

## MR. WATT TO SIR JOSEPH BANKS.

Birmingham, May 5, 1784.

DEAR SIR,—I had the honour of writing to you on Sunday last, informing you that I had sent to Mr. De Luc the sequel of my letter to him on dephlogisticated air; since which, having been stupified by headaches, I have not been able to revise the letter till to-day, when I perceived an obscurity, in the wording of the passage where I mention, that litmus is no test of the saturation of the phlogisticated nitrous acid by alkalies; the words which follow should run thus—“for “the infusion of litmus *added to such a mixture will* “turn red, &c.”

The words I have under-scored, are what I wish to be inserted instead of “mixt with it,” which at present stands in the letter. The passage is about two-thirds down the second page. I am quite ashamed to be so troublesome, but hope you will excuse; and I remain, &c.

JAMES WATT.

## EXTRACT—SIR JOSEPH BANKS TO MR. WATT.

Soho Square, 11th May, 1784.

DEAR SIR,—Your paper commenced reading to the Royal Society on Thursday se'nnight; and last Thursday the postscript was read. Both appeared to meet with great approbation from large meetings of Fellows.

On Friday I received your favour, requesting a small alteration to be made in the postscript, which I have delivered to Dr. Blagden, our new Secretary,

who has undertaken that it shall be made before the papers are printed. [The sequel of this letter communicated an account of some experiments by M. Lavoisier, on a mode of making inflammable air by passing the steam of water through a red-hot iron tube.]

## EXTRACT—MR. DE LUC TO MR. WATT.

Londres, le 12me Mai, 1784.

Je suis charmé du parti que vous aviez pris d'authentifier vos lettres, et leurs dates ; et le Chevalier Banks s'y est prêté volontiers. La lettre au Dr. Priestley fut lue pendant son séjour ici ; et celle du 26. Novembre à moi, ainsi que votre addition, durent être lues Jeudi dernier. Je n'ai pas voulu les garder pour corriger ma copie de la première, et tirer copie de la seconde ; préférant qu'elles furent lues d'abord, et de les r'avoir ensuite. [The rest of this letter contains some remarks on M. Lavoisier's experiment, mentioned by Sir J. Banks ; on a supposed invention of a new steam engine, by Kempelen ; and on private matters.]

## EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 14th May, 1784.

Sir Joseph Banks has behaved with great civility and kindness in the affair of the letters. I had a letter from him the other day, advising they were all read. \* \* \* \* I cannot be sufficiently thankful for the daily instances you give me of your friendship and regard to our interest, which is the more flattering as coming from you.

It is such consolations as experiencing the regard of the worthy few, which make the bitter pill of life palatable. It is the next thing to self-approbation, and to sensible minds a necessary appendage to it, for without it, self-approbation cannot properly exist.

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## EXTRACT—MR. WATT TO MR. FRY OF BRISTOL.

Birmingham, 15th May, 1784.

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The papers which I mentioned to you that I had written, on the composition of water and dephlogisticated air, have been read at the Royal Society; I am told, with applause. If they are printed, I shall do myself the pleasure to send you a copy. But I have had the honour, like other great men, to have had my ideas pirated. Soon after I wrote my first paper on the subject, Dr. Blagden explained my theory to M. Lavoisier at Paris; and soon after that, M. Lavoisier invented it himself, and read a paper on the subject to the Royal Academy of Sciences. Since that, Mr. Cavendish has read a paper to the Royal Society on the same idea, without making the least mention of me. The one is a French Financier; and the other a member of the illustrious house of Cavendish, worth above £100,000,\* and does not spend £1000 per year. Rich men may do mean actions. May you and I always persevere in our integrity, and despise such doings. Adieu, my worthy friend!

JAMES WATT.

\* Mr. Watt probably meant to say £1,000,000.—ED.

MR. WATT TO SIR JOSEPH BANKS.

Birmingham, 21st May, 1784.

[Sends him a paper on a new method of preparing tests for acids and alkalies, to be laid before the Royal Society. Makes some comments on M. La-vosier's experiments on the production of inflammable air in the iron tube, &c.]

DR. BLAGDEN TO MR. WATT.

London, May 25, 1784.

SIR,—The Committee of Papers have ordered your two letters and postscript, on the production of air from water, to be printed; subject to your judgment, as to the best form under which they can appear. I am, therefore, to request that you would inform me, whether your first letter to Dr. Priestley, dated April 26th, 1783, should be published entire as it is, or be incorporated into the second or corrected letter, bearing date the 26th of November 1783. The only reason for suggesting this latter method is, that the opinions are most digested in that second letter; and to avoid repetitions. The advantage of publishing the first letter at full length would be, to shew the exact state of your sentiments on that subject at a certain period. It is absolutely at your option to decide upon whichever of those methods you shall prefer. Should your choice fall upon that of incorporating the two letters, I must request you to let me know what parts of the former you choose to be struck out, and how the remainder is to be placed; and, at the same time, be so good as to send me what you think

the properest title to be inserted before these papers in the Transactions. I have the honour to be, Sir,

Your obedient humble servant,

C. BLAGDEN.

P.S. Sir Joseph Banks has just received your account of a new Test for acids and alkalies; which shall be read to the Royal Society next Thursday. In § 2 of your paper on the Test, there is an expression "putrid acid fermentation." Is it to stand so, or do you mean "putrid and acid?"

EXTRACT—MR. WATT TO DR. BLAGDEN.

Birmingham, 27th May, 1784.

SIR,—My only reason for wishing my letter to Dr. Priestley to be read before the Royal Society, was to shew them what my ideas on the subject were, at the time it was written. On some other accounts, I would rather have wished it to be suppressed.

I therefore would propose, if it meets the approbation of the gentlemen of the Committee of Papers, that that letter should be wholly left out; and that in place of it, a note should be added to the second paragraphs of the letter to Mr. De Luc, following the words, "April 26th, 1783," to the following purport:—"Which letter Dr. Priestley received at London; and, after shewing it to several members of the Royal Society, he delivered it to Sir Joseph Banks the President, with a request that it might be read at some of the public meetings of the Society; but, before that could be complied with, the

“ author, having heard of Dr. Priestley’s new experiments, begged that the reading might be delayed. “ The letter, therefore, remained in the custody of “ the President until —— ; when, at the author’s request, it was read before the Society. It has been judged unnecessary to print that letter, as the essential parts of it are repeated, almost verbatim, in this letter to Mr. De Luc ; but to authenticate the date of the author’s ideas, the parts of it which are contained in the present letter are marked with double commas.”

As I have marked some passages in my letter to Mr. De Luc with double commas, by way of directing the reader’s attention to my conclusions, it will be necessary by this new arrangement to print those passages in italics, to distinguish them from the quotations. I am very sorry to give you the trouble of collating the two letters, and of marking off the passages wherein the same ideas are expressed in each ; but I must beg it of you as a favour, as it will come with more propriety from the hand of the Secretary to the Royal Society, than from mine.

If you shall judge it to be proper to insert upon the margin, the dates of my experiments mentioned in my letter to Mr. De Luc, they are as follows, § 7th May, 1783, § 8th June, § 9th July, 3d, § 10th July, 4th, § 12th Nov. 1st, § 13th Nov. 22d.

I am really at a loss what title to give the paper, but propose the following ;—“ Thoughts (conjectures) on the constituent parts of water, and of dephlogisticated air ; with an account of some experiments on that subject.” I am much obliged to you for your

correction of the Test paper. It should be “putrid “and acid fermentation.” It undergoes both in a high degree, if we may judge of its putrescence by the smell, and of the acidity by the colour of the liquor. \* \* \* \* I beg the favour of you to return my thanks to the gentlemen of the Committee of Papers, for the honour they do me in ordering my communication to be printed ; and that you would also accept of my thanks for your obliging letter, communicating their intentions.

I remain, with sincere esteem, your most obliged humble servant,

JAMES WATT.

EXTRACT—DR. BLACK TO MR WATT.

May 28th, 1784.

MY DEAR FRIEND,—The great length of time during which I have been your debtor requires some apology from me. It has been occasioned by the following circumstances. I had made you a promise that I should, in the course of last summer, prepare some of my lectures for the press. When the summer came, I found myself so much worn out with my winter's labours, and in such bad health, with a cough, and defluxion from my breast, that I was quite unfit to sit down to serious business ; and during the rest of that season I had other things, in the way of College and other business, which broke my time, and took up my attention in such a manner that I got nothing done. All this while I was ashamed to write to you, after the promise I had made.

In the beginning of last winter, when it became

necessary to drop for some time all thoughts of such undertakings, I sat down to write to you, but something prevented me from finishing my letter, and it remains unfinished to this day. In short, I feel that I am unfit to come under such engagements. I have not sufficient activity and spirits to be sure of fulfilling them ; and they are a load on my mind, which increases my disability.

I received Lavoisier's and De La Place's Memoir.\* Their method for measuring quantities of heat is ingenious, but they have not used it with accuracy in some cases ; and there is reason to suspect, from Mr. Wedgewood's experiments in this way, that it cannot be practised with exactness. I am told it was contrived by La Place. Be so good as to return my best compliments to Mr. De Luc, and many thanks for his trouble and attention to me.

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Few things have given me so much pleasure, as the opportunity I had, in the beginning of winter, to form an acquaintance with Mr. Boulton. His connexion with you had raised a strong desire in me to be acquainted with him ; and I found so much reason to be satisfied that the connexion is a fortunate and a comfortable one, that I was made happy on your account, as well as in forming a friendship with a man of so much merit and worth. Present my most respectful compliments to him, and be assured that I ever am, my dear friend,

Yours most faithfully,

JOSEPH BLACK.

\* "Mémoire sur la Chaleur, par MM. Lavoisier et De La Place," dated 18th June, 1783, and printed in the Mémoires de l'Académie for 1780.

## EXTRACT—MR. WATT TO PROFESSOR ROBISON.

Birmingham, May 31st, 1784.

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I have lately, through the importunity of my friends, been prevailed upon to have read before the Royal Society of London, a paper containing a new hypothesis on the constituent parts of water and of dephlogisticated air, which has so far met their approbation as to be ordered to be printed. It may seem rather bold in me to commence my publications in science by a new theory; and my natural timidity and diffidence would certainly have prevented me, if Mr. Lavoisier in France, having learned something about it from Dr. Blagden, had not adopted it as his own, and Mr. Cavendish, a year after the broaching of mine, had not published one of the same kind.

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## EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, June 6th, 1784.

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The Committee of Papers referred it to me to decide, whether I would have both the letters printed. But I preferred to print only the one to you; to mark with double commas the parts of it which were contained in the letter to Dr. Priestley; to add a note giving a short history of that letter, and certifying that it had been seen by many of the members, and left in the possession of the President until it was read; and to print the conclusions, which I had marked with double commas, in italics.

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DR. BLAGDEN TO MR. WATT.

London, 9th August 1784.

DEAR SIR,—Your paper is now going to be printed. I have marked off the similar parts, &c., according to your request, as well as I could make them out ; but, if it would be any satisfaction to you, the proof-sheets shall be sent to Birmingham for your correction. Whatever separate copies you may choose to have, send your order to Mr. Nichols, the printer, Red Lion Court, Fleet Street. \* \* \* \* Should you determine to have the proof-sheets sent down to you, let me know it as soon as you can ; otherwise you need not give yourself the trouble of answering this letter. Your paper on the test-liquor, &c., will be printed, I believe, toward the end of this month. I am, Sir, your obedient humble servant,

C. BLAGDEN.

MR. WATT TO DR. BLAGDEN.

Birmingham, 11th August 1784.

DEAR SIR,—I am very much obliged to you for the attention you have been pleased to bestow on my paper on dephilogisticated air.

I have no desire to see the proof-sheets, as I am satisfied that you would mark off with propriety the passages in the second letter which were mentioned in the first, and also that you are much more capable of correcting any grammatical errors, or inaccuracies of style, than I am ; and that favour I take the liberty to request of you, so far as it can be done consistently with your own convenience, and in the correction of a proof-sheet. \* \* \* \*

Mr. De Luc did me the favour to write to you lately, requesting that you would desire the printer to print fifty separate copies of my paper, which liberty I hope you will excuse. Mr. De Luc is still here, and desires to join in compliments to you.

I remain, with respect, dear Sir, your obliged humble servant,

JAMES WATT.

EXTRACT—MR. WATT TO DR. BLACK.

Birmingham, 11th Nov. 1784.

DEAR DOCTOR,—I sent you lately a copy of the paper on dephlogisticated air, which I communicated to the Royal Society, and which will be printed in the next volume of their Transactions. It is far from being well written, but I am every day more and more satisfied that the doctrines it contains are true, however bold they appeared at first. My bad health, and my avoerations, prevented me from sitting close at it, or thinking continually on the subject; it should therefore be considered as a parcel of detached scraps, rather than any attempt at system; which made me put it into the form of a letter.

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EXTRACT—M. PICTET OF GENEVA TO MR. DE LUC.

Genève, le 9. Mai 1785.

\* \* \* \*

Aux expériences de M. Watt, pour le dire en passant, j'avois et j'ai encore la foi la plus implicite, en même tems que j'en aime et admire la belle et simple Théorie. Je l'ai exposée de mon mieux dans

mon cours, et il m' a paru qu' elle séduisoit la plupart de mes auditeurs.

Son fils, comme vous le savez sans doute, a suivi mes leçons avec beaucoup d'assiduité et d'attention. Lui et M. votre neveu étoient des modèles à cet égard.

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**EXTRACT—MR. WATT TO MR. DE LUC.**

Birmingham, 27th June 1786.

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It seems odd, but in the detached memoirs of Mr. Cavendish and myself, on the Composition of Water, they should be both wrong dated. Mr. Cavendish's dated, "read January 1783," when it was read January 1784,\* and my letter to Dr. Priestley,† dated April 1784, when it was written April 1783.

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**END OF THE EXTRACTS FROM MR. WATT'S CORRESPONDENCE.**

\* This refers to the copies of Mr. Cavendish's memoir for private circulation, which were circulated by him *before* the publication of the seventy-fourth volume of the Transactions for 1784, having on their title-page this date, "Read at the Royal Society, January. 15, 1783." The date at the head of the paper itself is rightly given in the Philosophical Transactions, but omitted in those copies.—ED.

† It is not the letter to Dr. Priestley, but that to Mr. De Luc, which is misdated in the Philosophical Transactions, being there dated "26th Nov. 1784," when the real date was 1783.

That letter to Dr. Priestley, written and dated 26th April 1783, was read at the Royal Society 22d April 1784. The letter to Mr. De Luc was written and dated 26th November 1783, and was read at the Royal Society 29th April 1784.—ED.

TRANSLATION OF A LETTER FROM DR. BLAGDEN, SEC. R. S. L.,  
TO DR. LORENZ CRELL. NOT DATED.\*

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I can certainly give you the best account of the little dispute about the first discoverer of the artificial generation of water, as I was the principal instrument through which the first news of the discovery that had been already made was communicated to Mr. Lavoisier. The following is a short statement of the history :—

In the Spring (“Frühjahr”) of 1783, Mr. Cavendish communicated to me and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us, that, out of them, he must draw the conclusion, that dephlogisticated air was nothing else than water deprived of its phlogiston ; and, *vice versa*, that water was dephlogisticated air united with phlogiston. About the same time (“um “dieselbe Zeit”) the news was brought to London, that Mr. Watt of Birmingham had been induced by some

\* Published in Crell’s “Chemische Annalen,” Helmstädt u. Leipzig, 1786, pp. 58-61.—ED.

observations, to form (“fassen”) a similar opinion. Soon after this (“bald darauf”) I went to Paris, and in the company of Mr. Lavoisier, and of some other members of the Royal Academy of Sciences, I gave some account of these new experiments, and of the opinions founded upon them. They replied that they had already heard something of these experiments ; and, particularly, that Dr. Priestley had repeated them. They did not doubt, that in such manner a considerable quantity of water might be obtained ; but they felt convinced that it did not come near to the weight of the two species of air employed ; on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in, and united with the airs, and deposited in their combustion. This opinion was held by Mr. Lavoisier, as well as by the rest of the gentlemen, who conferred on the subject ; but, as the experiment itself appeared to them very remarkable in all points of view, they unanimously requested Mr. Lavoisier, who possessed all the necessary preparations (“Vorrichtungen”) to repeat the experiment on a somewhat larger scale, as early as possible. This desire he complied with on the 24th June 1783, (as he relates in the latest volume of the Paris Memoirs.) From Mr. Lavoisier’s own account of his experiment, it sufficiently appears, that at that period he had not yet formed the opinion, that water was composed of dephlogisticated and inflammable airs ; for he expected that a sort of acid would be produced by their union. In general, Mr. Lavoisier cannot be convicted of having advanced any thing contrary to truth ;

but it can still less be denied, that he concealed a part of the truth. For he should have acknowledged that I had, some days before, apprized him of Mr. Cavendish's experiments ; instead of which, the expression "il nous apprit," gives rise to the idea that I had not informed him earlier than that very day. In like manner, Mr. Lavoisier has passed over a very remarkable circumstance, namely, that the experiment was made in consequence of what I had informed him of. He should likewise have stated in his publication, not only that Mr. Cavendish had obtained "une quantité d'eau très sensible," but that the water was equal to the weight of the two airs added together. Moreover, he should have added, that I had made him acquainted with Messrs. Cavendish and Watt's conclusions ; namely, that water, and not an acid or any other substance, ("Wesen"), arose from the combustion of the inflammable and dephlogisticated airs. But *those* conclusions opened the way to Mr. Lavoisier's present theory, which perfectly agrees with that of Mr. Cavendish; only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston. Mr. Monge's experiments, (of which Mr. Lavoisier speaks as if made about the same time,) were really not made until pretty long, I believe at least two months, later than Mr. Lavoisier's own, and were undertaken on receiving information of them. The course of all this history will clearly convince you, that Mr. Lavoisier, (instead of being led to the discovery, by following up the experiments which he and Mr. Bucquet had commenced in 1777,) was induced to institute again such experiments, solely by

the account he received from me, and of our English experiments ; and that he really discovered nothing, but what had before been pointed out to him to have been previously made out, and demonstrated in England.

END OF DR. BLAGDEN'S LETTER TO DR. CRELL.

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## APPENDIX.



## APPENDIX.

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### No. I.

THOUGHTS ON THE CONSTITUENT PARTS OF WATER AND  
OF DEPHLOGISTICATED AIR ; WITH AN ACCOUNT OF  
SOME EXPERIMENTS ON THAT SUBJECT. IN A LETTER  
FROM MR. JAMES WATT, ENGINEER, TO MR. DE LUC,  
F. R. S.\*

Read April 29, 1784.

Birmingham, November 26, 1783.

DEAR SIR,—In compliance with your desire, I send you an account of the hypothesis I have ventured to form on the probable causes of the production of water from the deflagration of a mixture of dephlogisticated and inflammable airs, in some of our friend Dr. Priestley's experiments.

I feel much reluctance to lay my thoughts on these subjects before the public in their present indigested

\* Reprinted from the Philosophical Transactions, vol. lxxiv. for 1784, p. 329 to 353 ;—the erroneous date of 1784 being now corrected to 1783, and the paper thus resuming its rightful precedence of that of Mr. Cavendish, hereafter also reprinted. In all the papers now reprinted, the numbering of the pages of that volume of the Philosophical Transactions in which they are to be found, is preserved, and is placed within brackets.—ED.

state, and without having been able to bring them to the test of such experiments as would confirm or refute them ; and should, therefore, have delayed the publication of them until these experiments had been made, if you, Sir, and some other of my philosophical friends, had not thought them as plausible as any other conjectures which have been formed on the subject ; and that though they should not be verified by further experiments, or approved of by men of science in general, they may perhaps merit a discussion, and give rise to experiments which may throw light on so important a subject.

I first thought of this way of solving the phænomena in endeavouring to account for an experiment of Dr. Priestley's, [330] wherein water appeared to be converted into air ; and I communicated my sentiments in a letter addressed to him, dated April 26, 1783,\* with a request that he would do me the honour to lay them before the Royal Society ; but as, before he had an opportunity of doing me that favour, he found, in the prosecution of his experiments, that

\* This letter Dr. Priestley received at London ; and, after showing it to several Members of the Royal Society, he delivered it to Sir Joseph Banks, the President, with a request that it might be read at some of the public meetings of the Society ; but before that could be complied with, the author, having heard of Dr. Priestley's new experiments, begged that the meeting might be delayed. The letter, therefore, was reserved until the 22d of April last ; when, at the author's request, it was read before the Society. It has been judged unnecessary to print that letter, as the essential parts of it are repeated, almost *verbatim*, in this letter to Mr. De Luc ; but, to authenticate the date of the author's ideas, the parts of it which are contained in the present letter are marked with double commas.

the apparent conversion of water into air, by exposing it to heat in porous earthen vessels, was not a real transmutation, but an exchange of the elastic fluid for the liquid, in some manner not yet accounted for ; therefore, as my theory was no ways applicable to the explaining these experiments, I thought proper to delay its publication, that I might examine the subject more deliberately, which my other avocations have prevented me from doing to this time.

1. It has been known for some time that inflammable air contained much phlogiston ; and Dr. Priestley has found, by some experiments made lately, that it "is either wholly pure phlogiston, or at least that "it contains no apparent mixture of any other matter." (In my opinion, however, it contains a small quantity of water, and much elementary [331] heat.)\* "He found, that by exposing the calces of metals to the "solar rays, concentrated by a lens, in a vessel containing inflammable air only, the calces of the softer "metals were reduced to their metallic state ;" and that the inflammable air was absorbed in proportion as they became phlogisticated ; and, by continually supplying the vessel with inflammable air, as it was absorbed, he found, that out of 101 ounce measures, which he had put into the vessel, 99 ounce measures were absorbed by the calces, and only two ounce

\* Previous to Dr. Priestley's making these experiments, Mr. Kirwan had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiston, in an aerial form. These arguments were perfectly convincing to me ; but it seems more proper to rest that part of the present hypothesis on the direct experiment.

measures remained, which, upon examination, he found to be nearly of the same quality the whole quantity had been of before the experiment, and to be still capable of deflagrating in conjunction with atmospheric or with dephlogisticated air. *Therefore, as so great a quantity of inflammable air had been absorbed by the metallic calces; the effect of reducing them to their metallic state had been produced; and the small remaining portion was still unchanged, at least had suffered no change which might not be attributed to its original want of purity; it was reasonable to conclude, that inflammable air must be the pure phlogiston, or the matter which reduced the calces to metals.*

2. "The same ingenious philosopher mixed together certain proportions of pure dry dephlogisticated air, and of pure dry inflammable air in a strong vessel, closely shut, and then set them on fire by means of the electric spark," in the same manner as is done in the inflammable air pistol. "The first effect was the appearance of red heat or inflammation [332] in the airs, which was soon followed by the glass vessel becoming hot. The heat gradually pervaded the glass, and was dissipated in the circumambient air, and as the glass grew cool, a mist or visible vapour appeared in it, which was condensed on the glass in the form of moisture or dew.\* When the glass was cooled to the tem-

\* I believe that Mr. Cavendish was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired. [This note was not in the original draft, nor in the press copy of the letter as sent to Mr. De Luc; but was afterwards added in pencil.—ED.]

" perature of the atmosphere, if the vessel was opened  
" with its mouth immersed in water or mercury, so  
" much of these liquids entered as was sufficient to  
" fill the glass within about  $\frac{1}{3}$  depth part of its whole  
" contents ; and this small residuum may safely be  
" concluded to have been occasioned by some impu-  
" rity in one or both kinds of air. The moisture ad-  
"hering to the glass after these deflagrations, being  
" wiped off, or sucked up by a small piece of sponge  
" paper, first carefully weighed, was found to be  
" exactly, or very nearly, equal in weight to the airs  
" employed."

" In some experiments, but not in all, a small  
" quantity of a sooty-like matter was found adhering  
" to the inside of the glass," the origin of which is  
not yet investigated ; but Dr. Priestley thinks that  
it arises from some minute grains of the mercury  
that was used in order to fill the glass with the air,  
which, being super-phlogisticated by the inflammable  
air, assumed that appearance ; but, from whatever  
cause it proceeded, " the whole quantity of sooty-like  
" matter was too small to be an object of considera-  
" tion, particularly as it did not occur in all the ex-  
" periments."

I am obliged to your friendship for the account of  
the experiments which have been lately made at  
Paris on this subject, [333] with large quantities of  
these two kinds of air, by which the essential point  
seems to be clearly proved, that the deflagration or  
union of dephlogisticated and inflammable air, by  
means of ignition, produces a quantity of water equal  
in weight to the airs ; and that the water thus pro-

duced, appeared, by every test, to be pure water. As I am not furnished with any particulars of the manner of making the experiment, I can make no observations on it, only that, from the character you give me of the gentlemen who made it, there is no reason to doubt of its being made with all necessary precautions and accuracy, which was further secured by the large quantities of the two airs consumed.

3. "Let us now consider what obviously happens in "the case of the deflagration of the inflammable and "dephlogisticated air. These two kinds of air unite "with violence, they become red-hot, and upon cool- "ing, totally disappear. When the vessel is cooled, "a quantity of water is found in it equal to the "weight of the air employed. This water is then the "only remaining product of the process, and *water*, "light, and *heat*, are all the products," unless there be some other matter set free which escapes our senses.

