*

I attempted to combine nitrous oxide with lime and ftrontian, by expofing fulphites of lime and ftrontian with excefs of earth, to nitrous gas; but this procefs did not fucceed: the diminution took place fo flowly as to deftroy all hopes of gaining any refults in a definite time. Sulphite of potafh is decompofable by barytes, ftrontian, and lime;* confequently it was impoffible to employ this fubftance to effect the combination.

As the dry fulphures, when well made, convert nitrous gas into nitrous oxide, it is probable that the union of the earths with nafcent nitrous oxide may be effected by expofing nitrous gas to their fulphures, containing an excefs of earth.

Perhaps the combination of nitrous oxide with

* See the above-mentioned elaborate memoir of Fourcroy and Vauquelin.

ſtrontian may be effected by introducing the combination of potaſh and nitrous oxide into ſtrontian lime water.

It is probable that nitrous oxide may be combined with clay and magneſia, by expoſing theſe bodies, mingled with ſulphite of potaſh or ſoda, to nitrous gas.

XI. *Additional Obſervations on the combinations of Nitrous Oxide with the Alkalies.*

A deſire to complete phyſiological inveſtigations relating to nitrous oxide, has hitherto prevented me from purſuing to a greater extent, the experiments on the combination of this ſubſtance with the alkalies, &c. As ſoon as an opportunity occurs, I purpoſe to reſume the ſubject.

The obſervations detailed in the foregoing ſections are ſufficient to ſhow that nitrous oxide is capable of entering into intimate union with the fixed alkalies: and as the compounds formed by this union are inſoluble in alcohol, decompofable by the acids, and heat, and pof-feffed of peculiar properties, they ought to be confidered as a new clafs of faline fubftances.

If it is thought proper, on a farther invefti-gation of their properties, to fignify them by fpecific names, they may, according to the ufu-ally adopted fafhion of nomenclature, be called *nitroxis*: thus the *nitroxi of potafh* would fignify the falt formed by the combination of nitrous oxide with potafh.

Future experiments muft determine the different affinities of nitrous oxide for the alka-lies, and alkaline earths.

With regard to the ufes of thefe new com-pounds it is difficult to form a guefs. When they are obtained pure, and fully faturated with nitrous oxide, on account of the low temperature at which their gas is liberated, they will proba-bly conftitute detonating compounds. From their facility of decompofition by the weaker acids, they may poffibly be ufed medicinally, if ever the evolution of nitrous oxide in the ftomach fhould be found beneficial in difeafes.

XII. *The properties of Nitrous Oxide resemble those of Acids.*

If we were inclined to generalise, and to place nitrous oxide among a known class of bodies, its properties would certainly induce us to consider it as more analogous to the acids than to any other substances; for it is capable of uniting with water and the alkalies, and is insoluble in most of the acids. It differs, however, from the stronger acids, in not possessing the sour taste,* and the power of reddening vegetable blues: and from both the stronger and weaker acids, in not being combinable when in a perfectly free state, at common tempera-

* The different persons who have respired nitrous oxide have, as will be seen hereafter, given different accounts of the taste; the greater number have called it sweet, some metallic. One of my friends, in a letter to me dated Nov. 13, 1799, containing a detail of some experiments made on the respiration of nitrous oxide, at Birmingham, denotes the taste of it by the term "sweetish faintly acidulous." To me the taste both of the gas and of its solution in water, has always appeared faintly sweetish.

tures, with the alkalies. If it ſhould be proved by future experiments, that condenſation by cold gave it the capability of immediately forming neutro-ſaline compounds with the alkalies; it ought to be conſidered as the weakeſt of the acids. Till thoſe experiments are made, its extraordinary chemical and phyſiological properties are ſufficient to induce us to conſider it as a body *ſui generis*.

It is a ſingular fact that nitrous gas, which contains in its compoſition a quantity of oxygene ſo much greater than nitrous oxide, ſhould nevertheleſs poſſeſs no acid properties. It is uncombinable with alkalies, very little ſoluble in water, and abſorbable by the acids,

# DIVISION II.

*On the DECOMPOSITION of NITROUS OXIDE by COMBUSTIBLE BODIES. Its ANALYSIS. OBSERVATIONS on the different combinations of OXYGENE and NITROGENE.*

## I. *Preliminaries.*

FROM the phænomena mentioned in Res. I. Divis. III.* it appears that the combustible bodies burn in nitrous oxide at certain temperatures. The experiments in this Division were instituted for the purpose of investigating the precise nature of these combustions, with a view of ascertaining exactly the composition of nitrous oxide.

It will be seen hereafter that very high temperatures are required for the decomposition of

* Section 2.

nitrous oxide, by moſt of the combuſtible bodies, and that in this proceſs heat and light are produced to a very great extent. Theſe agents alone are poſſeſſed of a conſiderable power of action on nitrous oxide; of which it is neceſſary to give an account, that we may be able to underſtand the phænomena in the following ſections.

II. *Converſion of Nitrous Oxide into Nitrous Acid, and a Gas analogous to Atmoſpheric Air, by Ignition.*

*a*. Dr. Prieſtley aſſerts, that nitrous oxide expoſed for a certain time to the action of the electric ſpark, is rendered immiſcible with water, and capable of diminution with nitrous gas, without ſuffering any alteration of volume; and likewiſe that the ſame changes are effected in it by expoſure to ignited incombuſtible bodies.*

The Dutch chemiſts ſtate, that the electric

* Vol. ii. pag. 91.

ſpark paſſed through nitrous oxide, occaſions a ſmall diminution of its volume, and that the gas remaining is analogous to common air.† They conclude that this change depends on the ſeparation of its conſtituent parts, oxygene and nitrogene, from each other.

None of theſe chemiſts have ſuſpected the production of nitrous acid in this proceſs.

*b.* Nitrous oxide undergoes no change whatever from the ſimple action of light. I expoſed ſome of it, confined by mercury, for many days to this agent, often paſſing through it concentrated rays by means of a ſmall lens. When examined it appeared, as well as I could eſtimate, of the ſame degree of purity as at the beginning of the experiment.

*c.* A temperature below that of ignition effects no alteration in the conſtitution of nitrous oxide. I paſſed nitrous oxide from a

† Journal de Phyſique, tom. xliii, part ii. pag. 330. They effected the ſame change by paſſing it through a heated tube. Dr. Prieſtley had publiſhed an account of ſimilar experiments more than two years before.

retort containing decompofing nitrate of ammoniac, through a green glafs tube, ftrongly heated in an air-furnace, but not fuffered to undergo ignition. The gas, received in a water apparatus exhibited the fame properties as the pureft nitrous oxide; fome of it abforbed by water, left a refiduum of not quite one thirteenth.

*d*. The action of the electric fpark for a long while continued, converts nitrous oxide into a gas analogous to atmofpheric air, and nitrous acid.

I paffed about 150 ftrong fhocks from a fmall Leyden phial, through 7 ten grain meafures of pure nitrous oxide. After this it filled a fpace rather lefs than fix meafures: the mercury was rendered white on the top, as if it had been acted on by nitric acid. Six meafures of nitrous gas mingled with the refidual gas of the experiment, over mercury covered by a little water, gave red fumes, and rapid diminution. In five minutes the volume of the gafes nearly equalled ten. Thermometer in this experiment was 58°.

Electric ſparks were paſſed for an hour and half through 7 ten grain meaſures of nitrous oxide over mercury covered with a little red cabbage juice, previouſly ſaturated with nitrous oxide, and rendered green by an alkali. After the proceſs the gas filled a ſpace equal to rather more than ſix meaſures and half, and the juice was become of a pale red. The gas was introduced into a ſmall tube filled with pure water, and agitated; no abſorption was perceptible; 7 meaſures of nitrous gas added to it gave red fumes, and after ſix minutes a diminution to $9\frac{1}{4}$ nearly. $6\frac{1}{2}$ meaſures of common air from the garden, with 7 of nitrous gas, gave exactly 9.

In this experiment it was evident that nitrous oxide was converted into a gas analogous to atmoſpheric air, at the ſame time that an acid was formed. There could be little doubt but that this was the nitrous acid. To aſcertain it, however, with greater certainty, the electric ſpark was paſſed through 6 meaſures of nitrous oxide, over a little ſolution of green ſulphate of iron, confined by mercury. As the proceſs went on, the color of the ſolution became rather darker. When the diminution was complete, a little pruſſiate of potaſh was added to the ſolution. A precipitate of pale blue pruſſiate of potaſh was produced.

*c.* Nitrous oxide was paſſed from decompoſing nitrate of ammoniac, through a porcelain tube well glazed inſide and outſide, ſtrongly ignited in an air-furnace, and communicating with the water apparatus. The gas collected was rendered opaque by denſe red vapor. It appeared wholly unabſorbable by water. After the precipitation of its vapor, a candle burnt in it with nearly the ſame brilliancy as in atmoſpheric air. 20 meaſures of it that had been agitated in water immediately after its production, mingled with 40 meaſures of nitrous gas, diminiſhed to about 47.5; whereas 20 meaſures that had remained unagitated for ſome time after their generation, introduced to the ſame quantity of nitrous gas, gave nearly 49. 20 meaſures of atmoſpheric air, with 40 of the ſame nitrous gas, were condenſed to 46.

The water with which the gas had been in contact, was ſtrongly acid. A little of it poured into a ſolution of green ſulphate of iron, and then mingled with pruſſian alkali, produced a green precipitate. Hence the acid it contained was evidently nitrous.

That no ſource of error could have exiſted in this experiment from fiſſure in the tube, I proved, by ſending water through it whilſt ignited, after the proceſs, from the ſame retort in which the nitrate of ammoniac had been decompoſed; a few globules of air only were produced, not equal to one tenth of the volume of the water boiled, and which were doubtleſs previouſly contained in it.

I have repeated this experiment two or three times, with ſimilar reſults; whenever the air was agitated in water immediately after its production, it gave *almoſt* the ſame diminution with nitrous gas as common air; when, on the contrary, it has been ſuffered to remain for ſome time in contact with the phlogiſticated nitrous acid ſuſpended in it, the condenſation has been leſs with nitrous gas by five or ſix hundred parts. Hence I am inclined to believe, that if it were poſſible to condenſe all the nitrous acid formed, immediately after its generation, ſo as to prevent it from abſorbing oxygene from the permanent gas, this gas would be found identical with the air of the atmoſphere.

The changes effected by fire on nitrous oxide are not analogous to thoſe produced by it in other bodies; for the power of this agent ſeems generally *uniform*, either in wholly ſeparating the conſtituent principles of bodies from each other, or in making them enter into more intimate union.*

It is a ſingular phænomenon, that whilſt it condenſes one part of the oxygene and nitrogene of nitrous oxide, in the form of nitrous acid;

* On the one hand, it decompoſes ammoniac into hydrogene and nitrogene, whilſt on the other, it converts free oxygene and nitrogene into nitrous acid. It likewiſe converts nitrous gas into nitrous acid and nitrogene. Till we are more accurately acquainted with the nature of heat, light, and electricity, we ſhall probably be unable to explain theſe phænomena.

it ſhould cauſe the remainder to expand, in the ſtate of atmoſpheric air. Does not this fact afford an inference in favor of the *chemical* compoſition of atmoſpheric air?

III. *Decompoſition of Nitrous Oxide by Hydrogene, at the temperature of Ignition.*

In the following experiments on the decompoſition of nitrous oxide by hydrogene, the gaſes were carefully generated in the mercurial apparatus, and their purity aſcertained by the teſts mentioned in Reſearch I. They were meaſured in ſmall tubes graduated to grains, and then transferred into the detonating tube, which was eight tenths of an inch in diameter, and graduated to ten grain meaſures.

The ſpace occupied by the gaſes being noted after the inflammation by the electric ſhock, green muriate of iron, and pruſſiate of potaſh, were ſucceſſively introduced, to aſcertain if any nitrous acid had been formed. The abſorption, if any took place, was marked, and the gaſes transferred into a narrow grain measure tube, and their bulk and composition accurately ascertained.

*b.* The hydrogene employed was procured from water by means of zinc and sulphuric acid. 50 grain measures of it fired by the electric spark, with 30 grain measures of oxygene containing one eleventh nitrogene, gave a residuum of about 4. Nitrous gas mingled with those 4, indicated the presence of rather less than 1 of unconsumed oxygene. In another experiment 23 of it, with 20 of the same oxygene left rather more than 6 residuum.

The nitrous oxide was apparently pure, for it left a remainder of about ,05 only, when absorbed by common water.

*c.* 50 of hydrogene were fired with 40 of nitrous oxide; the concussion was very great, and the light given out bright red; no perceptible quantity of nitrous acid was formed; the residual gas filled a space equal to 52. No part of it was absorbable by water, it gave no diminution with nitrous gas, when it was mingled with a little oxygene, and again acted on by the electric ſpark, an inflammation and ſlight diminution was produced.

*d.* 33 of hydrogene were fired with 35 of nitrous oxide: nitrous acid was produced in very minute quantity; the gas that remained was not abſorbable by water, and filled a ſpace equal to 37 grains. Nitrous gas mingled with theſe, underwent a very ſlight diminution.

*e.* 46 hydrogene were fired with 46 nitrous oxide. The quantity of nitrous acid formed was juſt ſufficient to tinge the white pruſſiate of potaſh. The gaſes filled a ſpace equal to 49, gave no perceptible diminution with nitrous gas, and did not inflame with oxygene.

*f.* 40 hydrogene were fired with 39 nitrous oxide; no perceptible quantity of nitrous acid was formed. The reſidual gas filled a ſpace equal to 41; was unabſorbable by water, underwent no diminution when mingled with nitrous gas; or when acted on by the electric ſpark in contact with oxygene.

*g.* 20 hydrogene were fired with 64 nitrous oxide; after detonation the expanſion of the gaſes was greater in this experiment than any of the preceding ones; denſe white fumes were obſerved in the cylinder, and a ſlow contraction of volume took place. After a little green muriate of iron had been admitted, the gaſes filled a ſpace equal to 73: pruſſiate of potaſh mingled with the muriate, gave a deeper blue than in any of the preceding experiments. The reſidual gas was unabſorbable by water: 65 of it, mingled with 65 of nitrous gas, diminiſhed to 93; whilſt 65 of common air, with 65 of nitrous gas, gave 84.

*h.* 8 of hydrogene were fired with 54 of nitrous oxide; the ſame phænomena as were obſerved in the laſt experiment took place; nitrous acid was formed; after the abſorption of which the reſidual gas filled a ſpace equal to 55. 50 of this, with an equal quantity of nitrous gas, diminiſhed to 76. In theſe proceſſes the temperatures were from 56° to 61°.

Theſe experiments are ſelected as the moſt accurate of nearly fifty, made on the inflammation of different quantities of nitrous oxide and hydrogene.

As Mr. Keir found muriatic acid in the fluid, produced by the inflammation of oxygene and hydrogene in clofed veffels, in Dr. Prieftley's experiments, I preferved the refidual gas of about 3 cubic inches of nitrous oxide, that had been detonated at different times with lefs than a cubic inch and half of hydrogene; but folution of nitrate of filver was not clouded when agitated in this gas, nor when introduced into the detonating tube in which the inflammation had been made.

From thefe experiments we learn that nitrous oxide is decompofable at the heat of ignition, by hydrogene, in a variety of proportions.

When the quantity of hydrogene very little exceeds that of the nitrous oxide, both of the gafes difappear, water is produced, no nitrous acid is formed, and the volume of nitrogene evolved is rather greater than that of the nitrous oxide decompofed.

When the quantity of hydrogene is lefs than that of the nitrous oxide, water, nitrous acid, oxygene and nitrogene, are generated in different proportions; one part of the nitrous oxide is moſt probably wholly decompoſed by the hydrogene, and the other part converted into nitrous acid and atmoſpheric air, in conſequence of the ignition.

From experiments *c*, *d*, and *e*, the compoſition of nitrous oxide may be deduced. In experiment *d*, 39 of nitrous oxide were decompoſed by 40 of hydrogene, and converted into 41 of nitrogene.

Now from *b* it appears that 40 of hydrogene require for their condenſation about 20.8 of oxygene in volume; ſo that founding the eſtimation upon the quantity of hydrogene conſumed, 100 parts of nitrous oxide would conſiſt nearly of 63.1 of nitrogene, and 36.9 of oxygene. But 41 of nitrogene weigh 12.4, Ref. I. Div. I. Conſequently, deducing the compoſition of nitrous oxide from the quantity of nitrogene evolved, 100 parts of it would conſiſt of 63.5 nitrogene, and 36.5 oxygene.

These eſtimations are very little different from thoſe which may be deduced from the other experiments, and the coincidence is in favor of their accuracy.

From the following experiment it appears that the temperature required for the decompoſition of nitrous oxide by hydrogene muſt be higher than that which is neceſſary to produce the inflammation of hydrogene with oxygene. I introduced into ſmall tubes filled with equal parts of nitrous oxide and hydrogene, ſtanding on a ſurface of mercury, iron wires ignited to different degrees, from the dull red to the vivid white heat. The gaſes were always inflamed by the white and vivid red heats; but never by the dull red heat, though the laſt uniformly inflamed mixtures of oxygene and hydrogene, and atmoſpheric air and hydrogene.

Dr. Prieſtley * firſt detonated together nitrous oxide and hydrogene; his experiment was repeated by the Dutch chemiſts, who found that when a ſmall quantity of hydrogene was

* Vol. ii. pag. 83.

employed, the nitrous oxide was partially converted into a gas analogous to common air. Their eftimation of its compofition, which is not far removed from the truth, was founded on this phænomenon.*

IV. *Decompofition of Nitrous Oxide by Phofphorus.*

*a.* Phofphorus introduced into pure nitrous oxide at common temperatures, is not at all luminous. It is capable of being fufed, and even fublimed in it, without undergoing acidification, and without effecting any alteration in its compofition.

About 2 grains of phofphorus were fufed, and gradually fublimed, in 2 cubic inches of pure nitrous oxide, over mercury, by the heat of a

* Journal de Phyfique, tom. xliii, part 2, pag. 331. They fuppofed it to confift of about 37,5 oxygene, and 62,5 nitrogene. The nearnefs of this account to the truth is fingular, when we confider that they were neither acquainted with the fpecific gravity of nitrous oxide, nor with the production of nitrous acid in this experiment.

burning lens. No alteration was produced in the volume of gas, and a portion of it abſorbed by water, left a reſiduum of one twelfth only.

Phoſphorus was ſublimed in pure nitrous oxide over mercury, in a dark room, by an iron heated *nearly* to ignition; but no luminous appearance was perceptible, nor was any gas decompoſed.

*b.* Phoſphorus decompoſes nitrous oxide at the temperature of ignition, with greater or leſs rapidity, according to the degree of heat. We have already ſeen, that when phoſphorus in active inflammation is introduced into nitrous oxide, it burns with intenſely vivid light.

Phoſphorus was ſublimed by a heated wire in a jar filled with nitrous oxide, ſtanding over warm mercury. In this ſtate of ſublimation an iron heated dull red was introduced to it by being rapidly paſſed through the mercury; a light blue flame ſurrounded the wire, and diſappeared as ſoon as it ceaſed to be red.

To phoſphorus ſublimed as before, in nitrous oxide, over warm mercury, a thick wire ignited to whiteneſs was introduced; a terrible detonation took place, and the jar was ſhattered in pieces.

By employing thick conical jars,* containing only a ſmall quantity of nitrous oxide, I effected the detonation ſeveral times with ſafety; but on account of the great expanſion of the elaſtic products, the jar was generally either raiſed from the mercury, or portions of gas were thrown out of it. Hence I was unable to aſcertain the exact changes produced by this mode of decompoſition.

*c.* As my firſt attempts to aſcertain the conſtitution of nitrous oxide were made on its decompoſition by phoſphorus, I employed many dif-

* Experiments on the detonation of nitrous oxide with phoſphorus in this way require great attention. The detonating jar ſhould be very conical; the nitrous oxide employed ſhould never equal more than one eighth of the capacity of the jar. The wire for the inflammation muſt be very thick, and curved ſo as to be eaſily introduced into the jar. When ignited, it muſt be inſtantaneouſly paſſed through the heated mercury into the jar.

Perhaps the electric ſpark might be advantageouſly applied for detonating phoſphoric vapor with nitrous oxide.

ferent modes of partially igniting this ſubſtance in it over mercury, ſo as to produce a combuſtion without exploſion.

The firſt method adopted was inflammation by means of oxygenated muriate of potaſh. A ſmall particle of oxygenated muriate of potaſh was inſerted into the phoſphorus to be burnt. On the application of a wire, moderately hot, to the point of inſertion, the ſalt was decompoſed by the phoſphorus, and ſufficient fire generated and partially applied by the ſlight exploſion, to produce the combuſtion of the phoſphorus, without the previous ſublimation of any part of it.

The ſecond way employed was the ignition of a part of the phoſphorus, by means of the combuſtion of a ſmall portion of tinder of cotton,* or paper, in contact with it, by the burning glaſs.

The third, and moſt ſucceſsful mode, was by introducing into the graduated jar containing

* It will be ſeen hereafter that theſe bodies are eaſily inflamed in nitrous oxide.

the nitrous oxide, the phoſphorus in a ſmall tube containing oxygene, ſo balanced as to ſwim on the ſurface of the mercury, without communicating with the nitrous oxide. The phoſphorus was fired in the oxygene with an ignited iron wire, by which at the moment of combuſtion, the tube containing it was raiſed into the nitrous oxide, and thus the inflammation continued.

*d.* In different experiments, made with accuracy, I found that the whole of a quantity of nitrous oxide was never decompoſable by ignited phoſphorus; the combuſtion always ſtopped when the nitrous oxide remaining was to the nitrogene evolved as about 1 to 5; likewiſe that the volume of nitrogene produced was rather leſs than that of the nitrous oxide decompoſed, and that this deficiency aroſe from the formation of nitrous acid by the intenſe ignition produced during the proceſs.

Of one experiment I ſhall give a detail.

Temperature being 48°, two cubic inches of pure nitrous oxide, which had been generated over mercury, were introduced into a jar of the capacity of 9 cubic inches, graduated to ,1 cubic inches, and much enlarged at the base. A grain of phosphorus was inserted into a small vessel about one third of an inch long, and half an inch in diameter, containing about 15 grain measures of very pure oxygene; this vessel, which swam on the surface of the mercury, was carefully introduced into the jar containing the nitrous oxide. The phosphorus was fired by means of a heated wire, and before the oxygene was wholly consumed, the vessel containing it elevated into the nitrous oxide. The combustion was extremely vivid and rapid. After the atmospheric temperature was restored, the gas was rendered opaque by dense white vapor. When this had been precipitated, and the small vessel removed from the jar, the gas filled a space nearly equal to 1.9 cubic inches. On introducing to it a little solution of green muriate of iron, and prussiate of potash, green prussiate of iron was produced: hence, evidently, nitrous acid had been formed.

On the admiſſion of pure water, an abſorption of rather more than ,3 took place.

The 16 meaſures remaining underwent no perceptible diminution with nitrous gas; the taper plunged into them was inſtantly extinguiſhed.

To aſcertain if the phoſphoric acid produced in the experiments made under mercury did not in ſome meaſure prevent the decompoſition of the whole of the nitrous oxide by the phoſphorus, I introduced into a mixture of 5 nitrogene and 1 nitrous oxide, ignited phoſphorus: but it was immediately extinguiſhed.*

The Dutch Chemiſts found that phoſphorus might be fuſed in nitrous oxide without being luminous. They aſſert that phoſphorus in a ſtate of inflammation, introduced into this gas, was immediately extinguiſhed; though when taken out into the atmoſphere, it again burnt of its own accord.† It is difficult to account for their miſtake.

* Phoſphorus burnt feebly with a white flame in a mixture of 4 nitrogene and 1 nitrous oxide.

† Journal de Phyſique, xliii. 328.

V. *Decompoſition of Nitrous Oxide by Phoſphorated Hydrogene.*

*a.* It has been mentioned in Ref. II. Div. I. that phoſphorated hydrogene and nitrous oxide poſſeſs no action on each other, at atmoſpheric temperatures.

Phoſphorated hydrogene mingled with nitrous oxide, is capable of being inflamed by the electric ſpark, or by ignition.

*b. E.* 1. 10 grain meaſures of phoſphorated hydrogene, carefully produced by means of phoſphorus and ſolution of cauſtic alkali, were mingled with 52 meaſures of nitrous oxide. The electric ſpark paſſed through them, produced a vivid inflammation. The elaſtic products were clouded with denſe white vapor, and after ſome minutes filled a ſpace nearly equal to 60. On the introduction of water, no abſorption took place. When 43 of nitrous gas were admitted, the whole diminiſhed to 70.

*E.* 2. 25 of nitrous oxide were fired with 10 of phofphorated hydrogene, by the electric fpark. After detonation* they filled a fpace exactly equal to 25. On the admiffion of folution of green fulphate of iron, and pruffiate of potafh, no blue or green precipitate was produced. On the introduction of water, no diminution was perceived. 25 of nitrous gas mingled with them, gave exactly 50.

*E.* 3. 10 of nitrous oxide, mingled with 20 of phofphorated hydrogene, could not be inflamed.

25 of nitrous oxide, with 20 phofphorated hydrogene, inflamed. The gas after detonation, was rendered opaque by denfe white vapor, and filled a fpace nearly equal to 45. No abforption took place when water was introduced. On admitting a little oxygene no white fumes, or diminution, was perceived. The electric fpark paffed through the mixture, produced an explofion, with great diminution.

*c.* From *E.* 1 it appears, that when a fmall quantity of phofphorated hydrogene is inflamed with

* In this experiment, as in the laft, denfe white vapor was produced.

nitrous oxide, both the phoſphorus and hydrogene are conſumed; whilſt the ſuperabundant nitrous oxide, is converted into nitrous acid and atmoſpheric air, by the ignition; or a certain quantity is partially decompoſed into atmoſpheric air by the combination of a portion of its oxygene with the combuſtible gas.

From *E*.2 we learn, that when the phoſphorated hydrogene and nitrous oxide are to each other as 25 to 10 nearly, they both diſappear, whilſt nitrogene is evolved, and water and phoſphoric acid produced. Reaſoning concerning the compoſition of nitrous oxide from this experiment, we ſhould conclude that it was compoſed of about 38 oxygene, and 62 nitrogene.

The reſult of *E*. 3 is intereſting; we are taught from it that the affinity of phoſphorus for the oxygene of nitrous oxide is ſtronger than that of hydrogene, at the temperature of ignition; ſo that when phoſphorated hydrogene is mingled with a quantity of nitrous oxide, not containing ſufficient oxygene to burn both its conſtituent parts, the phoſphorus only is conſumed, whilſt the hydrogene is liberated.

In repeating the experiments with phoſphorated hydrogene that had remained for ſome hours in the mercurial apparatus, I did not gain exactly the ſame reſults; for a larger quantity of it was required to decompoſe the nitrous oxide, than in the former experiments; doubtleſs from its having depoſited a portion of its phoſphorus. They confirm, however, the above mentioned concluſions.

In the courſe of experimenting, I paſſed the electric ſpark, for a quarter of an hour, through about 60 meaſures of phoſphorated hydrogene. It underwent no alteration of volume. Phoſphorus was apparently precipitated from it, and it had wholly loſt its power of inflaming, in contact with common air.

### VI. *Decompoſition of Nitrous Oxide by Sulphur.*

From the phænomena before mentioned,*

* Ref. I. Div. III. S. II.

relating to the combuſtion of ſulphur in nitrous oxide, it was evident that this gas was only decompoſable by it, at a much higher temperature than common air.

I introduced into ſulphur in contact with nitrous oxide, over mercury heated to 112°—114°, a wire intenſely ignited. It loſt much of its heat in paſſing through the mercury, but ſtill appeared red in the veſſel. The ſulphur rapidly fuſed, and ſublimed without being at all luminous. This experiment was repeated five or ſix times, but in no inſtance could the combuſtion of ſulphur, by means of the ignited wire, be effected.

I inflamed ſulphur in nitrous oxide in the ſame manner as phoſphorus; namely, by introducing it into the ſmall veſſel filled with oxygene, and igniting it by means of the heated wire. In theſe experiments the ſulphur burnt with a vivid roſe-colored light, and much ſulphuric, with a little ſulphureous acid, was formed.

Experimenting in this way I was never, however, able to decompoſe more than one third of the quantity of nitrous oxide employed; not only the nitrogene evolved, but likewife the fulphuric and fulphureous acids produced, ftopping the combuftion.

I found that fulphur in a ftate of vivid inflammation, when introduced into a mixture of one fourth nitrogene, and three fourths nitrous oxide, burnt with a flame very much enlarged, and of a vivid rofe color. In one third nitrogene, and two thirds nitrous oxide, it burnt feebly with a yellow flame. In equal parts of nitrous oxide and nitrogene, it was inftantly extinguifhed.

Sulphur burnt feebly, with a light yellow flame, when introduced ignited into a mixture of 5 nitrous gas, and 6 nitrous oxide. In one third nitrous oxide, and two thirds nitrous gas, it was inftantly extinguifhed. From many circumftances, I am inclined to believe that fulphur is incapable, at any temperature, of flowly decompofing nitrous oxide, fo as to burn in it with a blue flame, forming fulphureous acid alone. It appears to attract oxygene from it only when intenſely ignited, ſo as to form chiefly ſulphuric acid, and that with great rapidity, and vivid inflammation.

VII. *Decompoſition of Nitrous Oxide by Sulphurated Hydrogene.*

*a.* Though nitrous oxide and ſulphurated hydrogene do not act upon each other at common temperatures, yet they undergo a mutual decompoſition when mingled together in certain proportions, and ignited by the electric ſpark.

From more than twenty experiments made on the inflammation of ſulphurated hydrogene in nitrous oxide, I ſelect the following as the moſt concluſive and accurate. The temperature at which they were made was from 41° to 49°.

*b.* *E.* 1. About 35 meaſures of nitrous oxide were fired with 10 of ſulphurated hydrogene; the expanſion during inflammation was very great, and the flame ſky-blue. Immediately after, the gaſes filled a ſpace equal to 48 nearly. White fumes were then formed, and they gradually contracted to 40. On the admiſſion of a little ſtrontian lime water, a ſlight abſorption took place, with white precipitation; and the volume occupied by the reſidual gas nearly equalled 37. On admitting nitrous gas to theſe, no perceptible diminution took place.

*E.* 2. 20 ſulphurated hydrogene, with 25 nitrous oxide; could not be inflamed.

30 nitrous oxide, with 22 ſulphurated hydrogene, could not be inflamed.

35 nitrous oxide, with 20 ſulphurated hydrogene, inflamed with vivid blue light, and great expanſion. After the exploſion, the gaſes filled exactly the ſame ſpace as before the experiment; no white fumes were perceived, and no farther contraction occurred. On the addition of ſtrontian lime water, a copious precipitation, with diminution, took place; and the reſidual gas filled a ſpace nearly equal to $35\frac{1}{2}$.

*E.* 3. 47 nitrous oxide, and 14 ſulphurated hydrogene, inflamed. After the exploſion, the gaſes filled a ſpace nearly equal to 65; then white fumes formed, and they gradually diminiſhed to 52. On the introduction of muriate of ſtrontian, a copious white precipitate was produced; and on the addition of water, no further abſorption took place. To the reſidual 52, about 20 of nitrous gas were added; they filled together a ſpace equal to about 67.

*c.* In none of the experiments made on the inflammation of ſulphurated hydrogene and nitrous oxide, could I aſcertain with certainty the precipitation of ſulphur. In one or two proceſſes the detonating tube was rendered a little white at the points of contact with the mercury; but this was moſt probably owing to the oxydation of the mercury, either by the heated ſulphuric acid formed, or from nitrous acid produced by the ignition. The preſence of nitrous acid I could not aſcertain in theſe proceſſes by my uſual teſts, becauſe the combuſtion of ſulphur over white pruſſiate of iron, converts it into light green.

When I introduced an inflamed taper into about 3 parts of ſulphurated hydrogene, and 2 parts of nitrous oxide, in which proportions they could not have been fired by the electric ſpark, a blue flame paſſed through them, and much ſulphur was depoſited on the ſides of the veſſel. But this ſulphur moſt probably owed its formation to the decompoſition of a portion of ſulphurated hydrogene not burnt, by the ſulphureous acid formed from the combuſtion of the other portion.

We may then conclude with probability, that ſulphurated hydrogene and nitrous oxide will not decompoſe each other, when acted on by the electric ſpark, unleſs their proportions are ſuch as to enable the whole of the ſulphurated hydrogene to be decompoſed, ſo that both of its conſtituents may become oxygenated, by attracting oxygene from the nitrous oxide: likewiſe, that when the ſulphurated hydrogene is at its *maximum* of inflammation, the hydrogene and ſulphur form with the whole of the oxygene of nitrous oxide, water and ſulphureous acid; *E.* 2: whereas at its *minimum* they produce water, and chiefly, *perhaps* wholly, ſulphuric acid; at the ſame time that the nitrous oxide partially decompoſed, is converted into nitrogene, and a gas analogous to atmoſpheric air, or into nitrogene, nitrous acid, and atmoſpheric air. *E.* 1. E. 3.