"Are we not then authorized to conclude, that *water* "is composed of *dephlogisticated air and phlogiston*, "deprived of part of their latent or elementary heat; "that *dephlogisticated or pure air* is composed of "water deprived of its *phlogiston*, and united to ele- "mentary heat and light; and that the latter are con- "tained in it in a latent state, so as not to be sensible "to the thermometer or to the eye; and if light be "only a modification of heat, or a circumstance at- "tending it, or a component part of the inflammable "air, then *pure or dephlogisticated air* is composed "of water deprived of its *phlogiston* and united to "elementary heat?" [334]

4. "It appears that dephlogisticated water," or, which may be a better name for the basis of water and air, the element you call *humor*, "has a more powerful attraction for phlogiston than it has for latent heat, but that it cannot unite with it, at least not to the point of saturation, or to the total expulsion of the heat, unless it be first made red-hot," or nearly so. "The electric spark heats a portion of it red-hot, the attraction between the humor and the phlogiston takes place, and the heat which is let loose from this first portion heats a second, which operates in a like manner on the adjoining particles, and so continually, until the whole is heated red-hot and decomposed." Why this attraction does not take place to the same degree in the common temperature of the atmosphere, is a question I am not yet able to solve; but it appears, that, in some circumstances, "dephlogisticated air can unite, in certain degrees, with phlogiston, without being changed into water." Thus Dr. Priestley has found, that by taking clean filings of iron, which alone produce only inflammable air of the purest kind, and *mercurius calcinatus per se*, which gives only the purest dephlogisticated air, and exposing them to heat in the same vessel, he obtained neither dephlogisticated nor inflammable air, "but in their place fixed air." Yet it is well known, that a mixture of dephlogisticated and inflammable air will remain for years in close vessels in the common heat of the atmosphere, without suffering any change, the mixture being as capable of deflagration at the end of that time as it was when first shut up. These facts

the Doctor accounts for, by supposing that the two kinds of air, when formed at the same time in the same vessel, can unite in their *nascent* state; but that, when fully formed, they are incapable of acting upon one another, unless they are [335] first set in motion by external heat. "Phlogisticated air seems "also to be another composition of phlogiston and "dephlogisticated air;" but in what proportions they are united, or by what means, is still unknown. It appears to me to be very probable, that fixed air contains a greater quantity of phlogiston than phlogisticated air does, because it has a greater specific gravity, and because it has more affinity with water.

5. "For many years I have entertained an opinion, that air was a modification of water, which was originally founded on the facts, that in most cases wherein air was actually made," which should be distinguished from those wherein it is only extricated from substances containing it in their pores, or otherwise united to them in the state of air, "the substances were such as were known to contain water as one of their constituent parts, yet no water was obtained in the processes," except what was known to be only loosely connected with them, such as the water of the crystallization of salts. "This opinion arose from a discovery," that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased; or, in other words, "that the latent heat of steam was less when it was produced under a greater pressure, or in a more dense state, and greater when it was produced under a less pressure,

" or in a less dense state ; which led me to conclude,  
" that when a very great degree of heat was neces-  
" sary for the production of the steam, the latent heat  
" would be wholly changed into sensible heat ; and  
" that, in such cases, the steam itself might suffer  
" some remarkable change. I now abandon this  
" opinion in so far as relates to the change of water  
" into air, as I think that may be accounted for on  
" better principles." [336]

6. " In every case, wherein dephlogisticated air  
" has been produced, substances have been employed,  
" some of whose constituent parts have a strong at-  
" traction for phlogiston, and, as it would appear, a  
" stronger attraction for that substance than *humor*  
" has ; they should, therefore, dephlogisticate the  
" water" or fixed air, and the *humor* thus set free  
should unite to the matter of fire and light, and be-  
come pure air. Dephlogisticated air is produced in  
great abundance from melted nitre. " The acid of  
" nitre has a greater attraction for phlogiston than  
" any other substance is known to have ; and it is  
" also certain that nitre, besides its water of crystal-  
" lization, contains a quantity of water as one of its  
" elementary parts, which water adheres to the other  
" parts of the nitre, with a force sufficient to enable  
" it to sustain a red heat. When the nitre is melted,  
" or made red-hot, the acid acts upon the water, and  
" dephlogisticates it ; and the fire supplies the *humor*  
" with the due quantity of heat to constitute it air,  
" under which form it immediately issues. It is not  
" easy to tell what becomes of the acid of nitre and  
" phlogiston, which are supposed to be united," as

they seem to be lost in the process. Dr. Priestley has lately made some experiments, with a view to ascertain this point. He distilled dephlogisticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz. = 960 grains of nitre : the retort was placed in an air furnace, and, by means of an intense heat, he obtained from the nitre in one experiment 787, and in another experiment 800 ounce measures of dephlogisticated air ; and he found that, upon weighing the retort and nitre before and after the process, they had suffered a loss of weight equal to the weight of the air, and to the water of crystallization of the nitre, but nothing more. He remarked that the air had a pungent [337] smell, which he could not divest it of by washing ; and that the water in which the air was received had become slightly acid. I examined a portion of this water, which he was so kind as to send me, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grains of nitre. I also examined the residuum and the retort in which the distillation had been performed, and found the residuum highly alkaline, yet containing a minute quantity of phlogisticated nitrous acid. It had acted considerably upon the retort, and had dissolved a part of it, which was deposited in the form of a brownish powder, when the saline part was dissolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally consists of the earth of the retort. This experiment, and all others tried in earthen vessels, leave us still at a loss to determine what becomes

of the acid and phlogiston. They seem either to remain mixed with the air, in the form of an incoercible gas ; or to unite with the alkali or with the earth of the retort, in some manner so as not to be easily separated from them ; or else they are imbibed by the retorts themselves, which are sufficiently porous to admit of such a supposition.

*All that appears to be conclusive from this experiment is, that above one half of the weight of the nitre was obtained in the form of dephlogisticated air ; and that the residuum still contained some nitrous acid united to phlogiston.*

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourse to combinations of the nitrous acid with earths from which the dephlogisticated air is obtained with less heat than from nitre itself. As these processes have been particularly described by Dr. Priestley, by Mr. Scheele, and others, I [338] shall not enter into any detail of them ; but shall mention the general phenomena which I observed, and which relate to the present subject.

The earths I used were magnesia alba, calcareous earth, and minium or the red calx of lead. I dissolved them in the respective experiments in nitrous acid dephlogisticated by boiling, and diluted with proper proportions of water. I made use of glass retorts, coated with clay ; and I received the air in glass vessels, whose mouths were immersed in a glazed earthen bason, containing the smallest quantity of water that could be used for the purpose. As soon as the retort was heated a little above the heat

of boiling water, the solutions began to distil watery vapours containing nitrous acid. Soon after these vapours ceased, yellow fumes, and in some of the cases dark red fumes began to appear in the neck of the retort ; and at the same time there was a production of dephlogisticated air, which was greater in quantity from some of these mixtures than from others, but continued in all of them until the substances were reduced to dryness. I found in the receiving water, &c., very nearly the whole of the nitrous acid used for their solution, but highly phlogisticated, so as to emit nitrous air by the application of heat ; and there is reason to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogisticated air produced by these processes did not form a sufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the composition of the air obtained, I ceased to pursue them further, having learned from them the fact, *that however much the acid and the earths were dephlogisticated before the solution, the acid always became highly phlogisticated in the process.* [339]

In order to examine whether this phlogiston was furnished by the earths, some dephlogisticated nitrous acid was distilled from minium till no more acid or air came over. More of the same acid was added to the minium as soon as it was cold, and the distillation repeated, which produced the same appearance of red fumes and dephlogisticated air. This operation was repeated a third time on the same minium, without any sensible variation in the phenomena.

The process should have been still farther repeated, but the retort broke about the end of the third distillation. The quantity of minium used was 120 grains, and the quantity of nitrous acid added each time was 240 grains, of such strength that it could dissolve half its weight of mercury by means of heat.

*It appears from this experiment, that unless minium be supposed to consist principally of phlogiston, the source of the phlogiston, thus obtained, was either the nitrous acid itself, or the water with which it was diluted; or else that it came through the retort with the light, for the retort was in this case red-hot before any air was produced; yet this latter conclusion does not appear very satisfactory, when it is considered, that in the process wherein the earth made use of was magnesia, the retort was not red-hot, or very obscurely so, in any part of the process; and by no means luminous, when the yellow and red fumes first made their appearance.*

9. As the principal point in view was to determine whether any part of the acid entered into the composition of the air, I resolved to employ some substance which would part with the acid in a moderate heat, and also give larger quantities of air than had been obtained in the former processes. Mercury was thought a proper substance for this purpose. 240 grains of mercury were put into a glass retort with 480 grains [340] of diluted dephlogisticated nitrous acid, which was the quantity necessary to dissolve the whole of the mercury, a gentle heat was applied, and as soon as the common air contained in the retort was dissipated, a vessel was placed to receive the

nitrous air proceeding from the solution, which was 16 ounce measures. When it had ceased to give nitrous air, the neck of the retort became hot from the watery steams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condense the vapours, and a quantity of dilute, but highly phlogisticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about four ounces of very strong nitrous air passed up immediately, the fumes in the retort became red, and dephlogisticated air passed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the two kinds of air acting upon one another, their bulk was reduced to half of an ounce measure. At this period the fumes in the retort were of a dark red colour, and dephlogisticated air was produced very fast. After a short time, some orange-coloured sublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually disappeared, and the neck of the retort became quite clear. At the same time that this happened, small globules of mercury appeared in the neck of the retort, and accumulated there until they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which passed up with the air into the receiver, and mixed with the water, but did not dissolve in it. After giving about 36 ounce

measures of dephlogisticated air, [341] it suddenly ceased to give any more ; and the retort being cooled, the bulb was found to be quite empty, excepting a small quantity of black powder, which, on being rubbed on the hand, proved to be mostly running mercury. The orange-coloured sublimate was washed out of the neck of the retort, and what running mercury was in it was separated, and added to that which had run down into the basin among the water. The whole fluid mercury, when dried, weighed 218 grains ; therefore 22 grains remained in the form of sublimate, which, I believe, would also have been reduced if I could have applied heat in a proper manner to the neck of the retort, as some of it to which heat could be applied, disappeared.

10. The 16 ounce measures of nitrous air, which had been produced in the solution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admission of common air, and was taken up by the water ; this water was added to that in the basin, which had served to receive the dephlogisticated air. The whole quantity was about two quarts, was very acid to the taste, and sparkling with nitrous air. It was immediately put into bottles, and well corked, until it had lost the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the sublimate, I dissolved, first, alkali of tartar in water, and filtered the solution. 352 grains of this alkaline solution saturated 120 grains of the nitrous acid I had employed to dissolve the mercury, and 1395 grains of the same alkaline solu-

tion saturated the orange-coloured precipitate, and all the acid liquor obtained from the process: therefore we have the proportion as  $352 : 120 :: 1395 : 475$ , from which it appears, that all the acid employed was recovered again in the form of acid, excepting only five grains; [342] a smaller quantity than what might reasonably be supposed to be lost in the process by the extreme volatility of the nitrous air. In order to ascertain the exact point of saturation, slips of paper, stained by the juice of the petals of the scarlet rose, were employed, which were the nicest test I could procure, as litmus will not show the point of saturation of any liquor containing much phlogisticated nitrous acid, or even fixed air, but will turn red, and show it to be acid, when the test of those leaves, violets, and some other of the like kind, will turn green in the same liquor, and show it to be alkaline. But the exact point of saturation of so dilute a liquor is so very difficult to ascertain, that an error might easily be committed, notwithstanding the attention bestowed upon it. Supposing this experiment to be unexceptionable, the conclusions which may be drawn from it are very favourable to the hypothesis I endeavour to support. *Thirty-six ounce measures of dephlogisticated air were obtained, and only five grains of a weak nitrous acid were lost in the process. Two hundred and eighteen grains of mercury out of two hundred and forty were revived, and all the dephlogisticated nitrous acid employed is found to be highly phlogisticated in the process. It appears that the nitrous acid does not enter into the composition of dephlogisticated air; it seems only to*

*serve to absorb phlogiston from the watery part of the mercurial nitre.*

11. As this last process proved very tedious and complicated on account of the necessity of ascertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double source of error in the point of saturation, I resolved to try the distillation of dephlogisticated air from cubic nitre in a glass vessel, and to draw from it only such a quantity of air as it would yield without acting much upon the retort, which latter circumstance is [343] essentially necessary to be attended to. An ounce of the crystals of mineral alkali were dissolved in nitrous acid, and the mixture brought to an exact saturation by the test of litmus; 30 ounce measures of air were distilled from it, which, during the latter part of the process, was accompanied with slightly yellow fumes; the receiving water was found to be acid, and the residuum alkaline. The residuum being dissolved in the receiving water, the solution was neutral, or very nearly so, by every test; for in this case litmus might be used, as the acid was very slightly phlogisticated. On adding a few drops of a very dilute nitrous acid, the tests showed the liquor to be acid.

12. Encouraged by the success of this experiment, I took an ounce = 480 grains of pure common nitre, and put it into a flint-glass retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot, and during the latter part of the process this air was accompanied with yellowish fumes. I stopped the process when it had produced 50 ounce measures of air. The receiving water, and

particularly the air, had a strong but peculiar smell of nitrous acid. The air was well washed with the receiving water, but was not freed from the smell. The receiving water, which was 50 ounces, was slightly acid, and the residuum alkaline. I dissolved the latter in the former, and found the mixture alkaline, 10 grains of weak nitrous acid were added to it, which saturated it, and 105 grains of this spirit of nitre was found to contain the acid of 60 grains of nitre ; therefore the 10 grains contained the acid of 5.7 grains of nitre, which, by Mr. Kirwan's experiments, is equal to two grains of real nitrous acid. *We have, therefore, 34 grains weight of dephlogisticated air produced, and only two grains of real acid missing;* and it is not [344] certain that this quantity was destroyed, because some portion of the glass of the retort was dissolved by the nitre, and some part of the materials employed in making the glass being alkali, we may conclude that the alkali of the nitre would be augmented by the alkali of that part of the glass it had dissolved. As the glass cracked into small pieces on cooling, and some part of the coating adhered firmly to it, the quantity of the glass that was dissolved could not be ascertained. *From this experiment it appears, that if any of the acid of the nitre enters into the composition of the dephlogisticated air, it is a very small part ; and it rather seems that the acid, or part of it, unites itself so firmly to the phlogiston as to lose its attraction for water.*

13. "The vitriolic salts also yield dephlogisticated "air by heat ; and in these cases the dephlogisti- "cated air is always attended with a large quantity

"of vitriolic acid air or sulphureous vapour," even when the salts used are not known to contain any phlogistic matter. Mr. Scheele mentions his having obtained dephlogisticated air from manganese dissolved in acid of phosphorus, and also from the arsenical acid; from whence it appears that these acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogisticated air.

*It is necessary to remark, that no experiments have been yet published showing that dephlogisticated air can be produced from salts formed by the muriatic acid. The acids which produce salts suitable for this purpose have all a strong affinity with phlogiston; and the marine acid has either a very small affinity with it, or else is already saturated with it, at least so far saturated as not to be able to attract it from the humor.*

14. "The dephlogisticated air obtained from the pure calces of metals may be attributed to the calces themselves, attracting the phlogiston from water which they have imbibed from [345] the atmosphere, or from dephlogisticating the fixed air which they are known to contain."

It is very probable that the dephlogisticated air extruded from growing vegetables may be owing to their dephlogisticating the water they grow in; but it appears more probable that the plants have a power of dephlogisticating the fixed or phlogisticated air of the atmosphere.

"When dephlogisticated and nitrous air are mixed, the dephlogisticated air seizes part of the phlogiston of the nitrous air." The water contained in

the nitrous air, and the other part of the phlogiston, unite with the nitrous acid, which then assumes a liquid form, or at least that of a dense vapour ; " and " that part of the latent heat of the two airs not " essential to the new combination is set at liberty."\*

In the combustion of sulphur the same thing happens, but in a greater degree ; for the vitriolic acid having a much weaker attraction for phlogiston than air has, abandons it almost entirely to the latter, which is thereby converted into water, and in that form attracts the vitriolic acid, and reduces it to a liquid state. The same reasoning may be applied to the combustion of phosphorus, which is attended with similar effects. [346]

15. I shall not make, at present, any further deductions from what I myself consider still in the light of a conjectural hypothesis, which I have, perhaps, dwelt upon too long already. I shall only beg your attention to some general reasoning on the subject, which, however, may possibly serve more to show the uncertainty of other systems on the constituent parts of air, than the certainty of this. Some of those systems supposed dephlogisticated air to be

\* I cannot take upon me to determine, from any facts which have come to my knowledge, whether any part of the dephlogisticated air employed in this experiment is turned into fixed air ; but I am rather inclined to think that some part is, because the quantity of heat, which is separated by the union of the two airs, does not seem to be so great as that which is separated when the dephlogisticated air is wholly changed into water ; yet some water appears to be formed, because, when the mixture is made over mercury, the solution of the mercury in the nitrous acid assumes a crystallized form, which, however, may be due to the watery part of the nitrous air.

composed of an acid and something else, some say phlogiston. If an acid enters into the composition of it, why does not that acid appear again when the air is decomposed, by means of inflammable air and heat? And why is the water which is the product of this process pure water? And if an acid forms one of its constituent parts, why has nobody been able to detect any difference in the dephlogisticated air, made by the help of different acids, when compared with one another, or with the air extruded by vegetables? These airs, of such different origins, appear to be exactly the same. And if phlogiston be a constituent part of air, why does it attract phlogiston with such avidity? Some have, on the other hand, contended that air is composed of earth, united to acids, or phlogiston, or to both, or to some other matter. Here we must ask, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with some others, such as the phosphoric and the arsenical acids, will serve as bases for the formation of air, and the air produced from all of them appears by every test to be the same, when freed from accidental impurities. To this argument it is answered, that it is not any particular species of earth which is the basis of air, but elementary or simple earth, which is contained in all of them. If this were the [347] matter of fact, would not that earth be found after the decomposition of the air?

Mr. Scheele has formed an hypothesis on this subject, in which he supposes heat to be composed of dephlogisticated air united to phlogiston, and that

this combination is sufficiently subtle to pass through glass vessels. He affirms, that the nitrous and other acids, when in an ignited state, attract the phlogiston from the heat, and set the dephlogisticated air at liberty ; but he does not seem to have been more successful than myself in explaining what becomes of the acid of nitre and phlogiston in the case of the decomposition of nitre by heat. And since we know, from the late experiments, that water is a composition of air, or, more properly, *humor* and phlogiston, his whole theory must fall to the ground, unless that fact be otherwise accounted for, which it does not seem easy to do.

16. To return to the experiment of the deflagration of dephlogisticated and inflammable air, " it appears from the two airs becoming red-hot on their union, that the quantity of heat contained in one or both of them is much greater than that contained in steam, because, for the first moments after the explosion, the water deposited by the air remains in the form of steam, and consequently retains the latent heat due to that modification of water. This matter may be easily examined by firing the mixture of dephlogisticated and inflammable air in a vessel immersed in another vessel containing a given quantity of water of a known heat, and after the vessel in which the deflagration is performed is come to the same temperature with the water in which it is immersed, by examining how much heat that water has gained, which being divided by the quantity of water produced by the decomposition of the airs, will give the whole quan-

" tity of elementary [348] or latent heat which that  
" water had contained, both as air and as steam ;  
" and if from that quantity we deduct the latent  
" heat of the steam, the remainder will be the latent  
" or elementary heat contained more in air than in  
" steam." This experiment may be made more com-  
pletely by means of the excellent apparatus which  
MM. Lavoisier and De La Place have contrived for  
similar purposes.

Until direct experiments are made, we may con-  
clude, from those which have been made by the  
gentlemen just named, on the decompositions of air  
by burning phosphorus and charcoal, that the heat  
extricated during the combustion of inflammable and  
dephlogisticated air is much greater than it appears  
to be ; for they found that one Paris ounce (= 576  
Parisian grains) of dephlogisticated air, when decom-  
posed by burning phosphorus, melted 68,634 ounces  
of ice ; and as, according to another of their experi-  
ments, ice, upon being melted, absorbs 135° of heat,  
by Fahrenheit's scale, each ounce of air gave out  
 $68,634 \times 135^\circ = 9265^\circ,590$  ; that is to say, a quan-  
tity of heat which would have heated an ounce of  
water, or any other matter which has the same  
capacity for receiving heat as water has, from 32° to  
 $9265\frac{1}{2}^\circ$  : a surprising quantity ! (It is to be under-  
stood that all the latent heats mentioned herein are  
compared with the capacity of water.) And when  
an ounce of dephlogisticated air was changed into  
fixed air, by burning charcoal, or by the breathing  
of animals, it melted 29,547 ounces of ice ; conse-  
quently we have  $29,547 \times 135^\circ = 3988^\circ,845$ , the

quantity of heat which an ounce of dephlogisticated air loses when it is changed into fixed air. By the heat extricated during the detonation of one ounce of nitre with one ounce of sulphur, 32 ouncees of ice were melted ; and, by the experiment I have mentioned of Dr. Priestley's (6,) it appears that [349] nitre can produce one-half of its weight of dephlogisticated air. When the nitre and sulphur are kindled, the dephlogisticated air of the nitre unites with the phlogiston of the sulphur, and sets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogisticated air, united to the phlogiston, is turned into water, part of which is absorbed by the vitriolated tartar, and part is dissipated in the form of vapours, or unites to the nitrous air, or other air produced in the process.

As half an ounce of dephlogisticated air is, in this process, united by inflammation to a quantity of phlogiston sufficient to saturate it, and no fixed air is produced, it should melt a quantity of ice equal to the half of what was melted by the combination of an ounce of air with phlogiston in burning phosphorus, that is, it should melt 34,317 ouncees of ice ; and we find, by MM. Lavoisier and De La Place's experiment, that it actually melted 32 ounces of ice : the small difference may be accounted for by supposing that the heat produced by the combustion might not be quite so great as that Dr. Priestley employed in his experiment, or that the nitre might be less pure, and consequently not so much air formed. The two facts, however, agree near enough to permit us to conclude that *dephlogisticated air, in uniting to*

*the phlogiston of sulphur, produces as much heat as it does in uniting with the phlogiston of phosphorus.*

17. According to Dr. Priestley's experiments, dephlogisticated air unites completely with about twice its bulk of the inflammable air from metals. The inflammable air being supposed to be wholly phlogiston, and being  $\frac{1}{9.6}$  of the weight of an equal bulk of dephlogisticated air, and being double in quantity, will be  $\frac{1}{4.8}$  of the weight of the dephlogisticated air [350] it unites with. Therefore one ounce (576 grains) of dephlogisticated air, will require 120 grains of inflammable air, or phlogiston, to convert it into water. And supposing the heat extricated by the union of dephlogisticated and inflammable air to be equal to that extricated by the burning of phosphorus, we shall find that the union of 120 grains of inflammable air with 576 grains of dephlogisticated air, extricates 9265° of heat.

18. In the experiment on the deflagration of nitre with charcoal, by MM. Lavoisier and De La Place, an ounce of nitre and one-third of an ounce of charcoal melted twelve ounces of ice. Supposing the ounce of nitre to have produced half an ounce of dephlogisticated air, it ought to have consumed 0,1507 ounces of charcoal, and should have melted 14,773 ounces of ice ; and I suppose it fell short of its effect by the heat not being sufficiently intense to decompose the nitre perfectly.

19. By the above gentlemen's experiment an ounce of charcoal required for its combustion 3,3167 ounces of dephlogisticated air, and produced 3,6715 ounces of fixed air ; therefore there was united to each ounce

of air, when changed into fixed air, 61,5 grains of phlogiston, and 3988° of heat were extracted. *It appears by these facts that the union of phlogiston, in different proportions, with dephlogisticated air, does not extricate proportional quantities of heat.* For the addition of 61,5 grains produces 3988°, and the union of 120 grains produces 9265°. This difference may arise from a mistake in supposing the specific gravity of the inflammable air Dr. Priestley employed to have been only  $\frac{1}{9.6}$  of that of dephlogisticated air; for if it be supposed that its specific gravity was a little more than  $\frac{1}{8}$  of that of the dephlogisticated air, then equal additions of phlogiston would [351] have produced equal quantities of heat :\* this matter should therefore be put to the test of experiment, by deflagrating dephlogisticated air with inflammable air of a known specific gravity, or by finding how much dephlogisticated air is necessary for the combustion of an ounce of sulphur, the quantity of phlogiston in which has been accurately determined by Mr. Kirwan ; or by finding the quantity of phlogiston in phosphorus, the quantity of dephlogisticated air necessary for its decomposition being known from MM. Lavoisier and De La Place's experiments.

\* Or it may arise from my being mistaken, in supposing that the same quantity of heat is disengaged by the union of dephlogisticated air with phlogiston, in the form of inflammable air, as is by its union with the phlogiston of phosphorus or sulphur ; and there appears to be some reason why there should not ; because in these latter cases the water, being united to the acids, cannot retain so much elementary heat as it can do when left in the form of pure water, which is the case when the inflammable air is used.

On considering these latter gentlemen's experiments on the combustion of charcoal, a difficulty arises to know what became of the remainder of the ounce of charcoal; for the dephlogisticated air, in becoming fixed air, gained only the weight of 0,3548, or about  $\frac{1}{3}$  of an ounce; about  $\frac{2}{3}$  of an ounce are therefore unaccounted for. The weight of the ashes of an ounce of charcoal is very inconsiderable; and, by some experiments of Dr. Priestley's, charcoal, when freed from fixed air, and other air which it imbibes from the atmosphere, is almost wholly convertible into phlogiston. The cause of this apparent loss of matter, I doubt not, these gentlemen can explain satisfactorily, and very probably in such a manner as will throw other lights on the subject. [352]

It is also worthy of inquiry, whether all the amazing quantity of heat let loose in these experiments was contained in the dephlogisticated air; or whether the greatest portion of it was not contained in the phlogiston or inflammable air. If it was all contained in the dephlogisticated air, "*the general rule is not fact, that elastic fluids are enlarged in their dimensions in proportion to the quantity of heat they contain;*" because, then, inflammable air, which is ten times the bulk of dephlogisticated air, must be supposed to contain no heat at all; "and it is known, "from some experiments of my friend Dr. Black's, "and some of my own, that the steam of boiling "water, whose latent and sensible heat are only "1100°, reckoning from 60°, or temperate, is more "than twice the bulk of an equal weight of dephlo- "gisticated air." It seems, however, reasonable to

suppose, that the greater quantity of heat should be contained in the rarer fluid.

It may be alleged, that in proportion to the quantity of phlogiston that is contained in any fluid, the quantity of heat is lessened. But if we reason by analogy, the attraction of the particles of matter to one another in other cases is increased by phlogiston, and "bodies are thereby rendered specifically "heavier;" and we know of no other substance besides heat which can be supposed to separate the particles of inflammable air, and to endow it with so very great an elastic power, and so small a specific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the same reasoning cannot hold good in respect to the phlogiston of phosphorus, sulphur, charcoal, &c. But all these substances contain other matters besides phlogiston and heat. The acids in the sulphur [353] and phosphorus, and the alkali and earth in charcoal, may attract the phlogiston so powerfully that the heat they contain may not be able to overcome the adhesion of their particles, until, by the effect of external heat, they are once removed to such a distance from one another as to be out of the sphere of that kind of attraction.\*

If it be found to be a constant fact, that equal additions of phlogiston to dephlogisticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained

\* On the whole, this question seems to involve so many difficulties that it cannot be cleared up without many new experiments.

in phlogiston and dephlogisticated air respectively, and solve the problem.

Many other ideas on these subjects present themselves ; but I am not bold enough to trouble you, or the public, with any speculations but such as I think are supported by uncontroverted facts.

I must therefore bring this long letter to a conclusion, and leave to others the future prosecution of a subject which, however engaging, my necessary avocations prevent me from pursuing. I cannot however conclude, without acknowledging my obligations to Dr. Priestley, who has given me every information and assistance in his power, in the course of my inquiries, with that candour and liberality of sentiment which distinguish his character.

I return you my thanks for the obliging attention you have paid to this hypothesis ; and remain, with much esteem, &c.

JAMES WATT.

## No. II.

SEQUEL TO THE THOUGHTS ON THE CONSTITUENT PARTS  
OF WATER AND DEPHLOGISTICATED AIR. IN A SUB-  
SEQUENT LETTER FROM MR. JAMES WATT, ENGINEER,  
TO MR. DE LUC, F.R.S.\*

Read May 6, 1784.

Birmingham, April 30, 1784.

DEAR SIR,—On reconsidering the subject of my letter to you of the 26th of November last, I think it necessary to resume the subject, in order to mention some necessary cautions to those who may choose to repeat the experiments mentioned there, and to point out some circumstances that may cause variations in the results.

In experiments where the dephlogisticated air is to be distilled from common or cubic nitre, these salts should be purified as perfectly as possible, both from other salts and from phlogistic matter of any kind ; otherwise they will produce some nitrous air, or yellow fumes, which will lessen the quantity, and, perhaps, debase the quality of the dephlogisticated air. If the nitre is perfectly pure, no yellow fumes are

\* Reprinted from the Philosophical Transactions, vol. lxxiv. for 1784, p. 354 to 357.

perceptible, until the alkaline part begins to act upon the glass of the retort, and even then they are very slightly yellow.

When earthen retorts are used, and a large quantity of air is drawn from the nitre, it acts very much upon the retort, dissolves a great part of it, and becomes very alkaline, retaining only a small part of its acid, at least only a small part which [355] can be made appear in any of the known forms of that acid ; and unless retorts can be obtained of a true apyrous and compact porcelain, I should prefer glass retorts, properly coated, for making experiments for the present purpose.

In some of my experiments the nitre was left in the retort placed in a furnace, so that it took an hour or more to cool. In these cases there was always a deficiency of the acid part, which seemed, from some appearances on the coating, either to have penetrated the hot and soft glass, by passing from particle to particle, or to have escaped by small cracks which happened in the retort during the cooling. There was the least deficiency of the acid when the distillation was performed as quickly as was practicable, and the retort was removed from the fire immediately after the operation was finished. In order to shorten the duration of the experiment, and consequently to lessen the action of the nitre on the retort, it is advisable not to distil above 50 ounce measures of dephlogisticated air from an ounce of nitre. The experiment has succeeded best when the retort was placed in a charcoal fire in a chafing-dish or open furnace ; because it is easy in that case to stop the

operation, and to withdraw the retort at the proper period.

When the dephlogisticated air is distilled from the nitre of mercury, the solution should be performed in the retort itself, and the nitrous air produced by the solution should be caught in a proper receiver, and decomposed by the gradual admission of common air through water ; and the water, which thus becomes impregnated with the acid of the nitrous air, should be added after the process to the water through which the dephlogisticated air has passed. When the solution ceases to give any more nitrous air, the point of the tube of the retort should be raised out of the water ; otherwise, by the condensation of the [356] watery and acid vapours which follow, a partial exhaustion will take place, and the receiving water will rise up into the retort and break it, or at least spoil the experiment. A common receiver, such as is used in distilling spirit of nitre, should be applied, with a little water in it, to receive the acid steam ; and it should be kept as cool as can conveniently be done, as these fumes are very volatile. This receiver should remain as long as the fumes are colourless ; but when they appear, in the neck of the retort, of a yellow colour, it is a mark that the mercurial nitre will immediately produce dephlogisticated air ; the receiver should then be withdrawn, and an apparatus placed to receive the air. The rest of the process has been sufficiently explained in my former letter.

The phlogisticated nitrous acid, saturated by an alkali, will not crystallize ; and, if exposed to evapo-

ration, even in the heat of the air, will become alkaline again, which shows the weakness of its affinity with alkalies when dissolved in water ;\* a farther proof of which is, that it is expelled from them by all the acids, even by vinegar, (which fact has been observed by Mr. Scheele.) I have observed that litmus is no test of the saturation of this acid by alkalies ; for the infusion of litmus added to such a mixture will turn red, when the liquor appears to be highly alkaline, by its turning the infusions of violets, rose leaves, and most other red juices, green. This does not proceed from the infusion of litmus being more sensible to the presence of acids than other tests ; for I have lately discovered a test liquor (the preparation of which I mean to publish soon) which is more sensible to the presence of acids [357] than litmus is ; but which turns green in the same solution of phlogisticated nitre that turns litmus red.

The unavoidable little accidents which have attended these experiments, and which tend to render their results dubious, have prevented me from relying on them as *full* proofs of the position that no acid enters into the composition of dephlogisticated air ; though they give great probability to the supposition. I have, therefore, explained the whole of the hypothesis and experiments with the diffidence which ought to accompany every attempt to account for the phenomena of nature on other principles than

\* You have informed me that Mr. Cavendish has also observed this fact ; and that he has mentioned it in a paper lately read before the Royal Society ; but I had observed the fact previous to my knowledge of his paper.

those which are commonly received by philosophers in general. And in pursuance of the same motives it is proper to mention, that the alkali employed to saturate the phlogisticated nitrous acid, was always that of tartar which is partly mild ; and I have not examined whether highly phlogisticated nitrous acid can perfectly expel fixed air from an alkali, though I know no fact which proves the contrary. It should also be examined, whether the same quantity of real nitrous acid is requisite to saturate a given quantity of alkali, when the acid is phlogisticated, as is necessary when it is dephlogisticated.

As I am informed that you have done me the honour to communicate my former letter on this subject to the Royal Society, I shall be obliged to you to do me the same favour in respect to the present letter, if you judge that it merits it.

I remain, &c.

JAMES WATT.

## No. III.

EXPERIMENTS ON AIR. BY HENRY CAVENDISH, ESQ.,  
F.R.S. & S.A.\*

Read Jan. 15, 1784.

THE following experiments were made principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed ; and as they seem not only to determine this point, but also to throw great light on the constitution and manner of production of dephlogisticated air, I hope they may be not unworthy the acceptance of this Society.

Many gentlemen have supposed that fixed air is either generated or separated from atmospheric air by phlogistication, and that the observed diminution is owing to this cause ; my first experiments, therefore, were made in order to ascertain whether any fixed air is really produced thereby. Now, it must

\* Reprinted from the Philosophical Transactions, vol. lxxiv. for 1784, p. 119 to 153. The two interpolations by Dr. Blagden, and an addition by Mr. Cavendish, all made after the Paper itself had been read in January 1784, are now marked by being placed within brackets.—ED.

be observed, that as all animal and vegetable substances contain fixed air, and yield it by burning, distillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogisticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of sulphur or phosphorus, the mixture of nitrous air, and the explosion of inflammable air. Perhaps it may be supposed, that I ought to add to these the electric spark; but I [120] think it much most likely, that the phlogistication of the air, and production of fixed air, in this process, is owing to the burning of some inflammable matter in the apparatus. When the spark is taken from a solution of tournsol, the burning of the tournsol may produce this effect; when it is taken from lime-water, the burning of some foulness adhering to the tube, or perhaps of some inflammable matter contained in the lime, may have the same effect; and when quicksilver or metallic knobs are used, the calcination of them may contribute to the phlogistication of the air, though not to the production of fixed air.

There is no reason to think that any fixed air is produced by the first method of phlogistication. Dr. Priestley never found lime-water to become turbid by the calcination of metals over it: \* Mr. Lavoisier also found only a very slight and scarce perceptible turbid appearance, without any precipitation, to take place when lime-water was shaken in a glass vessel full of the air in which lead had been calcined; and

\* Experiments on Air, vol. i. p. 137.

even this small diminution of transparency in the lime-water might very likely arise, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in some places to the glass. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reason for supposing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though most metallic calces prepared over the fire, or by long exposure to the atmosphere, where they are in contact with fixed air, contain that substance, it by no means follows that they must [121] do so when prepared by methods in which they are not in contact with it.

Dr. Priestley also observed, that quicksilver, fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air;\* but it is by no means clear that this air was produced by the phlogistication of the air in which the quicksilver was shaken; as the powder was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces.

I never heard of any fixed air being produced by the burning of sulphur or phosphorus; but it has

\* Exper. in Nat. Phil. Vol. i. p. 144.

been asserted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air ; which, if true, would be a convincing proof that on mixing those two substances some fixed air is either generated or separated ; I therefore examined this carefully. Now it must be observed, that as common air usually contains a little fixed air, which is no essential part of it, but is easily separated by lime water ; and as nitrous air may also contain fixed air, either if the metal from which it is procured be rusty, or if the water of the vessel in which it is caught contain calcareous earth, suspended by fixed air, as most waters do, it is proper first to free both airs from it by previously washing them with lime water.\* Now I found, by repeated [122] experiments, that if the lime water was clean, and the two airs were previously washed with that substance, not the least cloud was produced, either immediately on mixing them, or on suffering them to stand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finished, that it was more than sufficient to saturate the acid formed by the decomposition of the nitrous air,

\* Though fixed air is absorbed in considerable quantity by water, as I showed in Phil. Trans., vol. lvi., yet it is not easy to deprive common air of all the fixed [122] air contained in it by means of water. On shaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of distilled water, not more than half of the fixed air was absorbed, and on transferring the air into fresh distilled water, only half the remainder was absorbed, as appeared by the diminution which it still suffered on adding lime water.

and consequently that if any fixed air had been produced, it must have become visible. Once indeed I found a small cloud to be formed on the surface, after the mixture had stood a few minutes. In this experiment the lime water was not quite clean ; but whether the cloud was owing to this circumstance, or to the air's having not been properly washed, I cannot pretend to say.

Neither does any fixed air seem to be produced by the explosion of the inflammable air obtained from metals, with either common or dephlogisticated air. This I tried by putting a little lime water into a glass globe, fitted with a brass cock, so as to make it air-tight, and an apparatus for firing air by electricity. This globe was exhausted by an air-pump, and the two airs, which had been previously washed with lime water, let in, and suffered to remain some time, to show whether they would affect the lime water, and then fired by electricity. The event was, that not the least cloud was produced in the lime-water, when the inflammable air was mixed with common air, and [123] only a very slight one, or rather diminution of transparency, when it was combined with dephlogisticated air. This, however, seemed not to be produced by fixed air ; as it appeared instantly after the explosion, and did not increase on standing, and was spread uniformly through the liquor ; whereas if it had been owing to fixed air, it would have taken up some short time before it appeared, and would have begun first at the surface, as was the case in the above-mentioned experiment with nitrous air. What it was really owing to I cannot pretend to say ; but

if it did proceed from fixed air it would show that only an excessively minute quantity was produced.\* On the whole, though it is not improbable that fixed air may be generated in some chymical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it.

As there seemed great reason to think, from Dr. Priestley's experiments, that the nitrous and vitriolic acids were convertible into dephlogisticated air, I tried whether the dephlogisticated part of common air might not, by phlogistication, be changed into nitrous or vitriolic acid. For this purpose I impregnated some milk of lime with the fumes of burning sulphur, by putting a little of it into a large glass receiver, and burning sulphur therein, taking care to keep the mouth of the receiver stopt till the fumes were all absorbed ; after which the air of the receiver was changed, and more sulphur burnt in it as before, and the process repeated till 122 grains of sulphur were consumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous salt, nor any other substance except selenite ; so that no sensible quantity of air was changed [124] into nitrous acid. It must be observed, that as the vitriolic acid produced by the burning sulphur is changed by its union with the lime into selenite, which is very little soluble in water, a very small quantity of nitrous salt,

\* Dr. Priestley also found no fixed air to be produced by the explosion of inflammable and common air. Vol. v. p. 124.

or any other substance which is soluble in water, would have been perceived.

I also tried whether any nitrous acid was produced by phlogisticating common air with liver of sulphur ; for this purpose I made a solution of flowers of sulphur by boiling it with lime, and put a little of it into a large receiver, and shook it frequently, changing now and then the air, till the yellow colour of the solution was quite gone ; a sign that all the sulphur was by the loss of its phlogiston, turned into vitriolic acid, and united to the lime, or precipitated ; the liquor was then filtered and evaporated, but it yielded not the least nitrous salt.

The experiment was repeated in nearly the same manner with dephlogisticated air procured from red precipitate ; but not the least nitrous acid was obtained.

It is well known that common selenite is very little soluble in water ; whereas that procured in the two last experiments was very soluble, and even crystallized readily, and was intensely bitter ; this, however, appeared to be owing merely to the acid with which it was formed being very much phlogisticated ; for on evaporating it to dryness, and exposing it to the air for a few days, it became much less soluble, so that on adding water to it not much dissolved ; and by repeating this process once or twice, it seemed to become not more soluble than selenite made in the common manner.

This solubility of the selenite caused some trouble in trying the experiment ; for while it continued much soluble it would have been impossible to have

distinguished a small mixture of nitrous salt ; but by the above-mentioned process I was able to [125] distinguish as small a proportion as if the selenite had been originally no more soluble than usual.

The nature of the neutral salts made with the phlogisticated vitriolic and nitrous acids has not been much examined by the chymists, though it seems well worth their attention ; and it is likely that many besides the foregoing may differ remarkably from those made with the same acids in their common state. Nitre formed with the phlogisticated nitrous acid has been found to differ considerably from common nitre, as well as Sal Polychrest from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogistication of air, I impregnated fifty ounces of distilled water with the fumes produced on mixing fifty-two ounce measures of nitrous air with a quantity of common air sufficient to decompound it. This was done by filling a bottle with some of this water, and inverting it into a basin of the same, and then, by a syphon, letting in as much nitrous air as filled it half-full ; after which common air was added slowly by the same syphon, till all the nitrous air was decompounded. When this was done, the distilled water was further impregnated in the same manner till the whole of the above-mentioned quantity of nitrous air was employed. This impregnated water, which was very sensibly acid to the taste, was distilled in a glass retort. The first runnings were very acid, and smelt pungent, being nitrous acid much phlogisticated ; what came next had no

sensible taste or smell ; but the last runnings were very acid, and consisted of nitrous acid not phlogisticated. Scarce any sediment was left behind. These different parcels of distilled liquor were then exactly saturated with salt of tartar, and evaporated ; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any [126] other substance, and consequently no sensible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as  $2\frac{3}{4}$  times its weight of saltpetre ; for fifty-two ounce measures of nitrous air weigh 32 grains, and, as was before said, yield as much acid as is contained in 87½ grains of saltpetre ; so that the acid in nitrous air is in a remarkably concentrated state, and I believe more than  $1\frac{1}{2}$  times as much so as the strongest spirit of nitre ever prepared.

Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceed to some experiments, which serve really to explain the matter.

In Dr. Priestley's last volume of experiments is related an experiment of Mr. Warltire's, in which it is said that, on firing a mixture of common and inflammable air by electricity in a close copper vessel holding about three pints, a loss of weight was always perceived, on an average about two grains, though the vessel was stopt in such a manner that no air could escape by the explosion. It is also related that on repeating the experiment in glass vessels, the in-

side of the glass, though clean and dry before, immediately became dewy ; which confirmed an opinion he had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and curious ; but it did not succeed with me ; for though the vessel I used held more than Mr. Warltire's, namely 24,000 grains of water, and though the experiment [127] was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than one-fifth of a grain, and commonly none at all. It must be observed, however, that though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased.\*

In all the experiments the inside of the glass globe became dewy, as observed by Mr. Warltire ; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows : the bulk of the inflammable air being expressed in decimals of the common air—

\* Dr. Priestley, I am informed, has since found the experiment not to succeed.

Common air.	Inflammable air.	Diminution.	Air remaining after the explosion.	Test of this air in first method.	Standard.
1	1,241	,686	1,555	,055	,0
	1,055	,642	1,413	,063	,0
	,706	,647	1,059	,066	,0
	,423	,612	,811	,097	,03
	,331	,476	,855	,339	,27
	,206	,294	,912	,048	,58

In these experiments the inflammable air was procured from zinc, as it was in all my experiments except where otherwise expressed : but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both, namely, 0,331 of the common ; but I could not find any difference to be depended on between the two kinds of air, [128] either in the diminution which they suffered by the explosion, or the test of the burnt air.

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air ; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed ; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500,000 grain measures of inflammable air were burnt with about  $2\frac{1}{2}$  times that quantity of common air, and the burnt air made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder ; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel ; and in order that the quantity of common air expelled should be  $2\frac{1}{2}$  times that of the inflammable, the water was let into the outer vessels by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in [129] which the common air was confined, being  $2\frac{1}{2}$  times as big as the other.

In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the two holes, into the outer vessels, till the airs began to issue from the ends of the copper pipes ; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment

when evaporated to dryness ; neither did it yield any pungent smell during the evaporation ; in short, it seemed pure water.

In my first experiment, the cylinder near that part where the air was fired was a little tinged with sooty matter, but very slightly so ; and that little seemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame ; for in another experiment, in which it was contrived so that the luting should not be much heated, scarce any sooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe, holding 8800 grain measures, furnished with a brass cock and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump, and then filled with [130] a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable ; so that, upon open-

ing the cock, some of this mixed air rushed through the bent tube, and filled the globe.\* The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was [131] known, could tell the quantity

\* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was stuck upon the end of it, which was rubbed off when raised above the surface of the water.

of burnt air in the small globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain measures ; its standard was 1,85.

The liquor condensed in the globe, in weight about 30 grains, was sensibly acid to the taste, and by saturation with fixed alkali, and evaporation, yielded near two grains of nitre ; so that it consisted of water united to a small quantity of nitrous acid. No sooty matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate, that is, from a solution of quicksilver in spirit of nitre distilled till it acquires a red colour.

As it was suspected that the acid contained in the condensed liquor was no essential part of the dephlogisticated air, but was owing to some acid vapour which came over in making it and had not been absorbed by the water, the experiment was repeated in the same manner, with some more of the same air, which had been previously washed with water, by keeping it a day or two in a bottle with some water, and shaking it frequently ; whereas that used in the preceding experiment had never passed through water, except in preparing it. The condensed liquor was still acid.

The experiment was also repeated with dephlogisticated air, procured from red lead by means of oil of vitriol ; the liquor condensed was acid, but by an accident I was prevented from determining the nature of the acid.

I also procured some dephlogisticated air from the

leaves of plants, in the manner of Doctors Ingenhouz and Priestley, and exploded it with inflammable air as before ; the condensed liquor still continued acid, and of the nitrous kind. [132]

In all these experiments the proportion of inflammable air was such that the burnt air was not much phlogisticated ; and it was observed, that the less phlogisticated it was the more acid was the condensed liquor. I therefore made another experiment, with some more of the same air from plants, in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being  $\frac{1}{10}$ . The condensed liquor was then not at all acid, but seemed pure water : so that it appears, that with this kind of dephlogisticated air, the condensed liquor is not at all acid, when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

In order to see whether the same thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the same bottle, and the experiment tried in the same manner, except that the proportions of inflammable air were different. In the first, in which the burnt air was almost completely phlogisticated, the condensed liquor was not at all acid. In the second, in which its standard was 1,86, that is, not much phlogisticated, it was considerably acid ; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or

more phlogisticated ; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air from turbith mineral, and exploded it with inflammable air, the [133] proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared, by saturation with a solution of salt of tartar, to be of the nitrous kind ; and it was found, by the addition of some terra ponderosa salita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in such a proportion that the standard of the burnt air was about  $\frac{4}{10}$ , the condensed liquor was not in the least acid. There is no difference, however, in this respect between common air and dephlogisticated air mixed with phlogisticated in such a proportion as to reduce it to the standard of common air ; for some dephlogisticated air from red precipitate, being reduced to this standard by the addition of perfectly phlogisticated air, and then exploded with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind,

whatever substance the dephlogisticated air is procured from ; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever ; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows, that almost the whole of the inflammable and dephlogisticated air is converted into pure water. It is not easy, indeed, to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the [134] small nor the large globe could be perfectly exhausted of air, and there was no saying with exactness what quantity was left in them ; but in most of them, after allowing for this uncertainty, the true quantity of burnt air seemed not more than  $\frac{1}{7}$ th of the dephlogisticated air employed, or  $\frac{1}{60}$ th of the mixture. It seems, however, unnecessary to determine this point exactly, as the quantity is so small, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogisticated and inflammable air, and consequently that, if those airs could be obtained perfectly pure, the whole would be condensed.

With respect to common air, and dephlogisticated air reduced by the addition of phlogisticated air to the standard of common air, the case is different ; as the liquor condensed in exploding them with inflammable air, I believe I may say in any proportion, is not at all acid ; perhaps, because if they are mixed in such a proportion as that the burnt air is not

much phlogisticated, the explosion is too weak, and not accompanied with sufficient heat.

[All the foregoing experiments, on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. Priestley, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions. During the last summer also, a friend of mine gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston ; but at that time so far was M. Lavoisier from thinking any such opinion warranted, that, till he was prevailed [135] upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable that neither of these gentlemen found any acid in the water produced by the combustion, which might proceed from the latter having burnt the two airs in a different manner from what I did ; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.\*]

Before I enter into the cause of these phenomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston ; for when nitre is deflagrated

\* Interpolation by Dr. Blagden, after the paper had been read.—ED.

with charcoal, the acid is almost entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common process for making what is called clyssus of nitre; for if the nitre and charcoal are dry, scarce any thing is found in the vessels prepared for condensing the fumes; but if they are moist a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, consisting of some of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive, too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a [136] considerable analogy between that and the vitriolic acid; for the vitriolic acid when united to a smaller proportion of phlogiston, forms the volatile sulphureous acid and vitriolic acid air, both of which, by exposure to the atmosphere, lose their phlogiston, though not very fast, and are turned back into vitriolic acid, but, when united to a greater proportion of phlogiston, it forms sulphur, which shows no signs of acidity, unless a small degree of affinity to alkalies can be called so, and in which the phlogiston is more strongly adherent,

so that it does not fly off when exposed to the air, unless assisted by a heat sufficient to set it on fire. In like manner, the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air, which readily quit their phlogiston to common air ; but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shows no signs of acidity, and is still less disposed to part with its phlogiston than sulphur.

This being premised, there seem two ways by which the phenomena of the acid found in the condensed liquor may be explained ; first, by supposing that dephlogisticated air contains a little nitrous acid, which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion ; and, secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air, but that, when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston and turned into nitrous acid ; whereas, when the dephlogisticated air is not more than sufficient to consume the inflammable air, [137] none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid.

If the latter explanation be true, I think we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its

phlogiston ; or, in other words, that water consists of dephlogisticated air united to phlogiston ; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston ;\* since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid and deprived of its phlogiston ; but still the nitrous acid in it must make only a very small part of the whole, [138] as it is found that the phlogisticated

\* Either of these suppositions will agree equally well with the following experiments ; but the latter seems to me much the most likely. What principally makes me think so is, that common or dephlogisticated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance ; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity ; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them. On the other hand, I know no experiment which shows inflammable air to be pure phlogiston rather than an union of it with water, unless it be Dr. Priestley's experiment of expelling inflammable air from iron by heat alone. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it ; but I think it much more likely that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated ; and in all probability this was the cause of the separation of the phlogiston, as iron seems not disposed to part with its phlogiston by heat alone, without being assisted by the air or some other substance.

air, which it is converted into, is very small in comparison of the dephlogisticated air.