By purſuing thoſe experiments, and uſing larger quantities of gas, we may probably be able to aſcertain from them with accuracy, the compoſition of ſulphuric and ſulphureous acids.

I own I was diſappointed in the reſults, for I expected to have been able to aſcertain from them, the relative affinities of ſulphur, and hydrogene for the oxygene of nitrous oxide, at the temperature of ignition. I conjectured that nitrous oxide, mingled with exceſs of ſulphurated hydrogene, would have been decompoſed, and one of the principles of it evolved unaltered, as was the caſe with phoſphorated hydrogene.

If we eſtimate the compoſition of nitrous oxide from the quantity of nitrogene produced in *E.* 2, it is compoſed of about 61 nitrogene, and 39 oxygene.

## VIII. *Decompoſition of Nitrous Oxide by Charcoal.*

An account of the analyſis of nitrous oxide by charcoal is given in Ref. I. Div. III. I have lately made two experiments on the combuſtion of charcoal in nitrous oxide, in which every precaution was taken to prevent the exiſtence of ſources of error. Of one of theſe I ſhall give a detail.

*E.* Temperature being 51°, about a grain of charcoal, which had been expoſed for ſome hours to a red heat, was introduced whilſt ignited, under mercury, and transferred into a graduated jar, containing 3 cubic inches of pure nitrous oxide, ſtanding over dry mercury.

The focus of a burning lens was thrown on the charcoal; it inſtantly inflamed, and burnt with great vividneſs for near a minute, the gas being much expanded. The focus was continually applied to it for ten minutes, when the proceſs appeared at an end. The gaſes, when the common temperature and preſſure were reſtored, filled a ſpace equal to 4,2 cubic inches.

On introducing into them a few grain meaſures of ſolution of green muriate of iron, for the double purpoſe of ſaturating them with moiſture, and aſcertaining if any nitrous acid had been formed, no change of volume took place; and pruſſiate of potaſh gave with the muriate a white precipitate only.

On the admiſſion of a ſmall quantity of concentrated ſolution of cauſtic potaſh, a diminution of the gas ſlowly took place; when it was complete the volume equalled about 3.05 cubic inches. By agitation in well boiled water, about ,9 of theſe were abſorbed; the remainder appeared to be pure nitrogene.

The difference between the eſtimation founded upon the nitrogene evolved, and that deduced from the carbonic acid generated in this experiment, is not nearly ſo great as in that Reſ. I. Div. III. Taking about the mean proportions, we ſhould conclude that nitrous oxide was compoſed of about 38 oxygene, and 62 nitrogene.

Charcoal burnt with greater vividneſs than in common air, in a mixture of one third nitrogene and two thirds nitrous oxide. In equal parts of nitrous oxide and nitrogene, its light was barely perceptible. In one third nitrous oxide, and two thirds nitrogene, it was almoſt immediately extinguiſhed.

As charcoal burns vividly in nitrous gas, when it has been previouſly ignited to whiteneſs, I introduced it into a mixture of equal parts of nitrous oxide and nitrous gas; it burnt with a deep and bright red.

### IX. *Decompoſition of Nitrous Oxide by Hydro-carbonate.*

Nitrous oxide, and hydro-carbonate, poſſeſs no action on each other, except at high temperatures. When mingled in certain proportions, and expoſed to the electric ſhock, a new arrangement of their principles takes place.

*E.* 1. Temperature being 53°, 35 of nitrous oxide, mingled with 15 of hydrocarbonate, were fired by the electric ſpark; the inflammation was very vivid, and the light produced, bright red. After the exploſion, the ſpace occupied by the gaſes equalled about 60. On the admiſſion of ſolution of ſtrontian, a copious white precipitate was produced, and the gas diminiſhed by agitation, to rather more than 35. When 36 of nitrous gas were added to theſe, white fumes appeared and the whole diminiſhed to 62. When a little muriatic acid was poured on the white precipitate from the ſolution of ſtrontian, gas was evolved from it, and it was gradually diſſolved.

*E*. 2. 22 nitrous oxide were inflamed with 20 hydro-carbonate; after the exploſion, they filled a ſpace equal to 45; when ſtrontian lime water was introduced, white precipitation took place, and the diminution was to 31.

To theſe 31, 14 of nitrous oxide were admitted, and the electric ſpark paſſed through them; an inflammation took place: carbonic acid was formed, after the abſorption of which, the gas remaining filled a ſpace equal to 43, and did not diminiſh with nitrous gas.

The hydro-carbonate employed in theſe experiments, was procured from alcohol by means of ſulphuric acid. In another ſet of experiments made with leſs accuracy, the ſame general reſults were obtained. Whenever hydro-carbonate inflamed with nitrous oxide, both its conſtituents were oxygenated; in all caſes carbonic acid was formed, and in no inſtance free hydrogene evolved, or charcoal precipitated.

In the decompoſition of nitrous oxide by hydro-carbonate, the reſidual nitrogene is leſs than in in other combuſtions. This circumſtance I am unable to explain.

Reaſoning from analogy, there can be little doubt, but that when hydro-carbonate is inflamed with exceſs of nitrous oxide, it will be only partially decompounded, or converted into nitrogene, nitrous acid, and atmoſpheric air.

The Dutch Chemiſts have aſſerted, that charcoal does not burn in nitrous oxide, except in conſequence of the previous decompoſition of the gas by the hydrogene always contained in this ſubſtance; and likewiſe, that when hydro-carbonate and nitrous oxide were mingled together, and fired by the electric ſpark, the hydrogene only was burnt, whilſt the charcoal was precipitated.

It is difficult to account for theſe numerous miſtakes. Their theory of the *non-reſpirability* of nitrous oxide was founded upon them. They ſuppoſed that the chief uſe of reſpiration was to deprive the blood of its ſuperabundant carbon, by the combination of atmoſpheric oxygene with that principle; and that nitrous oxide was highly fatal to life, becauſe it was incapable of de-carbonating the blood* ! !

X. *Combuſtion of Iron in Nitrous Oxide.*

I introduced into a jar of the capacity of 20 cubic inches, containing 11 cubic inches of nitrous oxide, over mercury, a ſmall quantity of fine iron wire twiſted together, and having

* Journal de Phyſique, xliii. 334.

affixed to it a particle of cork. On throwing the focus of a burning glafs on the cork, it inftantly inflamed, and the fire was communicated to the wire, which burnt with great vividnefs for fome moments, projecting brilliant white fparks. After it had ceafed to burn the gas was increafed in volume rather more than three tenths of an inch. The nitrous acid tefts were introduced, but no acid appeared to have been formed. On expofing the gas to water, near 4,2 cubic inches were abforbed: the 7,1 remaining appeared to be pure nitrogene.

From this experiment it is evident that iron at the temperature of ignition, is capable of decompofing nitrous oxide; likewife that it is incapable of burning in it when it contains more than three fifths nitrogene.

I attempted to inflame zinc in nitrous oxide, in the fame way as iron; but without fuccefs. By keeping the focus of a burning glafs upon fome zinc filings, in a fmall quantity of nitrous oxide, I converted a little of the zinc into white oxide, and confequently decompofed a portion of the gas.

XI. *Combuſtion of Pyrophorus in Nitrous Oxide.*

Pyrophorus, which inflames in nitrous gas, and atmoſpheric air, at or even below 40°, requires for its combuſtion in nitrous oxide a much higher temperature. It will not burn in it, or alter it, even at 212°.

I have often inflamed pyrophorus in nitrous oxide over mercury, by means of a wire ſtrongly heated, but not ignited. The light produced by the ignition of pyrophorus in nitrous oxide is white, like that produced by it in oxygene: in nitrous gas it is red.

When pyrophorus burns out in nitrous oxide, a little increaſe of the volume of gas is produced. Strontian lime water agitated in this gas becomes clouded; but the quantity of carbonic acid formed is extremely minute. I have never made any delicate experiments on the combuſtion of pyrophorus in nitrous oxide.

## XII. *Combuſtion of the Taper in Nitrous Oxide.*

It has been noticed by different experimentaliſts, that the taper burns with a flame conſiderably enlarged in nitrous oxide: ſometimes with a vivid light and crackling noiſe, as in oxygene; at other times with a white central flame, ſurrounded by a feeble blue one.

My experiments on the combuſtion of the taper in nitrous oxide, were chiefly made with a view to aſcertain the cauſe of the double flame.

When the inflamed taper is introduced into pure nitrous oxide, it burns at firſt with a brilliant white light, and ſparkles as in oxygene. As the combuſtion goes on, the brilliancy of the flame diminiſhes; it gradually lengthens, and becomes ſurrounded with a pale blue cone of light, from the apex of which much unburnt charcoal is thrown off, in the form of ſmoke. The flame continues double to the end of the proceſs.

When the refidual gafes are examined after combuftion, much nitrous acid is found fufpended in them; and they are compofed of carbonic acid, nitrogene, and about one fourth of undecompounded nitrous oxide.

The double flame depends upon the nitrous acid formed by the ignition; for it can be produced by plunging the taper into common air containing nitrous acid vapor, or into a mixture of nitrous oxide and nitrogene, through which nitrous acid has been diffufed. It is never perceived in the combuftion of the taper, till much nitrous acid is formed.

In attempting to refpire fome refidual gas of nitrous oxide, in which a taper had burnt out, I found it fo highly impregnated with nitrous acid, as to difable me from even taking it into my mouth.

The taper burns in a mixture of equal parts nitrous oxide and nitrogene, at firft with a flame nearly the fame as that of a candle in common air; white. Before its extinction the interior white flame, and exterior blue flame, are perceived.

The taper is inftantly extinguifhed in a mixture of one fourth nitrous oxide, and three fourths nitrogene.

In a mixture of equal parts nitrous oxide and nitrous gas, the taper burns at firft with nearly as much brilliancy as in pure nitrous oxide; gradually the double and feeble flame is produced.

XIII. *On the Combuftion of different Compound Bodies in Nitrous Oxide.*

All the folid and fluid compound inflammable bodies on which I have experimented, burn in nitrous oxide, at high temperatures. Wood, cotton, and paper, are eafily inflamed in it by the burning glafs. During their combuftion, nitrous acid is always formed, carbonic acid, and water produced, and nitrogene evolved, rather lefs in bulk than the nitrous oxide decompofed.

I have already mentioned that alcohol and ether are foluble in nitrous oxide. When an ignited body is introduced into the folution of alcohol, or ether in nitrous oxide, a ſlight exploſion takes place.

XIV. *General Concluſions relating to the Decompoſition of Nitrous Oxide, and to its Analyſis.*

From what has been ſaid in the preceding ſections, it appears that the inflammable bodies, in general, require for their combuſtion in nitrous oxide, much higher temperatures than thoſe at which they burn in atmoſpheric air, or oxygene.

When intenſely heated they decompoſe it, with the production of much heat and light, and become oxygenated.

During the combuſtion of ſolid or fluid bodies, producing flame, in nitrous oxide, nitrous acid is generated, moſt probably from a new arrangement of principles, analogous to thoſe obſerved in Sect. II, by the ignition of that part of the gas not in contact with the burning ſubſtance. Likewiſe when nitrous oxide in exceſs is decomposed by inflammable gases, nitrous acid, and sometimes a gas analogous to common air, is produced, doubtless from the same cause.

Pyrophorus is the only body that inflames in nitrous oxide, below the temperature of ignition.

Phosphorus burns in it with the blue flame, probably forming with its oxygene only phosphoreous acid at the dull red heat, and with the intensely vivid flame, producing phosphoric acid at the white heat.

Hydrogene, charcoal, sulphur, iron, and the compound inflammable bodies, decompose it only at heats equal to, or above, that of ignition; *probably* each a different temperature.

From the phænomena in Sect. V. it appears, that at the temperature of intense ignition, phosphorus has a stronger affinity for the oxygene of nitrous oxide than hydrogene; and reasoning from the different degrees of combustibility of the inflammable bodies, in mixtures of nitrous oxide and nitrogene, and from other phænomena, we may conclude with probability, that at about the white heat, the affinity of the combuſtible bodies for oxygene takes place in the following order. Phoſphorus, hydrogene, charcoal,* iron, ſulphur, &c.

This order of attraction is very different from that obtaining at the red heat; in which temperature charcoal and iron have a much ſtronger affinity for oxygene than either phoſphorus or hydrogene.†

The ſmalleſt quantity of oxygene given in the different analyſes of nitrous oxide juſt detailed, is thirty five hundred parts; the greateſt proportion is thirty-nine.

Taking the mean eſtimations from the moſt accurate experiments, we may conclude that 100 grains of the known ponderable matter of

* As is proved by the decompoſition of oxide of iron and ſulphuric acid by charcoal, at that temperature.

† Hydrogene at or about the red heat, appears to attract oxygene ſtronger than phoſphorus. See Dr. Prieſtley's experiments, vol. i. page 262.

nitrous oxide, consist of about 36,7 oxygene, and 63,3 nitrogene; or taking away decimals, of 37 oxygene to 63 nitrogene; which is identical with the estimation given in Research I.

XV. *Observations on the combinations of Oxygene and Nitrogene.*

During the decompositions of the combinations of oxygene and nitrogene by combustible bodies, a considerable momentary expansion of the acting substances, and the bodies in contact with them is generally produced, connected with increased temperature; whilst light is often generated to a great extent.

Of the causes of these phænomena we are at present ignorant. Our knowledge of them must depend upon the discovery of the precise nature of heat and light, and of the laws by which they are governed. The application of general hypotheses to isolated facts can be of little utility; for this reason I shall at present forbear to enter into any discussions concerning thoſe agents, which are imperceptible to the ſenſes, and known only by ſolitary effects.

Analyſis and ſyntheſis clearly prove that oxygene and nitrogene conſtitute the known ponderable matter of atmoſpheric air, nitrous oxide, nitrous gas, and nitric acid.

That the oxygene and nitrogene of atmoſpheric air exiſt in chemical union, appears almoſt demonſtrable from the following evidences.

1ſt. The equable diffuſion of oxygene and nitrogene through every part of the atmoſphere, which can hardly be ſuppoſed to depend on any other cauſe than an affinity between theſe principles.*

2dly. The difference between the ſpecific

* That attraction muſt be called chemical, which enables bodies of different ſpecific gavities to unite in ſuch a manner as to produce a compound, in every part of which the conſtituents are found in the ſame proportions to each other. Atmoſpheric air, examined after having been at perfect reſt in cloſed veſſels, for a great length of time, contains in every part the ſame proportions of oxygene and nitrogene; whereas if no affinity exiſted between theſe principles, following the laws of ſpecific gravity, they ought to ſepa-

gravity of atmofpheric air, and a mixture of 27 parts oxygene and 73 nitrogene, as found by calculation; a difference apparently owing to expanfion in confequence of combination.

3dly. The converfion of nitrous oxide into nitrous acid, and a gas analogous to common air, by ignition.

4thly. The folubility of atmofpheric air undecompounded in water.

ATMOSPHERIC AIR, then, may be confidered as the leaft intimate of the combinations of nitrogene and oxygene.

It is an elaftic fluid, permanent at all known temperatures, confifting of ,73 nitrogene, and ,27 oxygene. It is decompofable at certain temperatures, by moft of the bodies poffeffing affinity for oxygene. It is foluble in about thirty times its bulk of water, and as far as we are acquainted with its affinities, incapable of

rate; the oxygene forming the inferior, the nitrogene the fuperior ftratum.

The fuppofition of the *chemical* compofition of atmofpheric air, has been advanced by many philofophers. The two firft evidences have been often noticed.

combining with moſt of the ſimple and compound ſubſtances. 100 cubic inches of it weigh about 31 grains at 55° temperature, and 30 atmoſpheric preſſure.

NITROUS OXIDE is a gas unalterable in its conſtitution, at temperatures below ignition. It is compoſed of oxygene and nitrogene, exiſting *perhaps* in the moſt intimate union which thoſe ſubſtances are capable of aſſuming.* Its properties approach to thoſe of acids. It is decompoſable by the combuſtible bodies at very high temperatures, is ſoluble in double its volume of water, and in half its bulk of moſt of the inflammable fluids. It is combinable with the alkalies, and capable of forming with them peculiar ſalts. 100 grains of it are compoſed of about 63 nitrogene, and 37 oxygene. 100 cubic inches of it weigh 50 grains, at 55° temperature, and 30 atmoſpheric preſſure.

* For it is unalterable by thoſe bodies which are capable of attracting oxygene from nitrous gas and nitrous acid, at common temperatures.

Nitrous Gas is compoſed of about ,56 oxygene, and ,44 nitrogene, in intimate union. It is ſoluble in twelve times its bulk of water, and is combinable with the acids, and certain metallic ſolutions; it is poſſeſſed of no acid properties, and is decompoſable by moſt of the bodies that attract oxygene ſtrongly, at high temperatures. 100 cubic inches of it weigh about 34 grains, at the mean temperature and preſſure.

Nitric Acid is a ſubſtance permanently aëriform at common temperatures, compoſed of about 1 nitrogene, to 2,3 oxygene. It is ſoluble to a great extent in water, and combinable with the alkalies, and nitrous gas. It is decompoſable by moſt of the combuſtible bodies, at certain temperatures. 100 cubic inches of it weigh, at the mean temperature and preſſure, nearly 76 grains.

# RESEARCH III.

RELATING TO THE RESPIRATION OF

# NITROUS OXIDE,

AND OTHER

# GASES.

## RESEARCH III.

### DIVISION I.

*EXPERIMENTS and OBSERVATIONS on the EFFECTS produced upon ANIMALS by the RESPIRATION of NITROUS OXIDE.*

#### I. *Preliminaries.*

THE term *reſpirable*, in its phyſiological application, has been differently employed. Some times by the reſpirability of a gas has been meant, its power of ſupporting life for a great length of time, when repeatedly applied to the blood in the lungs. At other times all gaſes have been conſidered as reſpirable, which were capable of introduction into the lungs by voluntary efforts, without any relation to their vitality.

In the laſt ſenſe the word reſpirable is moſt properly employed. In this ſenſe it is uſed in the following ſections.

Non-reſpirable gaſes are thoſe, which when applied to the external organs of reſpiration, ſtimulate the muſcles of the epiglottis in ſuch a way as to keep it perfectly cloſe on the glottis; thus preventing the ſmalleſt particle of gas from entering into the bronchia, in ſpite of voluntary exertions; ſuch are carbonic acid, and acid gaſes in general.*

Of reſpirable gaſes, or thoſe which are capable of being taken into the lungs by voluntary efforts.

One only has the power of uniformly ſupporting life;—atmoſpheric air. Other gaſes, when reſpired, ſooner or later produce death; but in different modes.

Some, as nitrogene and hydrogene, effect no poſitive change in the venous blood. Animals

* See the curious experiments of Roſier, Journal de Phyſique, 1786, vol. 1, pag. 419.

immerſed in theſe gaſes die of a diſeaſe produced by privation of atmoſpheric air, analogous to that occaſioned by their ſubmerſion in water, or non-reſpirable gaſes.

Others, as the different varieties of hydrocarbonate, deſtroy life by producing ſome poſitive change* in the blood, which probably immediately renders it incapable of ſupplying the nervous and muſcular fibres with principles eſſential to ſenſibility and irritability.

Oxygene, which is capable of being reſpired for a much greater length of time than any other gas, except common air, finally deſtroys life; firſt producing changes in the blood, connected with new living action.†

After experiments, to be detailed hereafter, made upon myſelf and others, had proved that nitrous oxide was reſpirable, and capable of

* As appears from the experiments of Dr. Beddoes; likewiſe thoſe of Mr. Watt.

† As appears from the experiments of Lavoiſier and Dr. Beddoes; and as will be ſeen hereafter.

ſupporting life for a longer time than any of the gaſes, except atmoſpheric air and oxygene, I was anxious to aſcertain the effects of it upon animals, in caſes where its action could be carried to a full extent; and to compare the changes occaſioned by it in their organs, with thoſe produced by other powers.

II. *On the reſpiration of Nitrous Oxide by warm-blooded Animals.*

The nitrous oxide employed in the following experiments, was procured from nitrate of ammoniac, and received in large jars, filled with water previouſly ſaturated with the gas. The animal was introduced into the jar, by being carried under the water; after its introduction, the jar was made to reſt on a ſhelf, about half an inch below the ſurface of the water; and the animal carefully ſupported, ſo as to prevent his mouth from reſting in the water.

This mode of experimenting, either under water or mercury, is abſolutely neceſſary, to aſcertain with accuracy the effects of pure gaſes on living beings. In ſome experiments that I made on the reſpiration of nitrous oxide, by animals that were plunged into jars of it opened in the atmoſphere, and immediately cloſed after their introduction, the unknown quantities of common air carried in, were always ſufficient to render the reſults perfectly inaccurate.

Animals ſuffer little or nothing by being paſſed through water.

That the phænomena in theſe experiments might be more accurately obſerved, two or three perſons were always preſent at the time of their execution, and an account of them was noted down immediately after.

*a.* A ſtout and healthy young cat, of four or five months old, was introduced into a large jar of nitrous oxide. For ten or twelve moments he remained perfectly quiet, and then began to make violent motions, throwing himſelf round the jar in every direction. In two minutes he appeared quite exhauſted, and ſunk quietly to the bottom of the jar. On applying my hand to the thorax, I found that the heart beat with extreme violence ; on feeling about the neck, I could diſtinctly perceive a ſtrong and quick pulſation of the carotids. In about three minutes the animal revived, and panted very much ; but ſtill continued to lie on his ſide. His inſpirations then became longer and deeper, and he ſometimes uttered very feeble cries. In four minutes the pulſations of the heart appeared quicker and feebler. His inſpirations were at long intervals, and very irregular ; in five minutes the pulſe was hardly perceptible ; he made no motions, and appeared wholly ſenſeleſs. After five minutes and quarter he was taken out, and expoſed to the atmoſphere before a warm fire. In a few ſeconds he began to move, and to take deep inſpirations. In five minutes he attempted to riſe on his legs ; but ſoon fell again, the extremities being ſlightly convulſed. In eight or nine minutes he was able to walk, but his motions were ſtaggering and unequal, the right leg being convulſed, whilſt the other was apparently ſtiff and immoveable ; in about half an hour he was almoſt completely recovered.

*b.* A healthy kitten, of about ſix weeks old, was introduced into nitrous oxide. She very ſoon began to make violent exertions, and in leſs than a minute fell to the bottom of the receiver, as if apoplectic. At this moment, applying my hand to her ſide, I felt the heart beating with great violence. She continued gaſping, with long inſpirations, for three minutes and half; at the end of five minutes and half ſhe was taken out completely dead.

*c.* Another kitten of the ſame breed was introduced into nitrous oxide, the day after. She exhibited the ſame phænomena, and died in it in about five minutes and half.

*d.* A ſmall dog that had accidentally met with a diſlocation of the vertebræ of the loins, and was in great pain, as manifeſted by his moaning and whining, was introduced into a large jar of nitrous oxide. He immediately became quiet, and lay on his ſide in the jar, breathing very deeply. In four minutes his reſpiration became noiſy, and his eyes ſparkled very much. I was not able to apply my hand to the thorax. In five minutes he appeared fenfelefs, and in feven minutes was perfectly dead.

*e.* A ftrong rabbit, of ten or twelve months old, was introduced into nitrous oxide. He immediately began to ftruggle very much, and in a minute fell down fenfelefs: in two minutes the legs became convulfed, and his infpirations were deep and noify: in lefs than five minutes he appeared perfectly dead.

*f.* A rabbit of a month old introduced into nitrous oxide, became fenfelefs in lefs than a minute; the pulfations of the heart were very ftrong at this moment: they gradually became weaker, and in three minutes and half the animal was dead.

*g.* Another rabbit of the fame breed, after being rendered fenfelefs in nitrous oxide in a minute and half, was taken out. He foon became convulfed; in a minute began to breathe quickly; in two minutes attempted to rife, but ftaggered, and fell again on his fide. His hinder legs were paralytic for near five minutes. In twenty he had almoft recovered.

*g*. A middle fized guinea-pig was much convulfed, after being in nitrous oxide for a minute. In two minutes and half he was fenfelefs. Taken out at this period, he remained for fome minutes by the fide of a warm fire, without moving; his fore legs then became convulfed; his hind legs were perfectly paralytic. In this ftate he continued, without attempting to rife or move, for near an hour, when he died.

*h*. A large and old guinea-pig died in nitrous oxide, exhibiting the fame phænomena as the other animals, in about five minutes and quarter. A young one was killed in three minutes and half.

*i*. A fmall guinea-pig, after breathing nitrous oxide for a minute and half, was taken out, and placed before a warm fire. He was for a few minutes a little convulfed; but in a quarter of an hour got quite well, and did not relapfe.

*k*. A large moufe introduced into nitrous oxide, was for a few feconds very active. In half a minute he fell down fenfelefs; in a minute and quarter he appeared perfectly dead.

*l.* A moufe taken out of nitrous oxide, after being in it for half a minute, continued convulfed for fome minutes, but finally recovered.

*m.* A young hen was introduced into a veffel filled with nitrous oxide. She immediately began to ftruggle very much; fell on her breaft in lefs than half a minute, and in two minutes was quite dead.

*n.* A goldfinch died in nitrous oxide in lefs than a minute.

In each of thefe experiments a certain abforption of the gas was always perceived, the water rifing in the jar during the refpiration of the animal. From them we learn

1ft. That nitrous oxide is deftructive when refpired for a certain time to the warm blooded animals, apparently previoufly exciting them to a great extent.

2dly. That when its operation is ftopped before compleat exhauftion is brought on, the healthy living action is capable of being gradually reproduced, by enabling the animal to refpire atmofpheric air.

3dly. That exhauſtion and death is produced in the ſmall animals by nitrous oxide ſooner than in the larger ones, and in young animals of the ſame ſpecies, in a ſhorter time than in old ones, as indeed Dr. Beddoes had conjectured a priori would be the caſe.

Moſt of the animals deſtroyed in theſe experiments were examined after death; the appearances in their organs were peculiar. To prevent unneceſſary repetitions, an account of them will be given in the fourth ſection.

## III. *Effects of the reſpiration of Nitrous Oxide upon animals, as compared with thoſe produced by their immerſion in Hydrogene and Water.*

Before the following experiments were made, a number of circumſtances had convinced me that nitrous oxide acted on animals by producing ſome poſitive change in their blood, connected with new living action of the irritable and ſenſitive organs, and terminating in their death.

To aſcertain however, the difference between the effects of this gas and thoſe of hydrogene and non-reſpirable gaſes, I proceeded in the following way.

*a.* Of two healthy rabbits of about two months old, of the ſame breed, and nearly of the ſame ſize.

One was introduced into nitrous oxide. In a half a minute, it had fallen down apparently ſenſeleſs. On applying my hand to the thorax, the action of the heart appeared at firſt, very quick and ſtrong, it gradually became weaker, and in two minutes and half, the animal was taken out quite dead.

The other was introduced into a jar of pure hydrogene through water. He immediately began to ſtruggle very much, and in a quarter of a minute fell on his ſide. On feeling the thorax, the pulſations of the heart appeared very quick and feeble, they gradually diminiſhed; his breathing became momentarily ſhorter, and in rather more than three quarters of a minute, he was taken out dead. Dr. Kinglake was present at this experiment, and afterwards diffected both of the animals.

*b.* Of two fimilar rabbits of the fame breed, nearly three months old. One was introduced into nitrous oxide, and after being rendered fenfelefs by the refpiration of it for nearly a minute and half, was expofed to the atmofphere, before a warm fire. He recovered gradually, but was occafionally convulfed, and had a paralyfis of one of his hinder legs for fome minutes: in an hour he was able to walk. The other, after being immerged in hydrogene for near half a minute, was reftored to the atmofphere apparently inanimate. In lefs than a minute he began to breathe, and to utter a feeble noife; in two minutes was able to walk, and in lefs than three minutes appeared perfectly recovered.

*b.* A kitten of about two months old, was introduced into a jar of nitrous oxide, at the fame time that another of the fame breed was plunged under a jar of water. They both ftruggled very much. The animal in the nitrous oxide fell ſenſeleſs before that under water had ceaſed to ſtruggle, and to throw out air from its lungs. In two minutes and three quarters, the animal under water was quite dead: it was taken out and expoſed to heat and air, but did not ſhew the ſlighteſt ſigns of life. At the end of three minutes and half, the animal in nitrous oxide began to gaſp, breathing very ſlowly; at four minutes and three quarters it was yet alive; at the end of five minutes and quarter it appeared perfectly dead. It was taken out, and did not recover.

From theſe experiments it was evident, that animals lived at leaſt twice as long in nitrous oxide as in hydrogene or water. Conſequently from this circumſtance alone, there was every reaſon to ſuppoſe that their death in nitrous oxide could not depend on the ſimple privation of atmoſpheric air; but that it was owing to ſome peculiar changes effected in the blood by the gas.

## IV. *Of the changes effected in the Organisation of warm-blooded Animals, by the respiration of Nitrous Oxide.*

The external appearance of animals that have been deſtroyed in nitrous oxide, is very little different from that of thoſe killed by privation of atmoſpheric air. The fauces and tongue appear of a dark red, and the eyes are dull, and a little protruded. Their internal organs, however, exhibit a very peculiar change. The lungs are pale brown red, and covered here and there with purple ſpots; the liver is of a very bright red, and the muſcular fibre in general dark. Both the auricles and ventricles of the heart are filled with blood. The auricles contract for minutes after the death of the animal. The blood in the left ventricle, and the aorta, is of a tinge between purple and red, whilſt that in the right ventricle is of a dark color, rather more purple than the venous blood. But theſe appearances, and their cauſes, will be better underſtood after the following comparative obſervations are read.

*a*. Of two ſimilar rabbits, about eight months old, one A, was killed by expoſure for near ſix minutes to nitrous oxide, the other, B, was deſtroyed by a blow on the head.

They were both opened as ſpeedily as poſſible. The lungs of B were pale, and uniform in their appearance; this organ in A was redder, and every where marked with purple ſpots. The liver of A was of a dark and bright red, that of B of a pale red brown. The diaphragm of B, when cut, was ſtrongly irritable; that of A rather darker, and ſcarce at all contractile. All the cavities of the heart contracted for more than 50 minutes in B. The auricles contracted for near 25 minutes with force and velocity in A: but the ventricles were almoſt inactive. The vena cava, and the right auricle, in A, were filled with blood, apparently a ſhade darker than in B. The blood in the left auricle, and the aorta, appeared in A of a purple, a ſhade brighter than that of the venous blood. In the left auricle of B it was red.

I opened the head of each, but not without injuring the brains, ſo that I was unable to make any accurate comparifon. The color of the brain in A appeared rather darker than in B.

*b.* Two rabbits, C and D, were deftroyed, C by immerfion in nitrous oxide, D in hydrogene: they were both diffected by Dr. Kinglake. The blood in the pulmonary vein and the left auricle of C was of a different tinge, from that in D more inclined to purple red. The membrane of the lungs in C was covered with purple fpots, that of D was pale and uniform in its appearance. The brain in C was rather darker than in D; but there was no perceptible effufion of blood into the ventricles either in D or C. The liver in C was of a brighter red than in health, that in D rather paler.

*c.* In the laft experiment, the comparative irritability of the ventricles and auricles of the heart and the mufcular fibre in the two animals, had not been examined. That thefe circumftances might be noticed, two rabbits, E and F were killed; E under water in about a minute, and F in nitrous oxide in three minutes. They were immediately opened, and after a minute, the appearance of the heart, and organs of refpiration obferved.

Both the right and left ventricles of the heart in F contracted but very feebly; the auricles regularly and quickly contracted; the aorta appeared perfectly full of blood. In E, a feeble contraction of the left finus venosus and auricle was obferved; the left ventricle did not contract: the right contracted, but more flowly than in F. In a few minutes, the contractions of the ventricles in F had ceafed, whilst the auricles contracted as ftrongly and quickly as before. The blood in the pulmonary veins of F was rather of a redder purple than in E; the difference of the blood in the vena cava was hardly perceptible, perhaps it was a little more purple in F. The membranous fubftance of the lungs in F was fpotted with purple as from extravafated blood, whilft that in E was pale. The brain in F was darker than in E. On opening the ventricles no extravafation of blood was perceptible.

The auricles of the heart in F contracted ftrongly for near twenty minutes, and then gradually their motion became lefs frequent; in twenty-eight minutes it had wholly ceafed. The right auricle and ventricle in E, occafionally contracted for half an hour. The livers of both animals were fimilar when they were firft opened, of a dark red; that of F preferved its color for fome time when expofed to the atmofphere; whilft that of E almoft immediately became paler under the fame circumftances.

The periftaltic motion continued for nearly an equal time in both animals.