I think the second of these explanations seems much the most likely ; as it was found that the acid in the condensed liquor was of the nitrous kind, not only when the dephlogisticated air was prepared from red precipitate, but also when it was procured from plants or from turbith mineral ; and it seems not likely, that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol, should contain any nitrous acid.

Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur ; for if this air contains nitrous acid, and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

But what forms a stronger, and, I think, almost decisive argument in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air, as appears by the following experiments.

A mixture of 18,500 grain measures of inflammable air with 9750 of dephlogisticated air procured from red precipitate were exploded in the usual manner ; after which, a mixture of the same quantities of the same dephlogisticated and inflammable air, with the addition of 2500 of air phlogisticated by iron filings and sulphur, was treated in the same manner. The

condensed liquor, in both experiments, was acid, but that in the latter evidently more so, as appeared also by saturating each of them separately with marble powder, and precipitating [139] the earth by fixed alkali, the precipitate of the second experiment weighing one-fifth of a grain, and that of the first being several times less. The standard of the burnt air in the first experiment was 1,86, and in the second only 0,9.

It must be observed, that all circumstances were the same in these two experiments, except that in the latter the air to be exploded was mixed with some phlogisticated air, and that in consequence the burnt air was more phlogisticated than in the former ; and from what has been before said, it appears that this latter circumstance ought rather to have made the condensed liquor less acid ; and yet it was found to be much more so, which shows strongly that it was the phlogisticated air which furnished the acid.

As a further confirmation of this point, these two comparative experiments were repeated with a little variation, namely, in the first experiment there was first let into the globe 1500 of dephlogisticated air, and then the mixture, consisting of 12,200 of dephlogisticated air, and 25,900 of inflammable, was let in at different times as usual. In the second experiment, besides the 1500 of dephlogisticated air first let in, there was also admitted 2500 of phlogisticated air, after which the mixture, consisting of the same quantities of dephlogisticated and inflammable air as before, was let in as usual. The condensed liquor of the second experiment was about three times as acid

as that of the first, as it required 119 grains of a diluted solution of salt of tartar to saturate it, and the other only 37. The standard of the burnt air was 0,78 in the second experiment, and 1,96 in the first.

The intention of previously letting in some dephlogisticated air in the two last experiments was, that the condensed liquor [140] was expected to become more acid thereby, as proved actually to be the case.

In the first of these two experiments, in order that the air to be exploded should be as free as possible from common air, the globe was first filled with a mixture of dephlogisticated and inflammable air, it was then exhausted, and the air to be exploded let in ; by which means, though the globe was not perfectly exhausted, very little common air could be left in it. In the first set of experiments this circumstance was not attended to, and the purity of the dephlogisticated air was forgot to be examined in both sets.

From what has been said there seems the utmost reason to think that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water or else pure phlogiston ; but in all probability the former.

[As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. Watt says is true ;

but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat ; that solutions of sal ammoniac, and most other neutral salts, consist of the salt united to water and elementary heat ; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now, I have chosen to avoid this form of speaking [141] both because I think it more likely that there is no such thing as elementary heat, and because saying so in this instance, without using similar expressions in speaking of other chemical unions, would be improper, and would lead to false ideas ; and it may even admit of doubt, whether the doing it in general would not cause more trouble and perplexity than it is worth.\*]

There is the utmost reason to think that dephlogisticated and phlogisticated air, as M. Lavoisier and Scheele suppose, are quite distinct substances, and not differing only in their degree of phlogistication ; and that common air is a mixture of the two ; for if the dephlogisticated air is pretty pure, almost the whole of it loses its elasticity by phlogistication, and, as appears by the foregoing experiments, is turned into water, instead of being converted into phlogisticated air. In most of the foregoing experiments, at least  $\frac{16}{17}$ ths of the whole was

\* Second interpolation by Dr. Blagden, after the paper had been read.—Ed.

turned into water ; and by treating some dephlogisticated air with liver of sulphur, I have reduced it to less than  $\frac{1}{5}$ th of its original bulk, and other persons I believe have reduced it to a still less bulk ; so that there seems the utmost reason to suppose that the small residuum which remains after its phlogistication proceeds only from the impurities mixed with it.

It was just said, that some dephlogisticated air was reduced by liver of sulphur to  $\frac{1}{5}$ th of its original bulk ; the standard of this air was 4,8, and consequently the standard of perfectly pure dephlogisticated air should be very nearly 5, which is a confirmation of the foregoing opinion ; for if the standard of pure dephlogisticated air is 5, common air must, according to this opinion, contain one-fifth of it, and therefore ought to lose one-fifth of its bulk by phlogistication, which is what it is actually found to lose. [142]

From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say, it is deprived of, or receives, an addition of dephlogisticated air ; but as the other expression is convenient, and can scarcely be considered as improper, I shall still frequently make use of it in the remainder of this paper.

There seemed great reason to think, from Dr. Priestley's experiments, that both the nitrous and vitriolic acids were convertible into dephlogisticated air, as that air is procured in the greatest quantity from substances containing those acids, especially the

former. The foregoing experiments, however, seem to show that no part of the acid is converted into dephlogisticated air, and that their use in preparing it is owing only to the great power which they possess of depriving bodies of their phlogiston. A strong confirmation of this is, that red precipitate, which is one of the substances yielding dephlogisticated air in the greatest quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with spirits of sal ammoniac, and keeping them together for some days in a bottle, taking care to shake them frequently. The red colour of the precipitate was rendered pale, but not entirely destroyed ; being then washed with water and filtered, the clear liquor yielded on evaporation not the least ammoniacal salt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation ; but in order to determine more certainly whether this would be the case, I dried some of the same solution of quicksilver from which the red precipitate was prepared with a less heat, so that it acquired only an orange [143] colour, and treated the same quantity of it with volatile alkali in the same manner as before. It immediately caused an effervescence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmost reason to think, therefore, that red precipitate contains no nitrous acid ; and consequently that, in procuring dephlogisticated air from it, no acid is con-

verted into air ; and it is reasonable to conclude, therefore, that no such change is produced in procuring it from any other substance.

It remains to consider in what manner these acids act in producing dephlogisticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, seems to be as follows. On distilling the mixture of quicksilver and spirit of nitre, the acid comes over, loaded with phlogiston, in the form of nitrous vapour, and continues to do so till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but some of the watery part still remains behind, and adheres strongly to the quicksilver ; so that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or as quicksilver united to dephlogisticated air ;\* after which, on further increasing the heat, the water in it rises deprived of its phlogiston, that is, in the form of dephlogisticated [144] air, and at the same time the quicksilver distils over in its metallic form. It is justly remarked by Dr. Priestley, that the solution of quicksilver does not begin to

\* Unless we were much better acquainted than we are with the manner in which different substances are united together in compound bodies, it would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver ; all that we can say is, that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston. In like manner, during the preparation of the red precipitate, it is certain that the acid absorbs phlogiston, either from the quicksilver or the water ; but we are by no means authorised to say from which.

yield dephlogisticated air till it acquires its red colour.

Mercurius calcinatus appears to be only quicksilver which has absorbed dephlogisticated air from the atmosphere during its preparation ; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form. It seems, therefore, that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the same thing.

From what has been said it follows, that red precipitate and mercurius calcinatus contain as much phlogiston as the quicksilver they are prepared from ; but yet, as uniting dephlogisticated air to a metal comes to the same thing as depriving it of part of its phlogiston and adding water to it, the quicksilver may still be considered as deprived of its phlogiston ; but the imperfect metals seem not only to absorb dephlogisticated air during their calcination, but also to be really deprived of part of their phlogiston, as they do not acquire their metallic form by driving off the dephlogisticated air.

In procuring dephlogisticated air from nitre, the acid acts in a different manner, as, upon heating the nitre red hot, the dephlogisticated air rises mixed with a little nitrous acid, and at the same time the acid remaining in the nitre becomes very much phlogisticated ; which shows that the acid absorbs phlogiston from the water in the nitre, and becomes phlogisticated, while the water is thereby turned into dephlogisticated air. On distilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256,000

grain measures of dephlogisticated air,\* the [145] standard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort dissolved readily in water, and tasted alkaline and caustic. On adding diluted spirit of nitre to the solution, strong red fumes were produced ; a sign that the acid in it was very much phlogisticated, as no fumes whatever would have been produced on adding the same acid to a solution of common nitre ; that part of the solution also which was supersaturated with acid became blue ; a colour which the diluted nitrous acid is known to assume when much phlogisticated. The solution, when saturated with this acid, lost its alkaline and caustic taste, but yet tasted very different from true nitre, seeming as if it had been mixed with sea-salt, and also required much less water to dissolve it ; but on exposing it for some days to the air, and adding fresh acid as fast as by the flying off of the fumes the alkali predominated, it became true nitre, unmixed, as far as I could perceive, with any other salt.†

It has been remarked, that the dephlogisticated air procured from nitre is less pure than that from red precipitate and many other substances, which

\* This is about eighty-one grain measures from one grain of nitre ; and the [145] weight of the dephlogisticated air, supposing it 800 times lighter than water, is one-tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

† This phlogistication of the acid in nitre by heat has been observed by Mr. Scheele ; see his experiments on air and fire, p. 45, English translation.

may perhaps proceed from unglazed earthen retorts having been commonly used for this purpose, and which, conformably to Dr. Priestley's discovery, may possibly absorb some common air from without, and emit it along with the dephlogisticated air ; but if it should be found that the dephlogisticated air procured from nitre in glass or glazed earthen vessels is also impure, it would seem to show that part of [146] the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part.

From what has been said it appears, that there is a considerable difference in the manner in which the acid acts in the production of dephlogisticated air from red precipitate and from nitre ; in the former case the acid comes over first, leaving the remaining substance deprived of part of its phlogiston ; in the latter the dephlogisticated air comes first, leaving the acid loaded with the phlogiston of the water from which it was formed.

On distilling a mixture of quicksilver and oil of vitriol to dryness, part of the acid comes over, loaded with phlogiston, in the form of volatile sulphureous acid and vitriolic acid air ; so that the remaining white mass may be considered as consisting of quicksilver deprived of its phlogiston, and united to a certain proportion of acid and water, or of plain quicksilver united to a certain proportion of acid and dephlogisticated air. Accordingly, on urging this white mass with a more violent heat, the dephlogisticated air comes over, and at the same time part of the quicksilver rises in its metallic form, and also part of the white mass, united in all probability to a greater

proportion of acid than before, sublimes ; so that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar.

True turbith mineral consists of the above-mentioned white mass, well washed with water, by which means it acquires a yellow colour, and contains much less acid than the unwashed mass. Accordingly, it seems likely, that on exposing this to heat, less of it should sublime without being decompounded, and consequently that more dephlogisticated air should be procured from it than from the unwashed mass. [147]

This is an instance that the superabundant vitriolic acid may, in some cases, be better extracted from the base it is united to by water than by heat. Vitriolated tartar is another instance ; for, if vitriolated tartar be mixed with oil of vitriol and exposed even to a pretty strong red heat, the mass will be very acid ; but if this mass is dissolved in water, and evaporated, the crystals will be not sensibly so.

In all probability, the vitriolic acid acts in the same manner in the production of dephlogisticated air from alum, as the nitrous does in its production from nitre ; that is, the watery part comes over first in the form of dephlogisticated air, leaving the acid charged with its phlogiston. Whether this is also the case with regard to green and blue vitriol, or whether in them the acid does not rather act in the same manner as in turbith mineral, I cannot pretend to say, but I think the latter more likely.

There is another way by which dephlogisticated air has been found to be produced in great quantities,

namely, the growth of vegetables exposed to the sun or day-light ; the rationale of which, in all probability is, that plants, when assisted by the light, deprive part of the water sucked up by their roots of its phlogiston, and turn it into dephlogisticated air, while the phlogiston unites to, and forms part of, the substance of the plant.

There are many circumstances which show, that light has a remarkable power in enabling one body to absorb phlogiston from another. Mr. Senebier has observed, that the green tincture procured from the leaves of vegetables by spirit of wine, quickly loses its colour when exposed to the sun in a bottle not more than one-third part full, but does not do so in the dark, or if the bottle is quite full of the tincture, or if the air in it [148] is phlogisticated ; whence it is natural to conclude, that the light enables the dephlogisticated part of the air to absorb phlogiston from the tincture ; and this appears to be really the case, as I find that the air in the bottle is considerably phlogisticated thereby. Dephlogisticated spirit of nitre also acquires a yellow colour, and becomes phlogisticated by exposure to the sun's rays ;\* and I find on trial that the air in the bottle in which it is

\* If spirit of nitre is distilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourless, and fumes much less than other nitrous acid of the same strength, and the fumes are colourless. This is called dephlogisticated spirit of nitre, as it appears to be really deprived of phlogiston by the process. The manner of preparing it, as well as its property of regaining its yellow colour by exposure to the light, is mentioned by Mr. Scheele in the Stockholm Memoirs, 1774.

contained becomes dephlogisticated, or in other words, receives an increase of dephlogisticated air, which shows that the change in the acid is not owing to the sun's rays communicating phlogiston to it, but to their enabling it to absorb phlogiston from the water contained in it, and thereby to produce dephlogisticated air. Mr. Scheele also found, that the dark colour acquired by luna cornica on exposure to the light, is owing to part of the silver being revived ; and that gold, dissolved in aqua regia, and deprived by distillation of the nitrous and superfluous marine acid, is revived by the same means ; and there is the utmost reason to think, that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water.

Vegetables seem to consist almost entirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water, since by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere, and forms [149] water, they seem to be reduced almost entirely to water and those two kinds of air. Now plants growing in water without earth, can receive nourishment only from the water and air, and must therefore, in all probability, absorb their phlogiston from the water. It is known also that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when exposed to the light.

From what has been said, it seems likely that the use of light in promoting the growth of plants and the production of dephlogisticated air from them, is,

that it enables them to absorb phlogiston from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and should therefore, according to this hypothesis, absorb water from the atmosphere, and yield dephlogisticated air, which they have not been found to do. But we have no proof that they grew at all in any of those cases in which they were found not to yield dephlogisticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at first be entirely checked by it, especially considering the unnatural situation in which they must be placed in such experiments. Perhaps two plants growing in the dark may be able to absorb phlogiston from water not much impregnated with dephlogisticated air, but not from water strongly impregnated with it; and consequently, when kept under water in the dark, may perhaps at first yield some dephlogisticated air, which, instead of rising to the surface, may be absorbed by the water, and, before the water is so much impregnated as to suffer any to escape, the plant may cease to vegetate unless the water is changed. Unless, therefore, it could be shown that plants growing in the dark, in water alone, will increase in size, without yielding dephlogisticated [150] air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. Senebier finds that plants yield much more dephlogisticated air in distilled water impregnated with fixed air, than in plain distilled water, which is perfectly conformable to the above-mentioned hypo-

thesis ; for as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water.

\*[There are several memoirs of Mr. Lavoisier published by the Academy of Sciences, in which he entirely discards phlogiston, and explains those phenomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogisticated air ; and as not only the foregoing experiments, but most other phenomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phenomena ; the more so, as the foregoing experiments may, perhaps, induce the author himself to think some such additions proper.

According to this hypothesis, we must suppose, that water consists of inflammable air united to dephlogisticated air ; that nitrous air, vitriolic acid air, and the phosphoric acid, are also combinations of phlogisticated air, sulphur, and phosphorus, with dephlogisticated air ; and that the two former, by a further addition of the same substance, are reduced to the common [151] nitrous and vitriolic acids ; that

\* Addition by Mr. Cavendish after the paper had been read.—ED.

the metallic calces consist of the metals themselves united to the same substance, commonly, however, with a mixture of fixed air ; that on exposing the calces of the perfect metals to a sufficient heat, all the dephlogisticated air is driven off, and the calces are restored to their metallic form ; but as the calces of the imperfect metals are vitrified by heat, instead of recovering the metallic form, it should seem as if all the dephlogisticated air could not be driven off from them by heat alone. In like manner, according to this hypothesis, the rationale of the production of dephlogisticated air from red precipitate is, that during the solution of the quicksilver in the acid, and the subsequent calcination, the acid is decompounded, and quits part of its dephlogisticated air to the quicksilver, whereby it comes over in the form of nitrous air, and leaves the quicksilver behind united to dephlogisticated air, which, by a further increase of heat, is driven off, while the quicksilver reassumes its metallic form. In procuring dephlogisticated air from nitre, the acid is also decompounded ; but with this difference, that it suffers some of its dephlogisticated air to escape, while it remains united to the alkali itself, in the form of phlogisticated nitrous acid. As to the production of dephlogisticated air from plants, it may be said, that vegetable substances consist chiefly of various combinations of three different bases, one of which, when united to dephlogisticated air, forms water, another fixed air, and the third phlogisticated air ; and that by means of vegetation each of these substances are decomposed, and yield their dephlogisticated air ; and that in burning they again

acquire dephlogisticated air, and are restored to their pristine form.

It seems, therefore, from what has been said, as if the phenomena of nature might be explained very well on this principle [152] without the help of phlogiston ; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it, and as there are, perhaps, no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest ; but as the commonly received principle of phlogiston explains all phenomena, at least as well as Mr. Lavoisier's, I have adhered to that. There is one circumstance also, which though it may appear to many not to have much force, I own has some weight with me ; it is, that as plants seem to draw their nourishment almost entirely from water and fixed and phlogisticated air, and are restored back to those substances by burning, it seems reasonable to conclude, that notwithstanding their infinite variety they consist almost entirely of various combinations of water and fixed and phlogisticated air, united according to one of these opinions to phlogiston, and deprived according to the other of dephlogisticated air, so that, according to the latter opinion, the substance of a plant is less compounded than a mixture of those bodies into which it is resolved by burning ; and it is

more reasonable to look for great variety in the more compound than in the more simple substance.

Another thing which Mr. Lavoisier endeavours to prove is, that dephlogisticated air is the acidifying principle. From what has been explained it appears, that this is no more than saying, that acids lose their acidity by uniting to phlogiston, which, with regard to the nitrous, vitriolic, phosphoric, and arsenical acids, is certainly true. The same thing I believe, may be said of the acid of sugar; and Mr. Lavoisier's experiment is a [153] strong confirmation of Bergman's opinion, that none of the spirit of nitre enters into the composition of the acid, but that it only serves to deprive the sugar of part of its phlogiston. But as to the marine acid and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston. It is to be remarked also, that the acids of sugar and tartar, and in all probability almost all the vegetable and animal acids are by burning reduced to fixed and phlogisticated air and water, and therefore contain more phlogiston, or less dephlogisticated air than those three substances.]

## No. IV.

MEMOIRE OU L'ON PROUVE PAR LA DECOMPOSITION DE  
L'EAU, QUE CE FLUIDE N'EST POINT UNE SUBSTANCE  
SIMPLE, ET QU'IL Y A PLUSIEURS MOYENS D'OBTENIR  
EN GRAND L'AIR INFLAMMABLE QUI Y ENTRE COMME  
PRINCIPE CONSTITUANT. PAR MM. MEUSNIER ET LA-  
VOISIER.\*

Lû le 21 Avril 1784.

DEPUIS qu'on connoît l'expérience dans laquelle un mélange d'air inflammable et d'air déphlogistique, fait suivant les proportions convenables, ne produit en brûlant que de l'eau très-pure, à peu-près égale en poids à celui des deux airs réunis, il étoit difficile de ne pas reconnoître dans cette production d'eau, une preuve presque évidente que ce fluide, mis de tout temps au rang des substances simples, est réellement un corps composé ; et que les deux airs, du mélange desquels il résulte, en fournissent les principes constitutans. M. Lavoisier en tira cette conséquence dans un Mémoire qu'il lut à la dernière séance publique de cette Académie, en annonçant avec M. de la Place qu'ils avoient les premiers obtenu ainsi une quantité

\* Reprinted from the Mémoires de l'Académie des Sciences for 1781, (printed in 1784), pp. 269 to 283.

d'eau assez considérable pour la soumettre à quelques épreuves chimiques ;\* et en admettant quelqu'exactitude dans la détermination du poids des airs employés dans cette expérience, on ne voit pas comment il seroit possible de l'infirmer : on a cependant élevé des doutes sur cette réduction entière de deux fluides aéiformes en eau ; et malgré les soins apportés par M. Lavoisier, pour assurer, autant qu'il est possible, la précision d'une expérience aussi délicate ; malgré la conformité du résultat obtenu à peu-près en même temps par M. Monge, [270] dans le laboratoire de l'Ecole de Mézières, avec un appareil très-exact et les attentions les plus scrupuleuses, quelques personnes ont cru pouvoir attribuer l'eau qui provient de cette opération, à l'humidité dissoute par les airs, et privée de soutien au moment de leur combustion. Mais sans parler du peu de proportion d'une cause aussi légère avec la quantité d'eau dont il faut expliquer l'origine, si les airs eux-mêmes n'y entroient pour rien, il resteroit à trouver quel est le produit réel de leur combustion ; et puisqu'en en brûlant des volumes considérables, on n'obtient autre chose que cette eau très-pure qu'on voit couler de toutes parts, il s'ensuit

\* Ce Mémoire se trouve dans ce même volume. C'est par erreur qu'il a été imprimé postérieurement à celui-ci. [Notwithstanding this note, and a similar one which is printed with M. Lavoisier's subsequent Memoir, at p. 171, these two Memoirs have been allowed to retain here the same relative place which they occupy in the Mémoires de l'Académie for 1781. For although M. Lavoisier's paper was in part read before that by him and M. Meusnier, yet much of it contains express allusions to that other, and was therefore written later in order of time ; and we have in the Mémoires, as printed, no means of determining precisely the extent of the additions.—Ed.]

que même en admettant une erreur grossière dans la comparaison du poids des airs avec celui de l'eau qui se manifeste, l'explication qu'on vient de rappeler seraient encore sujettes aux difficultés les plus fortes. C'est au reste la multitude des faits, bien plutôt que le raisonnement, qui doit établir toute espèce de théorie nouvelle, et c'est la voie que nous avons prise dans le travail dont nous allons rendre compte, il est le fruit des recherches récentes auxquelles M. Lavoisier et moi avons eu occasion de nous livrer sur la production de l'air inflammable ; et voyant déjà tant de raisons de croire que c'est dans l'eau que la Nature a déposé tout celui dont elle fait usage pour ses diverses combinaisons, ayant éprouvé qu'en le tirant des corps plus composés, il est toujours altéré par le mélange des substances qui servoient à le fixer, nous ne pouvions être mieux conduits à le chercher directement dans ce fluide si abondant.

La question qu'il s'agissoit de résoudre étoit donc de décomposer l'eau, en lui présentant des intermèdes capables de s'unir à l'un de ses principes constituans, et tendans à cette union avec une force supérieure à celle qui lie ces principes entr'eux : et puisqu'il étoit si naturel de penser qu'outre l'air inflammable, l'eau contient encore l'air déphlogistique que nous avions vu contribuer à sa formation, il falloit chercher à en séparer ce dernier par le moyen des corps avec lesquels on lui connoît une grande affinité ; [271] c'étoit donc parmi les corps combustibles et les métaux calcinables que nous pouvions espérer de trouver les agens propres à opérer cette décomposition.

M. Lavoisier, conduit par ces principes, avoit déjà

tenté un mélange dont il rendit compte dans le Mémoire que je viens de citer, et avoit réussi par ce moyen à obtenir de l'air inflammable. De la limaille de fer et de l'eau mises en petite quantité dans la partie supérieure d'une cloche pleine de mercure, n'avoient pas tardé à laisser dégager ce fluide aéri-forme, qui au bout de quelques jours devint assez abondant pour en essayer la combustion, et le fer, calciné alors, annonçoit une absorption d'air déphlogistique, qu'il ne pouvoit avoir tiré que de l'eau dans laquelle il étoit plongé.

Cette expérience dans laquelle M. Lavoisier avoit opéré une vraie décomposition de l'eau, n'étoit cependant pas exempte de toute difficulté, et quoiqu'il eût employé de l'eau distillée, la petitessc du volume de l'air inflammable ainsi obtenu, pouvoit peut-être donner encore lieu aux objections qu'on a établies sur la supposition où cette eau n'eût pas été parfaitement pure. Il manquoit en effet quelque chose à ce procédé ; et puisque la matière de feu paroît un élément si essentiel à la formation de tous les fluides élastiques, qu'elle est presque toujours absorbée dans les expériences qui en produisent, et dégagée quand ils se condensent ; puisque sur-tout il s'en fait une production si considérable lorsque les deux airs qui constituent l'eau, la reforment par leur combustion ; et qu'enfin les métaux calcinables de même que les combustibles ne deviennent sensiblement altérables par l'air déphlogistique qu'à l'aide d'une température très élevée, il n'est pas étonnant qu'une opération, dans laquelle on n'employoit d'autre chaleur que celle de l'atmosphère, eût un effet si lent et si peu marqué.

La décomposition de l'eau exige donc, pour se faire rapidement, le concours d'une chaleur considérable, et c'est une condition principale que nous avions à remplir ; mais la difficulté de donner à l'eau une chaleur au-dessus du degré de son ébullition, étoit [272] encore un obstacle à nos vues ; et ce n'est qu'en la prenant déjà réduite en vapeurs, que nous avons pu la porter jusqu'à l'état d'incandescence auquel nous présumions qu'il étoit nécessaire de l'amener.

D'après ces considérations, l'appareil nécessaire se présente de lui-même et n'exigeroit pas une longue description ; mais quelqu'intéressantes qu'aient été pour nous les premières épreuves que nous en avons faites, et dont M. Berthollet a bien voulu être témoin et coopérateur, les bornes de ce Mémoire ne nous permettent pas d'entrer à ce sujet dans le détail qu'elles exigeroient, et nous passerons rapidement aux expériences plus concluantes que nous nous sommes empressés de tenter dès que notre appareil eut acquis successivement le degré de perfection nécessaire. Nous dirons seulement qu'en faisant passer dans un tube de fer incandescent, soit de l'eau en vapeurs fournie par une cornue à laquelle il étoit ajusté, soit de l'eau versée goutte à goutte au moyen d'un robinet ouvert imperceptiblement, et qui se vaporisant de même dès qu'elle commençoit à atteindre la partie rouge du fer, étoit également forcée, en la parcourant en entier, d'acquérir au passage le même degré de chaleur, nous avons constamment obtenu de grandes quantités d'air inflammable : que cet air présentoit, dans son inflammation et dans sa détonation avec l'air déphlogistique, tous les phénomènes qui caract-

térisent celui qu'on obtient par la dissolution de quelques métaux dans l'acide vitriolique : qu'il avoit de même une odeur très-marquée ; mais que n'offrant rien de semblable à celle de l'acide sulfureux qu'on démèle dans l'air inflammable ordinaire, celui-ci se rapprochoit insuffisamment plus de ce que les Chimistes ont nommé *empyreume* : que sa pésanteur spécifique déterminée avec des instrumens très-délicats, s'est toujours trouvée d'autant moindre que l'air atmosphérique qui remplissoit originairement l'appareil, s'y est mêlé en moindre proportion par rapport au volume total de l'air inflammable qu'on a fabriqué à chaque expérience, et que pour peu qu'on en [273] produise un volume décuple de la capacité des vaissceaux qu'on emploie, on l'obtient au moins neuf fois plus léger que celui de l'atmosphère : qu'enfin le tube de fer soumis à cette opération, éprouve successivement une altération considérable qui le rend de moins en moins propre à dégager l'air inflammable : que l'opération éprouve par cette raison, un ralentissement gradué jusqu'à ce qu'elle cesse enfin totalement, et qu'alors le fer calciné intérieurement se trouve converti sur une grande épaisseur en une matière singulière que nous décrirons plus bas, et qui annonce sa combinaison avec l'air déphlogistique qu'il devoit enlever à l'eau, pour mettre l'air inflammable en liberté.

Ces expériences expliquent donc l'observation faite assez récemment, que le fer rouge éteint dans l'eau, dégage de l'air inflammable ; en le plongeant au-dessous d'une cloche renversée et pleine d'eau, on voit en effet ce gaz se rassembler dans la partie supérieure de la cloche, et on lui trouve toutes les propriétés de celui

que nous venons de décrire : cette espèce d'épreuve est même extrêmement commode pour connoître sur le champ les diverses substances qui peuvent produire le même effet, et nous nous en sommes servis dans cette vue : nous allons encore rendre un compte succinct de ces tentatives générales.

Il étoit en effet bien essentiel de vérifier si les substances calcinables ou combustibles sont les seules qui puissent décomposer l'eau comme la théorie l'indiquoit ; et il étoit également intéressant de déterminer si elles ont toutes cette propriété : nous avons en conséquence soumis à l'expérience de l'extinction dans l'eau un assez grand nombre de corps incandescens, principalement des substances métalliques : celles qui sont facilement fusibles ont été mises dans des creusets, avec lesquels nous les avons plongées, et toutes ces épreuves ont été d'accord avec la théorie que nous avons exposée. Ainsi, l'or et l'argent, métaux parfaits, qui ne sont susceptibles d'aucune calcination, pris en masses considérables du poids de trente et quarante-cinq marcs, et plongés presque [247] fondans, n'ont point fourni d'air inflammable : des cailloux rougis, des creusets vides, substances également dénuées d'affinité pour l'air déphlogistique, n'ont dégagé, comme les premiers, qu'un air incombustible en très-petite quantité, que tout annonce être celui que l'eau tient naturellement en dissolution. Le cuivre rouge, quoique calcinable, a eu le même sort ; n'ayant pas sans doute avec l'air déphlogistique le degré d'affinité suffisante pour le séparer de l'air inflammable, et il est bien remarquable que, dissous par l'acide vitriolique, il n'en fournit pas non plus ; mais le zinc qui

à cet égard se comporte comme le fer, a donné aussi comme lui de l'air inflammable par son contact avec l'eau : le charbon végétal et le charbon de terre, plongés brûlans, en ont également fourni, quoiqu'on les eût épuisés par une longue combustion de tout celui qu'ils pouvoient donner par la seule chaleur ; et il faut bien que l'eau soit essentielle à ces divers phénomènes, puisque l'immersion dans le mercure ne produit rien de semblable : quant à l'étain et au régule d'antimoine, ils ont constamment occasioné des explosions si fortes que les cloches ont été brisées avec éclat, et ils nous ont appris à ne plus tenter ces sortes d'épreuves qu'avec des précautions particulières.

En même temps que nous voyions la théorie qui nous guidoit se confirmer de plus en plus, nous venions d'acquérir par ces dernières expériences une connoissance précieuse pour la pratique, en apprenant qu'un métal commun dans les Arts, tel que le cuivre rouge, qui peut, après le fer, supporter la plus grande chaleur, n'éprouve aucune altération de la part de l'eau, dans l'état d'incandescence. Si en effet ce métal se fût calciné comme le fer, on n'auroit pu fabriquer pour ces sortes d'expériences que des appareils exposés à une prompte destruction, et les recherches expérimentales y auroient presque autant perdu que les usages auxquels on appliquera les nouvelles méthodes qui résultent de ce travail pour la fabrication de l'air inflammable ; car le verre ou les poteries sont infiniment trop fragiles pour être employés en [275] grand à des opérations de ce genre, et l'on sait d'ailleurs que ces dernières ne sont plus imperméables à l'air, dès qu'elles sont échauffées au point de devenir

rouges. C'est donc de cuivre que doivent être faits par la suite les appareils que l'on destinera à ces sortes de décompositions de l'eau, et l'on y renfermera les substances que l'on jugera pouvoir y employer ; nous cherchâmes en conséquence à nous procurer des tubes de ce métal, coulés d'une seule pièce et sans soudure, mais l'empressement, bien naturel dans des recherches aussi neuves, nous engagea à continuer les nôtres avec les tubes de fer que nous avions sous la main.

Il ne s'agissoit plus alors de chercher de nouvelles méthodes pour fabriquer l'air inflammable, nous nous voyions en possession d'une théorie féconde, de laquelle dérive une multitude de ces moyens ; mais plus cette théorie cadroit avec les épreuves que nous avions déjà faites, plus nous devions l'examiner sévèrement, et multiplier pour cela les expériences de poids et de mesure, sans lesquels la Physique ni la Chimie ne peuvent plus guère rien admettre.

Nous cherchâmes donc d'abord à constater si en mesurant exactement toute l'eau qu'on fait passer dans l'appareil que nous avons indiqué, et recueillant de même celle qui se condense, après en avoir parcouru toute la longueur, il se trouveroit entre ces deux quantités une différence notable qu'on pût attribuer à l'eau décomposée qui auroit ainsi changé de nature : ainsi, au lieu de faire aboutir immédiatement le tube de fer à l'appareil pneumato-chimique, nous interposâmes un serpentin environné d'eau froide, et l'eau qui se condensoit dans ce réfrigérent, étoit versée dans un flacon tubulé, d'où les produits aériformes se rendoient, comme à l'ordinaire, sous le cloches de

l'appareil par un conduit particulier appliqué à la tubulure du flacon. La Planche jointe à ce Mémoire, donne une idée complète de toute cette disposition ; on y voit en détail l'entonnoir qui verse l'eau goutte à goutte, à l'aide d'un robinet qui en traverse la queue, le tube de fer où elle passe ensuite, le brasier qui [276] l'échauffe, le serpentin, le récipient, et enfin la cloche où est recueilli l'air inflammable : il est presque inutile d'observer que toutes les jointures de cet appareil étoient hermétiquement fermées par des luts, de l'exactitude desquels on s'est assuré avec le plus grand soin.

Plusieurs Membres de l'Académie voulurent bien être témoins de cette expérience importante, il en résulta cent vingt-cinq pintes d'air inflammable, et il s'en fallut trois onces un gros que l'eau reçue au sortir de l'appareil n'égalât celle que l'entonnoir supérieur y avoit versée ; ce *deficit*, beaucoup trop considérable pour qu'on pût l'attribuer à l'humidité qui avoit dû mouiller l'intérieur de la machine, annonce donc qu'une certaine quantité d'eau étoit vraiment disparue, et avoit contribué à former l'air inflammable ainsi obtenu : cet air fut pesé avec la plus scrupuleuse attention, il étoit neuf fois et demi plus léger que l'air atmosphérique, et le volume total qui en avoit été produit, pesoit par conséquent quatre gros et quelques grains : il est à remarquer que c'est, à quelques grains près, le sixième de la quantité d'eau que nous avons vu s'être dissipée, et que cette proportion est aussi précisément celle qui résulte de l'expérience capitale dans laquelle on forme de l'eau par la combustion des deux airs.

Une seconde expérience faite avec le même canon, dans la vue de la calciner entièrement, a encore fourni soixante-une pintes d'air inflammable, avec une déperdition d'eau d'une once sept gros, dont la sixième partie étoit encore, à quelques grains près, égale au poids total du gaz dégagé.

On avoit réussi parfaitement à préserver ce tube de fer de l'action de l'air extérieur, par des enveloppes et des luts d'argile arrangés avec soin ; il se cassa néanmoins avec facilité quand on voulut en visiter l'intérieur, et à l'exception d'une couche très-mince de fer doux qui le couvroit par dehors, il se trouva converti tout entier en une matière qui n'avoit plus du fer que la couleur, mais elle présentoit un grain composé de facettes brillantes qui lui donnoient quelque [277] ressemblance avec la mine de fer spéculaire ; la surface intérieure paroissoit même être devenue d'autant plus fusible, qu'elle étoit plus saturée d'air déphlogistique, et formoit ainsi sur un tiers de ligne d'épaisseur une doublure lisse et brillante, sur laquelle le burin ni la lime ne mordoiient plus, tandis que les parties plus éloignées du centre, présentoient un grain plus inégal et comme rempli de petites cavités : l'aimant attire d'autant moins les différentes parties de cette matière, qu'elles sont plus voisines de l'état de la doublure intérieure, mais son action paroît devoir être toujours sensible : enfin le métal avoit considérablement augmenté de volume en éprouvant ce changement, puisque le calibre intérieur fut réduit de sept lignes à quatre, sans que le diamètre extérieur eût changé.

Cette substance éprouvée par les acides, ne donne

plus aucune espèce de gaz, il en reste même une quantité considérable qui demeure indissoluble ; et quoiqu'ayant beaucoup de rapport avec le fer calciné par l'air déphlogistique qui se trouve dans l'air libre, c'est cependant, à beaucoup d'égards, une matière nouvelle qui mérite l'attention des Chimistes.

Indépendamment des connaissances acquises dans ces derniers temps, sur la cause de la calcination des métaux, tout annonçoit donc dans cet état du fer, l'admission d'une substance étrangère qui en avoit augmenté le volume et changé l'organisation : il falloit bien en effet que les cinq sixièmes du poids de l'eau qui nous manquoit, eussent été employés, et leur union avec le métal étoit la seule destination qu'on pût leur attribuer, puisqu'il n'y a point dans la Nature de déperdition proprement dite ; mais la persuasion où nous étions que notre tube de fer seroit calciné par dehors, nous ayant fait négliger de le pincer avant l'opération, nous ne pumes acquérir de cette conséquence une confirmation directe que son évidence ne pouvoit nous empêcher de désirer.

Nous entreprimes donc une nouvelle expérience, dont l'objet étoit de constater si le fer augmente de poids quand [278] il se calcine par le contact de l'eau, comme quand il se calcine dans l'air libre ou dans l'air déphlogistique. C'étoit d'ailleurs le moyen le plus direct de répondre à l'objection qu'on pourroit peut-être encore faire contre la décomposition de l'eau, en attribuant tout l'air inflammable que nous avons obtenu, au métal qui l'auroit fourni, et non à l'eau de laquelle nous croyons qu'il provient : dans cette manière de voir, le fer perdant un de ses princi-

pes, diminueroit de poids, tandis que dans la théorie que nous avons adoptée il doit au contraire augmenter. Cette expérience étoit donc la plus propre à décider la question d'une manière définitive.

N'ayant pu encore obtenir aucun des tubes de cuivre rouge que nous avions demandés afin d'y introduire un morceau de fer d'un poids connu et déterminé scrupuleusement, nous cherchames au moins à en faire une sorte d'imitation avec un nouveau tube de fer dans lequel nous fimes appliquer une feuille de cuivre rouge qui lui servoit de doublure : nous ne pumes à la vérité fermer exactement la jointure longitudinale, parée qu'il n'y a point de soudure qui ne soit trop fusible pour le degré de chaleur que nous avions intention de produire ; mais si nous ne préservames pas en entier le fer du canon de l'action de l'eau en vapeurs, nous diminuames au moins de beaucoup cette action étrangère à notre objet présent. Nous introduisimes dans cet appareil une baguette de fer plate, roulée sur elle-même comme le filet d'une vis, et occupant ainsi une longueur de 18 pouces ; et pour éviter que, devenue plus fusible, elle n'adhérât à la doublure de cuivre, nous la mimes dans un canal de même métal, avec lequel nous devions la retirer avec facilité quand l'opération seroit finie : notre baguette de fer pesoit exactement deux onces cinq gros quarante-sept grains.

Cette opération consomma une once cinq gros cinquante-quatre grains d'eau, et produisit cinquante-trois pintes d'air inflammable : la baguette de fer calcinée par l'eau, avoit [279] éprouvé à sa surface une sorte de fusion, qui en avoit arrondi les arêtes, et son

poids se trouva augmenté de deux gros cinquante-quatre grains, comme notre théorie le demandoit. Cette augmentation de poids fait presque un septième du total, mais nous nous sommes assurés qu'il restoit encore dans cette baguette une grande quantité de fer non calciné, qui en formoit le noyau, que le reste étoit composé de différentes couches inégalement calcinées, de sorte que n'étant pas à beaucoup près saturée d'air déphlogistique, elle ne peut servir à déterminer la vraie dose de cette saturation, mais il paroît qu'elle ne doit pas être éloignée de celle qu'on observe dans le fer caleiné par l'air libre, qui augmente d'environ un quart de son poids.

Après avoir ainsi varié les expériences pour constater les phénomènes que présente le coneours du fer et de l'eau dans l'état d'incandescence, et en avoir tiré des preuves démonstratives, que l'eau ne fournit l'air inflammable, qu'autant qu'elle dépose l'air déphlogistique dont elle contient encore la base, nous résolumes de prendre cette théorie pour toutes ses conséquences, et d'établir, en les vérifiant, autant d'expériences confirmatives : ainsi, voyant, par ce qui précède, que le fer a plus d'affinité avec l'air déphlogistique, que celui-ci n'en a pour l'air inflammable, puisqu'il les sépare l'un de l'autre en décomposant l'eau ; sachant d'ailleurs par l'opération la plus commune en Metallurgie, que le principe du charbon a plus d'affinité encore avec l'air déphlogistique, puisqu'il enlève celui-ci au fer, pour le ramener à l'état métallique, nous en conelumes que le charbon étoit à plus forte raison propre à décomposer l'eau, et qu'il devoit brûler sans le coneours de l'air, dès qu'on lui appli-

queroit cette autre substance. Nous avions en effet éprouvé, comme on l'a vu plus haut, que ce corps, plongé dans l'eau, en dégage de l'air inflammable ; mais une combustion complète étant la seule preuve propre à nous satisfaire, nous pensames à introduire du charbon dans le même appareil où nous venions de déterminer l'augmentation de poids du fer ; et pour priver ce charbon de tout l'air inflammable, [280] par lequel il pouvoit encore participer à l'état du bois dont il vient originairement, et que la simple chaleur auroit pu en dégager, nous l'épuisames entièrement en le tenant pendant deux heures et demie dans un creuset rougi à blanc, qui n'étoit fermé qu'autant qu'il falloit pour empêcher le libre accès de l'air extérieur.

Il étoit aisé de prévoir le résultat de cette expérience, d'après la théorie donnée antérieurement par M. Lavoisier, sur la combustion du charbon : ce corps uni avec l'air déphlogistique de l'eau devoit produire de l'air fixe, et l'air inflammable de l'eau devoit ainsi en être mêlé en grande quantité.

Nous mimes donc dans notre appareil quatre gros et quinze grains de charbon préparé, comme nous l'avons dit plus haut, et nous procédames d'ailleurs comme dans les autres expériences ; celle-ci dissipâ deux onces trois gros d'eau, qui avec le charbon composoient un total de près de trois onces, et nous ne retrouvames de toutes ces substances que six grains de cendre qui restèrent dans le canal de cuivre où le charbon avoit été arrangé ; mais il s'étoit formé cent dix-huit pines d'un fluide aériforme inflammable, qui éprouvé fréquemment par l'alkali caustique, contenoit

un peu plus du quart de son volume d'air fixe ; il pesoit à peu-près la moitié de l'air atmosphérique, et cette pesanteur cadroit parfaitement avec les proportions dans lesquelles la théorie indiquoit que l'air fixe et l'air inflammable de l'eau devoient se trouver mélangés.

Le volume total de l'air ainsi obtenu, pesoit donc environ neuf gros vingt-deux grains, c'est-à-dire, plus du double du charbon employé ; cette expérience suffiroit donc seule pour offrir une preuve démonstrative, que l'eau peut se réduire en fluide aériforme, puisque cet excédant ne pouvoit venir que de l'eau consommée, et le poids de celle-ci s'y seroit retrouvé en entier, si le canon mal défendu par la doublure de cuivre n'eût absorbé une partie de l'air déphlogistique qu'elle contenoit ; cette expérience montre enfin le [281] premier exemple d'une combustion entière, opérée sans le concours de l'air, et ne laisse plus de doute, tant sur la nature du vrai principe de la respiration et de la combustion, que sur son identité avec celui que l'eau dépose quand elle forme l'air inflammable.

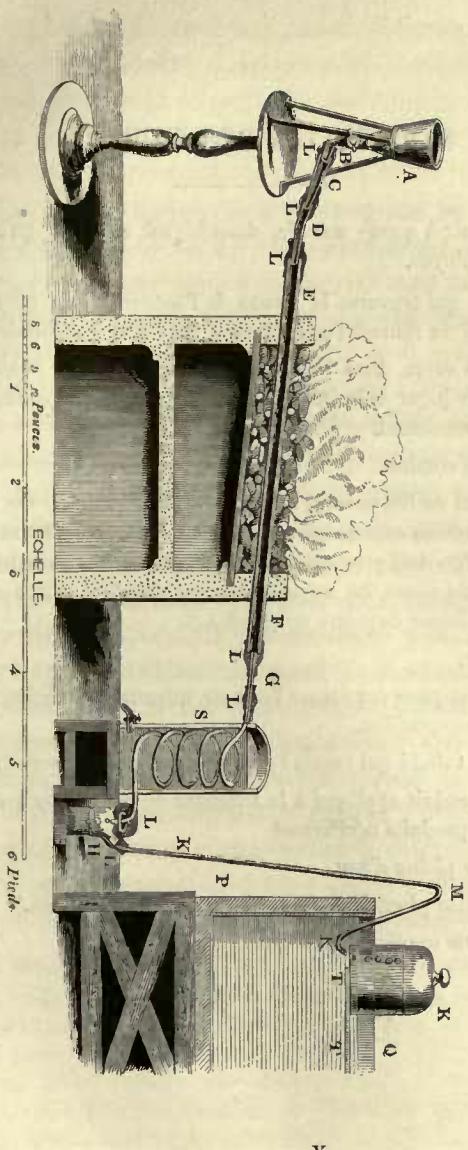
On demandera sans doute quel est, d'après notre travail, le vrai degré de légèreté de l'air inflammable de l'eau, et le poids qu'elle en contient : la petite quantité d'eau retenue par notre appareil, et l'air atmosphérique qui le remplissoit originairement, font que chacune de nos expériences ne peut pas seule déterminer ces données avec une précision mathématique ; mais en comparant ensemble plusieurs épreuves, on peut, à l'aide d'une analyse fort simple, en déduire ces éléments essentiels de la théorie générale. Nous réservons pour un Mémoire ultérieur, les détails de ce calcul, que nous nous proposons d'établir sur un plus

grand nombre d'expériences ; mais il résulte de celles que nous avons faites jusqu'ici, que l'air inflammable de l'eau dans son plus grand état de pureté, et séparé de celui des appareils qui s'y mêle pendant l'opération, seroit environ treize fois plus léger que celui de l'atmosphère, et que l'eau en contient à peu-près la septième partie de son poids ; d'où il suit qu'elle en peut fournir un volume quinze cents fois égal au sien.

On voit par ces proportions, pourquoi dans l'expérience de la combustion des deux airs, l'eau formée n'a jamais égalé rigoureusement leurs poids réunis ; ce *deficit*, que les soins les plus attentifs n'ont jamais pu annuler, et que M. Monge a trouvé lui-même avec un appareil fermé de toutes parts, qu'on peut regarder comme un modèle de précision, vient de ce que l'air inflammable que l'on a employé, pesant toujours au moins la dixième partie de celui de l'atmosphère, contenait un fluide plus pesant, outre l'air inflammable propre à constituer l'eau ; on peut même maintenant calculer ce *deficit*, et à l'aide de nos nouvelles données, on trouve *a priori* qu'il devoit aller à environ un douzième de la somme du poids des deux airs. [282]

L'application de cette théorie, à la fabrication de l'air inflammable en grand, ne laisse plus maintenant que le choix des moyens ; un fourneau fort simple, traversé d'un ou plusieurs tuyaux de cuivre, et un réservoir fournissant continuellement un filet d'eau, composeront généralement l'appareil propre à cette opération ; enfermant ensuite dans cet appareil celle des substances qu'on jugera devoir employer, ou fournissant encore un filet des matières fluides combustibles qui peuvent également y servir, on aura

l'air inflammable donné par l'eau décomposée ; ainsi le fer disposé de manière à présenter une grande surface, comme des rognures de tôle ou de fer battu, donnera sans acide vitriolique, et cependant en même quantité, l'air le plus léger qu'on connoisse, à raison de cinq à six pieds cubes par livre ; le charbon végétal opérera avec encore plus de vitesse et d'abondance, car une livre de cette substance peut dégager cinquante-quatre pieds cubes d'air inflammable de l'eau ; mais il se trouve mélangé d'environ un quart d'air fixe qu'il faut absorber par les lessives alkalines caustiques, et dont peut-être l'air inflammable retiendroit encore une petite portion : il en est de même des autres corps combustibles, tels que les huiles, l'esprit-de-vin ou l'eau-de-vie, et le charbon de terre. Plusieurs, quoique chers en apparence, comme l'esprit-de-vin et l'eau-de-vie, se résolvent seuls et en entier en une immense quantité d'air inflammable, dont le concours de l'eau convertit en air fixe la partie qui en altère la légèreté, ce qui la rend dès-lors absorbable par les alkalis ; et nous nous sommes assurés que par ce moyen on peut rendre tous ces airs environ quatre fois plus légers que l'air commun ; mais c'est la matière d'un travail de pratique qui ne peut être bien fait qu'en grand, et auquel nous avons le projet de nous livrer. [283]



Y

## EXPLICATION DES FIGURES.

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*A*, Entonnoir à queue coudée, dans lequel est l'eau qu'on veut employer.

*B*, Robinet qui traverse la queue de l'entonnoir, au moyen duquel on fournit l'eau goutte à goutte et à volonté.

*C*, Tube de verre dans lequel aboutit la queue de l'entonnoir, pour juger de la fréquence avec laquelle les gouttes d'eau se succèdent.

*D*, Allonge coudée.

*E F*, Canon de fer passant au travers d'un brasier. On a pour certaines expériences doublé ce canon de cuivre rouge, et l'on doit y substituer en pareil cas des tubes de cuivre ou de verre, en enveloppant ces derniers d'une certaine épaisseur de plâtre en poudre.

*G*, Allonge.

*S*, Serpentin pour condenser l'eau en vapeurs qui a échappé à la décomposition.

*H*, Flacon tubulé qui reçoit l'eau condensée par le serpentin.

*K K K*, Conduit appliqué à la tubulure du flacon, pour évacuer les produits aériformes.

*P Q*, Cuve pleine d'eau.

*T T*, Tablette plongée à un ou deux pouces sous l'eau.

*L L L*, Luts appliqués aux différentes jointures.