*d.* The fternum of a young rabbit was removed fo that the heart and lungs could be perceived, and he was introduced into a veffel filled with nitrous oxide; the blood in the pulmonary veins gradually became more purple, and the heart appeared to beat quicker than before, all the mufcles contracting with great force. After he had been in about a minute, fpots began to appear on the lungs, though the contractions of the heart became quicker and weaker; in three minutes and half he was quite dead; after death the ventricles contracted very feebly, though the contractions of the auricles were as strong almost after the end of five minutes as at first. This animal was passed through water saturated with nitrous oxide; possibly this fluid had some effect on his organs.

Besides these animals, many others, as guinea-pigs, mice and birds, were dissected after being destroyed in nitrous oxide; in all of them the same general appearance was observed. Their muscular fibre almost always appeared less irritable than that of animals destroyed, by organic læsion of part of the nervous system, in the atmosphere. The ventricles of the heart in general, contracted feebly and for a very short time; whilst the auricles continued to act for a great length of time. The lungs were dark in their appearance, and always suffused here and there with purple; the blood in the pulmonary veins when slightly obſerved, appeared dark, like venous blood, but when minutely examined, was evidently much more purple. The blood in the vena cava, was darker than that in the pulmonary veins. The cerebrum was dark.

In a late experiment, I thought I perceived a ſlight extravaſation of blood in one of the ventricles of the brain in a rabbit deſtroyed in nitrous oxide; but as this appearance had not occurred in the animals I had examined before, or in thoſe diſſected by Dr. Kinglake, and Mr. King, Surgeon, I am inclined to refer it to an accidental cauſe. At my requeſt, Mr. Smith, Surgeon, examined the brain of a young rabbit that had been killed in his preſence in nitrous oxide; he was of opinion that no effuſion of blood into the ventricles had taken place.

In comparing the external appearance of the crural nerves in two rabbits that had been diſſected by Dr. Kinglake, having been deſtroyed one in hydrogene, the other in nitrous oxide, we could perceive no perceptible difference.

It deſerves to be noticed, that whenever the gall bladder and the urinary bladder have been examined in animals deſtroyed in nitrous oxide, they have been always diſtended with fluid; which is hardly ever the caſe in animals killed by privation of atmoſpheric air.

In the infancy of my experiments on the action of nitrous oxide upon animals, I thought that it rendered the venous blood leſs coagulable; but this I now find to be a miſtake. The blood from the pulmonary veins of animals killed in nitrous oxide, does not ſenſibly differ in this reſpect from the arterial blood of thoſe deſtroyed in hydrogene, and both become vermilion nearly in the ſame time when expoſed to the atmoſphere.

In deſcribing the various ſhades of color of the blood in the preceding obſervations on the different diſſected animals, the poverty of the language of color, has obliged me to adopt terms, which I fear will hardly convey to the mind of the reader, diſtinct notions of the differences obſervable by minute examination in the venous and arterial blood of animals that die of privation of atmofpheric air, and of thofe deftroyed by the action of nitrous oxide. This difference can only be obferved in the veffels by means of a ftrong light; it may however be eafily noticed in the fluid blood by the introduction of it from the arteries or veins at the moment of their incifion, between two polifhed furfaces of white glafs,* fo clofely adapted to each other, as to prevent the blood from coming in contact with the atmofphere.

Having four or five times had an opportunity of bleeding people in the arm for trifling complaints, I have always received the blood in phials, filled with various gafes, in a mode to be defcribed hereafter. Venous blood agitated in nitrous oxide, compared with fimilar blood in common air, hydrogene, and nitrogene, was always darker and more purple

* The colour of common venous blood, examined in this way, refembles that of the paint called by colour-men red ochre; that of blood faturated with nitrous oxide, approaches to the tinge of lake.

than the firſt, and much brighter and more florid than the two laſt, which were not different in their color from venous blood, received between two ſurfaces of glaſs. It will be ſeen hereafter, that the coagulum of venous blood is rendered more purple when expoſed to nitrous oxide, whilſt the gas is abſorbed; likewiſe that blood altered by nitrous oxide, is capable of being again rendered vermilion, by expoſure to the air.

The appearances noticed in the above mentioned experiments, in the lungs of animals deſtroyed in nitrous oxide, are ſimilar to thoſe obſerved by Dr. Beddoes, in animals that had been made to breathe oxygene for a great length of time.

There were many reaſons for ſuppoſing that the large purple ſpots in the lungs of animals deſtroyed in nitrous oxide, were owing to extravaſation of venous blood from the capillary veſſels; their coats being broken by the highly increaſed arterial action. To aſcertain whether theſe phænomena exiſted at a period of the action of nitrous oxide, when the animal was recoverable by exposure to the atmosphere,

I introduced a rabbit of six months old, into a vessel of nitrous oxide, and after a minute, when it had fallen down apparently apoplectic, plunged him wholly under water; he immediately began to struggle, and what surprised me very much, died in less than a minute after submersion. On opening the thorax, the blood in the pulmonary veins was nearly of the color of that in animals that have been simply drowned. The lungs were here and there, marked with a few points; but there were no large purple spots, as in animals that have been wholly destroyed in nitrous oxide: the right side of the heart only contracted. In this experiment, the excitement from the action of the gas was probably carried to such an extent, as to produce indirect debility. There are reasons for supposing, that animals after having been excited to but a small extent by the respiration of nitrous oxide, will live under water for a greater length of time, than animals previously made to breathe common air.

V. *Of the respiration of mixtures of Nitrous Oxide, and other gases, by warm-blooded Animals.*

*a.* A rabbit of near two months old, was introduced into a mixture of equal parts hydrogene and nitrous oxide through water. He immediately began to ſtruggle; in a minute fell on his ſide; in three minutes gaſped, and made long inſpirations; and in four minutes and half, was dead. On diſſection, he exhibited the ſame appearances as animals deſtroyed in nitrous oxide.

*b.* A large and ſtrong mouſe was introduced into a mixture of three parts hydrogene to one part nitrous oxide. He immediately began to ſtruggle very much, in half a minute, became convulſed, and in about a minute, was quite dead.

*c.* Into a mixture of one oxygene, and three nitrous oxide, a ſmall guinea-pig was introduced. He immediately began to ſtruggle, and in two minutes repoſed on his ſide, breathing very deeply. He made afterwards no violent muſcular motion; but lived quietly for near fourteen minutes: at the end of which time, his legs were much convulſed. He was taken out, and recovered.

*d.* A mouſe lived apparently without ſuffering, for near ten minutes, in a mixture of 1 atmoſpheric air, and 3 nitrous oxide, at the end of eleven minutes he began to ſtruggle, and in thirteen minutes became much convulſed.

*e.* A cat of three months old, lived for ſeventeen minutes, in a very large quantity of a mixture of 1 atmoſpheric air, and 12 nitrous oxide. On her firſt introduction ſhe was very much agitated and convulſed, in a minute and half ſhe fell down as if apoplectic, and continued breathing very deeply during the remainder of the time, ſometimes uttering very feeble cries. When taken out, ſhe appeared almoſt inanimate, but on being laid before the fire, gradually began to breathe and move; being for ſome time, like moſt of the animals that have recovered after breathing nitrous oxide, convulſed on one ſide, and paralytic the other.

*f*. A goldfinch lived for near five minutes in a mixture of equal parts nitrous oxide and oxygene, without apparently ſuffering. Taken out, he appeared faint and languid, but finally recovered.*

VI. *Recapitulation of facts relating to the reſpiration of Nitrous Oxide, by warm-blooded Animals.*

1. Warm-blooded animals die in nitrous oxide infinitely ſooner than in common air or oxygene; but not nearly in ſo ſhort a time as in gaſes incapable of effecting poſitive changes in the venous blood, or in non-reſpirable gaſes.

2. The larger animals live longer in nitrous oxide than the ſmaller ones, and young animals

* Small birds ſuffer much from cold when introduced into gaſes through water. In this experiment, the goldfinch was immediately inſerted into a large mouthed phial, filled with the gaſes, and opened in the atmoſphere.

die in it ſooner than old ones of the ſame ſpecies.

3. When animals, after breathing nitrous oxide, are removed from it before compleat exhauſtion has taken place, they are capable of being reſtored to health under the action of atmoſpheric air.

4. Peculiar changes are effected in the organs of animals by the reſpiration of nitrous oxide. In animals deſtroyed by it, the arterial blood is purple red, the lungs are covered with purple ſpots, both the hollow and compact muſcles are *apparently* very inirritable, and the brain is dark colored.

5. Animals are deſtroyed by the reſpiration of mixtures of nitrous oxide and hydrogene nearly in the ſame time as by pure nitrous oxide; they are capable of living for a great length of time in nitrous oxide mingled with very minute quantities of oxygene or common air.

Theſe facts will be reaſoned upon in the next diviſion.

VII. *Of the reſpiration of Nitrous Oxide by amphibious Animals.*

As from the foregoing experiments, it appeared that the nitrous oxide deſtroyed warm-blooded animals by increaſing the living action of their organs to ſuch an extent, as finally to exhauſt their irritability and ſenſibility; it was reaſonable to conjecture that the cold-blooded animals, poſſeſſed of voluntary power over reſpiration, would ſo regulate the quantity of nitrous oxide applied to the blood in their lungs as to bear its action for a great length of time. This conjecture was put to the teſt of experiment; the following facts will prove its error.

*a.* Of two middle-ſized water-lizards, one was introduced into a ſmall jar filled with nitrous oxide, over moiſt mercury, by being paſſed through the mercury; the other was made to breathe hydrogene, by being carried into it in the ſame manner.

The lizard in nitrous oxide, in two or three minutes, began to make violent motions, appeared very uneaſy, and rolled about the jar in every direction, ſometimes attempting to climb to the top of it. The animal in hydrogene was all this time very quiet, and crawled about the veſſel without being apparently much affected. At the end of twelve minutes, the lizard in nitrous oxide was lying on his back ſeemingly dead; but on agitating the jar he moved a little; at the end of fifteen minutes he did not move on agitation, and his paws were reſting on his belly. He was now taken out ſtiff and apparently lifeleſs, but after being expoſed to the atmoſphere for three or four minutes, took an inſpiration, and moved his head a little; he then raiſed the end of his tail, though the middle of it was ſtill ſtiff and did not bend when touched. His four legs remained cloſe to his ſide, and were apparently uſeleſs; but on pricking them with the point of a lancet, they became convulſed. After being introduced into ſhallow water, he was able to crawl in a quarter of an hour, though his motions were very irregular. In an hour he was quite well. The animal in hydrogene appeared to have fuffered very little in three quarters of an hour, and had raifed himfelf againft the fide of the jar. At the end of an hour he was taken out, and very foon recovered.

*b.* Some hours after, the fame lizards were again experimented upon. That which had been inferted into hydrogene in the laft experiment, being now inferted into nitrous oxide.

This lizard was apparently lifelefs in fourteen minutes, having tumbled and writhed himfelf very much during the firft ten minutes. Taken out after being in twenty-five minutes, he did not recover. The other lizard lived in hydrogene for near an hour and quarter, taken out after an hour and twenty minutes, he was dead.

Thefe animals were both opened, but the vifcera of the nitrous oxide lizard were fo much injured by the knife, that no accurate comparifon of them with thofe of the other could be made, I thought that the lungs appeared rather redder.

γ. Of two ſimilar large water-lizards, one was introduced into a veſſel ſtanding over mercury, wholly filled with water that had been long boiled and ſuffered to cool under mercury. The animal very often roſe to the top of the jar as, if in ſearch of air, during the firſt half hour; but ſhewed no other ſigns of uneaſineſs. At the end of three quarters of an hour, he became very weak, and appeared ſcarcely able to ſwim in the water. Taken out at the end of fifty minutes, he recovered.

The other was inſerted into nitrous oxide. After much ſtruggling, he became ſenſeleſs in about fifteen minutes, and lay on his back. Taken out at the end of twenty minutes, he remained for a long time motionleſs and ſtiff, but in a quarter of an hour, began to move ſome of his limbs.

From theſe experiments, we may conclude, that water-lizards, and moſt probably the other amphibious animals, die in nitrous oxide in a much ſhorter time than in hydrogene or pure water; conſequently their death in it cannot dependon the ſimple privation of atmoſpheric air.

At the ſeaſon of the year in which this inveſtigation was carried on, I was unable to procure frogs or toads. This I regret very much.

Suppoſing that cold-blooded animals die in nitrous oxide from poſitive changes effected in their blood by the gas, it would be extremely intereſting to notice the apparent alterations taking place in their organs of reſpiration and circulation during its action, which could eaſily be done, the membranous ſubſtance of their lungs being tranſparent. The increaſe or diminution of the irritability of their muſcular fibre, might be determined by comparative galvanic experiments.

VIII. *Effects of ſolution of nitrous oxide in water on Fiſhes.*

*a.* A ſmall flounder was introduced into a veſſel filled with ſolution of nitrous oxide in water over mercury. He remained at reſt for ten minutes and then began to move about the jar in different directions. In a half an hour he was apparently dying, lying on his ſide in the water. He was now taken out, and introduced into a veſſel filled with water ſaturated with common air, he very ſoon recovered.

*b*. Of two large thornbacks,* equally briſk and lively. One, A, was introduced into a jar containing nearly 3 cubic inches of water, ſaturated with nitrous oxide, and which previous to its impregnation had been long boiled; the other, B, was introduced into an equal quantity of water which had been deprived of air by diſtillation through mercury.

A, appeared very quiet for two or three minutes, and then began to move up and down in the jar, as if agitated. In eight minutes his motions became very irregular, and he darted obliquely from one ſide of the jar to the other.

* I uſe the popular name. This fiſh is very common in every part of England; it is nearly of the ſame ſize and color as the minnow, and is diſtinguiſhed from it by two ſmall bony excreſences at the origin of the belly. It is extremely ſuſceptible.

In twelve minutes, he became ſtill, and moved his gills very ſlowly. In fifteen minutes he appeared dead. After ſixteen minutes he was taken out, but ſhewed no ſigns of life.

B was very quiet for four minutes and half. He then began to move about the jar. In ſeven minutes he had fallen on his back, but ſtill continued to move his gills. In eleven minutes he was motionleſs; taken out after thirteen minutes, he did not recover.

*c*. Of two thornbacks, one, C was introduced into about an ounce of boiled water in contact with hydrogene, ſtanding over mercury. The other, D, was introduced into well boiled water ſaturated with nitrous oxide, and ſtanding in contact with it over mercury. C lived near thirteen minutes, and died without being previouſly much agitated. D was apparently motionleſs, after having the ſame affections as A in the laſt experiment, in ſixteen minutes. At the end of this time he was taken out and introduced into common water. He ſoon began to move his gills, and in leſs than a quarter of an hour was ſo far recovered as to be able to ſwim.

The laſt experiment was repeated on two ſmaller thornbacks; that in the aqueous ſolution of nitrous oxide lived near ſeventeen minutes, that in the water in contact with hydrogene, about fifteen and half.

The experiments in Reſ. I. Div. 3, prove the difficulty, and indeed almoſt impoſſibility of driving from water by boiling, the whole of the atmoſpheric air held in ſolution by it; they likewiſe ſhow that nitrous oxide by its ſtrong affinity for water, is capable of expelling air from that fluid after no more can be procured from it by ebullition.

Hence, if water ſaturated with nitrous oxide had no poſitive effects upon fiſhes; they ought to die in it much ſooner than in water deprived of air by ebullition. From their living in it rather longer;* we may conclude, that they are deſtroyed not by privation of atmoſpheric air, but

* A priori I expected that fiſhes, like amphibious animals would have been very quickly deſtroyed by the action of nitrous oxide.

from ſome poſitive change effected in their blood by the gas.

A long while ago, from obſerving that the gills of fiſh became rather of a lighter red during their death, in the atmoſphere; I conjectured that the diſeaſe of which they died, was probably hyperoxygenation of the blood connected with highly increaſed animal heat. For not only is oxygene preſented to their blood in much larger quantities in atmoſpheric air than in its aqueous ſolution; but likewiſe, to uſe common language, in a ſtate in which it contains much more *latent heat.* Without however laying any ſtreſs on this ſuppoſition, I had the curioſity to try whether thornbacks would live longeſt in atmoſpheric air or nitrous oxide. In one experiment, they appeared to die in them nearly in the ſame time. In another, the fiſh in nitrous oxide lived nearly half as long again as that in atmoſpheric air.

## XI. *Effects of Nitrous Oxide on Inſects.*

The winged inſects furniſhed with breathing holes, become motionleſs in nitrous oxide very ſpeedily ; being however poſſeſſed of a certain voluntary power over reſpiration, they ſometimes recover, after having been expoſed to it for ſome minutes, under the action of atmoſpheric air.

A butterfly was introduced into a ſmall jar, filled with pure nitrous oxide, over mercury. He ſtruggled a little during the firſt two or three ſeconds ; in about ſeven ſeconds, his legs became convulſed, and his wings were wrapt round his body ; in about half a minute he was ſenſeleſs ; taken out after ſix minutes, he did not recover.

Another butterfly introduced into hydrogene, became convulſed in about a quarter of a minute, was ſenſeleſs in twenty ſeconds, and taken out after five minutes, did not revive.

A large drone, after being in nitrous oxide for a minute and a quarter, was taken out ſenſeleſs. After being for ſome time expoſed to the atmoſphere, he began to move, and at laſt roſe on his wings. For ſome time, however, he was unable to fly in a ſtraight line ; and often after deſcribing circles in the air, fell to the ground as if giddy.

A large fly, became motionleſs in nitrous oxide after being convulſed, in about half a minute. Another was rendered ſenſeleſs in hydrogene, in leſs than a quarter of a minute.

A fly introduced into hydrocarbonate, dropt immediately ſenſeleſs; taken out after about a quarter of a minute, he recovered; but like the fly that had lived after expoſure to nitrous oxide, was for ſome time vertiginous.

Flies live much longer under water, alcohol, or oil, than in non-reſpirable gaſes, or gaſes incapable of ſupporting life. A certain quantity of air always continues attached in the fluid to the fine hairs ſurrounding their breathing holes, ſufficient to ſupport life for a ſhort time.

Snails and earth-worms, live in nitrous oxide a long while, they die in it however, much ſooner than in water or hydrogene; probably from the ſame cauſes as the amphibious animals.

# DIVISION II.

## *Of the CHANGES effected in NITROUS OXIDE, and other GASES, by the RESPIRATION of ANIMALS.*

### I. *Preliminaries.*

As ſoon as I had diſcovered that nitrous oxide was reſpirable, and poſſeſſed of extraordinary powers of action on living beings, I was anxious to be acquainted with the changes effected in it by the venous blood. To inveſtigate theſe changes, appeared at firſt a ſimple problem; I ſoon however found that it involved much preliminary knowledge of the chemical properties and affinities of nitrous oxide. After I had aſcertained by experiments detailed in the preceding Reſearches, the compoſition of this gas its combinations, and the phyfical changes effected by it in living beings, I began my enquiry relating to the mode of its operation.

Finding that the refidual gas of nitrous oxide after it had been breathed for fome time in filk bags, was chiefly nitrogene, I at firft conjectured that nitrous oxide was decompofed in refpiration in the fame manner as atmofpheric air, and its oxygene only combined with the venous blood; the following experiments foon however convinced me of my error.

II. *Abforption of Nitrous Oxide by venous blood. Changes effected in the blood by different Gafes.*

*a.* Though the laws of the coagulability of the blood are unknown, yet we are certain that at the moment of coagulation, a perfectly new arrangement of its principles takes place; confequently, their powers of combination muft be newly modified. The affinities of living blood can only be aſcertained during its circulation in the veſſels of animals. At the moment of effuſion from thoſe veſſels, it begins to paſs through a ſeries of changes, which firſt produce coagulation, and finally its compleat decompoſition.

Conſequently, the action of fluid blood upon gaſes out of the veſſels, will be more analogous to that of circulating blood in proportion as it is more ſpeedily placed in contact with them.

*b.* To aſcertain the changes effected in nitrous oxide by fluid venous blood.

A jar, ſix inches long and half an inch wide, graduated to ,05 cubic inches, having a tight ſtopper adapted to it, was filled with nitrogene, which is a gas incapable of combining with, and poſſeſſing no power of action upon venous blood. A large orifice was made in the vein of a tolerably healthy man, and the ſtopper removed from the jar, which was brought in contact with the arm ſo as to receive the blood, and preſſed cloſe againſt the ſkin, in ſuch a way as to leave an orifice juſt ſufficient for the eſcape of the nitrogene, as the blood flowed in. When the jar was full, it was closed, and carried to the pneumatic apparatus, the mercury of which had been previously a little warmed. A small quantity of the blood was transferred into another jar to make room for the gas. The remaining quantity equalled exactly two cubic inches; to this was introduced as speedily as possible, eleven measures equal to ,55 cubic inches of nitrous oxide, which left a residuum of $\frac{1}{32}$ only, when absorbed by boiled water, and was consequently, perfectly pure. On agitation, a rapid diminution of the gas took place.

In the mass of blood which was opaque, but little change of color could be perceived; but that portion of it diffused over the sides of the jar, was evidently of a brighter purple than the venous blood.

It was agitated for two or three minutes, and then suffered to rest; in eight minutes it had wholly coagulated; a small quantity of serum had separated, and was diffused over the coagulum. This coagulum was dark; but evidently of a more purple tinge than that of venous blood ; no gas had apparently been liberated during its formation.

The nitrous oxide remaining, was not quite equal to ſeven meaſures; hence, at leaſt four meaſures of it had been abſorbed.

To aſcertain the nature of the reſiduum, it was neceſſary to transfer it into another veſſel , but this I found very difficult to accompliſh, on account of the coagulated blood. By piercing through the coagulum and removing part of it by means of curved iron forceps, I at laſt contrived to introduce about $4\frac{1}{2}$ meaſures of the gas into a ſmall cylinder, graduated to ,25 cubic inches, in which it occupied of courſe, nearly 9 meaſures ; when a little ſolution of ſtrontian was admitted to theſe, it became very ſlightly clouded ; but the abſorption that took place did not more than equal half its bulk. Conſequently, the quantity of carbonic acid evolved from the blood, or formed, muſt have been extremely minute.

On the introduction of pure water, a rapid abſorption of the gas took place, and after agitation, not quite 3 meaſures remained. Theſe did not *perceptibly* diminiſh with nitrous gas; their quantity was too ſmall to be examined by any other teſt; but there is reaſon to ſuppoſe that they were chiefly compoſed of nitrogene.

From this experiment, it appeared that nitrous oxide is abſorbed when placed in contact with venous blood; at the ſame time, that a very minute quantity of carbonic acid and probably nitrogene is produced.

*c*. In another ſimilar experiment when nearly half a cubic inch of nitrous oxide was abſorbed by about a cubic inch and three quarters of fluid blood, the reſidual gas did not equal more than $\frac{1}{8}$, the quantity abſorbed being taken as unity. This fact induced me to ſuppoſe that the abſorption of nitrous oxide by venous blood, was owing to a ſimple ſolution of the gas in that fluid, analogous to its ſolution in water or alcohol.

To aſcertain if nitrous oxide could be expelled from blood impregnated with it, by heat; I introduced to 2 cubic inches of fluid blood taken from the medial vein, about ,6 cubic inches of nitrous oxide. After agitation, in feven minutes nearly ,4 were abforbed. In ten minutes, after the blood had completely coagulated, the cylinder containing it, was transferred in contact with mercury, into a veffel of folution of falt in water ; this folution was heated and made to boil. During its ebullition, the whole of the blood became either white or pale brown, and formed a folid coherent mafs ; whilft fmall globules of gas were given out from it. In a few minutes, about ,25 of gas had collected. After the veffel had cooled, I attempted to transfer this gas into a fmall graduated jar in the mercurial apparatus, but in vain ; the mafs in the jar was fo folid and tough, that I could not remove it. By transferring it to the water apparatus, I fucceeded in difplacing enough of the coagulum to fuffer the water to come in contact with the gas ; an abforption of nearly half of it took place ; hence, *I conjecture,* that nitrous oxide had been given out by the impregnated blood.

*d.* Some fresh dark coagulum of venous blood, was exposed to nitrous oxide. A very slight alteration of color took place at the surface of the blood, perceptible only in a strong light, and a minute quantity of gas was absorbed. A taper burnt in the remaining gas as brilliantly as before, hence, it had apparently suffered no alteration.

*e.* To compare the physical changes effected in the venous blood by nitrous oxide, with those produced by other gases, I made the following experiments.—I filled a large phial, containg near 14 cubic inches, with blood from the vein of the arm of a man, and immediately transferred it to the mercurial apparatus. Different portions of it were thrown into small graduated cylinders, filled with the following gases: nitrogene, nitrous gas, common air, oxygene, nitrous oxide, carbonic acid, and hydrocarbonate.

The blood in each of them was successively agitated till it began to coagulate; and making allowances for the different periods of agitation, there was no marked difference in the times of coagulation.

The color of the coagulum in every part of the cylinder, containing nitrogene, was the ſame very dark red. When it was agitated ſo as to tinge the ſides of the jar, it appeared exactly of the color of venous blood received between two ſurfaces of glaſs; no perceptible abſorption of the gas had taken place.

The blood in nitrous gas was dark, and much more purple on the top than that in nitrogene. When agitated ſo as to adhere to the jar as a thin ſurface, this purple was evidently deep and bright. An abſorption of rather more than $\frac{1}{8}$ of the volume of gas had taken place.

The blood in oxygene and atmoſpheric air, were of a much brighter tinge than that in any of the other gaſes. On the top, the color was vermilion, but no perceptible abſorption had taken place.

The coagulum in nitrous oxide, when examined in the maſs was dark, and hardly diſtinguiſhable in its color from venous blood; but when minutely noticed at the ſurface where it was covered with ſerum, and in its diffuſion over the ſides of the jar, it appeared of a fine purple red, a tinge brighter than the blood in nitrous gas. An abſórption had taken place in this cylinder, more conſiderable than in any of the others.

In carbonic acid, the coagulum was of a brown red, much darker than the venous blood, and a ſlight diminution of gas had taken place.

In the hydrocarbonate,* the blood was red, a ſhade darker than the oxygenated blood, and a very ſlight diminution of the gas† was perceptible.

*f.* To human blood that had been ſaturated with nitrous oxide whilſt warm and conſtantly agitated for four or five minutes, to prevent its uniform coagulation, oxygene was introduced; the red purple on the ſurface of it,

* The hydrocarbonate employed, was procured from alcohol, by means of ſulphuric acid. This gas contains more carbon, than hydrocarbonate from water and charcoal.

† The curious fact of the reddening of venous blood by hydrocarbonate, was diſcovered by Dr. Beddoes.

immediately changed to vermilion; and on agitation, this color was diffufed through it. On comparing the tinge with that of oxygenated blood, no perceptible difference could be obferved. No change of volume of the oxygene introduced, had taken place; and confequently, no nitrous oxide had been evolved from the blood.

*g*. Blood, impregnated with nitrous gas, was expofed to oxygene; but after agitation in it for many minutes, no change of its dark purple tinge could be obferved, though a flight diminution of the oxygene appeared to take place.

*h*. Blood that had been rendered vermilion in every part by long agitation in atmofpheric air, the coagulum of which was broken and diffufed with the coagulable lymph through the ferum, was expofed to nitrous oxide; for fome minutes no perceptible change of color took place; but by agitation for two or three hours, it evidently affumed a purple tinge, whilft a a flight abforption of gas took place. It never however, became nearly ſo dark as venous blood that had been expoſed to nitrous oxide.

*i.* Blood, oxygenated in the ſame manner as in the laſt experiment, the coagulum of which had been broken, was expoſed to nitrous gas. The ſurface of it immediately became purple, and by agitation for a few minutes, this color was diffuſed through it. A ſlight diminution of the gas was obſerved. On comparing the tinge with that of venous blood that had been previouſly expoſed to nitrous gas, there was no perceptible difference.

*k.* Blood expoſed to oxygenated muriatic acid is wholly altered in its conſtitution and phyſical properties, as has been often noticed; the coagulum becomes black in ſome parts, and brown and white in others. Venous blood, after agitation in hydrogene or nitrogene, oxygenates when expoſed to the atmoſphere in the ſame manner as ſimple venous blood. I had the curioſity to try whether venous blood expoſed to hydrogene, would retain its power of being oxygenated longer than blood ſaturated with nitrous oxide: for this purpoſe ſome ſimilar black coagulum was agitated for ſometime in two phials, one filled with hydrogene, the other with nitrous oxide. They were then ſuffered to reſt for three days at a temperature from about 56° to 63°. After being opened, no offenſive ſmell was perceived in either of them, the blood in hydrogene was rather darker than at the time of their expoſure, whilſt that in nitrous oxide was of a brighter purple. On being agitated for ſome time in the atmoſphere, the blood in nitrous oxide became red, but not of ſo bright a tinge as oxygenated venous blood. The color of the blood in hydrogene did not at all alter.

*l.* To aſcertain whether impregnation with nitrous oxide accelerated or retarded the putrefaction of the blood; I expoſed venous blood in four phials, the firſt filled with hydrocarbonate, the ſecond with hydrogene; the third with atmoſpheric air, and the fourth with nitrous oxide. Examined after a fortnight, the blood in hydrogene and common air were both black, and ſtunk very much; that in hydrocarbonate was red, and perfectly ſweet; that in nitrous oxide appeared purple and had no diſagreeable ſmell.

In a ſecond experiment, when blood was expoſed for three weeks to hydrocarbonate and nitrous oxide, that in nitrous oxide was darker than before and ſtunk a little; that in hydrocarbonate was ſtill perfectly ſweet. The power of hydrocarbonate to prevent the putrefaction of animal matters, was long ago noticed by Mr. Watt.

*m.* Having accidentally cut one of my fingers ſo as to lay bare a little muſcular fibre, I introduced it whilſt bleeding into a bottle of nitrous oxide; the blood that trickled from the wound evidently became much more purple; but the pain was neither alleviated or increaſed. When however, the finger was taken out of the nitrous oxide and expoſed to the atmoſphere, the wound ſmarted more than it had done before. After it had ceaſed to bleed, I inſerted it through water into a veſſel of nitrous gas; but it did not become more painful than before.

From all these observations, we may conclude,

1st. That when nitrous oxide is agitated in fluid venous blood, a certain portion of the gas is absorbed; whilst the color of the blood changes from dark red to red purple.

2dly. That during the absorption of nitrous oxide by the venous blood, minute portions of nitrogene and carbonic acid are produced, either by evolution from the blood, or from a decomposition of part of the nitrous oxide.

3dly. That venous blood impregnated with nitrous oxide is capable of oxygenation; and vice versa; that oxygenated blood may be combined with nitrous oxide.

When blood separated into coagulum and serum, is exposed to nitrous oxide, it is most probable that the gas is chiefly absorbed by the serum. That nitrous oxide however is capable of acting upon the coagulum, is evident from *d*. In the fluid blood, as we shall see hereafter, nitrous oxide is absorbed by the attractions of the whole compound.

III. *Of the changes effected in Nitrous Oxide by Reſpiration.*

To aſcertain whether the changes effected in nitrous oxide by the circulating blood acting through the moiſt coats of the pulmonary veins of living animals, were highly analogous to thoſe produced in it by fluid venous blood removed from the veſſels, I found extremely difficult.

I have before obſerved, that when animals are made to reſpire nitrous oxide, a certain abſorption of the gas always takes place; but the ſmaller animals, the only ones that can be experimented upon in the mercurial apparatus, die in nitrous oxide ſo ſpeedily and occaſion ſo ſlight a diminution of gas, that I judged it uſeleſs to attempt to analiſe the reſiduum of their reſpiration, which ſupports flame as well as pure nitrous oxide, and is chiefly abſorbable by water.

In the infancy of my reſearches, I often reſpired nitrous oxide in a large glaſs bell, furnifhed with a breathing tube and ftopcock, and poifed in water faturated with the gas.

In two or three experiments in which the nof-trils being clofed after the exhauftion of the lungs, the gas was infpired from the bell and refpired into it, a confiderable diminution was perceived, and by the teft of lime water fome carbonic acid appeared to have been formed; but on account of the abforption of this carbonic acid by the impregnated water, and the liberation of nitrous oxide from it, it was impoffible to determine with the leaft accuracy, the quantities of products after refpiration.

About this time likewife, I often examined the refiduum of nitrous oxide, after it had been refpired in filk bags. In thefe experiments when the gas had been breathed for a long time, a confiderable diminution of it was obferved, and the remainder extinguifhed flame and gave a very flight diminution with nitrous gas. But the great quantity of this remainder as well as other phænomena, convinced me that though the oiled filk was apparently air tight when dry, under ſlight preſſure, yet during the action of reſpiration, the moiſt and warm gas expired, penetrated through it, whilſt common air entered through the wetted ſurface.

To aſcertain accurately, the changes effected in nitrous oxide by reſpiration, I was obliged to make uſe of the large mercurial airholder mentioned in Reſearch I. of the capacity of 200 cubic inches. The upper cylinder of it was accurately balanced ſo as to be conſtantly under the preſſure of the atmoſphere. To an aperture in it, a ſtop cock having a very large orifice was adapted, curved and flattened at its upper extremity, ſo as to form an air-tight mouth-piece.