## No. V.

MEMOIRE DANS LEQUEL ON A POUR OBJET DE PROUVER  
QUE L'EAU N'EST POINT UNE SUBSTANCE SIMPLE, UN  
ELEMENT PROPREMENT DIT, MAIS QU'ELLE EST SUSCEP-  
TIBLE DE DECOMPOSITION ET DE RECOMPOSITION.\*  
PAR M. LAVOISIER.†

Y A-T-IL plusieurs espèces d'airs inflammables ? ou bien celui que nous obtenons, est-il toujours le même, plus ou moins mélangé, plus ou moins altéré par l'union de différentes substances qu'il est susceptible de dissoudre ? C'est une question que je n'entreprendrai pas de résoudre dans ce moment ; il me suffira de dire que l'air inflammable dont j'entends parler dans ce Mémoire, est celui qu'on obtient, soit de la décomposition de l'eau par le fer seul, soit de la dissolution du fer et du zinc dans les acides vitriolique et marin ; que comme il paroît prouvé que dans tous les cas cet air vient originairement de l'eau, je l'appellerai, lorsqu'il se présentera dans l'état aériiforme, *air inflammable aqueux*; et lorsqu'il sera engagé dans quelque combinaison, *principe inflammable*

\* Ce Mémoire a été lu à la Rentrée publique de la Saint-Martin 1783 ; depuis on y a fait quelques additions relatives au travail fait en commun avec M. Meusnier, sur le même objet. Il auroit dû se trouver placé avant celui-là par M. Meusnier, à la Séance publique de Pâques 1784. Voyez, p. 269. [See Note on p. 152.—Ed.]

† Reprinted from the Mémoires de l'Académie des Sciences for 1781, (printed in 1784,) pp. 468 to 494.

aqueux. La suite de ce Mémoire éclaircira ce que ce premier énoncé peut présenter d'obscur. Cet air pèse douze fois et demie moins que l'air commun, lorsqu'il est porté au dernier degré de pureté dont il est susceptible ; c'est au moins ce qui résulte des expériences que nous avons faites en commun, M. Meusnier et moi, et qui sont imprimées dans ce volume ; mais il est souvent mêlé d'air fixe [469] ou acide charbonneux dont il est difficile de séparer les dernières portions ; plus souvent encore il tient de la substance charbonneuse en dissolution, et sa pesanteur spécifique en est considérablement augmentée.

Si on brûle ensemble sous une cloche de verre, au moyen des caisses pneumatiques que j'ai décrites dans un Mémoire particulier, un peu moins de deux parties d'air inflammable aqueux, contre une d'air vital, en supposant que l'un et l'autre soient parfaitement purs, la totalité des deux airs est absorbée, et l'on trouve à la surface du mercure sur lequel se fait cette expérience, une quantité d'eau égale en poids à celui des deux airs qu'on a employés : je suppose, comme je l'ai dit, que les deux airs soient parfaitement purs (et c'est une condition, il est vrai, difficile à obtenir;) mais dans le cas de mélange, il y a un résidu plus ou moins considérable, et il y a dans le poids de l'eau qui s'est formée un *deficit* égal à celui de ce résidu.

L'eau qu'on obtient par ce procédé, est parfaitement pure et dans l'état d'eau distillée ; quelquefois elle est imprégnée d'une légère portion d'air fixe, et c'est une preuve alors, ou que l'air inflammable aqueux tenoit de la substance charbonneuse en dissolution, ou que l'un des deux airs étoit mélangé d'air fixe.

Tel est en général le résultat de la combustion de l'air vital et de l'air inflammable; mais comme on a voulu éléver quelque doute sur l'antériorité de cette découverte, je me crois obligé d'entrer dans quelques détails sur la suite des expériences qui m'y ont conduit. Les premières tentatives qui aient été faites pour déterminer la nature du résultat de la combustion de l'air inflammable, remontent à 1776 ou 1777; à cette époque, M. Macquer ayant présenté une soucoupe de porcelaine blanche à la flamme de l'air inflammable qui brûloit tranquillement à l'orifice d'une bouteille, il observa que cette flamme n'étoit accompagnée d'aucune fumée fuligineuse; il trouva seulement la soucoupe mouillée de gouttelettes assez sensibles d'une liqueur blanche comme de l'eau, et [473] qu'il a reconnu, ainsi que M. Sigaud de la Fond qui assistoit à cette expérience, pour de l'eau pure. (Voyez *Dictionnaire de Chimie, seconde édition, article Gaz inflammable.*) Je n'eus pas connaissance alors de l'expérience de M. Macquer, et j'étois dans l'opinion que l'air inflammable en brûlant devoit donner de l'acide vitriolique ou de l'acide sulfureux. M. Bucquet au contraire pensoit qu'il devoit en résulter de l'air fixe. Pour éclaircir nos doutes, nous remplimes au mois de Septembre 1777, M. Bucquet et moi, d'air inflammable obtenu par la dissolution du fer dans l'acide vitriolique, une bouteille de cinq à six pintes; nous la retournames l'ouverture en haut, et pendant que l'un de nous allumoit l'air avec une bougie à l'orifice de la bouteille, l'autre y versa très-promptement, à travers de la flamme même, deux onces d'eau de chaux: l'air brûla d'abord paisiblement à l'ouverture

du gouleau qui étoit fort large; ensuite la flamme descendit dans l'intérieur de la bouteille, et elle s'y conserva encore quelques instans. Pendant tout le temps que la combustion dura, nous ne cessames d'agiter l'eau de chaux, et de la promener dans la bouteille, afin de la mettre, le plus qu'il seroit possible, en contact avec la flamme; mais la chaux ne fut point précipitée, l'eau de chaux ne fit que loucher très-légèrement, en sort que nous reconnumes évidemment que le résultat de la combustion de l'air inflammable et de l'air atmosphérique n'étoit point de l'air fixe.

Cette expérience, qui détruisoit l'opinion de M. Bucquet, ne suffisoit pas pour établir la mienne: j'étois en conséquence curieux de la répéter et d'en varier les circonstances, de manière à la confirmer ou à la détruire. Ce fut dans l'hiver de 1781 à 1782 que je m'en occupai et M. Gingembre, déjà connu de l'Académie, voulut bien être mon coopérateur pour une expérience qu'il m'étoit impossible de faire seul. Nous primes une bouteille de six pintes, que nous remplimes d'air inflammable; nous l'allumames très-promptement, et nous y versames en même temps deux onces d'eau de chaux; aussi-tôt nous bouchâmes la bouteille avec un bouchon [471] de liège, traversé d'un tube de cuivre terminé en pointe, et qui correspondoit par un tuyau flexible, avec une caisse pneumatique remplie d'air vital. Le bouchon ayant interrompu le contact de l'air inflammable et de l'air de l'atmosphère, la surface de l'air inflammable cessa de brûler, mais il se forma à l'extrémité du tube de cuivre, dans l'intérieur de la bouteille, un beau dard de

flamme très-brillant, et nous vimes avec beaucoup de plaisir l'air vital brûler dans l'air inflammable, de la même manière et avec les mêmes circonstances que l'air inflammable brûle dans l'air vital. Nous continuâmes assez long temps cette combustion, en agitant l'eau de chaux et en la promenant dans la bouteille sans qu'elle donnât la moindre apparence de précipitation ; enfin une légère détonation qui se fit, et que nous attribuâmes à quelques portions d'air commun qui sans doute étoit rentré, éteignit la flamme et mit fin à l'expérience.

Nous répétabmes deux fois cette expérience, en substituant à l'eau de chaux, dans l'une de l'eau distillée, dans l'autre de l'alkali affoibli ; l'eau après la combustion se trouva aussi pure qu'auparavant, elle ne donnoit aucun signe d'acidité, et la liqueur alkaline étoit précisément dans le même état qu'elle étoit avant l'expérience.

Ces résultats me surprisrent d'autant plus, que j'avois antérieurement reconnu que dans toute combustion il se formoit un acide, que cet acide étoit l'acide vitriolique si on brûloit du soufre, l'acide phosphorique si on brûloit du phosphore, l'air fixe si l'on brûloit du charbon ; et que l'analogie m'avoit porté invinciblement à conclure que la combustion de l'air inflammable devoit également produire un acide.

Cependant rien ne s'anéantit dans les expériences ; la seule matière du feu, de la chaleur et de la lumière, a la propriété de passer à travers les pores des vaisseaux ; les deux airs qui sont des corps pesans, ne pouvoient donc avoir disparu, ils ne pouvoient être anéantis : de-là la nécessité de faire les expériences

avec plus d'exactitude et plus en grand. Je fis construire en conséquence une seconde caisse pneumatique, [472] afin que l'une fournissant l'air inflammable, l'autre l'air vital, on pût continuer plus long-temps la combustion : au lieu d'un simple ajutoir de cuivre, j'en fis faire un double destiné à conduire les deux airs ; des robinets adaptés à chacun, donnaient la facilité de ménager à volonté les quantités d'airs : ces deux ajutages, ou plutôt ce double ajutage, car il n'en formoit qu'un, à deux tuyaux, s'appliquoit à frottement à la tubulure supérieure de la cloche, où devoit se faire l'expérience ; il avoit été usé dessus de la même manière qu'on use un bouchon de cristal pour l'ajuster à un flacon.

Ce fut le 24 Juin 1783 que nous fimes cette expérience, M. de la Place et moi, en présence de MM. le Roi, de Vandermonde, de plusieurs autres Académiciens, et de M. Blagden, aujourd'hui Secrétaire de la Société royale de Londres ; ce dernier nous apprit que M. Cavendish avoit déjà essayé, à Londres, de brûler de l'air inflammable dans des vaisseaux fermés, et qu'il avoit obtenu une quantité d'eau très-sensible.

Nous commençames d'abord à chercher par voie de tâtonnement, quelle devoit être l'ouverture de nos robinets pour fournir la juste proportion des deux airs ; nous y parvinmes aisément en observant la couleur et l'éclat du dard de flamme qui se formoit au bout de l'ajutoir ; la juste proportion des deux airs donnoit la flamme la plus lumineuse et la plus belle. Ce premier point trouvé, nous introduisimes l'ajutoir dans la tubulure de la cloche, laquelle étoit plongée

sur du mercure, et nous laissâmes brûler les airs jusqu'à ce que nous eussions épuisé la provision que nous en avions faite : dès les premiers instans, nous vimes les parois de la cloche s'obscurcir et se couvrir de vapeurs ; bientôt elles se rassemblèrent en gouttes, et ruisselèrent de toutes parts sur le mercure, et en quinze ou vingt minutes, sa surface s'en trouva couverte. L'embarras étoit de rassembler cette eau ; mais nous y parvinmes aisément en passant une assiette sous la cloche sans la sortir du mercure, et en versant ensuite l'eau et le mercure dans un entonnoir de verre : en laissant ensuite couler le mercure, l'eau se trouva réunie [473] dans le tube de l'entonnoir ; elle pesoit un peu moins de 5 gros.

Cette eau soumise à toutes les épreuves qu'on pût imaginer, parut aussi pure que l'eau distillée : elle ne rougissait nullement la teinture de tournesol ; elle ne verdissait pas le sirop de violettes ; elle ne précipitait pas l'eau de chaux ; enfin, par tous les réactifs connus on ne put y découvrir le moindre indice de mélange.

Comme les deux airs étoient conduits des caisses pneumatiques à la cloche, par des tuyaux flexibles de cuir, et qu'ils n'étoient pas absolument imperméables à l'air, il ne nous a pas été possible de nous assurer de la quantité exacte des deux airs dont nous avions ainsi opéré la combustion : mais comme il n'est pas moins vrai en Physique qu'en Géométrie, que le tout est égal à ses parties ; de ce que nous n'avions obtenu que de l'eau pure dans cette expérience sans aucun autre résidu, nous nous sommes cru en droit d'en conclure que le poids de cette eau étoit égal à celui des deux airs qui avoient servi à la former. On

ne pourroit faire qu'une objection raisonnable contre cette conclusion : en admettant que l'eau qui s'étoit formée, étoit égale en poids aux deux airs, c'étoit supposer que la matière de la chaleur et de la lumière qui se dégage en grande abondance dans cette opération, et qui passe à travers les pores des vaisseaux, n'avoit pas de pesanteur : or on pouvoit regarder cette supposition comme gratuite. Je me suis donc trouvé engagé dans cette question importante, savoir si la matière de la chaleur et de la lumière a une pesanteur sensible et appréciable dans les expériences physiques ; et j'ai été déterminé pour la négative, d'après des faits qui me paroissent très-concluans, et que j'ai exposés dans un Mémoire déposé depuis plusieurs mois au Secrétariat de l'Académie.

Comme l'expérience dont je viens de donner les détails avoit acquis beaucoup de publicité, nous en rendimes compte dès le lendemain 25 à l'Académie, et nous ne balançames pas à en conclure que l'eau n'est point une [474] substance simple, et qu'elle est composée poids pour poids d'air inflammable et d'air vital.

Nous ignorions alors que M. Monge s'occupât du même objet, et nous ne l'apprimes que quelques jours après par une lettre qu'il addressa à M. Vandermonde, et que ce dernier lut à l'Académie ; il y rendoit compte d'une expérience de même genre, et qui lui a donné un résultat tout semblable. L'appareil de M. Monge est extrêmement ingénieux : il a apporté infiniment de soin à déterminer la pesanteur spécifique des deux airs : il a opéré sans perte ; de sorte que son expérience est beaucoup plus concluante

encore que la nôtre, et ne laisse rien à désirer : le résultat qu'il a obtenu, a été de l'eau pure dont le poids s'est trouvé à très-peu de chose près égal à celui des deux airs.

En rapprochant le résultat de ces premières expériences de ceux que nous avons obtenus, M. Meusnier et moi, dans des expériences faites postérieurement en commun, et dont je parlerai bientôt, il paroîtroit que la proportion en volume du mélange des deux airs, en les supposant l'un et l'autre dans leur plus grand degré de pureté, est de 12 parties d'air vital, et de 22,924345 d'air inflammable ; mais on ne peut disconvenir qu'il ne reste encore quelque incertitude sur l'exactitude de cette proportion. En partant au surplus de cette donnée qui ne doit pas s'écartez de beaucoup du vrai, et en supposant qu'à 28 pouces de pression et à 10 degrés du thermomètre, l'air vital pèse 0 grains, 47317 le pouce cube, et l'air inflammable 0 grains, 037449, ainsi qu'il résulte des expériences faites avec M. Meusnier, on trouve qu'une livre d'eau est composée ainsi qu'il suit,

	livres.
Air vital ou plutôt principe oxygine, . . . . .	0,86866273
Air inflammable ou plutôt principe inflammable de l'eau . . . . .	0,13133727
<b>TOTAL, . . . . .</b>	<b>1,00000000</b>

[475] Ces nombres, exprimés en fractions vulgaires de livres, reviennent à

	onces.    gros.    grains.
Principe oxygine, . . . . .	13    7    13,6
Principe inflammable, . . . . .	2    0    58,4
<b>TOTAL, . . . . .</b>	<b>16    0    0</b>

Enfin, en réduisant ces quantités au volume, on trouve pour les quantités de pouces cubiques de chacun des deux airs,

	pouces cubiques.
Air vital, . . . . .	16919,07
Air inflammable, . . . . .	32321,29
<b>TOTAL, . . . . .</b>	<b>49240,36</b>

Cette seule expérience de la combustion des deux airs, et leur conversion en eau, poids pour poids, ne permettoit guère de douter que cette substance, regardée jusqu'ici comme un élément, ne fût un corps composé ; mais pour constater une vérité de cette importance, un seul fait ne suffisoit pas ; il falloit multiplier les preuves et après avoir composé artificiellement de l'eau, il falloit la décomposer : je m'en suis occupé pendant les vacances de 1783, et j'ai rendu compte très-sommairement du succès de mes tentatives, dans un Mémoire lû à la Rentrée publique de la Saint-Martin, et dont l'Extrait a été publié dans plusieurs Journaux.

Je fis observer alors, que si véritablement l'eau étoit composée, comme l'annonçoit la combustion des deux airs, de l'union du principe oxygine avec le principe inflammable aqueux, on ne pouvoit la décomposer, et obtenir séparément l'un de ces principes sans présenter à l'autre une substance avec laquelle il cût plus d'affinité : le principe inflammable aqueux ayant plus d'affinité avec le principe oxygine qu'avec aucun autre corps, comme je le ferai voir dans mon Mémoire sur [476] les Affinités, ce n'étoit pas par ce *latus* que pouvoit être tentée la décomposition ; c'étoit donc le principe oxygine qu'il falloit attaquer. Je savois à

cet égard, par des expériences déjà connues, que le fer, le zinc et le charbon, avoient une grand affinité avec lui ; en effet, M. Bergman nous avoit appris dans son Analyse du fer, que la limaille de ce métal se convertissoit dans l'eau distillée seule, en éthiops martial, et qu'en même-temps, il se dégageoit une grande quantité d'air inflammable ; d'un autre côté, M. l'abbé Fontana ayant éteint des charbons ardens dans de l'eau, sous une cloche remplie d'eau, en avoit retiré une quantité notable d'air inflammable ; et M. Sage m'avoit communiqué une observation qui lui avoit été envoyée d'Allemagne, par MM. Hassenfrast, Stoulz et d'Hellancourt, Elèves de l'école des Mines ; il en résultoit, que du fer rouge éteint dans l'eau, sous une cloche, comme M. l'abbé Fontana l'avoit fait pour le charbon, donnaoit également de l'air inflammable : enfin, M. de la Place, qui étoit au courant de mes expériences, qui les avoit partagées souvent, et qui m'aidoit de ses conseils, m'avoit répété bien des fois, qu'il ne doutoit pas que l'air inflammable qui se dégageoit de la dissolution du fer et du zinc, dans l'acide vitriolique et l'acide marin, ne fût dû à la décomposition de l'eau.

Il se fondoit sur les raisons suivantes, dont il me fit part dans le mois de Septembre 1783 : je vais transcrire ses propres expressions. “ Par l'action “ des acides, le métal se dissout sous forme de chaux, “ c'est-à-dire, uni à l'air vital, et relativement au fer “ cette quantité d'air forme le quart ou le tiers de son “ poids. La dissolution ayant également lieu dans “ les vaisseaux fermés, il est visible que l'air vital n'est “ point fourni par l'atmosphère ; il ne l'est pas non

“ plus par l’acide ; car on sait, d’après les expériences de M. Lavoisier, que l’acide vitriolique privé “ d’une partie de l’air vital qu’il renferme, donne de “ l’acide sulfureux ou du soufre ; or on n’a aucun de ces “ deux résultats lorsqu’on dissout le fer dans de l’acide “ vitriolique suffisamment affoibli : d’ailleurs, ce qui “ [477] prouve que l’acide n’est point altéré par son “ action sur le fer, c’est qu’après cette action, il faut “ pour le saturer, ainsi que M. Lavoisier l’a constaté, “ employer la même quantité d’alkali. Il ne reste donc “ que l’eau à laquelle on puisse attribuer l’air vital qui “ s’unit au métal dans sa dissolution ; elle se décompose donc, et son principe inflammable se développe “ sous forme d’air : il suivait de-là que si par la combustion on combinoit de nouveau ce même principe “ avec l’air vital, on reproduiroit l’eau qui s’est décomposée ; cette conséquence étant confirmée par “ plusieurs expériences incontestables, elle fournit une “ nouvelle preuve de la décomposition de l’eau par “ l’action des acides sur les métaux, lorsqu’il en résulte de l’air inflammable.

“ La considération de cet air nous conduit encore “ au même résultat ; car il n’est point dû aux acides “ qui, comme nous venons de l’observer, n’éprouvent “ point d’altération dans leur action sur les métaux ; “ et s’il venoit des métaux même, on devroit également obtenir de l’air inflammable par l’action de “ l’acide nitreux. On pourroit à la vérité supposer “ que cet air entre dans la formation de l’air nitreux “ qui se dégage dans cette opération ; mais alors l’air “ inflammable devroit reparoître, lorsqu’en combinant “ l’air nitreux avec l’air vital, on reproduit l’acide

" nitreux : d'ailleurs, l'action de l'acide nitreux sur " le mercure, développe de l'air nitreux ; il ne paroît " pas cependant que le mercure lui fournisse de l'air " inflammable, puisque la chaux mercurielle qui a ré- " sulté de cette action, se revivifie sans addition d'air " inflammable et par la simple chaleur. Les con- " sidérations sur les bases des airs vital et inflammable, " dont l'une se combine et dont l'autre se développe " dans les dissolutions métalliques, se réunissent donc " pour faire voir que l'eau se décompose dans ces " opérations."

Toutes ces considérations réunies, ne me permet-  
toient pas de douter que les métaux n'exerçassent  
une action marquée sur l'eau, et pour la constater je  
commençai mes expériences par le fer. [478]

Je remplis des jarres de mercure ; j'y fis ensuite passer de petites quantités d'eau distillée qui avoit bouilli, et de la limaille de fer bien pure, en différentes proportions, et je laissai le tout en repos pendant plusieurs mois ; je reconnus bientôt que ces deux substances avoient une action réciproque l'une sur l'autre ; il se détacha peu-à-peu de la limaille une poudre noir très légère, la quantité s'en augmenta, et au bout de quelques mois presque toute la limaille de fer, dans les jarres au moins où je n'en avois introduit qu'une petite quantité, se trouva convertie en éthiops martial ; en même temps il s'étoit dégagé une quantité d'air inflammable très-considérable, qui s'étoit rassemblée au haut des vaisseaux, et qui se trouva très-pur ; à l'égard des jarres où la quantité de limaille de fer étoit plus considérable, il s'y dégagea plus d'air inflammable, mais je fus obligé d'interrompre

avant que la totalité de la limaille fût convertie en éthiops, à cause de la lenteur de l'opération.

En rapprochant le résultat de ces différentes expériences, je reconnus qu'un quintal ou cent livres de limaille de fer, acquéroient, en se convertissant ainsi en éthiops par la seule action de l'eau, vingt-cinq livres d'augmentation de poids, et qu'il se dégageroit en même temps 538 pieds cube  $\frac{1}{3}$  d'air inflammable très-léger, pesant 3 livres 12 onces 3 gros 60 grains ; ces quantités sont même au moins du douzième plus fortes quand on opère avec du fer parfaitement pur et qui ne contient aucune portion de principe oxygénique.

Pendant que je m'occupois de ces expériences, M. Blagden qui étoit à Paris, nous donna une connoissance très-exacte des expériences faites par M. Priestley, sur la revivification des chaux métalliques dans l'air inflammable ; M. Magellan et plusieurs autres Physiciens Anglois en avoient déjà écrit à différens Membres de l'Académie ; ces expériences me confirment de plus en plus dans l'opinion où j'étois, que l'eau étoit un corps composé : voici la mauière dont opère M. Priestley,

Il emplit d'air inflammable tiré du fer par l'acide vitriolique, [479] une cloche de verre placée sur la tablette de l'appareil pneumato-chimique à l'eau ; il y introduit à travers l'eau, du *minium* qu'il a fait préalablement bien chauffer pour en chasser tout l'air ; ce *minium* est placé sur un tesson de creuset, et soutenu par un support ; enfin il fait tomber sur la chaux métallique le foyer d'une lentille de verre : d'abord la chaux se sèche par la chaleur de la lentille ; ensuite

le plomb se revivifie ; en même temps l'air inflammable est absorbé, et on parvient aussi à en faire disparaître des quantités très-considérables. Il est impossible, dans l'appareil de M. Priestley, de pousser cette expérience jusqu'au bout, c'est-à-dire, jusqu'à ce que tout l'air inflammable ait disparu, parce qu'on seroit forcé de faire tomber le foyer sur les parois même de la cloche, et elle se casseroit infailliblement ; d'ailleurs, la chaux de plomb seroit elle-même submergée : mais, malgré cette difficulté, M. Priestley est parvenu à réduire 101 mesures d'air inflammable à 2, et ce restant étoit encore de l'air inflammable pur. Il a conclu de cette expérience, que l'air inflammable se combinoit avec le plomb pour le revivifier, et que par conséquent l'air inflammable et le phlogistique n'étoient qu'une seule et même chose, comme l'avoit avancé M. Kirwan.

J'observeroi que M. Priestley n'a pas fait attention à une circonstance capitale qui a lieu dans cette expérience, c'est que le plomb, loin d'augmenter de poids, diminue au contraire de près d'un douzième : il s'en dégage donc une substance quelconque ; or cette substance est nécessairement de l'air vital dont le *minium* contient près d'un douzième : mais d'un autre côté, il ne reste après cette opération, de fluide élastique d'aucune espèce ; non-seulement on ne retrouve pas dans la cloche d'air vital, mais l'air inflammable lui-même qui la remplissoit, disparaît : donc les produits ne sont plus dans l'état aériforme ; et puisque d'un autre côté il est prouvé que l'eau est un composé d'air inflammable et d'air déphlogistique, il est clair que M. Priestley a formé de l'eau sans s'en douter. [480]

Cette expérience m'a rappelé qu'ayant fait des revivifications de chaux de plomb avec de la poudre de charbon, dans des vaisseaux fermés, j'avois obtenu de l'eau; j'ai consigné ce fait, dont j'ignorois alors l'explication, dans le volume d'*Opuscules* que j'ai publié en 1774. *Voyez*, p. 270.

Dans l'expérience que je viens de citer, j'avois revivifié dans une cornue 6 onces de *minium*, par le moyen de 6 gros de poudre de charbon, et j'avois reçu les produits aériformes dans un appareil pneumatique : la quantité d'air fixe qui passa se trouva de 560 pouces cubiques, à 15 degrés et demi du thermomètre, ce qui, réduit à 10 degrés de température, revient à 545,7 ; l'air fixe à 28 pouces de pression, et 10 degrés de température pèse 0 grains, 695 le pouce cube, ainsi la totalité de l'air fixe obtenu, pesoit . . . 0 onc. 5 gros.  $19\frac{1}{4}$  gr.