By accurately cloſing the noſe, and bringing the lips tight on the mouth-piece, after a few trials I was able to breathe oxygene or common air in this machine for two minutes or two minutes and half, without any other uneaſy feeling than that produced by the inclination of the neck and cheſt towards the cylinder. The power of uniformly exhauſting the lungs and fauces to the ſame extent, I did not acquire till after many experiments. At laſt, by preſerving exactly the ſame poſture after exhauſtion of the lungs before the inſpiration of the gas to be experimented upon, and during its compleat expiration, I found that I could always retain nearly the ſame quantity of gas in the bronchial veſſels and fauces; the difference in the volume expired at different times, never amounting to a cubic inch and half.

By connecting the conducting pipe of the mercurial airholder, during the reſpiration of the gas, with a ſmall trough of mercury by means of a curved tube, it became a perfect and excellent breathing machine. For by exerting a certain preſſure on the airholding cylinder, it was eaſy to throw a quantity of gas after every inſpiration or expiration, into tubes filled with mercury ſtanding in the trough. In theſe tubes it could be accurately analiſed, and thus the changes taking place at different periods of the proceſs aſcertained.

Whenever I breathed pure nitrous oxide in the mercurial airholder, after a compleat voluntary exhaustion of my lungs, the pleasurable delirium was very rapidly produced, and being obliged to stoop on the cylinder, the determination of blood to my head from the increased arterial action in less than a minute became so great, as often to deprive me of voluntary power over the muscles of the mouth. Hence, I could never rely on the accuracy of any experiment, in which the gas had been respired for more than three quarters of a minute.

I was able to respire the gas with great accuracy for more than half a minute; it at first, rather increasing than diminishing the power of volition; but even in this short time, very strong sensations were always produced, with sense of fulness about the head, somewhat alarming; a feeling which hardly ever occurs to me when the gas is breathed in the natural posture.

In all the numerous experiments that I made on the respiration of nitrous oxide in this way, a very considerable diminution of gas always took place; and the diminution was generally apparently greater to the eye during the first four or five inspirations.

The refidual gas of an experiment was always examined in the following manner. After being transferred through mercury into a graduated cylinder, a fmall quantity of concentrated folution of cauftic potafh was introduced to it, and fuffered to remain in contact with it for fome hours; the diminution was then noted, and the quantity of gas abforbed by the potafh, judged to be carbonic acid. To the remainder, twice its bulk of pure water was admitted. After agitation and reft for four or five hours, the abforption by this was noticed, and the gas abforbed confidered as nitrous oxide. The refidual unabforbable gas was mingled over water with twice its bulk of nitrous gas; and by this means, its compofition, whether it confifted wholly of nitrogene, or of nitrogene mingled with fmall quantities of oxygene, afcertained.

From a number of experiments made at different times on the refpiration of nitrous oxide, I felect the following as the moft accurate.

E. 1. At temperature 54°, I breathed 102 cubic inches of nitrous oxide, which contained near $\frac{1}{50}$ common air, for about half a minute, ſeven inſpirations and ſeven expirations being made. After every expiration, an evident diminution of gas was perceived ; and when the laſt full expiration was made, it filled a ſpace equal to 62 cubic inches.

Theſe 62 cubic inches analiſed, were found to conſiſt of

| | |
|---|---|
| Carbonic acid .. | 3,2 |
| Nitrous oxide .. | 29,0 |
| Oxygene .... | 4,1 |
| Nitrogene .... | 25,7 |
| | 62,0 |

Hence, accounting for the two cubic inches of common air previouſly mingled with the nitrous oxide, 71 cubic inches had diſappeared in this experiment.

In the laſt reſpirations, the quantity of gas was ſo much diminiſhed, as to prevent the full expanſion of the lungs ; and hence the apparent diminution was very much leſs after the firſt four inſpirations.

E. 2. At temperature 47°, I breathed 182 cubic inches of nitrous oxide, mingled with $2\frac{1}{2}$ cubic inches of atmoſpheric air, which previouſly exiſted in the airholder, for near 40 ſeconds; having in this time made 8 reſpirations. The diminution after the firſt full inſpiration, appeared to a by-ſtander nearly uniform. When the laſt compleat expiration was made, the gas filled a ſpace equal to 128 cubic inches, the common temperature being reſtored. These 128 cubic inches analiſed, were found to conſiſt of

| | | |
|---|---|---|
| Carbonic acid | .... | 5,25 |
| Nitrous oxide | .... | 88,75 |
| Oxygene | ..... | 5,00 |
| Nitrogene | ...... | 29,00 |

Conſequently, in this experiment, 93,25 cubic inches of nitrous oxide had diſappeared.

In each of theſe experiments, the cylinder was covered with condenſed watry vapor exactly in the ſame manner as if common air had been breathed in it. It ought to be obſerved that, E. 1. was made in the morning, four hours and half after a moderate breakfaſt; whereas, E. 2. was made but an hour and quarter after a plentiful dinner; at which near three-fourths of a pint of table-beer had been drank.

From theſe experiments we learn, that nitrous oxide is rapidly abſorbed by the venous blood, through the moiſt coats of the pulmonary veins. But as after a compleat voluntary exhauſtion of the lungs, much reſidual air muſt remain in the bronchial veſſels and fauces, as appears from their incapability of compleatly collapſing, it is evident that the gas expired after every inſpiration of nitrous oxide muſt be mingled with different quantities of the reſidual gas of the lungs;* whilſt after a complete expiration, much of the unabſorbed nitrous oxide muſt remain as reſidual gas in the lungs. Now when a complete expi-

* By lungs, I mean in this place, all the internal organs of reſpiration.

ration is made after the breathing of atmofpheric air, it is evident that the refidual gas of the lungs confifts of nitrogene,* mingled with fmall portions of oxygene and carbonic acid. And thefe are the only products found after the refpiration of nitrous oxide.

To afcertain whether thefe products were partially produced, during the procefs of refpiration, as I was inclined to believe from the experiments in the laft fection, or whether they were wholly the refidual gafes of the lungs, I found extremely difficult.

I at firft thought of breathing nitrous oxide immediately after my lungs had been filled with oxygene; and to compare the products remaining after the full expiration, with thofe produced after a full expiration of pure oxygene; but on the fuppofition that oxygene and nitrous oxide, when applied together to the venous blood, muft effect changes in it different from

* Becaufe thefe products are formed during the refpiration of common air.

either of them ſeparately, the idea was relinquiſhed.

I attempted to inſpire nitrous oxide, after having made two inſpirations and a complete expiration of hydrogene; but in this experiment the effects of the hydrogene were ſo debilitating, and the conſequent ſtimulation by the nitrous oxide ſo great, as to deprive me of ſenſe. After the firſt three inſpirations, I loſt all power of ſtanding, and fell on my back, carrying in my lips the mouth-piece ſeparated from the cylinder, to the great alarm of Mr. Patrick Dwyer, who was noting the periods of inſpiration.

Though experiments on ſucceſſive inſpirations of pure nitrous oxide might go far to determine whether or no any nitrogene, carbonic acid and oxygene were products of reſpiration, yet I diſtinctly ſaw that it was impoſſible in this way to aſcertain their quantities, ſuppoſing them produced, unleſs I could firſt determine the capacity of my lungs; and the different proportions of the gaſes remaining in the bronchial veſſels after a compleat expiration, when atmoſpheric air had been reſpired.

In ſome experiments (that I made on the reſpiration of hydrogene, with a view to determine whether carbonic acid was *produced* by the combination of carbon looſely combined in the venous blood, with the oxygene reſpired, or whether it was ſimply *given out* as excrementitious by this blood) I found, without however being able to ſolve the problem I had propoſed to myſelf, that in the reſpiration of pure hydrogene, little or no alteration of volume took place; and that the reſidual gas was mingled with ſome nitrogene, and a little oxygene and carbonic acid.

From the compariſon of theſe facts with thoſe noticed in the laſt ſection and in R. III. Div. I. there was every reaſon to ſuppoſe that hydrogene was not abſorbed or altered when reſpired; but only mingled with the reſidual gaſes of the lungs. Hence, by making a full expiration of atmoſpheric air, and afterwards taking ſix or ſeven reſpirations of hydrogene in the mercurial airholder, and then making a compleat expiration, I conjectured that the residual gas and the hydrogene would be so mingled, as that nearly the same proportions should remain in the bronchial vessels, as in the airholder. By ascertaining these proportions and calculating from them, I hoped to be able to ascertain with tolerable exactness, the capacity of my fauces and bronchia, as well as the composition of the gas remaining in them, after a complete expiration of common air.

## IV. *Respiration of Hydrogene.*

The hydrogene that I employed, was procured from the decomposition of water by means of clean iron filings and diluted sulphuric and muriatic acids. It was breathed in the same manner as nitrous oxide, in the large mercurial airholder.

After a compleat voluntary exhaustion of my lungs in the usual posture, I found great difficulty in breathing hydrogene for so long as half a minute, fo as to make a compleat expiration of it. It produced uneafy feelings in the cheft, momentary lofs of mufcular power, and fometimes a tranfient giddinefs.

In fome of the experiments that I made; on account of the giddinefs, the refults were rendered inconclufive, by my removing my mouth from the mouth-piece after expiration, before the affiftant could turn the ftopcock.

The purity of the hydrogene was afcertained immediately before the experiment by the teft of nitrous gas, and by detonation with oxygene or atmofpheric air; generally 12 meafures of atmofpheric air were fired with 4 of the hydrogene, and if the diminution was to ten or a little more, the gas was judged to be pure.

After the experiment, when the compleat expiration had been made and the common temperature reftored; the volume of the gas was noticed, and then a fmall quantity of it thrown into the mercurial apparatus by means of the conducting tube, to be examined. The carbonic acid was feparated by from it by means of folution of potafh or ftrontian; the quantity of oxygene it contained, was afcertained by means of nitrous gas of known compofition; the fuperabundant nitrous gas was abforbed by folution of muriate of iron; and the proportions of hydrogene and nitrogene in the remaining gas, difcovered by inflammation with atmofpheric air or oxygene in the detonating tube by the electric fpark.

*a.* The two following experiments made upon quantities of hydrogene, equal to thofe of the nitrous oxide refpired in the experiments in the laft fection, are given as the moft accurate of five.

E. 1. I refpired at 59° 102 cubic inches of hydrogene apparently pure, for rather lefs than half a minute, making in this time feven quick refpirations.

After the complete expiration, when the common temperature was reftored, the gas occupied a fpace equal to 103 cubic inches nearly. Thefe analifed were found to confift of

| | | |
|---|---|---|
| Carbonic acid | . . | 4,0 |
| Oxygene | . . . . . . | 3,7 |
| Nitrogene | . . . . | 17,3 |
| Hydrogene | . . . . | 78,0 |
| | | 103,0 |

Now as in this experiment, the gas was increafed in bulk only a cubic inch ; fuppofing that after the compleat expiration the gas in the lungs, bronchia and fauces was of nearly fimilar compofition with that in the airholder, and that no hydrogene had been abforbed by the blood, it would follow that 24 cubic inches of hydrogene remained in the internal organs of refpiration, and confequently, by the rule of proportion, about 7,8 of the mixed refidual gas of the common air. And then the whole quantity of refidual gas of the lungs, fuppofing the temperature 59°, would have been 31,8 cubic inches ; but as its temperature was nearly that of the internal parts of the body, 98°, it muft have filled a greater fpace; calculating from the experiments of Guyton and Vernois,* about 37,5† cubic inches.

From the increaſe of volume, it would appear that a minute quantity of gas had been generated during the reſpiration, and this was, as we ſhall ſee hereafter, moſt probably carbonic acid.§ Likewiſe there is reaſon to ſuppoſe, that a little of the reſidual oxygene muſt have been abſorbed. Making allowances for thoſe circumſtances, it would follow, that the 37,5 cubic inches of gas remaining in my lungs, after a compleat expiration of atmoſpheric air at animal heat 98°, equal to 31,8 cubic inches at 59°, were compoſed of

| | |
|---|---|
| Nitrogene .... | 21,9 |
| Carbonic acid .. | 4,9 |
| Oxygene .... | 5,0 |
| | 31,8 |

* Annales de Chimie, vol. 1, page 279.

† This is only an imperfect approximation; the ratio of the increaſe of expanſibility of gaſes to the increaſe of temperature, has not yet been aſcertained. It is probable that the expanſibility of gaſes is altered by their mixture.

§ For there is no reaſon to ſuppoſe the production of nitrogene.

E. 2. I refpired for near a half a minute in the mercurial airholder at 61°, 182 cubic inches of hydrogene; having made during this time, fix long infpirations. After the laft expiration, the gas filled a fpace nearly equal to 184 cubic inches, and analifed, was found to confift of

| | |
|---|---|
| Carbonic acid .... | 4,8 |
| Oxygene ...... | 4,6 |
| Nitrogene ...... | 21,0 |
| Hydrogene ...... | 153,6 |
| | 184. |

Now in this experiment, reafoning in the fame manner as before, 28,4 cubic inches of hydrogene muft have remained in the lungs, and likewife 5,5 of the atmofpheric refidual gas. Confequently, the whole refidual gas was nearly equal to 34 cubic inches at 61°, which at 98° would become about 40,4 cubic inches. And reafoning as before, it would appear from this experiment, that the quantity of gas remaining in my lungs after a compleat voluntary refpiration, equalled at 98, about 40 cubic inches, and at 61°, 34 nearly: making the neceſſary corrections; that after common air had been breathed, theſe 34 cubic inches conſiſted of

| | |
|---|---|
| Carbonic acid .... | 4,1 |
| Oxygene ........ | 5,5 |
| Nitrogene ...... | 24,4 |

*b.* It would have been poſſible to prove the truth of the poſtulate on which the experiments were founded, by reſpiring common air or oxygene after the compleat expiration of the hydrogene, for the ſame time as the hydrogene was reſpired and in equal quantities.

For if portions of hydrogene were found in the airholder equal to thoſe of the reſidual gaſes in the two experiments, it would prove that a *uniform* mixture of reſidual gas with the gas inſpired, was produced by the reſpiration. That this mixture muſt have taken place, appeared, however, ſo evident from analogous facts, that I judged the experimental proof unneceſſary.

Indeed, as moſt gaſes, though of different ſpecific gravities, when brought in contact with each other, aſſume ſome ſort of union, it is more than probable, that gas inſpired into the lungs, from being placed in contact with the reſidual gas on ſuch an extenſive ſurface, muſt inſtantly mingle with it. Hence, poſſibly one deep inſpiration and compleat expiration of the whole of a quantity of hydrogene, will be ſufficient to determine the capacity of the lungs after compleat voluntary exhauſtion, and the nature of the reſidual air.

That two inſpirations are ſufficient, appears probable from the following experiment.

E. 3. After a compleat voluntary expiration of common air, I made two deep inſpirations of 141 cubic inches of hydrogene. After the compleat expiration, they filled a ſpace equal to rather more than 142 cubic inches, and analiſed, were found to conſiſt of

| | |
|---|---|
| Carbonic acid .... | 3,1 |
| Oxygene ...... | 4,5 |
| Nitrogene ...... | 18,8 |
| Hydrogene .. ... | 115,6 |
| | 142. |

Now calculating on the exhaufted capacity of my lungs from this experiment, fuppofing uniform mixture, they would contain after expiration of common air, about 30,7 cubic inches at 58°, equal to 36 at 98°, compofed of about

| | |
|---|---|
| Nitrogene .... | 20,9 |
| Oxygene .... | 5,8 |
| Carbonic acid .. | 4,0 |
| | 30,7 |

One fhould fuppofe a priori that in this experiment much lefs of the refidual oxygene of the lungs muft have been abforbed, than in Expts. 1 and 2; yet there is no very marked difference in the portions evolved. That a tolerably accurate mixture took place, appears from the quantity of nitrogene. The fmaller quantity of carbonic acid is an evidence in favour of its evolution from the venous blood.

*c*. It is reafonable to fuppofe that the preffure upon the refidual gas of the exhaufted lungs, muft be nearly equal to that of the atmofphere. But as aqueous vapour is perpetually given out by the exhalents, and perhaps evolved from the moist coats of the pulmonary vessels, it is likely that the residual gas is not only fully saturated with moisture at 98°, but likewise impregnated with uncombined vapor; and hence its volume enlarged beyond the increment of expansion of temperature.

Considering all these circumstances, and calculating from the mean of the three experiments on the composition of the residual gas, I concluded,

1st. That the exhausted capacity of my lungs was equal to about 41 cubic inches.

2dly. That the gas contained in my bronchial vessels and fauces, after a compleat respiration of atmospheric air, was equal to about 32 cubic inches, its temperature being reduced to 55°.

3dly. That these 32 cubic inches were composed of about

| | | |
|---|---|---|
| Nitrogene | .. | 23,0 |
| Carbonic acid | .. | 4,1 |
| Oxygene | ... | 4,9 |

*d.* In many experiments made in the mercurial airholder on the capacity of my lungs under different circumſtances, I found that I threw out of my lungs by a full forced expiration at temperatures from 58° to 62°

| | cub. in. | | cub. in. |
|---|---|---|---|
| After a full voluntary inſpiration, from | 189 | to | 191 |
| After a natural inſpiration, from .. | 78 | to | 79 |
| After a natural expiration, from .. | 67 | to | 68 |

So that making the corrections for temperature, it would appear, that my lungs in a ſtate of voluntary inſpiration, contained about 254 cubic inches; in a ſtate of natural inſpiration about 135; in a ſtate of natural expiration, about 118; and in a ſtate of forced expiration 41.*

As the exhauſted capacity as well as impleted capacity of the internal organs of reſpiration muſt be different in different individuals, according as the forms and ſize of their thorax,

* This capacity is moſt probably below the medium, my cheſt is narrow, meaſuring in circumference, but 29 inches, and my neck rather long and ſlender.

fauces, and bronchia are different, it would be almoſt uſeleſs to endeavour to aſcertain a ſtandard capacity. It is however probable, that a ratio exiſts between the quantities of air inſpired in the natural and forced inſpiration, thoſe expired in the natural and forced expiration, and the whole capacity of the lungs. If this ratio were aſcertained, a ſingle experiment on the natural inſpiration and expiration of common air, would enable us to aſcertain the quantity of reſidual gas in the lungs of any individual after a compleat forced expiration.*

V. *Additional obſervations and experiments on the Reſpiration of Nitrous Oxide.*

*a.* Having thus aſcertained the capacity of my lungs, and the compoſition of the reſidual gas of expiration, I proceeded to reaſon concerning

* Dr. Goodwyn in his excellent work on the connexion of life with reſpiration, has detailed ſome experiments on the capacity of the lungs after natural expiration. He makes the medium capacity about 109 cubic inches, which agrees very well with my eſtimation.—page 27.

the experiments in fection III, on the refpiration of nitrous oxide.

In Exp. I. nearly 100 cubic inches of nitrous oxide, making the corrections on account of the common air, were refpired for half a minute. In this time, they were reduced to 62 cubic inches, which confifted of 3,2 carbonic acid, 29 nitrous oxide, 4,1 oxygene, and 25,7 nitrogene.

But, as appears from the laft fection, there exifted in the lungs before the infpiration of the nitrous oxide, about 32 cubic inches of gas, confifting of 23 nitrogene, 4,1 carbonic acid, and 4,9 oxygene, temperature being reduced to 59°. This gas muft have been perfectly mingled with the nitrous oxide during the experiment; and confequently, the refidual gas in the lungs after the experiment, was of the fame compofition as that in the airholder.

Suppofing it as before, to be about 32 cubic inches: from the rule of proportion, they will be compofed of

| | |
|---|---|
| Nitrous oxide .. | 14,7 |
| Nitrogene ..... | 13,3 |

| | | |
|---|---|---|
| Carbonic acid | .. | 1,9 |
| Oxygene | ...... | 2,1 |

And the whole quantity of gas in the lungs and the airholder, fuppofing the temperature 59°, will equal 94 cubic inches, which are compofed of

| | | |
|---|---|---|
| Nitrous oxide | .. | 43,7 |
| Nitrogene | ...... | 39,0 |
| Carbonic acid | .. | 5, 2 |
| Oxygene | ...... | 6, 1 |
| | | 94 |

But before the experiment, the gas in the lungs and airholder equalled 134 cubic inches, and thefe, reckoning for the common air, were compofed of

| | | |
|---|---|---|
| Nitrous oxide | .. | 100 |
| Nitrogene, | .... | 24,3 |
| Carbonic acid | .. | 4,1 |
| Oxygene | .... | 5,6 |

Hence, it appears, that 56,3 cubic inches of nitrous oxide were abforbed in this experiment, and 13,7 of nitrogene produced, either by evolution from the blood, or decompofition of the nitrous oxide. The quantities of carbonic acid and oxygene approach ſo near to thoſe exiſting after the reſpiration of hydrogene, that there is every reaſon to believe that no portion of them was produced in conſequence of the abſorption, or decompoſition of the nitrous oxide.

*b*. In Exp. 2, calculating in the ſame manner, before the firſt inſpiration, a quantity of gas equal to 216,5 cubic inches at 47°, exiſted in the lungs and airholder, and theſe 216,5 cubic inches were compoſed of

| | |
|---|---|
| Nitrous oxide, .... | 182,0 |
| Nitrogene ...... | 24,9 |
| Carbonic acid .... | 4,1 |
| Oxygene ....... | 5,5 |
| | 216,5 |

After the compleat expiration, 160 cubic inches remained in the lungs and airholder, which was compoſed of

| | |
|---|---|
| Nitrous oxide .. | 110,6 |
| Nitrogene ...... | 36,3 |
| Carbonic acid .... | 6,8 |
| Oxygene ........ | 6,3 |

Hence, it appears, that 71,4 cubic inches of nitrous oxide were abſorbed in this experiment, and about 12 of nitrogene produced. The quantity of carbonic acid and oxygene is rather greater than that which exiſted in the experiments on hydrogene.

*c*. From theſe eſtimations, I learned that a ſmall quantity of nitrogene was produced during the abſorption of nitrous oxide in reſpiration. It remained to determine, whether this nitrogene owed its production to evolution from the blood, or to the decompoſition of a portion of the nitrous oxide.

Analogical evidences were not in favour of the hypotheſis of decompoſition. It was difficult to ſuppoſe that a body requiring the temperature of ignition for its decompoſition by the moſt inflammable bodies, ſhould be partially abſorbed and partially decompounded at 98°, by a fluid apparently poſſeſſed of uniform attractions.

It was more eaſy to believe, that from the immenſe quantity of nitrogene taken into the blood in nitrous oxide; the ſyſtem ſoon became overcharged with this principle, which not being wholly expended in new combinations during living action, was liberated in the aëriform ſtate by the exhalents, or through the moiſt coats of the veins.

Now if the laſt rationale were true, it would follow, that the quantity of nitrogene produced in reſpiration, ought to be increaſed in proportion as a greater quantity of nitrous oxide entered into combination with the blood.

*d.* To aſcertain whether this was the caſe, I made after full voluntary exhauſtion of my lungs, one full voluntary inſpiration and expiration of 108 cubic inches of nitrous oxide. After this, it filled a ſpace nearly equal to 99 cubic inches. The quantities of carbonic acid and oxygene in theſe were not determined; but by the teſt of abſorption by water, they appeared to contain only 18 nitrogene; which is very little more than ſhould have been given from the reſidual gas of the lungs.

In a ſecond experiment, I made two reſpirations of 108 cubic inches of nitrous oi de.

nearly pure. The diminution was to 95. On analyſing theſe 95, I found to my great ſurpriſe, that they contained only 17 nitrogene. Hence, I could not but ſuſpect ſome ſource of error in the proceſs.

I now introduced into a ſtrong new ſilk bag, the ſides of which were in perfect contact, about 8 quarts of nitrous oxide. From the mode of introduction, this nitrous oxide muſt have been mingled with a little common air, not however ſufficient to diſturb the reſults.

I then adapted a cork cemented to a long curved tube to my right noſtril; the tube was made to communicate with the water apparatus; and the left noſtril being accurately cloſed, and the mouth-piece of the ſilk bag tightly adapted to the lips, I made a full expiration of the common air of my lungs, inſpired nitrous oxide from the bag, and by carefully cloſing the mouth-piece with my tongue, expired it through the curved tube into the water apparatus. In this way, I made nine reſpirations of nitrous oxide. The expired gas of the firſt reſpiration was not preferved ; but part of the gas of the fecond, third, fifth, feventh and ninth, were caught in feperate graduated cylinders. The fecond, analifed by abforption, confifted of about 29 abforbable gas, which muft have been chiefly nitrous oxide ; and 17 unabforbable gas, which muft have been chiefly nitrogene ; and the third of 22 abforbable gas, and 8 unabforbable. The fifth was compofed of 27 to 6 ; the feventh of 23 to 7, and the ninth of 26 to 11.

*e.* Though the refults of thefe experiments were not fo conclufive as could be wifhed ; yet, comparing them with thofe of the experiments in fection III. it feemed reafonable to conclude, that the production of nitrogene was increafed, in proportion as the blood became more fully impregnated with nitrous oxide.

From this conclufion, compared with the phænomenon noticed in fection 2, and in Div. I. fection 4, I am induced to believe that the production of nitrogene during the refpiration of nitrous oxide, is not owing to the decompofition of part of the nitrous oxide, in the aëriform ſtate *immediately* by the attraction of the red particles of venous blood for its oxygene; but that it is rather owing to a new arrangement produced in the principles of the impregnated blood, during circulation; from which, becoming ſuperſaturated with nitrogene, it gives it out through the moiſt coats of the veſſels.

For if any portion of nitrous oxide were decompoſed immediately by the red particles of the blood, one ſhould conjecture, that the quantity of nitrogene produced, ought to be greater during the firſt inſpirations, before theſe particles became fully combined with condenſed oxygene. If on the contrary, the whole of the nitrogene and oxygene of the nitrous oxide were both combined with the blood, and carried through the pulmonary veins and left chamber of the heart to the arteries; then, ſuppoſing the oxygene chiefly expended in living action, whilſt the nitrogene was only partially conſumed in new combinations, it would follow, that the venous blood of animals made to breathe nitrous oxide, hyperſaturated with nitrogene, muſt be different from common venous blood ; and this we have reaſon to believe from the phænomena in Div. I. ſection 4, is actually the caſe.

*f*. Beſides the nitrogene generated during the reſpiration of nitrous oxide, we have noticed the evolution of other products, carbonic acid,* and water.

Now as nearly equal quantities of carbonic acid are produced, whether hydrogene or nitrous oxide is reſpired, provided the proceſs is carried on for the ſame time ; there is every reaſon to believe, as we have ſaid before, that no part of the carbonic acid produced, is generated from the immediate decompoſition of nitrous oxide by carbon exiſting in the blood.

Conſequently, in theſe experiments, it muſt be either evolved from the venous blood ; or formed, by the ſlow combination of the oxygene of the reſidual air of reſpiration with the charcoal of the blood.

* The oxygene as we have before noticed, moſt probably wholly exiſted in the reſidual gas.

But if it was produced by the decompoſition of reſidual atmoſpheric air, it would follow, that its volume muſt be much leſs than that of the oxygene of the reſidual air, which had diſappeared; for ſome of this oxygene muſt have been *abſorbed* by the blood, and during the converſion of oxygene into carbonic acid by charcoal, a ſlight diminution of volume is produced.

In the experiments when nitrous oxide and hydrogene were reſpired for about half a minute, the medium quantity of carbonic acid produced, was 5,6 cubic inches nearly.

Now we will aſſume, that the quantity of carbonic acid produced, is in the ratio of the oxygene diminiſhed; and there is every reaſon to believe, that in the expiration of atmoſpheric air, the expired air and the reſidual air are nearly of the ſame compoſition.

Hence, no more carbonic acid can remain in the lungs or be produced from the reſidual gas after the compleat expiration of common air, than that which can be generated from a volume of atmofpheric air equal to the refidual gas of the lungs.

The refidual gas of the lungs, after compleat expiration, equals at 55°, 32 cubic inches, and 32 cubic inches of common air contain 8.6 cubic inches of oxygene.

But in the experiments on the refpiration of hydrogene, not only 5.6 cubic inches of carbonic acid were produced, but more than 4 of refidual oxygene remained unabforbed.

Hence it appears impoffible that all the carbonic acid evolved from the lungs during the refpiration of nitrous oxide or hydrogene could have been produced by the combination of charcoal in the venous blood with refidual atmofpheric oxygene: there is confequently every reafon to believe that it is wholly or partially liberated from the venous blood through the moift coats of the veffels.

*g.* The water carried out of the lungs in folution by the expired gas of nitrous oxide, could neither have been wholly or partially formed by the decompofition of nitrous oxide. The coats of the veſſels in the lungs, and indeed in the whole internal ſurface of the body, are always covered with moiſture, and the ſolution of part of this moiſture by the inſpired heated gas, and its depoſition by the expired gas, are ſufficient cauſes for the appearance of the phænomenon.

There are no reaſons for ſuppoſing that any of the reſidual atmoſpheric oxygene is immediately combined with fixed or naſcent hydrogene, or hydrocarbonate, in the venous blood at 98°, by ſlow combuſtion, and conſequently none for ſuppoſing that water is immediately formed in reſpiration.

The evolution of water from the veſſels in the lungs, is almoſt certain from numerous analogies.

*b*. As from the experiments in ſection II. it appeared that nitrous oxide was capable of being combined with oxygenated blood, and vice verſa, blood impregnated with nitrous oxide capable of oxygenation; I was curious to aſcertain what changes would be effected in nitrous oxide when it was respired, mingled with atmospheric air or oxygene. For this purpose, without making a very delicate experiment, I breathed in the large mercurial airholder about 112 cubic inches of nitrous oxide, mingled with 44 of common air, for near half a minute, in the usual mode. The gas, after expiration, filled a space nearly equal to 119. I did not exactly ascertain the composition of the residual gas; it supported flame rather better than common air, and after the nitrous oxide was absorbed, gave much less diminution with nitrous gas than atmospheric air.

*i*. I breathed a mixture of four quarts of nitrous oxide with three quarts of hydrogene, in a dry silk bag, for near a minute; an evident diminution was produced; but on account of the mode of experimenting it was impossible to determine the quantity of nitrous oxide absorbed, or the exact nature of the products. When a taper was introduced into a little of the residual gas, it inflamed with a very feeble explosion. Now a mixture of 4 parts nitrous oxide and 3 hydrogene, detonates when inflamed with very great violence.

*k* Nitrous oxide can be respired without danger by the human animal for a much longer time than that required for the death of the smaller quadrupeds in it.

I have breathed it two or three times in a considerable state of purity, in a dry silk bag, for four minutes and quarter and four minutes and half: some diseased individuals have respired it for upwards of five minutes.

In the infancy of my experiments, from general appearances, I thought that the proportion of nitrous oxide absorbed in respiration was greater in the first inspirations than the last; but this I have since found to be a mistake. In the last respirations the apparent absorption is indeed less; but this is on account of the increased evolution of nitrogene from the blood. When nitrous oxide is respired for a long time, the last inspirations are always fuller and quicker than the first; but the consumption by the same individual is nearly in the ratio of the time of respiration. Three quarts i. e. about 174 cubic inches, are confumed fo as to be unfit for refpiration, by an healthy individual with lungs of moderate capacity, in about a minute and quarter; fix quarts, or 348 cubic inches, laft generally for two minutes and half or two minutes and three quarters; eight quarts, or 464 cubic inches, for more than three minutes and half; and twelve, or 696 cubic inches, for nearly five.

The quantities of nitrous oxide abforbed by the fame individual, will, as there is every reafon to fuppofe, be different under different circumftances, and will probably be governed in fome meafure by the ftate of the health. It is reafonable to fuppofe, that the velocity of the circulation muft have a confiderable influence on the abforption of nitrous oxide; probably in proportion as it is greater a larger quantity of gas will be confumed in equal times.

I am inclined from two or three experiments, to believe that nitrous oxide is abforbed more rapidly after hearty meals or during ftimulation from wine or fpirits, than at other times. As its abſorption appears to depend on a ſimple ſolution in the venous blood ; probably diminution of temperature will increaſe its capability of being abſorbed.

*l.* The quantities of nitrous oxide abſorbed by different individuals, will probably be governed in ſome meaſure by the ſize of their lungs and the ſurface of the blood veſſels, all other circumſtances being the ſame.

From the obſervations that I have been able to make on the abſorption of nitrous oxide, as compared with the capacity of the lungs, the range of the conſumption of different individuals does not extend to more than a pint, or 30 cubic inches at the maximum doſe.

We may therefore conclude, that the medium conſumption of nitrous oxide by the reſpiration of different individuals, is not far from two cubic inches, or about a grain every ſecond, or 120 cubic inches, or 60 grains every minute.

*m.* When nitrous oxide is breathed in tight ſilk bags, towards the end of the experiment as the internal ſurface becomes moiſt, as I have before mentioned, a certain quantity of common air penetrates through it and becomes mixed with the refidual gas of the experiment; but this quantity is always too fmall to deftroy any of the effects of the nitrous oxide. The refidual gas of the common air, the nitrogene and carbonic acid produced in the procefs, and the refiduum of the admitted atmofpheric air, hardly ever amount after the experiment, to one half of the volume of the nitrous oxide abforbed. There is confequently, a perfect propriety in fucceffively infpiring and expiring the whole of a given quantity of nitrous oxide, till it is nearly confumed. In the refpiration of nitrous oxide as the gas is abforbed and not decompofed, little will be gained in effect, by perpetually infpiring and expiring new portions, whilft an immenfe quantity of gas will be idly wafted, and this circumftance, confidering the expence of the fubftance, is of importance.

## VI. *On the respiration of Atmospheric Air.*

Having thus ascertained the absorption of nitrous oxide in respiration, and the evolution of nitrogene and carbonic acid from the lungs during its absorption: considering atmospheric air as a compound in which principles identical with those in nitrous oxide existed, though in different quantities and looser combination, I was anxious to compare the changes effected in this gas by respiration, with those produced in nitrous oxide and oxygene; particularly as they are connected with the health and life of animals.

The ingenious experiments of Lavoisier and Goodwyn, prove the consumption of oxygene in respiration, and the production of carbonic acid. From many experiments on the respiration of common air, Dr. Priestley suspected that a certain portion of nitrogene, as well as oxygene, was absorbed by the venous blood.

*b.* In the following experiments on the refpiration of atmofpheric air in the mercurial air-holder; the compofition of the gas before infpiration and after expiration, was afcertained in the following manner.

Forty meafures of it were agitated over mercury in folution of cauftic potafh, and fuffered to remain in contact with it for two or three hours. The diminution was noted, and the gas abforbed judged to be carbonic acid. Twenty meafures of the gas, freed from carbonic acid, were mingled with thirty of nitrous gas, in a tube of ,5 inches diameter; they were not agitated,* but fuffered to reft for an hour or an hour and half, when the volume occupied by them was noticed: and 50 — *m* the volume occupied, divided by 3 confidered as the oxygene *x*, and 20 — *x* confidered as the nitrogene.

* When they are agitated, a greater proportion of nitrous gas is abforbed, condenfed in the nitric acid by the water; and to find the oxygene, $x = \frac{50 - m}{3,4}$ or $\frac{50 - m}{3,5}$

*c.* To aſcertain the changes effected in atmoſpheric air by ſingle inſpirations,

I made, after a compleat voluntary exhauſtion of my lungs, at temperature 61°, one inſpiration and expiration of 141 cubic inches of atmoſpheric air. After expiration, they filled a ſpace equal to 139 cubic inches nearly. Theſe 139 cubic inches analiſed were found to conſiſt of

| | |
|---|---|
| Nitrogene .... | 101 |
| Oxygene .... | 32 |
| Carbonic acid .. | 6 |

The 141 cubic inches before inſpiration, were compoſed of 103 nitrogene, 1 carbonic acid and 37 oxygene. The time taken to perform the inſpiration and full expiration, was nearly a quarter of a minute.

I repeated this experiment ſeven or eight times, and the quantity of oxygene abſorbed was generally from 5 to 6 cubic inches, the carbonic acid formed from 5 to 5,5, and the quantity of nitrogene apparently diminiſhed by from 1 to 3 cubic inches.

E. 2. I made, after a voluntary expiration of common air, one inspiration and full expiration of 100 cubic inches of atmospheric air. It was diminished nearly to 98¾ or 99 cubic inches, and analised, was found to consist of

| | |
|---|---|
| Nitrogene . . . . | 71,7 |
| Oxygene . . . . | 22,5 |
| Carbonic acid . . | 4,5 |

This experiment I likewise repeated four or five times, with very little difference of result, and there always seemed to be a small diminution of nitrogene. I made no corrections on account of the residual air of the lungs in these processes, because there was every reason to suppose that it was always of similar composition.

*c.* Before I could ascertain whether similar changes were effected in atmospheric air, by natural inspirations as by forced ones, I was obliged to practise respiration in the mercurial airholder, by suffering the conducting tube to communicate with the atmosphere till I had attained the power of breathing in it naturally, without labor or attention; I then found by a number of experiments, that I took into my lungs at every natural inſpiration, about 13 cubic inches of air, and that I threw out of my lungs at every expiration,* rather leſs than this quantity; about $12\frac{3}{4}$ cubic inches.

The mean compoſition of the 13 cubic inches of air inſpired, was

| | cub. in. |
|---|---|
| Nitrogene . . . | 9,5 |
| Oxygene . . . . | 3,4 |
| Carbonic acid | 0,1 |

That of the 12,7 of air expired

| | |
|---|---|
| Nitrogene . . | 9,3 |
| Oxygene . . | 2,2 |
| Carbonic acid | 1,2 |

Theſe reſults I gained from more than 20 experiments, ſo that I could not poſſibly entertain any doubt of this accuracy.

I found, by making a perſon obſerve my reſpirations when I was inattentive to the proceſs,

* The diminution of air by ſingle inſpirations, was particularly noticed by Dr. Goodwyn.

that I made about 26 or 27 natural inſpirations in a minute. So that calculating from the above eſtimations, it would follow, that 31,6 cubic inches of oxygene were conſumed, and 5,2 inches of nitrogene loſt in reſpiration every minute, whilſt 26,6 cubic inches of carbonic acid were produced.

To collect the products of a great number of natural expirations ſo as to aſcertain whether their compoſition correſponded with the above accounts, I proceeded in the following manner.

I faſtened my lips tight on the mouth-piece of the exhauſted airholder, and ſuffering my noſtrils to remain open, inſpired naturally through them, throwing the expired air through my mouth into the airholder.

In many experiments, I found that in about a half a minute, I made in this way 14 or 15 expirations. The mean quantity of air collected was 171 cubic inches, and conſiſted of

| | cub. in. |
|---|---|
| Nitrogene .. | 128 |
| Oxygene .. | 29 |
| Carbonic acid .. | 14 |

Comparing theſe reſults with the former ones, we find the mean quantities of air reſpired in equal terms rather leſs; but the proportions of carbonic acid, nitrogene and oxygene in the reſpired air, nearly identical.

*e.* To aſcertain the changes effected in a given quantity of atmoſpheric air by continued reſpirations, I breathed after a compleat expiration, at temperature 63°, 161 cubic inches of air for near a minute, making in this time, 19 deep inſpirations. After the compleat expiration, which was very carefully made, the gas filled a ſpace nearly equal to 152 cubic inches, ſo that 9 cubic inches of gas had diſappeared.

The 152 cubic inches analiſed, were found to conſiſt of

| | cub. in. |
|---|---|
| Nitrogene .. | 111,6 |
| Oxygene .. | 23, |
| Carbonic acid, | 17,4 |

The 161 cubic inches before inſpiration, were compoſed of

| | cub. in. |
|---|---|
| Nitrogene .. | 117.0 |
| Oxygene .. | 42,4 |
| Carbonic acid | 1,6 |

But the refidual gas in the lungs before the experiment, was of different compofition from that remaining in the lungs after the experiment. Making corrections on account of this circumftance, as in fection IV. it appears that about 5,1 of nitrogene were abforbed in refpiration, 23,9 of oxygene confumed, and 12 of carbonic acid produced.

I repeated this experiment three times; in each experiment the diminution after refpiration, was nearly the fame; and the refidual gas making the neceffary allowances, of fimilar compofition. So that fuppofing the exiftence of no fource of error in the experiments from which the quantity and compofition of the refidual gas of the lungs were eftimated in fection IV. the abforption of nitrogene by the venous blood, appears almoft demonftrated.

*f.* To compare the changes effected in atmofpheric air by refpiration of the fmaller quadrupeds, with thofe in the experiments juft detailed, I introduced into a jar of the capacity of 20 cubic inches filled with mercury in the mercurial trough, 15 cubic inches of atmofpheric air which had been deprived of its carbonic acid by long expofure, to folution of potafh.

Temperature being 64°, a healthy fmall moufe was quickly paffed under the mercury into the jar, and fuffered to reft on a very thin bit of cheefe, which was admitted immediately after.

He continued for near 40 minutes without apparently fuffering, occafionally raifing himfelf on his hind legs. At the end of 50 minutes, he was lying on his fide, and in 55 minutes was apparently dying. He was now carefully taken out through the mercury by the tail, and expofed before the fire, where he foon recovered. After the cheefe had been carefully removed, the gas in the jar filled a fpace nearly equal to 14 cubic inches; fo that a diminution of a cubic inch had taken place. These 14 cubic inches analised, were found to consist of

| | cub. in. |
|---|---|
| Carbonic acid .. | 2,0 |
| Oxygene .... | 1,4 |
| Nitrogene ... | 10,6 |

The 15 cubic inches before the experiment, consisted of

| | cub. in. |
|---|---|
| Oxygene .. | 4 |
| Nitrogene .. | 11 |

Hence it appeared, that 2,6 cubic inches of oxygene had been consumed, 2 cubic inches of carbonic acid produced, and about 0,4 of nitrogene lost.

The relation between the quantities of oxygene consumed in this experiment, and the carbonic acid produced, are nearly the same as that of those in the experiments just detailed; but the quantity of nitrogene lost is much smaller.

## VII. *Refpiration of Oxygene.*

The gafes before and after refpiration, were analifed in thefe experiments in the manner defcribed in the laft fection, except that 3 of nitrous gas were always employed to one of oxygene.

E. I. At temperature 53°, after a full forced refpiration, I refpired in the mercurial airholder, for half a minute, 102 cubic inches of oxygene, making feven very long and deep infpirations. After the compleat expiration, the gafes filled a fpace equal to 93 cubic inches; thefe 93 cubic inches analifed, were found to confift of

| | cub. in. |
|---|---|
| Carbonic acid .. | 5,9 |
| Nitrogene .... | 33,8 |
| Oxygene ...... | 53,3 |

The 102 cubic inches before the experiment, were compofed of

| | cub. in. |
|---|---|
| Oxygene .. | 78 |
| Nitrogene .. | 24 |

The refidual gas in the lungs before the experiment, was 32 cubic inches, and compofed of about 23 nitrogene, 4,1 carbonic acid, and 4,9 oxygene, Section IV. The refidual gas after expiration, was compofed of 18,2 oxygene, 2 carbonic acid, and 11,8 nitrogene.

Hence the whole of the gas in the lungs and airholder before infpiration, was 134 cubic inches, compofed of

| | cub. in. |
|---|---|
| Oxygene .. | 82,9 |
| Nitrogene .. | 47,0 |
| Carbonic acid .. | 4,1 |

And after refpiration, 125 cubic inches, confifting of

| | cub. in. |
|---|---|
| Oxygene .... | 71,5 |
| Nitrogene .... | 45,6 |
| Carbonic acid .. | 7,9 |

So that comparing the quantities, it appears, that 11,4 of oxygene and 1,4 of nitrogene, were confumed in this experiment, and 3,8 of carbonic acid produced.

I was much ſurpriſed at the ſmall quantity of oxygene that had been conſumed in this experiment. This quantity was leſs than that expended during the reſpiration of atmoſpheric air for half a minute: the portion of carbonic acid evolved was likewiſe ſmaller. I could detect no ſource of inaccuracy, and it was difficult to ſuppoſe that the greater depth and fulneſs of the inſpirations could make any difference.

E. 2. I now reſpired at the ſame temperature, after a full expiration, 162 cubic inches of gas, compoſed of 133 oxygene and 29 nitrogene for two minutes, imitating as much as poſſible, the natural reſpiration. After the experiment, they filled a ſpace equal to 123 cubic inches. And when the analyſis and calculations had been made as in the laſt experiment, it appeared that 57 cubic inches of oxygene, and 2 of nitrogene had been abſorbed, whilſt 21 cubic inches of carbonic acid had been formed.

Now from the eſtimations in the laſt ſection, it appears that 63 cubic inches of oxygene are confumed, and about 52 cubic inches of carbonic acid produced every two minutes during the natural refpiration of common air. So that fuppofing the experiment accurate, 6 cubic inches of oxygene lefs are abforbed, and 30 cubic inches lefs of carbonic acid produced every minute, when oxygene nearly pure is refpired, than when atmofpheric air is refpired.

Both thefe experiments were made in the morning, at a time when I was in perfect health; fo that there could be apparently no fource of error from accidental circumftances.

The uncommon and unexpected nature of the refults, made me however, very fceptical concerning them; and before I would draw any inferences, I refolved to afcertain the comparative confumption of atmofpheric air and oxygene by the fmaller quadrupeds, for which purpofe, I made the following experiment.

E. 3. Of two ftrong and healthy fmall mice, apparently of the fame breed, and exactly fimilar.

One was introduced into a jar containing 10 cubic inches and half of oxygene, and 3 cubic inches of nitrogene, and made to reſt on a bit of cheeſe.

The other was introduced into a jar containing fifteen cubic inches and half of atmoſpheric air, and made to reſt in the ſame manner on cheeſe.

The mouſe in oxygene began apparently to ſuffer in about half an hour, and occaſionally panted very much; in about an hour he lay down on his ſide as if dying. The jars were often agitated, that the gaſes might be well mingled.

The mouſe in atmoſpheric air became very feeble in 40 minutes, and at the end of 50 minutes was taken out through the mercury alive, but unable to ſtand.

The mouſe in oxygene was taken out in the ſame manner after an hour and quarter, alive, but motionleſs, and breathing very deeply.

The gas in the jars was examined. That in the oxygene jar filled a ſpace exactly equal to

12,7 cubic inches, and analifed, was found to confift of 1,7 carbonic acid, 2,6 of nitrogene, and 8,4 of oxygene. So that abfolutely, 2,1 cubic inches of oxygene and ,4 of nitrogene had been confumed, and 1,7 of carbonic acid produced.

The gas in the atmofpheric air jar was diminifhed nearly to 14,4, and confifted of 2,1 carbonic acid, 1,4 oxygene; and 10,9 nitrogene. So that 2,7 of oxygene and ,5 of nitrogene, had been confumed by the moufe; and 2,1 of carbonic acid produced.

Hence it appears, that the moufe in atmofpheric air confumed nearly one-third more oxygene and produced nearly one-fourth more carbonic acid in refpiration in 55 minutes, than the other in an hour and quarter in oxygene. And if we confider the perpetual diminution of the oxygene of the atmofpheric air; from which at laft it became almoft incapable of fupporting the life of the animal; we may conclude, that the quantity of oxygene confumed by it, had the air been perpetually renovated, would have been much more confiderable.

I defign very fhortly, to repeat thefe experiments, and to make others on the comparative confumption of oxygene and atmofpheric air, by the larger quadrupeds. Whatever may be the refults, I hope to be able to afcertain from them, why pure oxygene is incapable of fupporting life.

VIII. *Obfervations on the changes effected in the blood, by atmofpheric air and oxygene.*

From the experiments of Mr. Cigna and Dr. Prieftley,* it appears that the coagulum of the venous blood becomes florid at its furface when expofed to the atmofphere, though covered and defended from the immediate contact of air by a very thick ftratum of ferum.

* Dr. Prieftley found that it likewife became florid at the furface when covered by milk; but that it underwent little or no alteration of color under water and moft other fluids.—Vol. 3. p. 372.

Hence it is evident, that ſerum is capable of diſſolving either the whole compound atmoſpheric air, or the oxygene of it.

Suppoſing what indeed is moſt probable from numerous analogies, that it diſſolves the whole compound; it would follow, that the coloring of the coagulum of blood under ſerum, depended upon the decompoſition of the atmoſpheric air condenſed in the ſerum, the oxygene† of it combining with the red particles, and the nitrogene either remaining diſſolved in the fluid, or being liberated through it into the atmoſphere.

Now the circulating blood conſiſts of red particles, floating in and diffuſed through ſerum and coagulable lymph.

† There are many analogous decompoſitions. Dr. Prieſtley noticed (and I have often made the obſervation) that green oxide of iron, or the precipitate from pale green ſulphate of iron by cauſtic alkali, became red at the ſurface, when covered by a thick ſtratum of water. In my experiments on the green muriate and ſulphate of iron, I obſerved that part of ſome dark oxide of iron which was at the bottom of a trough of water 9 inches deep, became red at the ſurface nearly in the ſame time as another portion of the ſame precipitation that was expoſed to the atmoſphere. This oxygenation muſt depend upon the decompoſition of atmoſpheric air conſtantly diſſolved by the water.

In natural refpiration, the red particles are rendered of a brighter tinge during the paffage of the blood through the pulmonary veins. And as we have feen in the laft fections, during refpiration atmofpheric air is decompofed; all the oxygene of it confumed, *apparently* a fmall portion of the nitrogene loft, and a confiderable quantity of carbonic acid produced.

It feems therefore reafonable to fuppofe, that the whole compound atmofpheric air paffing through the moift coats of the veffels is firft diffolved by the ferum of the venous blood, and in its condenfed ftate, decompofed by the affinity of the red particles for its oxygene; the greater part of the nitrogene being liberated unaltered; but a minute portion of it poffibly remaining condenfed in the ferum and coagulable lymph, and paffing with them into the left chamber of the heart.

From the experiments on the refpiration of nitrous oxide and hydrogene, it appears that a certain portion of the carbonic acid produced in refpiration, is evolved from the venous blood;

but as a much greater quantity is generated during the reſpiration of common air and oxygene, than during that of hydrogene in equal times, it is not impoſſible but that ſome portion of it may be formed by the combination of charcoal in the red particles with the oxygene diſſolved in the ſerum; but this can only be determined by farther experiments.

Suppoſing that no part of the water evolved in ſolution by the expired gas of common air is formed immediately in reſpiration, it will follow that a very conſiderable quantity of oxygene muſt be conſtantly *combined* with the red particles, even allowing the conſumption of a certain portion of it to form carbonic acid; for the carbonic acid evolved, rarely amounts to more than three-fourths of the volume of the oxygene conſumed.

Perhaps the ſerum of the blood is capable of diſſolving a larger quantity of atmoſpheric air than of pure oxygene. On this ſuppoſition, it would be eaſy to explain the ſmaller conſumption of oxygene in the experiments in the laſt ſection.

## IX. *Observations on the respiration of Nitrous Oxide.*

The experiments in the first Division of this Research, prove that nitrous oxide when respired by animals, produces peculiar changes in their blood and in their organs, first connected with increased living action; but terminating in death.

From the experiments in this Division, it appears, that nitrous oxide is rapidly absorbed by the circulating venous blood, and of course its condensed oxygene and nitrogene distributed in the blood over the whole of the system.

Concerning the changes effected in the principles of the impregnated blood during circulation and its action upon the nervous and muscular fibre; it is useless to reason in the present state of our knowledge.

It would be easy to form theories referring the action of blood impregnated with nitrous oxide, to its power of supplying the nervous and muscular fibre with such proportions of condenſed nitrogene, oxygene and light or etherial fluid, as enabled them more rapidly to paſs through thoſe changes which conſtitute their life: but ſuch theories would be only collections of terms derived from known phænomena and applied by looſe analogies of language to unknown things.

We are unacquainted with the compoſition of dead organiſed matter; and new inſtruments of experiment and new modes of reſearch muſt be found, before we can aſcertain even our capabilities of diſcovering the laws of life.

# RESEARCH IV.

RELATING TO THE

EFFECTS PRODUCED BY THE RESPIRATION

OF

# NITROUS OXIDE

UPON DIFFERENT

# INDIVIDUALS.

# RESEARCH IV.

RELATING TO

THE EFFECTS

PRODUCED BY THE

# RESPIRATION OF NITROUS OXIDE.

## DIVISION I.

*HISTORY of the DISCOVERY.—Effects produced by the RESPIRATION of different GASES.*

A SHORT time after I began the ſtudy of Chemiſtry, in March 1798, my attention was directed to the dephlogiſticated nitrous gas of Prieſtley, by Dr. Mitchill's Theory of Contagion.*

The fallacy of this Theory was ſoon demonſtrated, by a few coarſe experiments made on ſmall quantities of the gas procured from zinc

* Dr. Mitchill attempted to prove from ſome phænomena connected with contagious diſeaſes, that dephlogiſticated nitrous gas which he called oxide of ſepton, was the principle of contagion, and capable of producing the moſt terrible effects when reſpired by animals in the minuteſt quantities or even when applied to the ſkin or muſcular fibre.

and diluted nitrous acid. Wounds were expoſed to its action, the bodies of animals were immerſed in it without injury; and I breathed it mingled in ſmall quantities with common air, without remarkable effects. An inability to procure it in ſufficient quantities, prevented me at this time, from purſuing the experiments to any greater extent. I communicated an account of them to Dr. Beddoes.

In 1799, my ſituation in the Medical Pneumatic Inſtitution, made it my duty to inveſtigate the phyſiological effects of the aëriform fluids, the properties of which preſented a chance of uſeful agency. At this period I recommenced the inveſtigation.

A conſiderable time elapſed before I was able to procure the gas in a ſtate of purity, and my firſt experiments were made on the mixtures of nitrous oxide, nitrogene and nitrous gas, which are produced during metallic ſolutions. In the beginning of March, I prepared a large quantity of impure nitrous oxide from the nitrous ſolution of zinc. Of this I often breathed the quantities of a quart and two quarts generally mingled with more than equal parts of oxygene or common air. In the moſt deciſive of thoſe trials, its effects appeared to be depreſſing, and I imagined that it produced a tendency to fainting: the pulſe was certainly rendered ſlower under its operation.

At this time, Mr. Southey reſpired it in an highly diluted ſtate; it occaſioned a ſlight degree of giddineſs, and conſiderably diminiſhed the quickneſs of his pulſe.

Mr. C. Coates likewiſe reſpired it highly diluted, with ſimilar effects.

In April, I obtained nitrous oxide in a ſtate of purity, and aſcertained many of its chemical properties. Reflections upon theſe properties and upon the former trials, made me reſolve to endeavour to inſpire it in its pure form, for I ſaw no other way in which its reſpirability, or powers could be determined.*

* I did not attempt to experiment upon animals, becauſe they die nearly in equal times in non-reſpirable gaſes, and gaſes incapable of ſupporting life and poſſeſſed of no action on the venous blood.

I was aware of the danger of this experiment. It certainly would never have been made if the hypothesis of Dr. Mitchill had in the least influenced my mind. I thought that the effects might be possibly depressing and painful, but there were many reasons which induced me to believe that a single inspiration of a gas apparently possessing no immediate action on the irritable fibre, could neither destroy or materially injure the powers of life.

On April 11th, I made the first inspiration of pure nitrous oxide; it passed through the bronchia without stimulating the glottis, and produced no uneasy feeling in the lungs.

The result of this experiment, proved that the gas was respirable, and induced me to believe that a farther trial of its effects might be made without danger.

On April 16th, Dr. Kinglake being accidentally present, I breathed three quarts of nitrous oxide from and into a silk bag for more than half a minute, without previously closing my nose or exhausting my lungs.

The firſt inſpirations occaſioned a ſlight degree of giddineſs. This was ſucceeded by an uncommon ſenſe of fulneſs of the head, accompanied with loſs of diſtinct ſenſation and voluntary power, a feeling analogous to that produced in the firſt ſtage of intoxication; but unattended by pleaſurable ſenſation. Dr. Kinglake, who felt my pulſe, informed me that it was rendered quicker and fuller.

This trial did not ſatisfy me with regard to its powers; comparing it with the former ones I was unable to determine whether the operation was ſtimulant or depreſſing.

I communicated the reſult to Dr. Beddoes, and on April the 17th, he was preſent, when the following experiment was made.

Having previouſly cloſed my noſtrils and exhauſted my lungs, I breathed four quarts of nitrous oxide from and into a ſilk bag. The firſt feelings were ſimilar to thoſe produced in the laſt experiment; but in leſs than half a minute, the reſpiration being continued, they diminiſhed gradually, and were ſucceeded by a ſenſation analogous to gentle preſſure on all the muſcles, attended by an highly pleaſurable thrilling, particularly in the cheſt and the extremities. The objects around me became dazzling and my hearing more acute. Towards the laſt inſpirations, the thrilling increaſed, the ſenſe of muſcular power became greater, and at laſt an irreſiſtible propenſity to action was indulged in; I recollect but indiſtinctly what followed; I know that my motions were various and violent.

Theſe effects very ſoon ceaſed after reſpiration. In ten minutes, I had recovered my natural ſtate of mind. The thrilling in the extremities, continued longer than the other ſenſations.*

This experiment was made in the morning; no languor or exhauſtion was conſequent, my feelings throughout the day were as uſual, and I paſſed the night in undiſturbed repoſe.

* Dr. Beddoes has given ſome account of this experiment, in his Notice of ſome obſervations made at the Medical Pneumatic Inſtitution. It was noticed in Mr. Nicholſon's Phil. Journal for May 1799.

The next morning the recollections of the effects of the gas were very indistinct, and had not remarks written immediately after the experiment recalled them to my mind, I should have even doubted of their reality. I was willing indeed to attribute some of the strong emotion to the enthusiasm, which I supposed must have been necessarily connected with the perception of agreeable feelings, when I was prepared to experience painful sensations. Two experiments however, made in the course of this day, with sceptism, convinced me that the effects were solely owing to the specific operation of the gas.

In each of them I breathed five quarts of nitrous oxide for rather a longer time than before. The sensations produced were similar, perhaps not quite so pleasurable; the muscular motions were much less violent.

Having thus ascertained the powers of the gas, I made many experiments to ascertain the length of time for which it might be breathed with safety, its effects on the pulse, and its general effects on the health when often respired.

I found that I could breathe nine quarts of nitrous oxide for three minutes, and twelve quarts for rather more than four. I could never breathe it in any quantity, so long as five minutes. Whenever its operation was carried to the highest extent, the pleasurable thrilling at its height about the middle of the experiment, gradually diminished, the sense of pressure on the muscles was lost; impressions ceased to be perceived; vivid ideas passed rapidly through the mind, and voluntary power was altogether destroyed, so that the mouth-piece generally dropt from my unclosed lips.

Whenever the gas was in a high state of purity, it tasted distinctly sweet to the tongue and palate, and had an agreeable odor. I often thought that it produced a feeling somewhat analogous to taste, in its application to my lungs. In one or two experiments, I perceived a distinct sense of warmth in my chest.

I never felt from it any thing like oppressive reſpiration: my inſpirations became deep in proportion as I breathed it longer; but this phænomenon aroſe from increaſed energy of the muſcles of reſpiration, and from a deſire of increaſing the pleaſurable feelings.

Generally when I breathed from ſix to ſeven quarts, muſcular motions were produced to a certain extent; ſometimes I manifeſted my pleaſure by ſtamping or laughing only; at other times, by dancing round the room and vociferating.

After the reſpiration of ſmall doſes, the exhilaration generally laſted for five or ſix minutes only. In one or two experiments when ten quarts had been breathed for near four minutes, an exhilaration and a ſenſe of ſlight intoxication laſted for two or three hours.

On May 3d. To aſcertain whether the gas would accelerate or retard the progreſs of ſleep, I breathed at about 8 o'clock in the evening, 25 quarts of nitrous oxide, in quantities of ſix at a time, allowing but ſhort intervals between each doſe. The feelings were much leſs pleaſurable than uſual, and during the conſumption of the two laſt doſes, almoſt indifferent ; indeed the gas was breathed rather too ſoon after its production and contained ſome ſuſpended acid vapour which ſtimulated the lungs ſo as to induce coughing.

After the experiments, for the firſt time I was ſomewhat depreſſed and debilitated ; my propenſity to ſleep however, came on at the uſual hour, and as uſual was indulged in, my repoſe was ſound and unbroken.

Between May and July, I habitually breathed the gas, occaſionally three or four times a day for a week together ; at other periods, four or five times a week only.

The doſes were generally from ſix to nine quarts ; their effects appeared undiminiſhed by habit, and were hardly ever exactly ſimilar. Sometimes I had the feelings of intenſe intoxication, attended with but little pleaſure ; at other times, ſublime emotions connected with highly vivid ideas ; my pulſe was generally increaſed in fulneſs, but rarely in velocity.

The general effects of its operation upon my health and ſtate of mind, are extremely difficult of deſcription; nor can I well diſcriminate between its agency and that of other phyſical and moral cauſes.

I ſlept much leſs than uſual, and previous to ſleep, my mind was long occupied by viſible imagery. I had a conſtant deſire of action, a reſtleſſneſs, and an uneaſy feeling about the præcordia analogous to the ſickneſs of hope.

But perhaps theſe phænomena in ſome meaſure depended on the intereſt and labour connected with the experimental inveſtigation relating to the production of nitrous oxide, by which I was at this time inceſſantly occupied.

My appetite was as uſual, and my pulſe not materially altered. Sometimes for an hour after the inſpiration of the gas, I experienced a ſpecies of mental indolence* pleaſing rather than

* Mild phyſical pleaſure is perhaps always deſtructive to action. Almoſt all our powerful voluntary actions, ariſe either from hope, fear, or deſire; and the moſt powerful from deſire, which is an emotion produced by the coaleſcence of hope or ideal pleaſure with phyſical pain.

otherwiſe, and never ending in liſtleſneſs.

During the laſt week in which I breathed it uniformly, I imagined that I had increaſed ſenſibility of touch: my fingers were pained by any thing rough, and the tooth edge produced from ſlighter cauſes than uſual. I was certainly more irritable, and felt more acutely from trifling circumſtances. My bodily ſtrength was rather diminiſhed than increaſed.

At the latter end of July, I left off my habitual courſe of reſpiration; but I continued occaſionally to breathe the gas, either for the ſake of enjoyment, or with a view of aſcertaining its operation under particular circumſtances.

In one inſtance, when I had head-ache from indigeſtion, it was immediately removed by the effects of a large doſe of gas; though it afterwards returned, but with much leſs violence. In a ſecond inſtance, a ſlighter degree of head-ache was wholly removed by two doſes of gas.

The power of the immediate operation of the gas in removing intenſe phyſical pain, I had a very good opportunity of aſcertaining.

In cutting one of the unlucky teeth called dentes ſapientiæ, I experienced an extenſive inflammation of the gum, accompanied with great pain, which equally deſtroyed the power of repoſe, and of conſiſtent action.

On the day when the inflammation was moſt troubleſome, I breathed three large doſes of nitrous oxide. The pain always diminiſhed after the firſt four or five inſpirations; the thrilling came on as uſual, and uneaſineſs was for a few minutes, ſwallowed up in pleaſure. As the former ſtate of mind however returned, the ſtate of organ returned with it; and I once imagined that the pain was more ſevere after the experiment than before.

In Auguſt, I made many experiments with a view of aſcertaining whether any analogy exiſted between the ſenſible effects of the different gaſes which are ſooner or later fatal to life when reſpired, and thoſe of nitrous oxide.

I reſpired four quarts of Hydrogene* nearly

* Pure hydrogene has been often reſpired by different Philoſophers; particularly by Scheele, Fontana, and the adventurous and unfortunate Roſier.

pure produced from zinc and muriatic acid, ſor near a minute, my lungs being previouſly exhauſted and my noſtrils carefully cloſed. The firſt ſix or ſeven inſpirations produced no ſenſations whatever; in half a minute, I perceived a diſagreeable oppreſſion of the cheſt, which obliged me to reſpire very quickly; this oppreſſion gradually increaſed, till at laſt the pain of ſuffocation compelled me to leave off breathing. I felt no giddineſs during or after the experiment; my pulſe was rendered feebler and quicker; and a by-ſtander informed me that towards the laſt, my cheeks became purple.

In a ſecond experiment, when the hydrogene was procured from iron and diluted ſulphuric acid, I was unable to reſpire it for ſo long as three quarters of a minute; a tranſient giddineſs and muſcular debility were produced, the pulſe was rendered very feeble, and the pain of ſuffocation was greater than before.

I breathed three quarts of Nitrogene mingled with a very ſmall portion of carbonic acid, for near a minute. It produced no alteration in my senſations for the firſt twenty ſeconds; then the painful ſenſe of ſuffocation gradually came on, and increaſed rapidly in the laſt quarter of the minute, ſo as to oblige me to deſiſt from the experiment. My pulſe was rendered feebler and quicker. I felt no affection whatever in the head.

Mr. Watt's obſervations on the reſpiration of diluted Hydrocarbonate by men, and Dr. Beddoes's experiments on the deſtruction of animals by pure hydrocarbonate, proved that its effects were highly deleterious.

As it deſtroyed life apparently by rendering the muſcular fibre inirritable without producing any previous excitement, I was anxious to compare its ſenſible effects with thoſe of nitrous oxide, which at this time I believed to deſtroy life by producing the higheſt poſſible excitement, ending in læſion of organiſation.

In the firſt experiment, I breathed for near a minute, three quarts of hydrocarbonate mingled with nearly two quarts of atmoſpheric air.*

* I believe it had never been breathed before by any individual, in a ſtate ſo little diluted.