Il m'est resté dans la cornue,

		onces.	gros.	grains.	onces.	gros.	grains.
Plomb réduit,	.	5	3	12	5	7	66
Charbon non brûlé,	.	0	4	54			
TOTAL du produit,	.				6	5	$13\frac{1}{4}$
J'avois employé de matière,	.				6	6	0
Donc, perte de poids ou manquant,					0	0	$58\frac{3}{4}$

J'ai prouvé ensuite par une expérience directe, que cette perte de poids étoit due à l'eau qui passoit dans la distillation.

Mais  $58\frac{3}{4}$  grains d'eau, sont composés, d'après les expériences faites par M. Meusnier et par moi, des quantités suivantes d'air inflammable et de principe oxygine.

	grains.
Principe oxygine, . . . . .	51,05
Air inflammable, . . . . .	7,70
<b>TOTAL, . . . . .</b>	<b>58<math>\frac{3}{4}</math></b>

Ainsi sur 1 gros 18 grains de charbon qui a été consommé dans cette expérience, il n'y avoit réellement que 1 gros 10 grains  $\frac{3}{4}$  de vraie matière charbonneuse, et le reste étoit de l'air inflammable aqueux. [481]

D'un autre côté, les 4 gros 60 grains que les 6 onces de *minium* ont perdus par leur transformation en plomb, sont composés

	gros.	grains.
1°. De la quantité de principe oxygine qui a servi à former de l'eau et qui est de, . . . . .	0	51,05
2°. De la quantité de principe oxygine nécessaire pour convertir 1 gros 10,3 grains de charbon en air fixe, et qui est de, . . . . .	2	68,95
3°. De l'air fixe qui est tout formé dans le <i>minium</i> , et dont la quantité monte à, . . . . .	1	12,00
<b>TOTAL, . . . . .</b>	<b>4</b>	<b>60,00</b>

D'après cela, il est aisé de connoître la véritable combinaison du *minium*, et l'on voit que 6 onces de cette substance, sont composées comme il suit,

	onces.	gros.	grains.
Plomb, . . . . .	5	3	12
Air fixe tout formé, . . . . .	0	1	12
Principe oxygine, . . . . .	0	3	48
<b>TOTAL, . . . . .</b>	<b>6</b>	<b>0</b>	<b>0</b>

## COMPOSITION DU MINIUM PAR QUINTAL.

	livres.
Plomb, . . . . .	89,9306
Air fixe tout formé, . . . . .	2,4306
Principe oxygine, . . . . .	7,6388
<b>TOTAL, . . . . .</b>	<b>100,0000</b>

Si on veut connoître, d'après ces proportions, les quantités de principe oxygine et d'air fixe qu'un quintal de plomb absorbe en se convertissant en *minium*, on trouvera le résultat qui suit,

	livres.
Plomb, . . . . .	100,00000
Air fixe, . . . . .	2,70275
Principe oxygine, . . . . .	8,49410
<b>TOTAL, . . . . .</b>	<b>111,19685</b>

[482] On peut également connoître, d'après cette expérience, la composition de l'air fixe, et on trouve qu'un quintal de cet acide contient,

	livres.
Principe oxygine, . . . . .	72,125
Charbon, . . . . .	27,875
<b>TOTAL, . . . . .</b>	<b>100,000</b>

J'observerai que le *minium* dont s'est servi M. Priestley, ne devoit pas contenir tout-à-fait autant de principe oxygine que celui que j'ai employé : en effet, il avoit fait passer dessus de l'acide nitreux ; mais on sait que cet acide enlève du principe oxygine au *minium*, et qu'on l'en surcharge en le distillant sur

cette chaux métallique; et c'est ce que prouve encore le résultat de ses expériences. Pour réduire une once de *minium*, il a employé cent huit mesures d'air inflammable, c'est-à-dire 166 pouces cubiques  $\frac{2}{3}$ .

	grains.
Cette quantité d'air inflammable, en la supposant pure, devoit peser,	6,24
La quantité de principe oxygine, correspondante pour former de l'eau, a dû être de,	41,27
Donc, quantité d'eau formée,	<hr/> 47,51

Le *minium* de M. Priestley, ne contenoit donc par once que 41,27 de principe oxygine, contre 7 gros 30,73 grains de plomb réduit, c'est-à-dire, 7 livres 11 onces 5 gros de principe oxygine pour un quintal de plomb, tandis que celui que j'ai employé, en contenoit près de 8 livres et demie; ainsi le premier par la réduction, ne devoit absorber que 1 livre 2 onces  $5\frac{1}{2}$  gros d'air inflammable par quintal, et ne donner que 8 livres 14 onces  $2\frac{1}{2}$  gros d'eau, tandis que le second devoit absorber 1 livre 4 onces  $4\frac{1}{2}$  gros d'air inflammable, et fournir 9 livres 12 onces  $4\frac{1}{2}$  gros d'eau: cette différence qui est d'un onzième, est peu considérable; elle tient sans doute, comme je l'ai dit, au degré [483] de saturation du *minium*; peut-être aussi peut-on l'attribuer au défaut d'exactitude dans les expériences. Je crois pouvoir répondre de celles qui me sont propres; mais il pourroit arriver que M. Priestley, dans sa réduction du *minium* par l'air inflammable, n'ayant pas pour objet de déterminer les quantités ni les augmentations ou diminu-

tions de poids, n'eût pas cherché à apporter une grande précision dans les résultats.

Presque toutes les chaux métalliques, à l'exception de celle de zinc, de celle d'arsenic, de celle de régule d'antimoine et de manganèse, sont susceptibles de se réduire dans l'air inflammable, et de former de l'eau. Il est à remarquer que celle d'arenic et celle de régule d'antimoine, se subliment dans cette expérience qui n'a été tentée encore qu'à l'aide du verre ardent; elles éludent par conséquent la chaleur du foyer, et il seroit possible que ce fût cette cause qui s'opposât à leur revivification. Dans toutes ces réductions par l'air inflammable, la quantité qui en est absorbée, est toujours proportionnelle à la quantité de principe oxygine propre à la saturation de chaque métal: ainsi pour revivifier cent huit livres de précipité rouge ou chaux de mercure, il faut employer 297633 pouces cubiques d'air inflammable, pesant 1 livre, 20955544 ou 1 livre 3 onces 2 gros 58 grains, et il se forme 9 livres 3 onces 2 gros 58 grains d'eau.

M. Priestley, en annonçant qu'il a revivifié la chaux d'étain dans l'air inflammable, ne spécifie pas l'espèce de chaux qu'il a employée; c'étoit sans doute de l'étain précipité d'une dissolution par les acides, car il n'est pas possible d'unir autant de principe oxygine à ce métal par voie de calcination.

	gros.	grains.
Une once de cette chaux a absorbé 581 pouces cubiques $\frac{3}{4}$ d'air inflammable, pesant,	0	$21\frac{8}{10}$
La quantité d'air vital ou de principe oxygine correspondante pour former de l'eau, est de;	2	0
Donc, eau formée,	2	21,8

[484] La quantité de principe oxygine, combinée avec l'étain dans la chaux qu'a employée M. Priestley, étoit donc de  $33\frac{1}{3}$  pour cent environ, tandis que par la calcination, ce métal ne se charge guère que de quatorze livres par quintal.

Les chaux de fer se revivifient également dans l'air inflammable, mais il n'est pas possible de les porter par cette voie à l'état de métal parfait ; il retient constamment la quantité de principe oxygine nécessaire pour le constituer dans l'état d'éthiops martial, et il n'est pas possible de porter la réduction plus loin. La raison de ce phénomène est facile à saisir ; puisque le fer décompose l'eau et ce calcine par cette voie, jusqu'à ce qu'il soit parvenu à l'état d'éthiops martial, il en résulte que le principe oxygine a plus d'affinité avec le fer dans son état métallique, qu'avec le principe inflammable de l'eau ; mais lorsque le fer est arrivé à l'état d'éthiops, alors il n'exerce plus une action assez forte sur le principe oxygine pour décomposer l'eau. Par une suite de cette plus grande affinité du principe oxygine pour le fer, ce métal ne doit se revivifier dans l'air inflammable, que jusqu'à ce qu'il soit parvenu à l'état d'éthiops ; et c'est ce qu'on observe en effet.

L'air inflammable tiré des végétaux par la distillation, opère la revivification du *minium*, et forme de l'eau avec le principe oxygine qui étoit combiné avec le plomb ; mais cette opération est plus lente et plus difficile que dans l'air inflammable pur. Le résidu qu'on obtient est de l'air fixe, qui peut-être, étoit tout formé dans l'air inflammable des végétaux, ou qui, plus vraisemblablement, est dû à la combustion de la

matière charbonneuse que l'air inflammable des végétaux tient abondamment en dissolution.

Le *minium* se revivifie tout aussi-bien dans l'alkali volatil aériforme, que dans l'air inflammable aqueux. Il seroit bien intéressant d'examiner avec soin ce qui résulte de cette combinaison de l'alkali volatil avec l'air vital ou principe oxygine. Il se forme dans cette expérience une substance qui, sans être de l'eau, est très-analogue à l'eau, et qui en a [485] toutes les principales propriétés : j'ai obtenu une assez grande quantité de cette nouvelle espèce d'eau, de la détonation spontanée du nitre ammoniacal dans les vaisseaux fermés. Il se dégage de l'air nitreux dans cette expérience, et le principe oxygine de l'acide nitreux, combiné avec l'alkali volatil, forme la nouvelle liqueur dont il est question : les expériences nombreuses que j'ai déjà faites sur cet objet, me paroissent pouvoir conduire à des découvertes très-importantes ; j'en entretiendrai particulièrement l'Académie.

L'acide sulfureux aériforme est, comme je l'ai dit ailleurs, de l'acide vitriolique privé d'une portion de principe oxygine. C'est un être intermédiaire entre le soufre et l'acide vitriolique ; aussi a-t-il une grande affinité pour le principe oxygine, et il l'enlève au *minium* : mais M. Priestley a observé que le plomb n'étoit pas complètement réduit dans cette expérience.

Dans toutes les autres espèces d'air, il n'y a nulle apparence de réduction, et le *minium* se convertit en verre de plomb.

Tel étoit l'état de nos connaissances sur la décomposition et la recomposition de l'eau, lorsque nous

nous trouvâmes insensiblement engagés, M. Meusnier et moi, à reprendre cette question sous un autre point de vue, pendant l'hiver de 1783 à 1784. La commission dont nous fûmes chargés par l'Académie, d'après les ordres du Roi, pour la perfection des machines aérostatiques, nous conduisit nécessairement à des recherches sur les moyens les plus économiques de faire de l'air inflammable en grand, et il étoit naturel que nous nous attachassions à le tirer de l'eau dans laquelle nous avions déjà de si fortes raisons de croire qu'il existoit en grand abondance. Le Mémoire que nous avons donné en commun à la rentrée publique de Pâques 1784, sur ce sujet, ayant été imprimé avant celui-ci,\* j'y renvoie les lecteurs, et je me bornerai à présenter ici ce qui rentre le plus immédiatement dans mon objet. [486]

Le fer, par la voie humide, m'ayant donné, ainsi que je l'ai déjà exposé, des signes d'une action non équivoque sur l'eau, nous résolvimes M. Meusnier et moi de suivre cette indication ; mais comme la production de l'air inflammable à froid étoit extrêmement lente, que je n'en avois même obtenu que des volumes peu considérables, nous pensâmes qu'il étoit important de tenter cette expérience à un degré de chaleur beaucoup plus fort, et que ce seroit probablement un moyen d'abréger beaucoup le temps de l'expérience.

Nous étions confirmés dans cette opinion, 1<sup>o</sup> parce que l'affinité du fer pour le principe oxygène, augmente à mesure qu'il est plus échauffé ; 2<sup>o</sup> parce que

\* Voyez ci-dessus, p 269.

la chaleur produit un effet contraire sur les deux principes de l'eau, et que nous ne pouvions douter que leur adhérence entr'eux ne diminuât à un certain degré de chaleur ; 3°. enfin parce que la matière de la chaleur étant un des élémens nécessaires à la formation des fluides aériformes, c'étoit se placer dans des circonstances favorables, que d'opérer à un degré de chaleur considérable. La difficulté étoit de faire éprouver à l'eau un degré supérieur à celui de l'ébullition ; on sait que ce fluide se vaporise à 80 degrés du thermomètre de Réaumur, quand il n'est chargé que de 28 pouees de mercure : nous n'avions donc que deux moyens de remplir notre objet, ou en faisant supporter à l'eau un très grand degré de pression dans un appareil analogue à la machine de Papin, ou en la prenant dans l'état de vapeurs ; le premier de ces moyens nous parut trop dangereux, et nous nous arrêtames au second : nous primes en conséquence un canon de fusil dont on avoit ôté la culasse, c'est-à-dire qui étoit ouvert par les deux bouts ; comme nous le destinions à éprouver un grand degré de chaleur, pour éviter la calcination extérieure, nous le recouvrimes en dehors dans toute sa région moyenne, avec deux couches de fil-de-fer tournées en spirales, et nous appliquames par-dessus une couche d'un lut formé avec de la terre grasse, du sable et de la poudre de charbon ; nous fimes passer ce canon à travers un fourneau, en l'inclinant [487] de quelques degrés avec l'horizon, afn de donner à l'eau une pente suffisante pour la déterminer à couler ; un entonnoir de fer-blanc, dont la queue étoit garnie d'un robinet, s'ajustoit et se lutoit solidement à l'ex-

trémite la plus élevée du canon, tandis que l'extrémité inférieure répondait à un serpentin d'étain ; enfin au bas du serpentin étoit luté un flacon tubulé, destiné à recevoir la liqueur qui pourroit s'écouler et en même temps à transmettre par un tuyau adapté et luté à la tubulure, les produits aériformes dans l'appareil pneu-mato-chimique. Tous ces détails sont rendus sensibles dans la *planche* jointe au Mémoire que nous avons donné en commun M. Meusnier et moi (*voyez* p. 269.) Comme les canons de fusil sont rarement assez longs pour ce genre d'expériences, nous avons souvent été obligé d'y faire ajouter des bouts de tuyaux de cuivre jaune brasé ; et comme il n'y a que le milieu du canon qui supporte l'ardeur du feu dans ces expériences, la chaleur dans l'endroit des soudures, n'étoit pas assez forte pour qu'elles en souffrissent.

Cet appareil nous a donné lieu de faire les observations qui suivent : si lorsque le canon de fusil est rouge et incandescent, on y laisse couler de l'eau goutte à goutte et en très-petite quantité, elle s'y décompose en entier, et il n'en ressort aucune portion par l'ouverture inférieure du canon : le principe oxygine de l'eau se combine avec le fer et le calcine ; en même temps le principe inflammable aqueux, devenu libre, passe dans l'état aériforme, et avec une pesanteur spécifique qui est environ de deux vingt-cinqièmes de celle de l'air commun. Dans le commencement de l'expérience, la production d'air inflammable est très-rapide, elle se ralentit bientôt ensuite, et elle arrive à une uniformité qui dure pendant plusieurs heures ; enfin au bout de huit à dix heures, plus ou moins, suivant l'épaisseur du canon, le passage de

l'air inflammable se ralentit, et l'eau finit par ressortir en totalité du canon, comme elle y étoit entrée, sans se décomposer. Si cette opération a été poussée [488] jusqu'au bout, toute la substance du fer qui formoit le canon de fusil, se trouve convertie en une substance noire brillante, cristallisée en facettes comme la mine de fer spéculaire ; cette substance est fragile et cassante, médiocrement attirable à l'aimant ; on peut la réduire en poudre dans un mortier, et elle ne diffère alors en rien de ce qu'on désigne en Chimie et en Pharmacie, sous le nom d'*éthiops martial* : cette matière occupe un volume beaucoup plus considérable que le fer qui a servi à la former ; le canon de fusil se trouve en conséquence augmenté d'épaisseur, et son diamètre intérieur considérablement diminué. Le fer, dans cette expérience, acquiert une augmentation de poids de vingt-cinq à trente livres par quintal, mais ce n'est pas par cet appareil qu'on peut en déterminer exactement la quantité, parce que quelque précaution que l'on prenne, il s'opère une calcination plus ou moins forte du fer à l'extérieur du canon, et qu'il est impossible de savoir si l'augmentation de poids observée, appartient à la calcination intérieure ou à celle extérieure.

Les phénomènes sont fort différens si on emploie un métal pour lequel le principe oxygine ait moins d'affinité que pour le principe inflammable aqueux : si par exemple, on substitue dans l'expérience précédente un canon de cuivre rouge à celui de fer, l'eau se réduit bien en vapeurs en passant par la partie incandescente du tube, mais elle se condense ensuite par le refroidissement dans le serpentin ; il ne s'opère

alors qu'une simple distillation sans perte, et il n'y a ni calcination du cuivre, ni production d'air inflammable.

Cette propriété du cuivre nous a fourni un moyen commode de faire des expériences plus exactes sur la calcination du fer et sur la combustion du charbon : en effet, étant une fois assurés que l'instrument dont nous nous servions ne fournissait rien et n'absorboit rien, les produits que nous obtenions étoient nécessairement dûs à l'eau et aux corps employés pour la décomposer. Le canon de cuivre dont nous nous sommes servis, avoit été fondu dans les [489] ateliers de MM. Perier ; il avoit 3 pouces de diamètre en dedans, et six lignes d'épaisseur ; nous y avons d'abord introduit du fer, soit en feuilles minees roulées, soit en petites barres tournées en élée ; nous lutions exactement toutes les jointures, et après avoir fait rougir le tuyau, nous y faisions passer de l'eau : nous avons continué quelques-unes de ces expériences jusqu'à ce que le fer fût parfaitement saturé, et qu'il n'y eût plus de production d'air. L'expérience finie, nous avons reconnu, 1°. que le fer s'étoit réduit en une substance cassante noire attirable à l'aimant, et qui, réduite en poudre, ne différoit point de l'éthiops martial obtenu par l'eau à froid ; 2°. que le fer dans cette opération, avoit acquis une augmentation de poids d'environ 25 livres par quintal ; 3°. que la quantité d'air inflammable dégagée étoit en volume pour un quintal de fer de 930198 pouces cubiques, ou 538 pieds cubes  $\frac{1}{3}$ , ce que revient en poids à 3 livres, 77986075 ; il est au surplus difficile d'amener le fer à ce degré de saturation complet.

D'après cette expérience, on ne pouvoit plus douter que la production d'air inflammable obtenu par M. l'Abbé Fontana, en éteignant des charbons ardens dans l'eau, et sur-tout celle obtenu par MM. Hassenfrast, Stoulz et d'Hellancourt, dans l'extinction du fer rouge, ne fût une véritable décomposition de l'eau. Il étoit sensible en effet, que faire passer l'eau à travers le fer rouge, ou le fer rouge à travers l'eau, étoit une expérience analogue, et que dans les deux cas, on devoit produire les mêmes effets. Nous nous sommes en conséquence servis de ce moyen pour déterminer quelles étoient les substances, principalement les métaux, susceptibles de décomposer l'eau, c'est-à-dire, quels étoient ceux avec lesquels le principe oxygine avoit plus d'affinité qu'avec le principe inflammable aqueux. Les appareils dont nous nous sommes servis pour ce genre d'expérience, sont extrêmement simples : nous suspendions au plancher, par le moyen d'un fil-de-fer, une cloche de verre pleine d'eau, et dont la bouche entroit d'un-demi pouce ou d'un pouce dans [490] l'eau de la cuve ou appareil pneumatochimique ; nous faisions rougir les matières sur les-quelles nous opérions, et lorsqu'elles étoient dans l'état d'incandescence, nous les plongions rapidement à travers l'eau sous la cloche. A l'égard des matières métalliques susceptibles de se fondre à un degré de feu médiocre, nous les placions dans un creuset dans lequel nous les faisions fondre et rougir, et nous plongions à la fois sous la cloche le métal et le creuset. Indépendamment des substances métalliques, nous avons cru devoir soumettre à cette même épreuve le verre, le silex, le quartz, le grès, le charbon allumé,

le soufre, et nous avons reconnu qu'il n'y avoit, parmi les substances métalliques, que le fer et le zinc qui donnassent de l'air inflammable ; que celui fourni par le charbon étoit mélangé d'air fixe ; qu'on obtenoit bien, en éteignant ainsi dans l'eau, même le quartz et le caillou, une très-petite portion d'air ; mais il nous a paru évident qu'elle provenoit de l'eau, qui en tient toujours une portion en dissolution : cet air étoit dans l'état d'air commun ou à peu-près. Pour avoir des résultats plus exacts, nous avons opéré en général sur de grandes masses ; par exemple, pour l'or, sur des lingots de 30 marcs effectifs, et sur l'argent, de 45 ; au moyen de quoi, s'il s'étoit dégagé de l'air inflammable en quantité sensible, il n'auroit pu nous échapper. Nous avons été obligés de renoncer à faire cette expérience sur le régule d'antimoine et sur l'étain, à cause des explosions dangereuses que font ces métaux un moment après qu'on les a plongés dans l'eau, et à l'instant, à ce qu'il paroît, où ils se figent.

Cette méthode de mettre les corps incandescens en contact avec l'eau, en les y plongeant entièrement, a au surplus un grand inconvénient : la surface du métal ou de quelqu'autre corps que ce soit, se refroidit promptement par l'application de l'eau froide, et surtout par la grande quantité de matière de la chaleur employée à former le fluide aériforme dans les expériences où il s'en dégage, en sorte que la production d'air inflammable n'a lieu qu'un instant, et qu'il faut répéter [491] plusieurs fois les immersions pour obtenir des quantités d'air suffisantes pour les soumettre à des épreuves.

Ces différents expériences fournissent des moyens multipliés de décomposer l'eau, et de séparer en quelque façon par l'art, les principes qui la constituent : la nature nous en offre un grand nombre d'autres, et nous n'avons à cet égard qu'à suivre ses opérations. L'eau est le grand réservoir où elle trouve la masse de combustibles qu'elle forme continuellement sous nos yeux, et la végétation, paroît être son grand moyen. Il est évident, en rapprochant les expériences de MM. Vanhelmont, du Hamel, Vallérius et Tillet, avec celles faites dernièrement par MM. Ingenhouse et Scnnebier, d'un côté, que l'eau est le principal agent de la végétation, de l'autre, qu'il se dégage habituellement pendant son cours une grande quantité d'air vital par les vaisseaux des feuilles : l'eau se décompose donc dans les plantes par l'acte de la végétation ; mais elle s'y décompose dans un ordre inverse à celui que nous avons observé jusqu'ici. En effet, dans la végétation c'est l'air vital qui devient libre, et c'est le principe inflammable aqueux qui reste engagé pour former la matière charbonneuse des plantes, leurs huiles, tout ce qu'elles ont de combustible ; ces différentes substances ne paroissent plus être aujourd'hui que des modifications encore inconnues du principe inflammable de l'eau.

La fermentation spiritueuse est encore un moyen de décomposer l'eau par la voie humide : le sucre, comme je l'ai fait voir, contient une quantité très-considérable de matière charbonneuse toute formée ; puis donc que la matière charbonneuse a plus d'affinité avec le principe oxygine, que ce dernier n'en a avec le principe inflammable aqueux, puisqu'en vertu

de cet excès d'affinité le charbon décompose l'eau, par la voie sèche, pourquoi ne la décomposeroit-il pas par la voie humide ?

Il paroît donc que, dans la fermentation spiritueuse, la matière charbonneuse du sucre ou du corps sucré se combine avec le principe oxygine de l'eau, et que le principe inflammable [492] aqueux, devenu libre, se fixe dans la combinaison en s'unissant avec une portion assez considérable du principe charbonneux, et que c'est ce principe inflammable qui forme la partie spiritueuse, l'esprit-de-vin : la décomposition de l'eau dans la fermentation spiritueuse, se fait donc en vertu d'une double action ; d'une part, la matière charbonneuse tend à se combiner avec le principe oxygine ; de l'autre, cette même matière charbonneuse tend à se combiner avec le principe inflammable aqueux.

Cette double combinaison me paroît déjà établie par des expériences décisives ; celle du principe oxygine avec le charbon est prouvée par la quantité énorme d'air fixe qui se dégage pendant la fermentation ; or, on ne peut plus doutier aujourd'hui que l'air fixe ne soit un composé de principe charbonneux et de principe oxygine : la combinaison du principe inflammable aqueux avec la matière charbonneuse est prouvée, parce que l'esprit-de-vin, en brûlant, donne de l'air fixe ; donc il contient le principe charbonneux, qui seul forme de l'air fixe en brûlant.

L'existence du principe inflammable aqueux dans l'esprit-de-vin n'est pas moins certaine, parce qu'il se reforme de l'eau dans sa combustion ; or, il n'y a que le principe inflammable aqueux, qui, combiné avec le

principe oxygine, ait cette propriété. Cette combustion de l'esprit-de-vin présente des résultats bien extraordinaires; et quoique je me propose de donner sur cet objet, un Mémoire particulier, je ne puis me dispenser de rapporter ici ce qui tient le plus immédiatement à la formation de l'eau.