It produced a flight giddinefs and pain in the head, and a momentary lofs of voluntary power: my pulfe was rendered much quicker and feebler. Thefe effects however, went off in five minutes, and I had no return of giddinefs.

Emboldened by this trial, in which the feelings were not unlike thofe I experienced in the firft experiments on nitrous oxide, I refolved to breathe pure hydrocarbonate.

For this purpofe, I introduced into a filk bag, four quarts of gas nearly pure, which was carefully produced from the decompofition of water by charcoal an hour before, and which had a very ftrong and difagreeable fmell.

My friend, Mr. James Tobin, Junr. being prefent, after a forced exhauftion of my lungs, the nofe being accurately clofed, I made three infpirations and expirations of the hydrocarbonate. The firft infpiration produced a fort of numbnefs and lofs of feeling in the cheft and about the pectoral mufcles. After the fecond infpiration, I loft all power of perceiving external things, and had no diftinct fenfation except a terrible oppreſſion on the cheſt. During the third expiration, this feeling diſappeared, I ſeemed ſinking into annihilation, and had juſt power enough to drop the mouth-piece from my uncloſed lips. A ſhort interval muſt have paſſed during which I reſpired common air, before the objects about me were diſtinguiſhable. On recollecting myſelf, I faintly articulated, "*I do not think I ſhall die.*" Putting my finger on the wriſt, I found my pulſe thread-like and beating with exceſſive quickneſs.

In leſs than a minute, I was able to walk, and the painful oppreſſion on the cheſt directed me to the open air.

After making a few ſteps which carried me to the garden, my head became giddy, my knees trembled, and I had juſt ſufficient voluntary power to throw myſelf on the graſs. Here the painful feeling of the cheſt increaſed with ſuch violence as to threaten ſuffocation. At this moment, I aſked for ſome nitrous oxide. Mr. Dwyer brought me a mixture of oxygene and nitrous oxide. I breathed this for a minute, and *believed* myſelf relieved. In five minutes, the painful feelings began gradually to diminiſh. In an hour they had nearly diſappeared, and I felt only exceſſive weakneſs and a ſlight ſwimming of the head. My voice was very feeble and indiſtinct. This was at two o'clock in the afternoon.

I afterwards walked ſlowly for about half an hour, with Mr. Tobin, Junr. and on my return, was ſo much ſtronger and better, as to believe that the effects of the gas had diſappeared; though my pulſe was 120 and very feeble. I continued without pain for near three quarters of an hour; when the giddineſs returned with ſuch violence as to oblige me to lie on the bed; it was accompanied with nauſea, loſs of memory, and deficient ſenſation. In about an hour and half, the giddineſs went off, and was ſucceeded by an excruciating pain in the forehead and between the eyes, with tranſient pains in the cheſt and extremities.

Towards night theſe affections gradually diminiſhed. At ten,† no diſagreeable feeling except weakneſs remained. I ſlept ſound, and awoke in the morning very feeble and very hungry. No recurrence of the ſymptoms took place, and I had nearly recovered my ſtrength by the evening.

I have been minute in the account of this experiment becauſe it proves, that hydrocarbonate acts as a ſedative, i. e. that it produces diminution of vital action, and debility, without previouſly exciting. There is every reaſon to believe, that if I had taken four or five inſpirations inſtead of three, they would have deſtroyed life immediately without producing any painful ſenſation. Perhaps moſt of the uneaſy feelings after the experiment, were connected with the return of the healthy condition of organs.*

† I ought to obſerve, that between eight and ten, I took by the advice of Dr. Beddoes, two or three doſes of diluted nitric acid.

* By whatever cauſe the exhauſtion of organs is produced, pain is almoſt uniformly connected with their returning health. Pain is rarely ever perceived in limbs debilitated

About a week after this experiment, I attempted to refpire Carbonic acid, not being at the time acquainted with the experiments of Rofier.

I introduced into a filk bag four quarts of well wafhed carbonic acid produced from carbonate of ammoniac* by heat, and after a compleat voluntary exhauftion of my lungs, attempted to infpire it. It tafted ftrongly acid in the mouth and fauces, and produced a fenfe of burning at the top of the uvula. In vain I made powerful voluntary efforts to draw it into the windpipe; at the moment that the epiglottis was raifed a little, a painful ftimulation was induced, fo as to clofe it fpafmodically on the glottis; and thus in repeated trials I was prevented from taking a fingle particle of carbonic acid into my lungs.

by fatigue till after they have been for fome hours at reft. Pain is uniformly connected with the recovery from the debility induced by typhus, often with the recovery from that produced by the ftimulation of opium and alcohol.

* Carbonic acid is produced in this way in a high ftate of purity, and with great readinefs.

I tried to breathe a mixture of two quarts of common air and three of carbonic acid, without succefs; it ftimulated the epiglottis nearly in the fame manner as pure carbonic acid, and was perfectly non-refpirable.

I found that a mixture of three quarts of carbonic acid with feven of common air was refpirable, I breathed it for near a minute. At the time, it produced a flight degree of giddinefs, and an inclination to fleep. Thefe effects however, very rapidly difappeared after I had ceafed to breathe,* and no other affections followed.

During the courfe of experiments on nitrous oxide, I feveral times breathed Oxygene procured from manganefe by heat, for from three to five minutes.

In refpiring eight or ten quarts; for the firft

* Carbonic acid poffeffes no action on arterial blood. Hence perhaps, its flight effects when breathed mingled with large quantities of common air. Its effects are very marked upon venous blood! If it were thrown forcibly into the lungs of animals, the momentary application of it to the pulmonary venous blood would probably deftroy life.

two or three minutes I could perceive no effects. Towards the end, even when I breathed very slowly, my respiration became oppressed, and I felt a sensation analogous to that produced by the want of fresh air; though but little of the oxygene had been consumed.

In one experiment when I breathed from and into a bag containing 20 quarts of oxygene for near six minutes; Dr. Kinglake felt my pulse, and found it not altered in velocity, but rather harder than before. I perceived no effects but those of oppression on the chest*.

* In a conversation with Mr. Watt, relating to the powers of gases, that excellent philosopher told me he had for some time entertained a suspicion, that the effects attributed to oxygene produced from manganese by heat, in some measure depended upon nitrous acid suspended in the gas, formed during ignition by the union of some of the oxygene of the manganese with nitrogene likewise condensed in it.

In the course of experiments on nitrous acid, detailed in Research I. made in September, October, and December, 1799, I several times experienced a severe oppression on the chest and difficulty of respiration, not unanalogous to that produced by oxygene, but much more violent, from breathing an atmosphere loaded with nitrous acid vapour. This fact seemed to confirm Mr. Watt's suspicion. I con-

Having obſerved in my experiments upon venous blood, that Nitrous gas rendered that fluid of a purple tinge, very like the color generated in it by nitrous oxide; and finding no painful effects produced by the application of nitrous gas to the bare muſcular fibre, I began to imagine that this gas might be breathed with impunity, provided it were poſſible in any way to free the lungs of common air before inſpiration, ſo as to prevent the formation of nitrous acid.

On this ſuppoſition, during a fit of enthuſiaſm produced by the reſpiration of nitrous oxide, I reſolved to endeavour to breathe Nitrous gas.

114 cubic inches of nitrous gas were introduced into the large mercurial airholder; two

feſs, however, that I have never been able to detect any ſmell of nitrous acid, either by means of my own organs or thoſe of others, during the production of oxygene; when the gas is ſuffered to paſs into the atmoſphere. The oxygene breathed in the experiments detailed in the text, had been for ſome days in contact with water.

ſmall ſilk bags of the capacity of ſeven quarts were filled with nitrous oxide.

After a forced exhauſtion of my lungs, my noſe being accurately cloſed, I made three inſpirations and expirations of nitrous oxide in one of the bags, to free my lungs as much as poſſible from atmoſpheric oxygene; then, after a full expiration of the nitrous oxide, I transferred my mouth from the mouth-piece of the bag to that of the airholder, and turning the ſtop-cock, attempted to inſpire the nitrous gas.— In paſſing through my mouth and fauces, it taſted aſtringent and highly diſagreeable; it occaſioned a ſenſe of burning in the throat, and produced a ſpaſm of the epiglottis ſo painful as to oblige me to deſiſt inſtantly from attempts to inſpire it. After moving my lips from the mouth-piece, when I opened them to inſpire common air, aëriform nitrous acid was inſtantly formed in my mouth, which burnt the tongue and palate, injured the teeth, and produced an inflammation of the mucous membrane which laſted for ſome hours.

As after the reſpiration of nitrous oxide in the experiments in the laſt Reſearch, a ſmall portion of the reſidual atmoſpheric air remained in the lungs, mingled with the gas, after forced expiration; it is moſt probable that a minute portion of nitrous acid was formed in this experiment, when the nitrous gas was taken into the mouth and fauces, which might produce its ſtimulating properties. If ſo, perhaps I owe my life to the circumſtance; for ſuppoſing I had taken an inſpiration of nitrous gas, and even that it had produced no poſitive effects, it is highly improbable, that by breathing nitrous oxide, I ſhould have freed my lungs from it, ſo as to have prevented the formation of nitrous acid when I again inſpired common air. I never deſign again to attempt ſo raſh an experiment.

In the beginning of September I often reſpired nitrous oxide mingled with different proportions of common air or oxygene. The effects produced by the diluted gas were much leſs violent than thoſe produced by pure nitrous oxide. They were generally pleaſant : the thrilling was not often perceived, but a ſenſe of exhilaration was almoſt conſtant.

Between September and the end of October, I made but few experiments on reſpiration, almoſt the whole of my time being devoted to chemical experiments on the production and analyſis of nitrous oxide.

At this period my health being ſomewhat injured by the conſtant labour of experimenting, and the perpetual inhalation of the acid vapours of the laboratory, I went into Cornwal; where new aſſociations of ideas and feelings, common exerciſe, a pure atmoſphere, luxurious diet and moderate indulgence in wine, in a month reſtored me to health and vigor.

Nov. 27th. Immediately after my return, being fatigued by a long journey, I reſpired nine quarts of nitrous oxide, having been preciſely thirty-three days without breathing any. The feelings were different from thoſe I had experienced in former experiments. After the firſt ſix or ſeven inſpirations, I gradually began to loſe the perception of external things, and a vivid and intenſe recollection of ſome former experiments paſſed through my mind, ſo that I called out "*what an amazing concatenation of ideas!*" I had no pleaſurable feeling whatever, I uſed no muſcular motion, nor did I feel any diſpoſition to it; after a minute, when I made the note of the experiment, all the uncommon ſenſations had vaniſhed; they were ſucceeded by a ſlight ſoreneſs in one of the arms and in the leg: in three minutes theſe affections likewiſe diſappeared.

From this experiment I was inclined to ſuppoſe that my newly acquired health had diminiſhed my ſuſceptibility to the effects of the gas. About ten days after, however, I had an opportunity of proving the fallacy of this ſuppoſition.

Immediately after a journey of 126 miles, in which I had no ſleep the preceding night, being much exhauſted, I reſpired ſeven quarts of gas for near three minutes. It produced the uſual pleaſurable effects, and ſlight muſcular motion.

I continued exhilarated for fome minutes afterwards; but in half an hour found myfelf neither more or lefs exhaufted than before the experiment. I had a great propenfity to fleep.

I repeated the experiment four or five times in the following week, with fimilar effects. My fufceptibility was certainly not diminifhed. I even thought that I was more affected than formerly by equal dofes.

Though, except in one inftance, when indeed the gas was impure, I had experienced no decifive exhauftion after the excitement from nitrous oxide, yet ftill I was far from being fatisfied that it was unanalogous to ftimulants in general.— No experiment had been made in which the excitement from nitrous oxide had been kept up for fo great a length of time and carried to fo great an extent as that in which it is uniformly fucceeded by exceffive debility under the agency of other powers.

It occurred to me, that fuppofing nitrous oxide to be a ftimulant of the common clafs, it would follow that the debility produced in conſequence of exceſſive ſtimulation by a known agent, ought to be *increaſed* after excitement from nitrous oxide.*

To aſcertain whether this was the caſe, I made on December 23d, at four P. M. the following experiment. I drank a bottle of wine in large draughts in leſs than eight minutes. Whilſt I was drinking, I perceived a ſenſe of fulneſs in the head, and throbbing of the arteries, not unanalogous to that produced in the firſt ſtage of nitrous oxide excitement. After I had finiſhed the bottle, this fulneſs increaſed, the objects around me became dazzling, the power of diſtinct articulation was loſt, and I was unable to walk ſteadily. At this moment the ſenſations were rather pleaſurable than otherwiſe, the ſenſe of fulneſs in the head ſoon however increaſed ſo as to become painful, and in

* In the ſame manner as the debility from intoxication by two bottles of wine is increaſed by a third.

less than an hour I sunk into a state of insensibility.*

In this situation I must have remained for two hours or two hours and half.

I was awakened by head-ache and painful nausea. The nausea continued even after the contents of the stomach had been ejected. The pain in the head every minute increased; I was neither feverish or thirsty; my bodily and mental debility were excessive, and the pulse feeble and quick.

In this state I breathed for near a minute and half five quarts of gas, which was brought to me by the operator for nitrous oxide; but as it produced no sensations whatever, and apparently rather increased my debility, I am almost convinced that it was from some accident, either common air, or very impure nitrous oxide.

Immediately after this trial, I respired 12 quarts

* I ought to observe that my usual drink is water, that I had been little accustomed to take wine or spirits, and had never been compleatly intoxicated but once before in the course of my life. This will account for the powerful effects of a single bottle of wine.

of oxygene for near four minutes. It produced no alteration in my sensations at the time; but immediately after I imagined that I was a little exhilarated.

The head-ache and debility still however continuing with violence, I examined some nitrous oxide which had been prepared in the morning, and finding it very pure, respired seven quarts of it for two minutes and half.

I was unconscious of head-ache after the third inspiration; the usual pleasurable thrilling was produced, voluntary power was destroyed, and vivid ideas rapidly passed through my mind; I made strides across the room, and continued for some minutes much exhilarated. Immediately after the exhilaration had disappeared, I felt a slight return of the head-ache; it was connected with transient nausea. After two minutes, when a small quantity of acidified wine had been thrown from the stomach, both the nausea and head-ache disappeared; but languor and depression not very different in degree from those existing before the experiment, succeeded.

They however, gradually went off before bed time. I slept sound the whole of the night except for a few minutes, during which I was kept awake by a trifling head-ache. In the morning, I had no longer any debility. No head-ache or giddiness came on after I had arisen, and my appetite was very great.

This experiment proved, that debility from intoxication was not increased by excitement from nitrous oxide. The head-ache and depression, it is probable, would have continued longer if it had not been administered. Is it not likely that the slight nausea following the effects of the gas was produced by new excitability given to the stomach?

To ascertain with certainty, whether the most extensive action of nitrous oxide compatible with life, was capable of producing debility, I resolved to breathe the gas for such a time and in such quantities, as to produce excitement equal in duration and superior in intensity to that occasioned by high intoxication from opium or alcohol.

To habituate myſelf to the excitement, and to carry it on gradually.

On December 26th, I was incloſed in an air-tight breathing-box,* of the capacity of about 9 cubic feet and half, in the preſence of Dr. Kinglake.

After I had taken a ſituation in which I could by means of a curved thermometer inſerted under the arm, and a ſtop-watch, aſcertain the alterations in my pulſe and animal heat, 20 quarts of nitrous oxide were thrown into the box.

For three minutes I experienced no alteration in my ſenſations, though immediately after the introduction of the nitrous oxide the ſmell and taſte of it were very evident.†

In four minutes I began to feel a ſlight glow

* The plan of this box was communicated by Mr. Watt. An account of it will be detailed in the *Reſearches*.

† The nitrous oxide was too diluted to act much; it was mingled with near 32 times its bulk of atmoſpheric air.

in the cheeks, and a generally diffufed warmth over the cheft, though the temperature of the box was not quite 50°. I had neglected to feel my pulfe before I went in; at this time it was 104 and hard, the animal heat was 98°. In ten minutes the animal heat was near 99°, in a quarter of an hour 99.5°, when the pulfe was 102, and fuller than before.

At this period 20 quarts more of nitrous oxide were thrown into the box, and well-mingled with the mafs of air by agitation.

In 25 minutes the animal heat was 100°, pulfe 124. In 30 minutes, 20 quarts more of gas were introduced.

My fenfations were now pleafant; I had a generally diffufed warmth without the flighteft moifture of the fkin, a fenfe of exhilaration fimilar to that produced by a fmall dofe of wine, and a difpofition to mufcular motion and to merriment.

In three quarters of an hour the pulfe was 104, and animal heat not 99,5°, the temperature of the chamber was 64°. The pleafurable feelings continued to increase, the pulse became fuller and slower, till in about an hour it was 88°, when the animal heat was 99°.

20 quarts more of air were admitted. I had now a great disposition to laugh, luminous points seemed frequently to pass before my eyes, my hearing was certainly more acute and I felt a pleasant lightness and power of exertion in my muscles. In a short time the symptoms became stationary; breathing was rather oppressed, and on account of the great desire of action, rest was painful.

I now came out of the box, having been in precisely an hour and quarter.

The moment after, I began to respire 20 quarts of unmingled nitrous oxide. A thrilling extending from the chest to the extremities was almost immediately produced. I felt a sense of tangible extension highly pleasurable in every limb; my visible impressions were dazzling and apparently magnified, I heard distinctly every sound in the room and was perfectly aware of my situation.* By degrees as the pleasurable sensations increased, I lost all connection with external things; trains of vivid visible images rapidly passed through my mind and were connected with words in such a manner, as to produce perceptions perfectly novel. I existed in a world of newly connected and newly modified ideas. I theorised; I imagined that I made discoveries. When I was awakened from this semi-delirious trance by Dr. Kinglake, who took the bag from my mouth, Indignation and pride were the first feelings produced by the sight of the persons about me. My emotions were enthusiastic and sublime; and for a minute I walked round the room perfectly regardless of what was said to me. As I recovered my former state of mind, I felt an inclination to communicate the discoveries I had made during the experiment. I endeavoured to recall the ideas, they were feeble and indistinct; one collection of terms, however, presented itself:

* In all these experiments after the first minute, my cheeks became purple.

and with the most intense belief and prophetic manner, I exclaimed to Dr. Kinglake, "*Nothing exists but thoughts! — the universe is composed of impressions, ideas, pleasures, and pains!*"

About three minutes and half only, had elapsed during this experiment, though the time as measured by the relative vividness of the recollected ideas, appeared to me much longer.

Not more than half of the nitrous oxide was consumed. After a minute, before the thrilling of the extremities had disappeared, I breathed the remainder. Similar sensations were again produced; I was quickly thrown into the pleasurable trance, and continued in it longer than before. For many minutes after the experiment, I experienced the thrilling in the extremities, the exhilaration continued nearly two hours. For a much longer time I experienced the mild enjoyment before described connected with indolence; no depression or feebleness followed. I ate my dinner with great appetite and found myself lively and disposed to action immediately after. I passed the evening in executing experiments. At night I found myself unusually cheerful and active; and the hours between eleven and two, were spent in copying the foregoing detail from the common-place book, and in arranging the experiments. In bed I enjoyed profound repose. When I awoke in the morning, it was with consciousness of pleasurable existence, and this consciousness more or less, continued through the day.

Since December, I have very often breathed nitrous oxide. My susceptibility to its power is rather increased than diminished. I find six quarts a full dose, and I am rarely able to respire it in any quantity for more than two minutes and half.

The mode of its operation is somewhat altered. It is indeed very different at different times.

I am scarcely ever excited into violent muscular action, the emotions are generally much less intense and sublime than in the former experiments, and not often connected with thrilling in the extremities.

When troubled with indigeſtion, I have been two or three times unpleaſantly affected after the excitement of the gas. Cardialgia, eructations and unpleaſant fulneſs of the head were produced.

I have often felt very great pleaſure when breathing it alone, in darkneſs and ſilence, occupied only by ideal exiſtence. In two or three inſtances when I have breathed it amidſt noiſe, the ſenſe of hearing has been painfully affected even by moderate intenſity of ſound. The light of the ſun has ſometimes been diſagreeably dazzling. I have once or twice felt an uneaſy ſenſe of tenſion in the cheeks and tranſient pains in the teeth.

Whenever I have breathed the gas after excitement from moral or phyſical cauſes, the delight has been often intenſe and ſublime.

On May 5th, at night, after walking for an hour amidſt the ſcenery of the Avon, at this period rendered exquiſitely beautiful by bright moonſhine; my mind being in a ſtate of agreeable feeling, I respired six quarts of newly prepared nitrous oxide.

The thrilling was very rapidly produced. The objects around me were perfectly distinct, and the light of the candle not as usual dazzling. The pleasurable sensation was at first local and perceived in the lips and about the cheeks. It gradually, however, diffused itself over the whole body, and in the middle of the experiment was for a moment so intense and pure as to absorb existence. At this moment, and not before, I lost consciousness; it was however, quickly restored, and I endeavoured to make a by-stander acquainted with the pleasure I experienced by laughing and stamping. I had no vivid ideas. The thrilling and the pleasurable feeling continued for many minutes; I felt two hours afterwards, a slight recurrence of them, in the intermediate state between sleeping and waking; and I had during the whole of the night, vivid and agreeable dreams. I awoke in the morning with the feeling of restless energy; or that desire of action connected with no definite object, which I had often experienced in the course of experiments in 1799.

I have two or three times since respired nitrous oxide under similar circumstances; but never with equal pleasure.

During the last fortnight, I have breathed it very often; the effects have been powerful and the sensations uncommon; but pleasurable only in a slight degree.

I ought to have observed that a desire to breathe the gas is always awakened in me by the sight of a person breathing, or even by that of an air-bag or an air-holder.

I have this day, June 5th, respired four large doses of gas. The first two taken in the morning acted very powerfully; but produced no thrilling or other pleasurable feelings. The effects of the third breathed immediately after a hearty dinner were pleasant; but neither intense or intoxicating. The fourth was respired at night in darkness and silence after the occurrence of a circumstance which had produced some anxiety. This dose affected me powerfully and pleasantly; a slight thrilling in the extremities was produced; an exhilaration continued for some time, and I have had but little return of uneasiness. 11 P. M.

From the nature of the language of feeling, the preceding detail contains many imperfections; I have endeavoured to give as accurate an account as possible of the strange effects of nitrous oxide, by making use of terms standing for the most similar common feelings.

We are incapable of recollecting pleasures and pains of sense.* It is impossible to reason concerning them, except by means of terms which have been associated with them at the moment of their existence, and which are afterwards called up amidst trains of concomitant ideas.

* Physical pleasure and pain generally occur connected with a compound impression, i. e. an organ and some object. When the idea left by the compound impression, is called up by being linked accidentally to some other idea or impression, no recurrence, or the slightest possible, of the pleasure or pain in any form will take place. But when the compound impression itself exists *without* the physical pleasure or pain, it will awaken ideal or intellectual

When pleafures and pains are new, or connected with new ideas, they can never be intelligibly detailed unlefs affociated during their exiftence with terms ftanding for analogous feelings.

I have fometimes experienced from nitrous oxide, fenfations fimilar to no others, and they have confequently been indefcribable. This has been likewife often the cafe with other perfons. Of two paralytic patients who were afked what they felt after breathing nitrous

pleafure or pain, i. e. hope or fear. So that phyfical pleafure and pain are to hope and fear, what impreffions are to ideas. For inftance, affuming no accidental affociation, the child does not fear the fire before he is burnt. When he puts his finger to the fire he feels the phyfical pain of burning, which is connected with a vifible compound impreffion, the fire and his finger. Now when the compound idea of the fire and his finger, left by the compound impreffion are called up by his mother, faying, "*You have burnt your finger*," nothing like fear or the pain of burning is connected with it. But when the finger is brought near the fire, i. e. when the compound impreffion again exifts; the ideal pain of burning or the paffion of fear is awakened, and it becomes connected with thofe very actions which removed the finger from the fire.

oxide, the first answered, "*I do not know how, but very queer.*" The second said, "*I felt like the sound of a harp.*" Probably in the one case, no analogous feelings had ever occurred. In the other, the pleasurable thrillings were similar to the sensations produced by music; and hence, they were connected with terms formerly applied to music.

# DIVISION II.

## *DETAILS of the EFFECTS produced by the RESPIRATION of NITROUS OXIDE upon different INDIVIDUALS furnished by THEMSELVES.*

THE experiments related in the following details, were made in the Medical Pneumatic Inſtitution.

Abſtracts from many of them have been publiſhed by Dr. Beddoes.*

### I. *Detail of* Mr. J. W. TOBIN.

Having ſeen the remarkable effects produced on Mr. Davy, by breathing nitrous oxide, the 18th of April; I became deſirous of taking ſome. A day or two after I breathed 2 quarts of this

* Notice of ſome Obſervations made at the Medical Pneumatic Inſtitution.

gas, returning it back again into the ſame bag, after two or three inſpirations, breathing became difficult, and I occaſionally admitted common air into my lungs. While the reſpiration was continued, my ſenſations became more pleaſant. On taking the bag from my mouth, I ſtaggered a little, but felt no other effect.

On the ſecond time of making the experiment, I took nearly four quarts, but ſtill found it difficult to continue breathing long, though the air which was left in the bag was far from being impure.

The effects however, in this caſe, were more ſtriking than in the former. Increaſed muſcular action was accompanied by very pleaſurable feelings, and a ſtrong deſire to continue the inſpiration. On removing the bag from my mouth, I laughed, ſtaggered, and attempted to ſpeak, but ſtammered exceedingly, and was utterly unable to pronounce ſome words. My uſual ſtate of mind, however, ſoon returned.

On the 29th, I again breathed four quarts. The pleaſant feelings produced at firſt, urged me to continue the inſpiration with great eager-neſs. Theſe feelings however, went off towards the end of the experiment, and no other effects followed. The gas had probably been breathed too long, as it would not ſupport flame. I then propoſed to Mr. Davy, to inhale the air by the mouth from one bag, and to expire it from the noſe into another. This method was purſued with leſs than three quarts, but the effects were ſo powerful as to oblige me to take in a little common air occaſionally. I ſoon found my nervous ſyſtem agitated by the higheſt ſenſations of pleaſure, which are difficult of deſcription; my muſcular powers were very much increaſed, and I went on breathing with great vehemence, not from a difficulty of inſpiration, but from an eager avidity for more air. When the bags were exhauſted and taken from me, I continued breathing with the ſame violence; then ſuddenly ſtarting from the chair, and vociferating with pleaſure, I made towards thoſe that were preſent, as I wiſhed they ſhould participate in my feelings. I ſtruck gently at

Mr. Davy and a ſtranger entering the room at the moment, I made towards him, and gave him ſeveral blows, but more in the ſpirit of good humour than of anger. I then ran through different rooms in the houſe, and at laſt returned to the laboratory ſomewhat more compoſed; my ſpirits continued much elevated for ſome hours after the experiment, and I felt no conſequent depreſſion either in the evening or the day following, but ſlept as ſoundly as uſual.

On the 5th of May, I again attempted to breathe nitrous oxide, but it happened to contain ſuſpended nitrous vapour which rendered it non-reſpirable.

On the 7th, I inſpired 7 quarts of pure gas mingled with an equal quantity of common air, the ſenſations were pleaſant, and my muſcular power much increaſed.

On the 8th, I inſpired five quarts without any mixture of common air, but the effects were not equal to thoſe produced the day before; Indeed there were reaſons for ſuppoſing that the gas was impure.

On the 18th, I breathed nearly ſix quarts of the pure nitrous oxide. It is not eaſy to deſcribe my ſenſations; they were ſuperior to any thing I ever before experienced. My ſtep was firm, and all my muſcular powers increaſed. My ſenſes were more alive to every ſurrounding impreſſion; I threw myſelf into ſeveral theatrical attitudes, and traverſed the laboratory with a quick ſtep; my mind was elevated to a moſt ſublime height. It is giving but a faint idea of the feelings to ſay, that they reſembled thoſe produced by a repreſentation of an heroic ſcene on the ſtage, or by reading a ſublime paſſage in poetry when circumſtances contribute to awaken the fineſt ſympathies of the ſoul. In a few minutes the uſual ſtate of mind returned. I continued in good ſpirits for the reſt of the day, and ſlept ſoundly.

Since the 18th of May, I have very often breathed nitrous oxide. In the firſt experiments when pure, its effects were generally ſimilar to thoſe juſt deſcribed.

Lately I have ſeldom experienced vivid ſenſations. The pleaſure produced by it is ſlight and tranquil, I rarely feel ſublime emotions or increaſed muſcular power.

J. W. Tobin.

*October*, 1799.

### II. *Detail of* Mr. Wm. Clayfield.

The firſt time that I breathed the nitrous oxide, it produced feelings analogous to thoſe of intoxication. I was for ſome time unconſcious of exiſtence, but at no period of the experiment experienced agreeable ſenſations, a momentary nauſea followed it; but unconnected with languor or head-ache.

After this I ſeveral times reſpired the gas, but on account of the fulneſs in the head and apparent throbbing of the arteries in the brain,* always deſiſted to breathe before the full effects were produced. In two experiments however, when by powerful voluntary efforts I ſucceeded in breathing a large quantity of gas for ſome mi-

* In ſome of theſe experiments, hearing was rendered more acute.

nutes, I had highly pleaſurable thrillings in the extremities, and ſuch increaſe of muſcular power, as to be obliged to exert my limbs with violence. After theſe experiments, no languor or depreſſion followed.

WILLIAM CLAYFIELD.

### III. *Letter from* DR. KINGLAKE.

In compliance with your deſire, I will endeavour to give you a faithful detail of the effects produced on my ſenſations by the inhalation of nitrous oxide.

My firſt inſpiration of it was limited to four quarts, diluted with an equal quantity of atmoſpheric air. After a few inſpirations, a ſenſe of additional freedom and power (call it energy if you pleaſe) agreeably pervaded the region of the lungs; this was quickly ſucceeded by an almoſt delirious but highly pleaſurable ſenſation in the brain, which was ſoon diffuſed over the whole frame, imparting to the muſcular power at once an encreaſed diſpoſition and tone for action; but the mental effect of the excitement was ſuch as to abſorb in a ſort of intoxicating placidity, and delight, volition, or rather the power of voluntary motion. Theſe effects were in a greater or leſs degree protracted during about five minutes, when the former ſtate returned, with the difference however of feeling more cheerful and alert, for ſeveral hours after.

It ſeemed alſo to have had the further effect of reviving rheumatic irritations in the ſhoulder and knee-joints, which had not been previouſly felt for many months. No perceptible change was induced in the pulſe either at or ſubſequent to the time of inhaling the gas.

The effects produced by a ſecond trial of its powers, were more extenſive, and concentrated on the brain. In this inſtance, nearly ſix quarts undiluted, were accurately and fully inhaled. As on the former occaſion, it immediately proved agreeably reſpirable, but before the whole quantity was quite exhauſted, its agency was exerted ſo ſtrongly on the brain, as progreſſively to ſuſpend the ſenſes of ſeeing, hearing, feeling, and ultimately the power of volition itſelf. At this period, the pulſe was much augmented both in force and frequency ; ſlight convulſive twitches of the muſcles of the arms were alſo induced ; no painful ſenſation, nauſea, or languor, however, either preceded, accompanied, or followed this ſtate, nor did a minute elapſe before the brain rallied, and reſumed its wonted faculties, when a ſenſe of glowing warmth extending over the ſyſtem, was ſpeedily ſucceeded by a re-inſtatement of the equilibrium of health.

The more permanent effects were (as in the firſt experiment) an invigorated feel of vital power, improved ſpirits, tranſient irritations in different parts, but not ſo characteriſtically rheumatic as in the former inſtance.

Among the circumſtances moſt worthy of regard in conſidering the properties and adminiſtration of this powerful aërial agent, may be ranked, the fact of its being (contrary to the prevailing opinion*) both highly reſpirable, and

* Dr. Mitchill (an American Chemiſt) has erroneouſly ſuppoſed its full admiſſion to the lungs, in its concentrated ſtate, to be incompatible with animal life, and that in a more diluted form it operates as a principal agent in the

ſalutary, that it impreſſes the brain and ſyſtem at large with a more or leſs ſtrong and durable degree of pleaſurable ſenſation, that unlike the effect of other violently exciting agents, no ſenſible exhauſtion or diminution of vital power accrues from the exertions of its ſtimulant property, that its moſt exceſſive operation even, is neither permanently nor tranſiently debilitating; and finally, that it fairly promiſes under judicious application, to prove an extremely efficient remedy, as well in the vaſt tribe of diſeaſes originating from deficient irritability and ſenſibility, as in thoſe proceeding from morbid aſſociations, and modifications, of thoſe vital principles.

production of contagious diſeaſes, &c. This gratuitous poſition is thus unqualifiedly affirmed. " If a full inſpira-" tion of gaſeous oxyd be made, there will be a ſudden " extinction of life; and this accordingly accounts for the " fact related by Ruſſel (Hiſtory of Aleppo, p. 232.) and " confirmed by other obſervers, of many perſons falling " down dead ſuddenly, when ſtruck with the contagion of " the plague."

Vide Remarks on the Gaſeous Oxyd of Azote, by Samuel Latham Mitchill, M. D.

If you ſhould deem any thing contained in this curſory narrative capable of ſubſerving in any degree the practical advantages likely to reſult from your ſcientific and valuable inveſtigation of the genuine properties of the nitrous oxide, it is perfectly at your diſpoſal.

I am

Your ſincere friend,

ROBERT KINGLAKE.

*Briſtol, June 14th,* 1799.

TO MR. DAVY.

## IV. *Detail of* MR. SOUTHEY.

In breathing the nitrous oxide, I could not diſtinguiſh between the firſt feelings it occaſioned and an apprehenſion of which I was unable to diveſt myſelf. My firſt definite ſenſation was a dizzineſs, a fulneſs in the head, ſuch as to induce a fear of falling. This was momentary. When I took the bag from my mouth, I immediately laughed. The laugh was involuntary but highly pleafurable, accompanied by a thrill all through me; and a tingling in my toes and fingers, a fenfation perfectly new and delightful. I felt a fulnefs in my cheft afterwards; and during the remainder of the day, imagined that my tafte and hearing were more than commonly quick. Certain I am that I felt myfelf more than ufually ftrong and chearful.

In a fecond trial, by continuing the inhalation longer, I felt a thrill in my teeth; and breathing ftill longer the third time, became fo full of ftrength as to be compelled to exercife my arms and feet.

Now after an interval of fome months, during which my health has been materially impaired, the nitrous oxide produces an effect upon me totally different. Half the quantity affects me, and its operation is more violent; a flight laughter is firft induced,* and a defire to continue the

* In the former experiments, Mr. Southey generally refpired fix quarts, now he is unable to confume two.

In an experiment made fince this paper was drawn up, the effect was rather pleafurable.

inhalation, which is counteracted by fear from the rapidity of respiration; indeed my breath becomes so short and quick, that I have no doubt but the quantity which I formerly breathed, would now destroy me. The sensation is not painful, neither is it in the slightest degree pleasurable.

ROBERT SOUTHEY.

### V. *Letter from* DR. ROGET.

The effect of the first inspirations of the nitrous oxide was that of making me vertiginous, and producing a tingling sensation in my hands and feet: as these feelings increased, I seemed to lose the sense of my own weight, and imagined I was sinking into the ground. I then felt a drowsiness gradually steal upon me, and a disinclination to motion; even the actions of inspiring and expiring were not performed without effort: and it also required some attention of mind to keep my nostrils closed with my fingers. I was gradually roused from this torpor by a kind of delirium, which came on ſo rapidly that the air-bag dropt from my hands. This ſenſation increaſed for about a minute after I had ceaſed to breathe, to a much greater degree than before, and I ſuddenly loſt ſight of all the objects around me, they being apparently obſcured by clouds, in which were many luminous points, ſimilar to what is often experienced on riſing ſuddenly and ſtretching out the arms, after ſitting long in one poſition.

I felt myſelf totally incapable of ſpeaking, and for ſome time loſt all conſciouſneſs of where I was, or who was near me. My whole frame felt as if violently agitated: I thought I panted violently: my heart ſeemed to palpitate, and every artery to throb with violence; I felt a ſinging in my ears; all the vital motions ſeemed to be irreſiſtibly hurried on, as if their equilibrium had been deſtroyed, and every thing was running headlong into confuſion. My ideas ſucceeded one another with extreme rapidity, thoughts ruſhed like a torrent through my mind, as if their velocity had been ſuddenly accelerated by the burfting of a barrier which had before retained them in their natural and equable courfe. This ftate of extreme hurry, agitation, and tumult, was but tranfient. Every unnatural fenfation gradually fubfided; and in about a quarter of an hour after I had ceafed to breathe the gas, I was nearly in the fame ftate in which I had been at the commencement of the experiment.

I cannot remember that I experienced the leaft pleafure from any of thefe fenfations. I can however, eafily conceive, that by frequent repetition I might reconcile myfelf to them, and poffibly even receive pleafure from the fame fenfations which were then unpleafant.

I am fenfible that the account I have been able to give of my feelings is very imperfect. For however calculated their violence and novelty were to leave a lafting impreffion on the memory, thefe circumftances were for that very reafon unfavourable to accuracy of comparifon with fenfations already familiar.

The nature of the fenfations themfelves, which bore greater resemblance to a half delirious dream than to any distinct state of mind capable of being accurately remembered, contributes very much to increase the difficulty. And as it is above two months since I made the experiment, many of the minuter circumstances have probably escaped me.

I remain

Yours, &c.

P. Roget.

To Mr. Davy.

VI. *Letter from* Mr. James Thomson.

The first time I respired nitrous oxide, the experiment was made under a strong impression of fear, and the quantity I breathed not sufficient, as you informed me, to produce the usual effect. I did not note very accurately my sensations. I remember I experienced a slight degree of vertigo after the third or fourth inspiration; and breathed with increased vigor, my inspirations being much deeper and more vehement than ordinary. I was enabled the next time I made the experiment, to attend more accurately to my ſenſations, and you have the obſervations I made on them at the time.

After the fourth inſpiration, I experienced the ſame increaſed action of the lungs, as in the former caſe. My inſpirations became uncommonly full and ſtrong, attended with a thrilling ſenſation about the cheſt, highly pleaſurable, which increaſed to ſuch a degree as to induce a fit of involuntary laughter, which I in vain endeavoured to repreſs. I felt a ſlight giddineſs which laſted for a few moments only. My inſpirations now became more vehement and frequent; and I inhaled the air with an avidity ſtrongly indicative of the pleaſure I received. That peculiar thrill which I had at firſt experienced at the cheſt, now pervaded my whole frame; and during the two or three laſt inſpirations, was attended with a remarkable tingling in my fingers and toes. My feelings at this moment are not to be deſcribed: I felt a high, an extraordinary degree of pleaſure, different from that produced by wine, being diveſted of all its groſs accompaniments, and yet approaching nearer to it than to any other ſenſation I am acquainted with.

I am certain that my muſcular ſtrength was for a time much increaſed. My diſpoſition to exert it was ſuch as I could not repreſs, and the ſatisfaction I felt in any violent exertion of my legs and arms is hardly to be conceived. Theſe vivid ſenſations were not of long duration; they diminiſhed inſenſibly, and in little more than a quarter of an hour I could perceive no difference between the ſtate I was then in, and that previous to the reſpiration of the air.

The obſervations I made on repeating the experiment, do not differ from the preceding, except in the circumſtance of the involuntary laughter, which I never afterwards experienced, though I breathed the air ſeveral times; and in the following curious fact, which, as it was dependent on circumſtances, did not always occur.

Having reſpired the ſame quantity of air as uſual, and with preciſely the ſame effects, I was ſurpriſed to find myſelf affected a few minutes afterwards with the recurrence of a pain in my back and knees, which I had experienced the preceding day from fatigue in walking. I was rather inclined to deem this an accidental coincidence than an effect of the air; but the ſame thing conſtantly occurring whenever I breathed the air, ſhortly after ſuffering pain either from fatigue, or any other accidental cauſe, left no doubt on my mind as to the accuracy of the obſervation.

I have now given you the ſubſtance of the notes I made whilſt the impreſſions were ſtrong on my mind. I cannot add any thing from recollection that will at all add to the accuracy of this account, or aſſiſt thoſe who have not reſpired this air, in forming a clearer idea of its extraordinary effects. It is extremely difficult to convey to others by means of words, any idea of particular ſenſations, of which they have had no experience. It can only be done by making uſe of ſuch terms as are expreſſive of ſenſations that reſemble them, and in theſe our vocabulary is very defective. To be able at all to comprehend the effects of nitrous oxide, it is neceffary to refpire it, and after that, we muft either invent new terms to exprefs thefe new and particular fenfations, or attach new ideas to old ones, before we can communicate intelligibly with each other on the operation of this extraordinary gas.

I am &c.

JAMES THOMSON.

*London, Sept.* 21, 1799.

TO MR. DAVY.

## VII. *Detail of* MR. COLERIDGE.

The firft time I infpired the nitrous oxide, I felt an highly pleafurable fenfation of warmth over my whole frame, refembling that which I remember once to have experienced after returning from a walk in the fnow into a warm room. The only motion which I felt inclined to make, was that of laughing at thofe who were looking at me. My eyes felt diftended, and towards the laſt, my heart beat as if it were leaping up and down. On removing the mouth-piece the whole ſenſation went off almoſt inſtantly.

The ſecond time, I felt the ſame pleaſurable ſenſation of warmth, but not I think, in quite ſo great a degree. I wiſhed to know what effect it would have on my impreſſions; I fixed my eye on ſome trees in the diſtance, but I did not find any other effect except that they became dimmer and dimmer, and looked at laſt as if I had ſeen them through tears. My heart beat more violently than the firſt time. This was after a hearty dinner.

The third time I was more violently acted on than in the two former. Towards the laſt, I could not avoid, nor indeed felt any wiſh to avoid, beating the ground with my feet; and after the mouth-piece was removed, I remained for a few ſeconds motionleſs, in great extacy.

The fourth time was immediately after breakfaſt. The few firſt inſpirations affected me ſo little that I thought Mr. Davy had given me atmoſpheric air: but ſoon felt the warmth beginning about my cheſt, and ſpreading upward and downward, ſo that I could feel its progreſs over my whole frame. My heart did not beat ſo violently; my ſenſations were highly pleaſurable, not ſo intenſe or apparently local, but of more unmingled pleaſure than I had ever before experienced.*

S. T. COLERIDGE.

## VIII. *Detail of* MR. WEDGWOOD.

July 23, I called on Mr. Davy at the Medical Inſtitution, who aſked me to breathe ſome of the nitrous oxide, to which I conſented, being rather a ſceptic as to its effects, never having ſeen any perſon affected. I firſt breathed about ſix quarts of air which proved to be only common atmoſpheric air, and which conſequently produced no effect.

I then had 6 quarts of the oxide given me in

* The doſes in theſe experiments were from five to ſeven quarts.

a bag undiluted, and as ſoon as I had breathed three or four reſpirations, I felt myſelf affected and my reſpiration hurried, which effect increaſed rapidly until I became as it were entranced, when I threw the bag from me and kept breathing on furiouſly with an open mouth and holding my noſe with my left hand, having no power to take it away though aware of the ridiculouſneſs of my ſituation. Though apparently deprived of all voluntary motion, I was ſenſible of all that paſſed, and heard every thing that was ſaid; but the moſt ſingular ſenſation I had, I feel it impoſſible accurately to deſcribe. It was as if all the muſcles of the body were put into a violent vibratory motion; I had a very ſtrong inclination to make odd antic motions with my hands and feet. When the firſt ſtrong ſenſations went off, I felt as if I were lighter than the atmoſphere, and as if I was going to mount to the top of the room. I had a metallic taſte left in my mouth, which ſoon went off.

Before I breathed the air, I felt a good deal fatigued from a very long ride I had had the day before, but after breathing, I loſt all ſenſe of fatigue.

## IX. *Detail of* Mr. George Burnet.

I had never heard of the effects of the nitrous oxide, when I breathed ſix quarts of it. I felt a delicious tremor of nerve, which was rapidly propagated over the whole nervous ſyſtem. As the action of inhaling proceeds, an irreſiſtible *appetite* to repeat it is excited. There is now a general ſwell of ſenſations, vivid, ſtrong, and inconceivably pleaſurable. They ſtill become more vigorous and glowing till they are communicated to the brain, when an ardent fluſh overſpreads the face. At this moment the tube inſerted in the air-bag was taken from my mouth, or I muſt have fainted in extacy.

The operation being over, the ſtrength and turbulence of my ſenſations ſubſided. To this ſucceeded a ſtate of feeling uncommonly ſerene and tranquil. Every nerve being gently agitated with a lively enjoyment. It was natural to expect that the effect of this experiment, would eventually prove debilitating. So far from this I continued in a state of high excitement the remainder of the day after two o'clock, the time of the experiment, and experienced a flow of spirits not merely chearful, but unusually joyous.

GEORGE BURNET.

## X. *Detail of* MR. T. POPLE.

A disagreeable sensation as if breaking out into a profuse perspiration, tension of the tympanum, cheeks and forehead; almost total loss of muscular power; afterwards increased powers both of body and mind, very vivid sensations and highly pleasurable. Those pleasant feelings were not new, they were felt, but in a less degree, on ascending some high mountains in Glamorganshire.

On taking it the second time, there was a disagreeable feeling about the face. In a few feconds, the feelings became pleafurable ; all the faculties abforbed by the fine pleafing feelings of exiftence without confcioufnefs ; an involuntary burft of laughter.

THOMAS POPLE.

## XI. *Detail of* MR. HAMMICK.

Having never heard any thing of the mode of operation of nitrous oxide, I breathed gas in a filk bag for fome time, and found no effects, but oppreffion of refpiration. Afterwards Mr. Davy told me that I had been breathing atmofpheric air..

In a fecond experiment made without knowing what gas was in the bag, I had not breathed half a minute, when from the extreme pleafure I felt, I unconcioufly removed the bag from my mouth ; but when Mr. Davy offered to take it from me, I refufed to let him have it, and faid eagerly, " let me breathe it again, it is highly pleafant ! it is the ftrongeft ftimulant I ever felt !" I was cold when I began to refpire, but had immediately a pleaſant glow extending to my toes and fingers. I experienced from the air a pleaſant taſte which I can only call ſweetly aſtringent; it continued for ſome time: the ſenſe of exhilaration was laſting. This air Mr. Davy told me was nitrous oxide.

In another experiment, when I breathed a ſmall doſe of nitrous oxide, the effects were ſlight, and ſometime afterwards I felt an unuſual yawning and languor.

The laſt time that I breathed the gas, the feelings were the moſt pleaſurable I ever experienced; my head appeared light, there was a great warmth in the back and a general unuſual glow; the taſte was diſtinguiſhable for ſome time as in the former experiment. My ideas were more vivid, and followed the natural order of aſſociation. I could not refrain from muſcular action.

STEPHEN HAMMICK, Junr.

Sept. 15th.

## XII. *Detail of* Dr. Blake.

Dr. Blake inhaled about ſix quarts of the air, was affected during the proceſs of reſpiring it with a ſlight degree of vertigo, which was almoſt immediately ſucceeded by a thrilling ſenſation extending even to the extremities, accompanied by a moſt happy ſtate of mind and highly pleaſurable ideas. He felt a great propenſity to laugh, and his behaviour in ſome meaſure appeared ludicrous to thoſe around him. Muſcular power ſeemed agreeably increaſed, the pulſe acquired ſtrength and firmneſs, but its frequency was ſomewhat diminiſhed. He perceived rather an unpleaſant taſte in the mouth and about the fauces for ſome hours afterwards, but in every other reſpect, his feelings were comfortable during the remainder of the day.

*December*, 30*th*.

To Mr. Davy.

## XIII. *Detail of* MR. WANSEY.

I breathed the gas out of a ſilk bag, believing it to be nitrous oxide, and was much ſurpriſed to find that it produced no ſenſations. After the experiment, Mr. Davy told me it was common air.

I then breathed a mixture of common air and nitrous oxide. I felt a kind of intoxication in the middle of the experiment, and ſtopping to expreſs this, deſtroyed any farther effects.

I now breathed pure nitrous oxide; the effect was gradual, and I at firſt experienced fulneſs in the head, and afterwards ſenſations ſo delightful, that I can compare them to no others, except thoſe which I felt (being a lover of muſic) about five years ſince in Weſtminſter Abbey, in ſome of the grand choruſſes in the Meſſiah, from the united powers of 700 inſtruments of muſic. I continued exhilarated throughout the day, ſlept at night remarkably ſound, and experienced when I awoke in the morning, a recurrence of pleafing fenfation.

In another experiment, the effect was ftill greater, the pulfe was rendered fuller and quicker, I felt a fenfe of throbbing in the head with highly pleafurable thrillings all over the frame. The new feelings were at laft fo powerful as to abforb all perception. I diftinguifhed during and after the experiment, a tafte on the tongue, like that produced by the contact of zinc and filver.

HENRY WANSEY.

## XIV. *Detail of* MR. RICKMAN.

On inhaling about fix quarts, the firft altered feeling was a tingling in the elbows not unlike the effect of a flight electric fhock. Soon afterwards, an involuntary and provoking dizzinefs as in drunkennefs. Towards the clofe of the inhalation, this fymptom decreafed; though the nofe was ftill involuntary held faft after the air-bag was removed. The dofe was probably an undercharge, as no extraordinary ſenſation was felt more than half a minute after the inhalation.

J. RICKMAN.

### XV. *Detail of* MR. LOVELL EDGWORTH.

My firſt ſenſation was an univerſal and conſiderable tremor. I then perceived ſome giddineſs in my head, and a violent dizzineſs in my ſight; thoſe ſenſations by degrees ſubſided, and I felt a great propenſity to bite through the wooden mouth-piece, or the tube of the bag through which I inſpired the air. After I had breathed all the air that was in the bag, I eagerly wiſhed for more. I then felt a ſtrong propenſity to laugh, and did burſt into a violent fit of laughter, and capered about the room without having the power of reſtraining myſelf. By degrees theſe feelings ſubſided, except the tremor which laſted for an hour after I had breathed the air, and I felt a weakneſs in my knees. The principal feeling through the whole of the time, or what I ſhould call the characteriſtical part of the effect, was a total difficulty of reſtraining my feelings, both corporeal and mental, or in other words, not having any command of one'self.

## XVI. *Detail of* MR. G. BEDFORD.

I inhaled 6 quarts. Experienced a ſenſation of fulneſs in the extremities and in the face with a deſire and power of expanſion of the lungs very pleaſurable. Feelings ſimilar to intoxication were produced, without being diſagreeable. When the bag was taken away, an involuntary though agreeable laughter took place, and the extremities were warm.

In about a quarter of an hour after the above experiment, I inhaled 8 quarts. The warmth and fulneſs of the face and extremities were ſooner produced during the inſpiration. The candle and the perſons about me, aſſumed the ſame appearances as took place during the effect produced by wine, and I could perceive no determinate outline. The defire and power to expand the lungs was increafed beyond that in the former experiment, and the whole body and limbs feemed dilated without the fenfe of tenfion, it was as if the bulk was increafed without any addition to the fpecific gravity of the body, which was highly pleafant. The provocation to laughter was not fo great as in the former experiment, and when the bag was removed, the warmth almoft fuddenly gave place to a coldnefs of the extremities, particularly of the hands which were the firft to become warm during the infpiration. A flight fenfation of fulnefs not amounting to pain in the head, has continued for fome minutes. After the firft experiment, a fenfation in the wrifts and elbows took place, fimilar to that produced by the electric fhock.

G. C. Bedford.

*March* 30*th*, 1800.

## XVII. *Detail of* Miss Ryland.

After having breathed five quarts of gas, I experienced for a ſhort time a quickneſs and difficulty of breathing, which was ſucceeded by extreme languor, reſembling fainting, without the very unpleaſant ſenſation with which it is uſually attended. It entirely deprived me of the power of ſpeaking, but not of recollection, for I heard every thing that was ſaid in the room during the time; and Mr. Davy's remark "that my pulſe was very quick and full." When the languor began to ſubſide, it was ſucceeded by reſtleſsneſs, accompanied by involuntary muſcular motions. I was warmer than uſual, and very ſleepy for ſeveral hours.

## XVIII. *Letter from* Mr. M. M. Coates.

I will, as you requeſt, endeavour to deſcribe to you the effect produced on me laſt Sunday ſe'nnight by the nitrous oxide, and will at the ſame time tell you what was the previous ſtate of my mind on the ſubject.

When I ſat down to breathe the gas, I believed that it owed much of its effect to the prediſpoſing agency of the imagination, and had no expectation of its ſenſible influence on myſelf. Having ignorantly breathed a bag of common air without any effect, my doubts then aroſe to poſitive unbelief.

After a few inſpirations of the nitrous oxide, I felt a fulneſs in my head, which increaſed with each inhalation, until, experiencing ſymptoms which I thought indicated approaching fainting, I ceaſed to breathe it, and was then confirmed in my belief of its inability to produce in me any pleaſurable ſenſation.

But after a few ſeconds, I felt an immoderate flow of ſpirits, and an irreſiſtible propenſity to violent laughter and dancing, which, being fully conſcious of the violence of my feelings, and of their irrational exhibition, I made great but ineffectual efforts to reſtrain; this was my ſtate for ſeveral minutes. During the reſt of the day, I experienced a degree of hilarity altogether new to me. For ſix or ſeven days afterwards, I ſeemed to feel moſt exquiſitely at every nerve, and was much indiſpoſed to my ſedentary purſuits; this acute ſenſibility has been gradually diminiſhing; but I ſtill feel ſomewhat of the effects of this novel agent.

Your's truly,

To Mr. DAVY. M. M. COATES.

*June* 11*th*, 1800.

# DIVISION III.

## *ABSTRACTS from ADDITIONAL DETAILS.——OBSERVATIONS on the EFFECTS of NITROUS OXIDE, by Dr. BEDDOES.—CONCLUSION.*

### I. *Abstracts from additional Details.*

THE trials related in the following abſtracts, have been chiefly made ſince the publication of Dr. Beddoes's Notice. Many of the individuals breathed the gas from pure curioſity. Others with a diſbelief of its powers.

MR. WYNNE, M. P. breathed five quarts of diluted nitrous oxide, without any ſenſation. Six quarts produced fulneſs in the cheſt, heat in the hands and feet, and ſenſe of tenſion in the fingers, ſlight but pleaſant ſenſations. Seven quarts produced no new or different effects.

Mr. MACKINTOSH ſeveral times breathed nitrous oxide. He had ſenſe of fulneſs in the head, thrillings, tingling in the fingers, and generally pleaſurable feelings.

Mr. JOHN CAVE, Junr. from breathing four quarts of nitrous oxide, felt ſenſations as from ſuperior wine, and general pleaſant feelings.

Mr. MICHAEL CASTLE, from five quarts, experienced ſenſations of heat and thrilling, general ſpirits heightened conſiderably as from wine; afterwards, ſlight pain in the back of the head.

Mr. H. CARDWELL, from five quarts, had feelings ſo pleaſurable as almoſt to deſtroy conſciouſneſs; almoſt convulſed with laughter; for a long time could not think of the feeling without laughing; ſenſation of lightneſs for ſome time after.

Mr. JARMAN, from five quarts, great pleaſure, laughter, certainly better ſpirits, glow in the cheeks which continued long.

The gentleman who furnifhed the preceding detail, had heard of the effects of nitrous oxide, and was prepared to experience new fenfations: I therefore gave him a bag of common air which he refpired, believing it to be nitrous oxide; and was much furprifed that no effects were produced. He then breathed five quarts of nitrous oxide, and after the experiment, gave this account of his fenfations.

Rev. W. A. CANE, after inhaling the gas, felt the moft delicious fenfations accompanied by a thrill through every part of his body. He did not think it poffible fo charming an effect could have been produced. He had heard of the gas; but the refult of the experiment far exceeded his expectations.

*May 6th,* 1800.

Mr. JOSEPH PRIESTLEY from breathing nitrous oxide, generally had unpleafant fulnefs of the head and throbbing of the arteries, which prevented him from continuing the refpiration.

Dr. Beddoes mentioned in his Notice, that Mr. Josiah Wedgwood and Mr. Thomas Wedgwood, experienced rather unpleaſant feelings from the gas. Mr. Josiah Wedgwood has ſince repeated the trial, the effects were powerful, but not in the ſlighteſt degree pleaſant.

Mr. R. Boulton and Mr. G. Watt have been much leſs affected than any individuals.

Many other perſons have reſpired the gas, but as their accounts contain nothing unnoticed in the details, it is uſeleſs to particulariſe them.

The caſes of all the males who have been unpleaſantly affected ſince we have learnt to prepare the gas with accuracy, are related in this Section and in the laſt Diviſion. Thoſe who have been pleaſurably affected after a fair trial and whoſe caſes are not noticed, generally experienced fulneſs in the head, heat in the cheſt, pleaſurable thrillings, and conſequent exhilaration.

To perſons who have been unaccuſtomed to breathe through a tube, we have uſually given common air till they have learnt to reſpire with accuracy: and in cafes where the form of the mouth has prevented the lips from being accurately clofed on the breathing tube, by the advice of Mr. Watt, we have ufed a tin plate conical mouth-piece fixed to the cheeks, and accurately adapted to the lips; by means of which precautions, all our later trials have been perfectly conclufive.

II. *Of the effects of Nitrous Oxide upon perfons inclined to hyfterical and nervous affections.*

The cafe of Mifs —— N. and other cafes, detailed by Dr. Beddoes in his Notice, feemed to prove that the action of nitrous oxide was capable of producing hyfterical and nervous affections in delicate and irritable conftitutions.

On this fubject, we have lately acquired additional facts.

Mifs E. a young lady who had been fubject to hyfteric fits, breathed three quarts of nitrous oxide mingled with much common air, and felt no effects but a slight tendency to fainting. She then breathed four quarts of pure nitrous oxide: her first inspirations were deep, her last very feeble. At the end she dropt the bag from her lips, and continued for some moments motionless. Her pulse which at the beginning of the experiment was strong, appeared to me to be at this time, quicker and weaker. She soon began to move her hands and talked for some minutes incoherently, as if ignorant of what had passed. In less than a quarter of an hour, she had recovered, but could give no account of her sensations. A certain degree of languor continued through the day.

A young lady who never had hysterical attacks, wished to breathe the gas. I informed her of the disagreeable effects it had sometimes produced, and advised her if she had the slightest tendency to nervous affection, not to make the trial. She persisted in her resolution.

To ascertain the influence of imagination, I first gave her a bag of common air, which ſhe declared produced no effect. I then ordered for her a quart of nitrous oxide mingled with two quarts of common air; but from the miſtake of the perſon who prepared it, three quarts of nitrous oxide were adminiſtered with one of common air. She breathed this for near a minute, and after the experiment, deſcribed her ſenſations as unpleaſant, and ſaid ſhe felt at the moment as if ſhe was dying. The unpleaſant feelings quickly went off, and a few minutes after, ſhe had apparently recovered her former ſtate of mind. In the courſe of the day, however, a violent head-ache came on, and in the evening after ſhe had taken a medicine which operated violently, hyſterical affections were produced, followed by great debility. They occaſionally returned for many days, and ſhe continued weak and debilitated for a great length of time.

Mrs. S. a delicate lady, liable to nervous affections who had heard of the caſes juſt detailed, chofe to breathe the gas. By three quarts fhe was thrown into a trance, which lafted for three or four minutes. On recovering, fhe could give no account of her feelings, and had fome languor for half an hour afterwards.

Thefe phænomena have rendered us cautious in adminiftering the gas to delicate females. In a few inftances however, it has been taken by perfons of this clafs, and even by thofe inclined to hyfterical and nervous complaints with pleafurable effects.

Mifs L. a young lady who had formerly had hyfterical fits, breathed a quart of nitrous oxide with three quarts of common air without effects. Two quarts of nitrous oxide with one of common air produced a flight giddinefs; four quarts of nitrous oxide produced a fit of immoderate laughter, which was fucceeded by flight exhilaration, her fpirits were good throughout the day, and no depreffion followed.

Miſs B. Y—— and Miſs S. Y—— both delicate but healthy young ladies, were affected very pleaſantly; each by three quarts of nitrous oxide, the firſt time of reſpiring it. Miſs B Y—— continued exhilarated and in high ſpirits for ſome hours after the doſe. Miſs S. Y— had a ſlight head-ache, which did not go off for ſome hours.

Mrs. F. inclined to be hyſterical, breathed four quarts of nitrous oxide mingled with common air. She was giddy and deſcribed her feelings as odd; but had not the ſlighteſt languor after the experiment.

III. *Obſervations on the effects of Nitrous Oxide, by* DR. BEDDOES.

Neither my notes nor my recollection ſupply much in addition to what I formerly ſtated in the *Notice of Obſervations at the Pneumatic Inſtitution. Longman.* The gas maintains its firſt character as well in its effects on me, as in the benefit it confers on ſome of the paralytic, and the injury it does or threatens to the hyſterical and the exquiſitely ſenſible. I find that five or ſix quarts operate as powerfully as ever. I ſeem to make a given quantity go farther by holding my breath ſo that the gas may be abſorbed in a great degree without returning into the bag, and therefore, be as little heated before inſpiration as poſſible.—This may be fancy.

After innumerable trials, I have never once felt laſſitude or depreſſion* Moſt commonly

* Of the facts on which Brown founded his law of indirect debility, no prudent man will loſe ſight either in practiſing or ſtudying medicine. They are incontrovertible.—And our new facts may doubtleſs be conciliated to the Brunonian doctrine.

But to ſuppoſe that the expenditure of a quality or a ſubſtance or a ſpirit, and its renewal or accumulation are the general principles of animal phænomena, ſeems to me a grievous and baneful error. I believe it often happens that excitement and excitability increaſe, and that they oftener decreaſe together;—In ſhort, without generalizing in a manner, of which Brown and ſimilar theoriſts had no conception, our notions of the living world will in my opinion, continue to be as confuſed as the elements are ſaid to have been in chaos. On ſome future occaſion, I may preſume to point

I am ſenſible of a grateful glow *circum præcordia*. This has continued for hours.—In two or three inſtances only has exhalation failed to be followed by pleaſurable feeling, it has never been followed by the contrary. On a few occaſions before the gas was exhauſted, I have found it impoſſible to continue breathing.

The pulſe at firſt becomes fuller and ſtronger. Whenever, after expoſure to a cold wind, the warmth of the room has created a glow in the cheeks, the gas has increaſed this to ſtrong fluſhing—which common air breathed in the ſame way, failed to do.

Several times I have found that a cut which had ceaſed to be painful has ſmarted afreſh, and on taking two doſes in ſucceſſion, the ſmarting ceaſed in the interval and returned during the ſecond reſpiration. I had no previous expectation of the firſt ſmarting.

out the region through which I imagine the path to wind, that will lead the obſervers of ſome diſtant generation to a point, whence they may enjoy a view of the ſubtle, buſy and intricate movements of the organic creation as clear as Newton obtained of the movements of the heavenly maſſes.

The only time I was near rendering myſelf inſenſible to preſent objects by very carefully breathing ſeveral doſes in quick ſucceſſion, I forcibly exclaimed, TONES !—In fact, beſides a general thrilling, there ſeemed to be quick and ſtrong alterations in the degree of illumination of all ſurrounding objects ; and I felt as if compoſed of finely vibrating ſtrings. On this occaſion, the ſkin ſeemed in a ſtate of conſtriction and the lips glued to the mouth-piece, and the mucous membrane of the lungs contracted, but not painfully. However, no conſtriction or corrugation of the ſkin could be ſeen. I am conſcious of having made a great number of obſervations while breathing, which I could never recover.

Immediately afterwards I have often caught myſelf walking with a hurried ſtep and buſy in ſoliloquy. The condition of general ſenſation being as while hearing chearful muſic, or after good news, or a moderate quantity of wine.

Mr. John Cave, Junr. and his three friends, as well as others, compared the effects to Champagne. Moſt perſons have had the idea of the effect of fermented liquors excited by the gas. It were to be wiſhed that we had, for a ſtandard of compariſon, obſervations on the effect of theſe liquors as diverſified and as accurate as we have obtained concerning the gas; nor would more uniformity in the action of theſe ſubſtances be obſerved if the enquiry were ſtrictly purſued. Opium and ſpirits ſeem, in particular ſtates to ſicken and diſtreſs in the firſt inſtance; how differently does wine at an early hour and faſting act upon thoſe who are accuſtomed to take it only after dinner!

I thought it might be an amuſing ſpectacle to ſee the different tints of blood flowing from a wound by a leech in conſequence of breathing different airs. The purple from the nitrous oxide was very evident. Oxygene, we thought, occaſioned a quicker flow and brighter color in the blood. In another experiment, an inflamed area round the puncture from a leech applied the day before, was judged by ſeveral ſpectators to become much more crimſon on the reſpiration of about 20 quarts of oxygene gas, which poſſibly acts more powerfully on inflamed parts.* Theſe and many ſimilar experiments, require to be repeated on the blood of ſingle arteries opened in warm and cold animals.

It has appeared to me that I could hold my breath uncommonly long when reſpiring oxygene gas mixed with nitrous oxide. While trying this to-day, (17th June), I thought the ſenſe of ſmell much more acute after the nitrous oxide than before I began to reſpire at all; and then I felt conſcious that this increaſed acuteneſs had before repeatedly occurred—a

* After writing this, I was preſent when an invalid, in whoſe foot the gout, after much wandering, had at laſt fixed, breathed 12 quarts of oxygene gas. While breathing, he eagerly pointed to the inflamed leg; and afterwards ſaid he had felt in it a new ſenſation, ſomewhat like tenſion.—I never had ſeen oxygene reſpired where there was ſo much local inflammation.

June 18. After four quarts of oxygene with 6 of nitrous oxide and then 6 of nitrous oxide alone, violent itching of the wounds made by the leech; and redneſs and tumour.—Both had healed, and I did not expect to feel any thing more from them.—I tried this again with two doſes of nitrous oxide—The yellow halo round one wound changed to crimſon, and there was ſo much ſtinging and ſwelling that I feared ſuppuration.—Abſorption here was rapid.

fact very capable, I apprehend, of a pneumatological interpretation.

Time by my feelings has always appeared longer than by a watch.

I thought of trying to obferve whether while I alternately breathed quantities of nitrous oxide and oxygene gas and common air, I could obferve any difference in the operation of a blifter beginning to bite the fkin. It would be of confequence to afcertain the effect of regulating by compreffion the flow of blood, while ftimulants of various kinds (and heated bodies among the reft) were applied to or near the extremities—becaufe in crifipelas and various inflammatory affections, a ready and pleafant cure might be effected by partial compreffion of the arteries going to the difeafed part; and a great improvement in practice thus obtained.

But I fhould run into an endlefs digreffion, were I to enumerate poffible phyfiological experiments with artificial airs, or to fpeculate on the mechanical improvement of medicine, which at prefent as far as mechanical means of affecting the living ſyſtem are concerned, is with us in a ſtate that would almoſt diſgrace a nation of ſavages.

## IV. Conclusion.

From the facts detailed in the preceding pages, it appears that the immediate effects of nitrous oxide upon the living ſyſtem, are analogous to thoſe of diffuſible ſtimuli. Both increaſe the force of circulation, produce pleaſurable feeling, alter the condition of the organs of ſenſation, and in their moſt extenſive action deſtroy life.

In the mode of operation of nitrous oxide and diffuſible ſtimuli, conſiderable differences however, exiſt.

Diffuſible ſtimuli act immediately on the muſcular and nervous fibre. Nitrous oxide operates upon them only by producing peculiar changes in the compoſition of the blood.

Diffuſible ſtimuli affect that part of the ſyſtem moſt powerfully to which they are applied, and act on the whole only by means of its ſympathy with that part. Nitrous oxide in combination with the blood, is univerſal in its application and action.

We know very little of the nature of excitement; as however, life depends immediately on certain changes effected in the blood in reſpiration, and ultimately on the ſupply of certain nutritive matter by the lymphatics; it is reaſonable to conclude, that during the action of ſtimulating ſubſtances, from the increaſed force of circulation, not only more oxygene and perhaps nitrogene muſt be combined with the blood in reſpiration,* but likewiſe more fluid nutritive matter ſupplied to it in circulation.

* See Dr. Beddoes's *Conſiderations, part* 1. *page* 26. His obſervations in the note in the laſt ſection, will likewiſe apply here.—Is not healthy living action dependant upon a certain equilibrium between the principles ſupplied to the blood by the pulmonary veins from reſpiration and by the lymphatics from abſorption? Does not ſenſibility more immediately depend upon reſpiration? Deprive an animal under ſtimulation, of air, and it inſtantly dies; probably

By this oxygene and nutritive matter excitability may be kept up: and exhaustion consequent to excitement only produced, in consequence of a deficiency of some of the nutritive principles, which are supplied by absorption.

When nitrous oxide is breathed, nitrogene (a principle under common circumstances chiefly carried into the blood by the absorbents in fluid compounds) is supplied in respiration; a greater quantity of oxygene is combined with the blood than in common respiration, whilst less carbonic acid and probably less water are evolved.

Hence a smaller quantity of nutritive matter is probably required from the absorbents during the excitement from nitrous oxide, than during the operation of stimulants; and in consequence, exhaustion rom the expenditure of nutritive matter more seldom occasioned.

if absorption could be prevented, it would likewise speedily die. It would be curious to try whether intoxication from fermented liquors cannot be prevented by breathing during their operation, an atmosphere deprived of part of its oxygene.

Since Reſearch III. has been printed, I have endeavoured to aſcertain the quantities of nitrogene produced when nitrous oxide is reſpired for a conſiderable time. In one experiment, when I breathed about four quarts of gas in a glaſs bell over impregnated water for near a minute, it was diminiſhed to about two quarts; and the reſiduum extinguiſhed flame.

Now the experiments in Reſearch II. prove that when nitrous oxide is decompoſed by combuſtible bodies, the quantity of nitrogene evolved is rather greater in volume than the pre-exiſting nitrous oxide. Hence much of the nitrogene taken into the ſyſtem during the reſpiration of nitrous oxide, muſt be either carried into new combinations, or given out by the capillary veſſels through the ſkin.

It would be curious to aſcertain whether the quantity of ammoniac in the ſaline matters held in ſolution by the ſecreted fluids is increaſed after the reſpiration of nitrous oxide. Experiments made upon the conſumption of nitrous oxide mingled with atmoſpheric air by the ſmaller animals, would go far to determine whether any nitrogene is given out through the ſkin.

The various effects of nitrous oxide upon different individuals and upon the ſame individuals at different times, prove that its powers are capable of being modified both by the peculiar condition of organs, and by the ſtate of general feeling.

Reaſoning from common phænomena of ſenſation, particularly thoſe relating to heat, it is probable that pleaſurable feeling is uniformly connected with a moderate increaſe of nervous action; and that this increaſe when carried to certain limits, produces mixed emotion or ſublime pleaſure; and beyond thoſe limits occaſions abſolute pain.

Comparing the facts in the laſt diviſion, it is likely that individuals poſſeſſed of high health and little ſenſibility, will generally be leſs pleaſurably affected by nitrous oxide than ſuch as have more ſenſibility, in whom the emotions will ſometimes ſo far enter the limits of pain as to become ſublime;* whilſt the nervous action in ſuch as have exquiſite ſenſibility, will be ſo much increaſed as often to produce diſagreeable feeling.

Modification of the powers of nitrous oxide by mixture of the gas with oxygene or common air, will probably enable the moſt delicately ſenſible to reſpire it without danger, and even with pleaſurable effects: heretofore it has been adminiſtered to ſuch only in its pure form or mingled with ſmall quantities of atmoſpheric air, and in its pure form even the moſt robuſt are unable to reſpire it with ſafety for more than five minutes.

The muſcular actions† ſometimes connected

* Sublime emotion with regard to natural objects, is generally produced by the connection of the pleaſure of beauty with the paſſion of fear.

† The immortal HARTLEY has demonſtrated that all our motions are originally automatic, and generally produced by the action of tangible things on the muſcular fibre.

The common actions of adults may be diſtinguiſhed into two kinds; voluntary actions, and mixed automatic actions. The firſt are produced by ideas, or by ideas connected with paſſions. The ſecond by impreſſion, or by pleaſure and pain.

with the feelings produced by nitrous oxide, ſeem to depend in a great meaſure upon the particular habits of the individual; they will uſually be of that kind which is produced either by common pleaſurable feelings or ſtrong emotions.

Hyſterical affection is occaſioned by nitrous oxide, probably only in conſequence of the ſtrong emotion produced, which deſtroys the power of the will, and calls up ſeries of automatic motions formerly connected with a variety of leſs powerful but ſimilar feelings. *

The quickneſs of the operation of nitrous oxide, will probably render it uſeful in caſes of extreme debility produced by deficiency of

In voluntary action, regular aſſociations of ideas and muſcular motions exiſt: as when a chemiſt performs a preconceived experiment.

In mixed automatic actions, the ſimple motions produced by impreſſion are connected with ſeries of motions formerly voluntary, but now produced without the intervention of ideas: as when a perſon accuſtomed to play on the harpſichord, from accidentally ſtriking a key, is induced to perform the ſeries of motions which produce a well-remembered tune.

Evidently the muſcular actions produced by nitrous oxide are mixed automatic motions.

common exciting powers. Perhaps it may be advantageoufly applied mingled with oxygene or common air, to the recovery of perfons apparently dead from fuffocation by drowing or hanging.

The only difeafes in which nitrous oxide has been hitherto employed, are thofe of defficient fenfibility.—An account of its agency in paralytic affections, will be fpeedily publifhed by Dr. Beddoes.

As by its immediate operation the tone of the irritable fibre is increafed, and as exhauftion rarely follows the violent mufcular motions fometimes produced by it, it is not unreafonable to expect advantages from it in cafes of fimple mufcular debility.

The apparent general tranfiency of its operation in the pure form in fingle dofes has been confidered as offering arguments againft its power of producing lafting changes in the conftitution. It will, however, be eafy to keep up excitement of different degrees of intenfity for a great length of time, either by adminiftering the unmingled gas in rapid ſucceſſive doſes, or by preſerving a permanent atmoſphere, containing different proportions of nitrous oxide and common air, by means of a breathing chamber.* That ſingle doſes nevertheleſs, are capable of producing permanent effects in ſome conſtitutions, is evident, as well from the hyſterical caſes as from ſome of the details—particularly that of Mr. M. M. Coates.

As nitrous oxide in its extenſive operation appears capable of deſtroying phyſical pain, it may probably be uſed with advantage during ſurgical operations in which no great effuſion of blood takes place.

From the ſtrong inclination of thoſe who have been pleaſantly affected by the gas to reſpire it again, it is evident, that the pleaſure produced, is not loſt, but that it mingles with the maſs of feelings, and becomes intellectual pleaſure, or hope. The deſire of ſome individuals acquainted with the pleaſures of nitrous oxide for the gas has been often ſo ſtrong as to induce them to

* See R. IV. Div. I. page 478.

breathe with eagerneſs, the air remaining in the bags after the reſpiration of others.

As hydrocarbonate acts as a ſedative,† and diminiſhes living action as rapidly as nitrous oxide increaſes it, on the common theory of excitability‡ it would follow, that by differently modifying the atmoſphere by means of this gas and nitrous oxide, we ſhould be in poſſeſſion of a regular ſeries of exciting and depreſſing* powers applicable to every deviation of the conſtitution from health: but the common theory

† R. IV. Div. I. page 467.

‡ That of Brown modified by his diſciples.

* Suppoſing the increaſe or diminution of living action when produced by different agents, uniform, ſimilar and differing only in degree; it would follow, that certain mixtures of hydrocarbonate and nitrous oxide, or hydrogene and nitrous oxide, ought to be capable of ſupporting the life of animals for a much longer time than pure nitrous oxide. From the experiments in Ref. III. Div. I. it appears however, that this is not the caſe.

It would ſeem, that in life, a variety of different corpuſcular changes are capable of producing phænomena apparently ſimilar; ſo that in the ſcience of living action, we are incapable of reaſoning concerning cauſes from effects.

of excitability is moſt probably founded on a falſe generaliſation. The modifications of diſeaſed action may be infinite and ſpecific in different organs; and hence out of the power of agents operating on the whole of the ſyſtem.

Whenever we attempt to combine our ſcattered phyſiological facts, we are ſtopped by the want of numerous intermediate analogies; and ſo looſely connected or ſo independant of each other, are the different ſeries of phænomena, that we are rarely able to make probable conjectures, much leſs certain predictions concerning the reſults of new experiments.

An immenſe maſs of pneumatological, chemical, and medical information muſt be collected, before we ſhall be able to operate with certainty, on the human conſtitution.

Pneumatic chemiſtry in its application to medicine, is an art in infancy, weak, almoſt uſeleſs, but apparently poſſeſſed of capabilities of improvement. To be rendered ſtrong and mature, ſhe muſt be nouriſhed by facts, ſtrengthened by exerciſe, and cautiouſly directed in the application of her powers by rational ſcepticiſm.

# APPENDIX.

## No. I.

### *Effects of Nitrous Oxide on Vegetation.*

In July 1799, I introduced two ſmall plants of ſpurge into nitrous oxide, in contact with a little water over mercury; after remaining in it two days, they preſerved their healthy appearance, and I could not perceive that any gas had been abſorbed. I was prevented by an accident, from keeping them longer in the gas.

A ſmall plant of mint introduced into nitrous oxide and expoſed to light, in three days became dark olive and ſpotted with brown; and in about ſix days was quite dead.—Another ſimilar plant, kept in the dark in nitrous oxide, did not alter in color for five days, and at the end of feven days, was only a little yellower than before. I could not afcertain whether any gas had been abforbed.

I introduced into nitrous oxide through water, a healthy budding rofe, thinking that its colors might be rendered brighter by the gas. I was difappointed, it very fpeedily faded and died; poffibly injured by the folution of nitrous oxide in water.

Of two rows of peas juft appearing above ground; I watered one with folution of nitrous oxide in water, and the other with common water daily, for a fortnight. At the end of this time, I could perceive no difference in their growth, and afterwards they continued to grow equally faft.

I introduced through water into fix phials, one of which contained hydrogene, one oxygene, one common air, one hydrocarbonate, one carbonic acid, and one nitrous oxide, fix fimilar plants of mint, their roots being in contact with water and their leaves expoſed to light.

The plant in carbonic acid began to fade in leſs than two days, and in four was dead. That in hydrogene died in leſs than five days; that in nitrous oxide did not fade much for the firſt two days, but on the third, drooped very much, and was dead at the ſame time as that in hydrogene. The plant in oxygene for the firſt four days, looked flouriſhing and was certainly of a finer green than before, gradually however, its leaves became ſpotted with black and dropped off one by one, till at the end of ten days they had all diſappeared. At this time the plant in common air looked ſickly and yellow, whilſt that in hydrocarbonate was greener and more flouriſhing than ever.

I have detailed theſe experiments not on account of any important concluſions that may be drawn from them; but with a view of inducing others to repeat them, and to examine the changes effected in the gaſes. If it ſhould be found by future experiments, that hydrocarbonate generally increaſed vegetation, it would throw ſome light upon the uſe of manures, containing putrefying animal and vegetable ſubſtances, from which this gas is perpetually evolved.

The chemiſtry of vegetation though immediately connected with agriculture, the art on which we depend for ſubſiſtence, has been but little inveſtigated. The diſcoveries of Prieſtley and Ingenhouſz, ſeem to prove that it is within the reach of our inſtruments of experiment.

# No. II.

## APPROXIMATIONS

TO THE

*Composition and Weight of the aëriform*

*COMBINATIONS of NITROGENE.*

At temperature 55°, and atmofpheric preffure 30.

| Nitrogene | | 100 Cubic In. | | grains | | Nitrogene | Oxygene |
|---|---|---|---|---|---|---|---|
| | | Nitrogene | | 30.04 | | | |
| | | Oxygene | | 35.06 | | | |
| | With Oxygene | Atmofpher.air | weigh | 31.10 | 100 grains are compofed of | 73.00 | 27.00 |
| | With Oxygene | Nitrous oxide | weigh | 50.20 | 100 grains are compofed of | 63.30 | 36.70 |
| | With Oxygene | Nitrous gas | weigh | 34.26 | 100 grains are compofed of | 44.05 | 55.95 |
| | With Oxygene | Nitric acid | weigh | 76.00 | 100 grains are compofed of | 29.50 | 70.50 |
| | | | | | | Nitrogene | Hydrogene |
| | With hydrogene | Ammoniac | weigh | 18.05 | 100 grains are compofed of | 80.00 | 20.00 |

## No. III.

### *Additional Obſervations.*

*a.* In Reſ. 1ſt. Div. IV. Sect. III. in the analyſis of nitrous gas by pyrophorus, as no abſorption took place when the reſidual nitrogene was expoſed to water, I inferred that if any carbonic acid was formed it was in quantity ſo minute, as to be unworthy of notice. A few days ago, I compleatly decompoſed a quantity of nitrous gas by pyrophorus, when the reſidual nitrogene was expoſed to ſolution of ſtrontian, the fluid became ſlightly clouded; but no perceptible abſorption took place.

*b.* If there was the leaſt probability in any of Dr. Girtanner's ſpeculations on the compoſition of Azote,* the experiments on the exhauſted capacity ‡ of the lungs in Reſ. III. might be ſuppoſed inconcluſive. But there appears to

* Annales de Chimie, 100; and Mr. Tilloch's Phil. Magazine. 24.

‡ I regret much that I could not procure Dr. Menzies's obſervations on Reſpiration, while I was making the experiments on the capacity of the lungs: they would probably have ſaved me ſome labor.

be no more reafon for fuppofing that hydrogene is converted into nitrogene by refpiration, than for fuppofing that it is converted into water, carbonic acid or oxygene; for all thefe products are evolved when that gas is refpired. From the comparifon of Exp. 1 with Exp. 3, Ref. iii. Div. ii. Sec. 4, it is almoft demonftrated that no afcertainable change is effected in hydrogene by refpiration. The experiment of the accurate Scheele in which hydrogene after being refpired thirty times in a bladder wholly loft its inflammability, may be eafily accounted for from its mixture with the refidual gafes of the lungs.

About a fortnight ago, I refpired, after forced voluntary exhauftion of my lungs, my nofe being accurately clofed, three quarts of hydrogene in a filk bag, at four intervals, for near five minutes. After this it was highly inflammable, and burnt with a greenifh white flame in contact with the atmofphere; but was not fo explofive as before.*

* If loofely combined carbon exifts in venous blood, hydrogene may probably diffolve a portion of it when

c. From what we have lately heard of the curious experiments of Mr. Volta and Mr. Carlifle, it is very probable that the converfion of nitrous gas into nitrous oxide when expofed to wetted zinc, copper and tin, in contact with mercury, as defcribed in Ref. I. Div. V. may in fome meafure depend on the action of the galvanic fluid. Whilft I was engaged in the experiments on this converfion, Dr. Beddoes * mentioned to me fome curious facts noticed by Humboldt and Ritter, relating to the oxydation of metals by the decompofition of water, which induced me

refpired and become flightly carbonated. At leaft there is as much probability in the fuppofition that carbon in loofe affinity may combine with hydrogene at 98° as that it may combine with oxygene.

* Dr. BEDDOES has fince favoured me with the following account of thefe facts.

" Mr. Humboldt (ueber die gereizte Fafer I. 473, 1797) quotes part of a letter from Dr. Afh, in which it is faid that *if two finely polifhed plates of homogeneous zinc be moiftened and laid together, little effect follows—but if zinc and filver be tried in the fame way, the whole furface of the filver will be covered with oxydated zinc. Lead and quickfilver act as powerfully on each other, and fo do iron and* to examine the phænomena with more attention than I ſhould have otherwiſe done.—I recollect obſerving that ſome of the wetted zinc

*copper.*—Mr. Humboldt (p. 474) ſays that, in repeating this experiment, he ſaw air-bubbles aſcend, which he ſuppoſes to have been hydrogene gas from the decompoſition of water—When he placed zinc ſimply on moiſt glaſs, the ſame phænomena took place, but more ſlowly and later. The quantity of oxyd of zinc upon the glaſs alone was in 20 hours to that on the ſilver as one to three.

In a very ingenious but obſcurely written tract by Mr. Ritter, entitled, *Evidence that the galvanic action exiſts in organic nature, 8vo. Jena,* 1800—The author obſerves, that the care of Dr. Aſh and Mr. Humboldt that the metals ſhould touch each other in as many points as poſſible was ſuperfluous, even if we could grant that two metallic plates might be made by poliſhing, to touch in a number of points. To ſhew that it was ſufficient if by touching in one point only they ſhould form a compleat galvanic circle, he dropped a ſingle drop of diſtilled water upon the buſt of a large ſilver coin. A piece of pure zinc was placed with its one end on the edge of the coin, while the other was ſupported by a bit of glaſs. The drop of water was neither in contact with the glaſs nor with the point at which the metals touched. The materials were left in this ſituation for four hours at the temperature of 68°. On taking them apart, the water had become quite milky and had half diſappeared; and Mr. Ritter actually ſeparated a quantity of white oxide that had been produced in the experiment.

The pieces of metal were cleaned and laid together in filings in nitrous gas on the ſide of the jar not in contact with the ſurface of mercury, were very ſlowly oxydated. Whilſt on the ſurface of the mercury where ſmall globules of that ſubſtance were mingled with the filings of zinc, the decompoſition went on much more rapidly;

the ſame manner, only that now a piece of paper was put between the metals at their former point of contact. In four hours firſt, and afterwards in ten, a faint ring of oxide only had been produced of which the quantity could not be eſtimated, nor could it be ſeparated. In this caſe, the zinc had ſcarce loſt any thing of its ſplendour; in the former it had been corroded. In many repetitions of the experiment, he found that far more oxide was formed when the metals touched, than when they were ſeparated to the ſlighteſt diſtance by an inſolating body, even air.

On expoſing theſe apparatuſes with ſomewhat more water to a conſiderable heat for four minutes, the water in the interrupted circle continued quite clear, while that in the other had become milk-white.

The ſame phænomena were preſented by other pairs of metals in a degree proportional to their galvanic activity; viz. by zinc and molybdæna, zinc and biſmuth, zinc and copper, as alſo with tin and ſilver, tin and molybdæna, and lead and ſilver. The experiment with tin was particularly deciſive, for when in contact with no other metal it was ſcarcely at all oxydated by water, though oxydation took place when tin was brought into contact with ſilver, poſſibly through the medium of the moiſture, a ſeries of galvanic circles were formed.

*d.* In Reſ. II. Div. I. it is ſtated, that nitrous oxide during its ſolution by common water, expels about $\frac{1}{16}$ of atmoſpheric air the volume of the water being unity.

and both were connected at the other end by a drop of water—What therefore took place in Dr. Aſh's experiment, aroſe from an aggregation of galvanic circles of different forms.

By the foregoing experiments, concludes Mr. Ritter, which though capable of the moſt various modifications, uniformly coincide in their main reſult, it is abundantly proved that *galvanic circles can be formed of merely inorganic bodies, by whoſe completion there is produced an action which ceaſes when the circle is opened.* The manner in which this has been ſhewn, proves alſo that *this action can effectuate ſenſible modifications in organic bodies*; and the proceſs by which theſe modifications have been effected, made it evident that they *were not conſequences of a momentary action of the circle, but of an action that is kept up while the circle remains entire*; for the proceſs which brought this action under the cognizance of the ſenſes went on, while the circle was unbroken, and its figure not brought back to that of a line.

It is ſcarce neceſſary to obſerve that the experiments here quoted, are far from being the only ones on which the above concluſions reſt." *T. B.*

From the delicate experiments of Dr. Pearſon, on the paſſage of the electric ſpark through water, it appears however probable, that much more than $\frac{1}{16}$ of atmoſpheric air is ſometimes held in ſolution by that fluid,* poſſibly the whole of the air is not expelled by nitrous oxide, owing to ſome unknown law of ſaturation by which an equilibrium of affinity is produced, forming a triple compound.

* Poſſibly a ratio exists between the ſolubility of gaſes in water, and the ſolubility of water in gaſes. It is probable from Mr. Wm. Henry's curious experiments on the muriatic acid, that the abſolute quantity of water in *many* gaſes, may be aſcertained by means of its decompoſition by the electric ſpark.

## No. IV.

DESCRIPTION OF A

# MERCURIAL AIR-HOLDER,

Suggeſted by an inſpection of Mr. WATT's Machine for containing Factitious Airs,

*By WILLIAM CLAYFIELD.*

SEVERAL modes of counteracting the preſſure of a decreaſing column of mercury having been thought of in conjunction with Mr. W. Cox, the following was at laſt adopted as the moſt ſimple and effectual.

Plate 1 Fig. 1, repreſents a ſection of the machine, which conſiſts of a ſtrong glaſs cylinder A cemented to one of the ſame kind B, fitted to the ſolid block C, into which the glaſs tube D is cemented for conveying air into the moveable receiver E.

The braſs axis F, Fig. 2, having a double bearing at *a, a*, is terminated at one end by the wheel G, the circumference of which is equal to the depth of the receiver, ſo that it may be drawn to the ſurface of the mercury by the cord *b* in one revolution; to the other end is fitted the wheel H, over which the balance cord *c* runs in an oppoſite direction in the ſpiral groove *e*, a front view of the wheel H is ſhewn at Fig. 3.

Having loaded the receiver with the weight I, ſomething heavier than may be neceſſary to force it through the mercury, it is balanced by the ſmall weight K, which hangs from that part of the ſpiral where the radius is equal to that of the wheel G, from this point the radius of the ſpiral muſt be increaſed in ſuch proportion, that in every part of its circuit, the weight K may be an exact counterpoiſe to the air-holder. In this way, ſo little friction will be produced, that merely plunging the lower orifice of the tube D under mercury contained in the ſmall veſſel L, will be ſufficient to overcome every reſiſtance, and to force the gas diſcharged from the beak of a retort into the receiver, where whatever may be its quantity, it will be ſubjected to a preſſure exactly correſponding to that of the atmoſphere. The edge of the wheel H being graduated, the balance cord *c* may be made to indicate its volume.

Should it at any time be neceſſary to reduce the preſſure to the medium ſtandard of the barometer, it may eaſily be done by graduating the lower end of the tube D, and adding to the weights I or K, as may be found neceſſary ; the ſurface of the mercury in the tube pointing out the increaſe or diminution.

The concavity at the top of the internal cylinder is intended to contain any liquid it may be thought proper to expoſe to the action of the gas.

The upper orifice *f*, with its ground-ſtopper, is particularly uſeful in conveying air from the retort *g*, with its curved neck, into the receiver, without its paſſing through the tube D. In all caſes where a rapid extrication of gas is expected the retort *g*, ſhould be firmly luted to the orifice, and the weight I, removed from the top of the receiver, this by diminiſhing the preſſure, will admit the gas to expand freely in the air-holder at the inſtant of its formation, and prevent an exploſion of the veſſels. The ſame caution muſt be obſerved whenever any inflammation of gas is produced by the electric ſpark.

The air may be readily transferred through water or even mercury by the tube *h*, Fig. 1.

To prevent an abſorption of mercury in caſe of a condenſation taking place in the retort made uſe of for generating air, Mr. Davy has applied the ſtop-cock *i*, to which the neck is firmly luted. This ſtop-cock is likewiſe of great ſervice in ſaturating water with acid or alkaline gaſes, which may be effected by luting one end of the tube *k* to the ſtop-cock, and plunging the other into the fluid in the ſmall veſſel *l*, cemented at top, and terminating in the bent funnel *m*—the tube *h* having been previouſly removed, and the lower orifice of the tube D either ſunk to a conſiderable depth in mercury, or cloſed with a ground ſtopper. The bend of the funnel *m*, may be accurately cloſed by the introduction of a few lines of mercury.

The application of the ſtop-cock *n*, has enabled Mr. Davy to perform ſome experiments on reſpiration with conſiderable accuracy.

*Note.* This apparatus was firſt deſcribed in the third part of Dr. Beddoes's Conſiderations; its relation to Mr. Davy's experiments with the improvements it has lately received, may probably be deemed ſufficient to excuſe the re-printing it.—The weight I. Fig. 2, having been omitted in the plate, the reader muſt ſupply the deficiency,

W. C.

# PROPOSAL

FOR

## THE PRESERVATION

OF

# ACCIDENTAL OBSERVATIONS

IN

# MEDICINE.

IN times beyond the reach of hiſtory, the medicinal application of ſubſtances could have ariſen from no other ſource than accident. Among articles of the materia medica of known origin, we are indebted to accident for ſome of the moſt precious.

Accident is every day preſenting to different individuals the ſpectacle of phænomena, ariſing from uncommon quantities of drugs on the one hand, and on the other, from uncommon conditions of the ſyſtem, where ordinary powers only have been knowingly or recently applied. What is ſaid of drugs may be extended to natural agents and mental affections.

From converſation with a variety both of medical practitioners and unprofeſſional obſervers, the author of this propoſal is perſuaded that ſuch authentic occurrences only, as have preſented themſelves to perſons now living would, if they could be brought together, compoſe a body of fact, ſo inſtructive to the philoſopher, and uſeful to the phyſician, that he deſpairs of finding a term worthy to characterize it.

In ſome caſes, the influence of unſuſpected powers would be detected. In others, reſources available to the purpoſe of reſtoring health in deſperate ſituations, would be directly preſented, or could be detected by a ſhort and eaſy proceſs of reaſoning. Some anomalous obſervations, by ſhewing the abſence or agency of conteſted cauſes, would perform the office of *experimenta crucis*—Unuſual affections occur of which an exact account would be among the means of removing from phyſic its opprobrious uncertainty: for this uncertainty frequently depends upon our inability to diſtinguiſh the ſubtler differences in caſes which reſemble each other in their groſſer features.

No ſtriking fact can be accurately ſtated, in conjunction with its antecedent and concomitant circumſtances, without improving our acquaintance with human nature. Our acquiſitions in this moſt important branch of knowledge, may be compared to a number of broken ſeries, of which we have not always more than one or two members. But every new acceſſion bids fair to fill up ſome deficiency; and a large ſupply would contribute towards connecting ſeries apparently independent, and working up the whole into one grand all-comprehending chain.

There are complaints, and thoſe by far too frequent, where no known proceſs has a claim to the title of *remedial*. Here the whole chance of preſervation depends on the phyſician's capacity for bringing together facts that have heretofore ſtood remote. But no power of combination can avail where there are no ideas to combine.

Every new obſervation therefore, may be conſidered as a ſtandard trunk, ſending forth analogies as ſo many branches crowned with bloſſoms, ſome of which cannot fail to be ſucceeded by ſalutary fruits. And were it not abſurd to extend the illuſtration of ſo plain a point, it might be added, that when by the continual interpofition of new trunks, the branches are brought near together, the produce of each will be ennobled by the action of their refpective principles of fecundation.

Whenever the author has been able to obtain certain information concerning any unufual appearance in animal nature, it has been his cuftom to preferve it; and among his papers he has memorandums which prove that to our prefent circumfcribed ideas concerning the dofe of medicines may be fometimes imputed failures in practice; that certain figns are not to be taken in the received fignification; and that many meafures are adopted or omitted to the detriment of invalids, becaufe it is affumed that circumftances are neceffarily connected which may exift feparately, or that one given natural operation is inconfiftent with another, to which it may really be fynchronous or next in order.

Affiduous obfervation of the daily ftates of the human microcofm will be the unfailing confequence of attention to its ftriking phænomena. Such is the progrefs of curiofity. Such the origin of all the fciences. The more uniformly clear the fky under which they tended their flocks, the lefs likely were the fhepherds of Chaldæa, to found the fcience of the ftars. And however the difpofition to ftudy aftronomy might have been ftrengthened by the coincidence between the heliacal rifing of Sirius and the overflowing of the Nile, it muft, I conceive, have been awakened by the afpect of meteors and eclipfes.

Whatever minute and authentic information this imperfect ftatement may produce, as foon as it fhall amount to a certain mafs, the author will prefent it to the public arranged. He flatters himfelf that no correfpondent will eke out by fuppofition the defect of genuine obfervation, without clearly diſtinguiſhing the one from the other. He ſtill more confidently hopes that none will be inſtigated by this advertiſement to exerciſe his invention in the manner of Pſalmanaſar and Chatterton. Whether any literary forgery can be innocent is queſtioned—but a forged medical report is a drawn dagger which the arm of a credulous phyſician may any day plunge into the heart of his defenceleſs patient. The author has heard ſome inconſiderate wits avow, that they have tranſmitted to the venders of quack medicines imaginary cures, atteſted by fictitious ſignatures; and it is not without apprehenſion from the propenſity of men to diſplay ingenuity and to relate wonders that he announces the preſent deſign. But he ſhall be on his guard, and hopes to baffle attempts at impoſition.

THOMAS BEDDOES.

Rodney-Place, Clifton, June 1800.

END.

# ERRATA.

| | | | |
|---|---|---|---|
| Page | 19 line | 15 | for *is* read *are* |
| — | 35 — | 7 — | for *principle* read *principles* |
| — | 42 — | 11 — | for *take* read *takes* |
| — | 68 Table | 5 — | for 5,88 read 15,88 |
| — | 94 — | 4 — | for $1\frac{1}{12}$ read $\frac{1}{12}$ |
| — | 95 — | 4 — | for 37 read 30,7 |
| — | 96 — | 3 — | for 38 read $\frac{1}{38}$ |
| — | 105 — | 9 — | for *exactitude* read *exactness* |
| — | 129 — | 21 — | for 41 read 4,1 |
| — | 132 — | 4 — | for *into* read *in* |
| — | 143 — | 13 — | for 25 read .25 |
| — | 186 — | 15 — | for *by* read *from* |
| — | 208 last line — | | for *abstracted* read *attracted* |
| — | 238 — | 5 — | for *gas* read *oxide* |
| — | 259 — | 4 — | for 12 read 2 |
| — | 283 — | 4 — | for *potash* read *iron* |
| — | 315 — | 14 — | dele *in* |
| — | 409 — | 15 — | for *respiration* read *expiration* |
| — | 464 — | 10 — | for *latter end* read *end* |
| — | 543 — | 3 | for *exhalation* read *inhalation.* |

A few literal errors are left to the reader's correction.

N. B. The term ignited is sometimes used to signify any temperature equal to or above a red heat, whether applied to solids, fluids, or aëriform substances.

The reasons for the use of the terms nitrogene and nitrous oxide, are given in Mr. Nicholson's Journal for January.

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