J'ai introduit, suivant ma méthode ordinaire, une lampe à esprit-de-vin, sous une cloche de verre remplie d'air commun, et qui étoit renversée sur du mercure: dès que la lampe a été allumée, il y a eu, comme je m'y attendois, une diminution considérable du volume de l'air, production d'air fixe et d'eau; mais ce qui m'a beaucoup surpris, c'est que le poids de cette eau s'est trouvé plus considérable que celui de l'esprit-de-vin que j'avois brûlé. [493]

Comme j'avois opéré sur de très-petites quantités, et que dans ce genre d'expérience, il y a des évaluations et des erreurs inévitables, qui peuvent influer sur l'exactitude du résultat, je desirois trouver un moyen de répéter cette combustion plus en grand, et de manière à ne laisser aucunes ressources à l'incrédulité. M. Meusnier, avec lequel j'en ai conféré, a imaginé un appareil très-simple pour remplir cet objet. Il consiste en une lampe à esprit-de-vin, disposée à la Quinquet, qu'on allume sous une petite cheminée circulaire de cuivre, de deux pieds de haut environ: cette cheminée, par sa partie supérieure, s'adapte à un serpentin ordinaire, dont le tuyau doit fournir un développement de quinze à dix-huit pieds; le seau du serpentin doit être rempli d'eau, qu'on ramène continuellement à la température de l'atmosphère, en y ajoutant un peu de glace à mesure qu'elle s'échauffe.

Les parois de la cheminée prennent, pendant que l'esprit-de-vin brûle, une chaleur considérable ; pour que cette chaleur s'y conservât plus long-temps, nous l'avons revêtue d'une seconde enveloppe, et nous avons rempli l'intervalle avec du sable. Il résulte de cette disposition, que l'eau qui est produite par la combustion de l'esprit-de-vin, se conserve dans l'état de vapeurs dans toute l'étendue de la cheminée ; mais que, lorsque cette même vapeur est une fois engagée dans le tuyau du serpentin, elle se condense par le refroidissement qu'elle éprouve, et coule dans le vase destiné à la recevoir. On peut brûler dans cet appareil autant d'esprit-de-vin qu'on le juge à propos, et chaque livre de seize onces donne, quand on opère avec toutes les précautions convenables, dix-huit onces quatre à cinq gros d'eau très-pure, ce qui fait deux onces et demie d'augmentation par livre ; c'est à très-peu-près un septième.

Dans des temps moins éclairés, on auroit présenté cette opération comme une transmutation d'esprit-de-vin en eau, et les Alehimistes en auroient tiré des inductions favorables à leurs idées sur les transmutations métalliques. Aujourd'hui [494] que l'esprit d'expérience et d'observation nous apprennent à tout apprécier à sa juste valeur, nous ne verrons autre chose dans cette expérience, que la preuve qu'il s'ajoute quelque chose à l'esprit-de-vin dans sa combustion, et que ce quelque chose est de l'air. Nous en conclurons, que l'augmentation de poids, la fixation d'air, est un phénomène général de toute combustion ; que tout concourt à prouver que la partie inflammable de l'esprit-de-vin est toute formée dans l'eau, qu'il ne

s'agit que de la dégager d'avec le principe oxygine avec lequel elle est combinée; enfin, que l'eau est un composé du principe oxygine uni à un principe inflammable.

Une autre circonstance très-remarquable de la fermentation spiritueuse, c'est que si on en rassemble soigneusement les produits, on voit clairement, qu'en réunissant le poids de l'air fixe qui s'est dégagé, celui de la portion de sucre qui reste sans être décomposée, enfin, la partie spiritueuse, on a un produit en poids beaucoup plus considérable que celui du sucre qu'on a employé, tandis qu'au contraire on trouve un manquant égal sur le poids de l'eau.

Il résulte évidemment de cette observation, que ni l'air fixe, ni la partie spiritueuse ne sont formés aux dépens du sucre seul, puisqu'un corps ne peut donner un résultat plus pesant qu'il ne l'est lui-même, et que l'eau par conséquent y contribue pour une portion très-notable.

Je ne donne ici qu'un résumé très-succinct de mes expériences sur la fermentation spiritueuse, parce qu'elles ne sont point encore complètes, et que d'ailleurs elles doivent faire le sujet d'un Mémoire particulier, uniquement dirigé vers cet objet.

## No. VI.

MEMOIRE SUR LE RESULTAT DE L'INFLAMMATION DU  
GAZ INFAMMABLE ET DE L'AIR DEPHLOGISTIQUE,  
DANS DES VAISSEAUX CLOS. PAR M. MONGE.\*

LORSQU'A la manière de M. de Volta on enflamme un mélange d'air déphlogistique et de gaz inflammable par le moyen d'une étincelle électrique, ou par une élévation suffisante de température, les deux fluides se décomposent, et se dépouillent réciproquement d'une très-grande partie de la matière de la chaleur qui entroit auparavant dans leur composition. Ce feu abandonné à lui-même quitte l'état de compression où le tenoit son adhérence pour les autres parties constituantes des fluides, il entre en expansion, il heurte d'une manière mécanique les parois des vaisseaux dans lesquels se fait l'opération et il les brise lorsque leur résistance n'est pas assez grande ; mais lorsque cette résistance est suffisante, le feu, après avoir perdu son mouvement contre les parois, passe par leurs pores comme matière de température, et il échauffe les corps circonvoisins ; il se trouve alors du vide dans le récipient qui ne contient plus

\* Reprinted from the Mémoires de l'Académie des Sciences for 1783, (printed in 1786,) pp. 78 to 88.

que les autres substances qui entrent dans la composition des fluides élastiques, et qui sont privées du ressort et de la légèreté que leur communiquoient auparavant la matière de la chaleur et celle de la lumière qu'elles ont abandonnées.

Malgré le grand nombre d'expériences que tous les Physiciens avoient répétées sur l'inflammation dans l'eudiomètre de M. de Volta, on n'avoit encore aucune connoissance sur la nature de ce résidu, parce que les expériences avoient été faites trop en petit, ou parce qu'on avoit opéré les inflammations sur de l'eau qui masquoit ce résidu et empêchoit [79] qu'on ne pût l'apercevoir.\* Ce résultat pouvant fournir une substance nouvelle, ou procurer des lumières sur la composition d'une substance déjà connue, il étoit important de répéter les expériences sur des quantités considérables de fluides élastiques, et dans des vaisseaux clos, secs et à l'abri du contact de toute matière étrangère : c'est ce que j'ai fait, et ce dont je vais rendre compte à l'Académie.

L'air déphlogistique que j'ai employé a été produit par la réduction du précipité rouge ; et pour que le gaz ne fut point altéré par l'air atmosphérique, j'ai d'abord mis dans une cornue le nitre mercuriel avec du mercure coulant, et j'ai poussé doucement la cal-

\* Les Expériences dont il s'agit dans ce Mémoire, ont été faites à Mézières, dans les mois de Juin et de Juillet 1783, et répétées en Octobre de la même année : je ne savois pas alors que M. Cavendish les eût faites plusieurs mois auparavant en Angleterre, mais plus en petit ; ni que MM. Lavoisier et de la Place les fissent à peu-près dans le même temps à Paris, dans un appareil qui ne comportoit pas toute la précision de celui que j'ai employé.

cination jusqu'à ce qu'il ne se dégagât plus de gaz nitreux que je recevois dans l'appareil hydropneumatique ; alors en augmentant le feu, et avec les précautions qu'exige la combinaison des premières portions d'air déphlogistique avec les dernières de gaz nitreux, j'ai obtenu l'air déphlogistique sans faire communiquer l'atmosphère avec l'intérieur de la cornue, et j'ai rejeté les premiers produits qui pouvoient contenir l'acide nitreux résultant de la combinaison des deux gaz. Quant à l'air inflammable je me le suis procuré en faisant dissoudre du fil-de-fer bien nettoyé dans de l'acide vitriolique affoibli, et en employant un vase assez grand pour que tout l'air qui m'étoit nécessaire fut produit d'un seul jet, et sans être obligé de l'ouvrir pour y introduire de nouveau ou du fer ou de l'acide, ce qui auroit donné passage à l'air de l'atmosphère et altéré mes résultats.

Après avoir obtenu l'air déphlogistique et l'air inflammable, j'ai mesuré le poids d'un volume déterminé de chacun de ces fluides : pour cela, sur un appareil hydropneumatique ABCD, [80] (figure 1,) dans lequel le niveau de l'eau EF étoit à une hauteur constante et déterminée, j'ai établi un bocal de verre I de la capacité de vingt-deux pintes, ouvert par en bas, et garni à son ouverture supérieure d'un robinet bien luté ; à côté de ce bocal étoit fixée une règle GH, destinée à recevoir les divisions du volume du bocal en parties qui continssent chacune la même masse d'air, malgré le poids variable de la colonne d'eau suspendue, et je me suis procuré ces divisions de la manière suivante. Dans un matras à col étroit, j'ai introduit une pinte d'eau, mesure de Paris ; cette

pinte contenoit 1 livre 14 onces 7 gros 44 grains d'eau de pluie filtrée, à la température de 12 degrés du thermomètre de Réaumur, et j'ai coupé le col du matras à l'endroit où se trouvoit la surface de la pinte d'eau; ensuite j'ai aspiré par en haut l'air du bocal 1 jusqu'à ce que l'eau fût arrivée au robinet, et que j'en eusse une gorgée dans la bouche; j'ai fermé le robinet, et dans cet état l'eau restoit suspendue, et il n'entroit point d'air dans le bocal ni par les luts, ni par le robinet. J'ai plongé dans l'eau de l'appareil le matras renversé et plein d'une pinte d'air atmosphérique sous le poids de l'atmosphère, j'ai versé cet air dans le bocal par-dessous, l'eau s'est abaissée, et j'ai marqué sur la règle la hauteur à laquelle s'arrêtait la surface: j'ai recommencé cette opération jusqu'à ce que le bocal fût entièrement vide d'eau, et j'ai eu sur la règle, des divisions inégales, et qui indiquoient des volumes inégaux, mais ces volumes contenoient des masses égales d'air sous le poids constant de l'atmosphère: cette opération préliminaire étant faite, j'ai de nouveau rempli d'eau le bocal, et j'y ai introduit par en bas le gaz dont je voulois mesurer le poids.

Ensuite j'ai fait le vide dans un grand ballon K, garni d'un robinet bien luté, et dont la capacité étoit à peu-près de 14 pintes; après l'avoir pesé dans cet état, je l'ai vissé sur le bocal, et en ouvrant les deux robinets j'ai permis à l'air du bocal d'entrer dans le ballon jusqu'à refus. La marche de la surface de l'eau dans le bocal, m'a donné le volume d'air [81] introduit dans le ballon, et j'en ai eu le poids par l'excès du poids du ballon plein, sur ce qu'il pesoit

étant vide: par ce moyen, j'ai trouvé que le baromètre étant à 27 pouces 5 lignes, et la température à 15 degrés du thermomètre de Réaumur.

		gros.	grains.
12 pintoes $\frac{2}{4} \frac{7}{8}$ de gaz déphlogistique pesoient,	.	4	13
12 pintoes $\frac{3}{4} \frac{8}{8}$ d'air atmosphérique,	.	3	$56\frac{1}{2}$
12 pintoes $\frac{4}{4} \frac{1}{8}$ d'air inflammable,	.	0	$39\frac{1}{4}$

Par des recherches antérieures je m'étois assuré que le pied cube d'eau de pluie filtrée, à la température de 12 degrés, pèse 69 livres 6 onces 0 gros 39 grains, et qu'il contient 35,865 fois la pinte qui me servoit alors d'unité, j'ai donc pu former la Table suivante, qui donne les poids de la pinte et du pied cube de chacun des trois fluides élastiques.

Noms des Gaz.	Poids de la Pinte.	Poids du Pied Cube.		
		Onces.	Gros.	Grains.
Air déphlogistique,	23 $\frac{1}{2} \frac{9}{10} \frac{5}{8}$	1	3	67,36
Air atmosphérique,	21 $\frac{3}{4} \frac{5}{7}$	1	2	44,03
Air inflammable,	3 $\frac{8}{2} \frac{7}{4} \frac{7}{8}$	0	1	36,86

Pour produire l'inflammation de l'air inflammable et de l'air déphlogistique dans des vaisseaux clos et à l'abri du mélange de toute matière étrangère, je me suis servi de l'appareil suivant.

Dans une caisse hydropneumatique, dont la coupe est représentée par ABCD (figure 2) et dans laquelle le niveau EF de la surface de l'eau étoit entretenu constamment à la même hauteur, j'ai établi deux grands bocaux G et H, semblables à celui qui m'avoit servi à prendre le poids [82] des gaz, et gradués

séparément par le même procédé; ces deux bocaux qui devoient servir de réservoirs, l'un à l'air déphlogistique, l'autre à l'air inflammable, étoient ouverts par en bas, dans le haut ils communiquoient, par des tuyaux de métal garnis des robinets  $\text{I}$  et  $\text{K}$ , à un ballon  $\text{M}$  destiné à servir de récipient, et dans lequel étoit un excitateur pour produire une étincelle électrique à la manière de M. de Volta; cet excitateur étoit d'argent, parce qu'une première expérience m'avoit appris que le cuivre se calcine par la chaleur des inflammations, et donne de la chaux métallique qui altère la pureté des résultats. Un troisième tuyau de métal, pareillement garni d'un robinet  $\text{L}$ , établissoit la communication du ballon à une excellente machine pneumatique  $\text{o}$ , destinée à faire le vide dans le ballon, et à en extraire les fluides élastiques: je m'étois assuré de l'exactitude des luts, des soudures et des robinets, en tenant l'eau suspendue pendant plusieurs jours à 18 pouces de hauteur par chaque robinet en particulier, sans qu'il soit entré la moindre quantité d'air dans l'appareil.

Cela fait, pour introduire le gaz déphlogistique dans le bocal  $\text{H}$ , j'ai ouvert les robinets  $\text{L}$  et  $\text{K}$ , puis en pompant avec la machine pneumatique, j'ai élevé l'eau dans le bocal jusqu'à ce que sa surface fût prête à être cachée par la calotte métallique qui étoit au haut, et j'ai fermé le robinet  $\text{K}$ . Il restoit alors un peu d'air atmosphérique entre la surface de l'eau et le robinet: pour enlever cet air sans faire passer de l'eau par le robinet, j'avois introduit dans le bocal un tube de verre  $\text{PQR}$ , recourbé par en bas, j'ai poussé l'extrémité supérieure de ce tube dans le tuyau de

métal jusqu'à ce qu'elle touchât le robinet, et en aspirant par le bout extérieur R, qui étoit garni d'une soupape de vessie, j'ai totalement vidé d'air le bocal H ; enfin j'y ai introduit le gaz par en bas : de la même manière, et avec les mêmes précautions, j'ai rempli le bocal G d'air inflammable.

Tout étant ainsi préparé, les deux robinets I et K étant fermés, et le robinet L étant seul ouvert, j'ai fait le vide [83] dans le ballon M aussi parfaitement qu'il m'a été possible, et j'ai fermé le robinet L ; puis ouvrant le robinet K, j'ai laissé entrer dans le ballon le douzième de son volume d'air déphlogistique, ce que je pouvois mesurer d'une manière très-précise par la marche de la surface de l'eau dans le bocal H ; ensuite ouvrant le robinet I, j'y ai laissé entrer du gaz inflammable jusqu'à refus, et tous les robinets étant fermés, j'ai tiré une étincelle qui a produit une première explosion. J'ai laissé entrer une seconde fois un douzième d'air déphlogistique, et j'ai eu une seconde explosion, et ainsi de suite jusqu'à six explosions consécutives ; le gaz inflammable étant tout employé, j'ai rendu un douzième d'air déphlogistique, et j'ai laissé entrer de nouveau de l'air inflammable jusqu'à refus ; mais dans ce cas il en entroit moins que la première fois, tant parce que le ballon étoit extrêmement chaud, que parce que la portion des gaz qui ne pouvoit servir à l'inflammation, commençoit à l'engorger, et je n'ai pu obtenir que cinq explosions consécutives : en continuant de cette manière, j'ai pu produire cent trente-sept explosions.

Le ballon étant alors engorgé, parce qu'il étoit trop

petit, j'ai laissé tomber le nuage qui le remplissoit, ensuite j'ai recommencé l'opération du vide, et pour ne rien perdre de tous les produits, j'ai recueilli dans un appareil pneumatique particulier que j'avois adapté à la pompe, tout l'air extrait du ballon pour le soumettre ensuite à l'examen.

Par ce procédé, et en trois suites d'explosions dont le nombre a été porté à trois cents soixante-douze, j'ai consommé

145 pintes  $\frac{9}{14}$  d'air inflammable  
Et 74 pintes  $\frac{9}{16}$  d'air déphlogistique.

Le poids de ces gaz, si leurs densités avoient été les mêmes que lorsque je les pesai, auroit été

	ounces.	gros.	grains.
Pour l'air inflammable,	. . .	0 6	10,03
Pour l'air déphlogistique,	. . .	3 0	58,53
TOTAL, . . .	<hr/>	3 6	68,56

[84] Mais pendant les explosions le poids de l'atmosphère étoit diminué, et sa hauteur moyenne n'étoit plus que de 26 pouces 11 lignes, la température de l'appartement étoit encore la même. Il faut donc diminuer le poids total des deux airs dans le rapport de 27 pouces 5 lignes à 26 pouces 11 lignes; car quoique les différens fluides élastiques ne soient pas tous également dilatables par la chaleur, il est très-probable qu'ils sont tous compressibles suivant la même loi, du moins dans l'état moyen, c'est-à-dire en raison des poids comprimans: d'après cela on trouve

que le poids total des airs que j'ai employés, est de 3 onces 6 gros 27,56 grains.

Avant que d'aller plus loin, je rapporterai quelques circonstances qui ont accompagné ces expériences : 1<sup>o</sup>. chaque explosion occasionnoit une chaleur très-forte, subite, et qui se faisoit sentir d'une manière tres-sensible au visage, même à la distance de trois pieds du ballon ; j'ai été obligé de mettre de l'intervalle entre les explosions, et de refroidir le ballon avec des linges mouillés pour empêcher les luts de se ramollir, et de laisser échapper les fluides élastiques : 2<sup>o</sup>. en refroidissant de cette manière le ballon, le fluide qu'il contenoit perdoit sa transparence et présentoit un brouillard très-épais qui disparaisoit sur le champ à l'explosion suivante, parce que les gouttes de liquide qui le composoient, étoient subitement converties en vapeurs par la haute température qu'exitoit l'inflammation : 3<sup>o</sup>. dans les commencemens de chaque suite d'explosions les étineclles produisoient un certain bruit ; mais sur la fin de la suite et lorsque le ballon commençoit à s'engorger sensiblement, ce bruit changeoit de nature, ou plutôt il étoit accompagné d'un sifflement éclatant qui me donnoit de l'inquiétude et me faisoit craindre qu'il ne s'échappât quelque chose par les luts : j'ai été pleinement convaincu par la suite que ce sifflement étoit occasionné par la grande et subite compression qu'éprouvoit le fluide élastique intérieur, en vertu de la haute température à laquelle l'élevoit l'explosion.

Ces opérations étant finies, j'ai déluté le ballon, je l'ai [85] d'abord pesé avec la liqueur qu'il contenoit, puis j'ai transvasé ce produit, et après avoir bien séché le

ballon je l'ai repesé de nouveau, et j'ai trouvé pour difference, . . . . . 3<sup>oncées.</sup> 2<sup>gros.</sup> 45,1<sup>grains.</sup>  
ce poids est celui du produit en liqueur de l'inflammation des deux gaz.

J'ai ensuite pesé tout l'air que j'avois extrait du ballon par les trois opérations du vide, son volume étoit de sept pintes, et j'ai trouvé son poids de, . . . . . 2 27,91

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Ainsi le poids total des substances qui résultent de l'opération, est de, . . . . . 3 5 1,01

et il s'en faut 1 gros 26,55 grains que ce poids ne soit égal à celui des gaz que j'ai employés. Cette différence peut venir 1°. de ce que j'ai corrigé les volumes d'airs d'après l'état moyen du baromètre pendant l'opération, tandis qu'il faudroit corriger chaque volume d'après la hauteur du baromètre pendant sa consommation particulière : 2°. et principalement de ce que je n'ai pas tenu compte des changemens de température dans les réservoirs qui ont dû s'échauffer par le voisinage du ballon, quoique le thermomètre n'ait pas varié sensiblement dans l'appartement : 3°. enfin de la perte occasionnée par la vaporisation dans chaque opération du vide.

#### *Examen de l'Air extrait du Ballon.*

Les sept pintes d'air que j'ai retirées du ballon, par la machine pneumatique, contenoient un peu d'air fixe : j'en ai agité une partie dans de l'eau de chaux

qu'elle a blanchie, et par cette agitation elle a diminué d'un dix-huitième de son volume : je l'ai fait passer ensuite dans l'eudiomètre de M. de Volta, où elle a détonné par l'étincelle électrique, et par cette opération elle a encore été diminuée d'un cinquième de son volume ; ce qui prouve qu'elle contenoit un mélange de gaz inflammable et de gaz déphlogistiqué. J'ai essayé de faire brûler, à l'air libre, le résidu de cette inflammation, [86] et il a refusé de s'enflammer ; mais par son mélange avec l'air nitreux, il a rutilé et s'est encore réduit comme l'air atmosphérique. Il contenoit donc encore à cette époque un quart de son volume d'air déphlogistiqué. Il suit de tout cela que cet air ne peut être regardé comme le produit de l'inflammation, et qu'il est le résultat des impuretés des deux gaz, impuretés qui peuvent venir en partie de l'air du vaisseau dans lequel j'ai fait le gaz inflammable, malgré l'attention que j'ai eue de ne pas recevoir le produit de la première effervescence, en partie de l'eau de l'appareil qui a été agitée plusieurs fois pour transvaser les gaz, enfin de l'eau employée pour affoiblir l'acide vitriolique.

*Examen du produit en liqueur.*

Cette liqueur, parfaitement transparente, a rougi imperceptiblement le papier teint en bleu par le tournesol, beaucoup moins que celle que j'avois obtenu dans une expérience antérieure, moins encore que la salive. Cette acidité ne peut pas être attribuée à l'air fixe, parce que la liqueur ne précipitoit pas l'eau de chaux, et parce que l'eau distillée, également acidulée par l'air fixe, rendoit sur le champ l'eau

de chaux laiteuse ; elle blanchit à peine la dissolution d'argent dans l'acide nitreux, et un peu plus sensiblement celle de mercure dans le même acide. Outre sa légère acidité, elle a encore la saveur empyreumatique que prend toujours l'eau dans la distillation ; ce résultat doit donc être regardé comme de l'eau pure chargée de la petite quantité d'acide vitriolique qu'entraîne nécessairement avec lui l'air inflammable lorsqu'on le retire de la dissolution de fer.

Une partie de cette eau vient certainement de celle que les deux airs tenoient en dissolution dans leur état aériforme, mais on ne peut pas admettre qu'elle en vienne entièrement, car l'air inflammable et l'air déphlogistique ne seroient alors essentiellement composés l'un et l'autre que de la matière du feu et de celle de la lumière, substances qui ne peuvent être rendues coëreibles ainsi qu'elles le sont dans les fluides<sup>[87]</sup> élastiques, que par leur combinaison avec une matière incapable de passer au travers des parois des vaisseaux.

Il suit de cette expérience, que lorsqu'on fait détonner le gaz inflammable et le gaz déphlogistique, considérés l'un et l'autre comme purs, on n'a d'autre résultat que de l'eau pure, de la matière de la chaleur et de celle de la lumière.

Il reste à savoir actuellement si les deux gaz étant des dissolutions de substances différentes dans le fluide du feu considéré comme dissolvant commun, ces substances, par l'inflammation, abandonnent le dissolvant et se combinent pour produire de l'eau qui ne seroit plus alors une substance simple ; ou bien si les deux gaz étant les dissolutions de l'eau dans des fluides élastiques différens, ces fluides quittent l'eau qu'ils

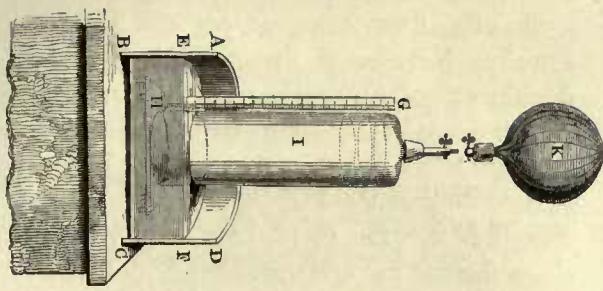
dissolvoient pour se combiner et former le fluide du feu et de la lumière qui s'échappe à travers les parois des vaisseaux : et alors le feu seroit une matière composée. Les deux conséquences sont également extraordinaires, et l'on ne pourra se décider pour l'une d'elles que d'après des expériences d'un autre genre.

En admettant la première, c'est-à-dire, en regardant l'eau comme composée des bases de l'air déphlogistiqué et de l'air inflammable, la végétation seroit une opération par laquelle la Nature décomposeroit l'eau et lui enlèveroit la base de l'air inflammable pour la combiner avec les végétaux qui en sont éminemment pourvus, tandis que la base de l'air déphlogistiqué, à l'aide de la chaleur et de la lumière qui nous viennent du Soleil, reprendroit l'état aériforme pour se porter au dehors, comme l'a observé M. Ingénouz. L'eau ne seroit donc pas nécessaire à la végétation simplement comme véhicule, elle en seroit un des matériaux ; et l'on expliqueroit à-la-fois pourquoi cette opération ne peut pas avoir lieu sans le concours de l'eau, de la chaleur et de la lumière. On rendroit pareillement raison d'un grand nombre d'autres phénomènes ; on expliqueroit, par exemple, pourquoi la flamme des végétaux mouille considérablement les corps froids qu'elle touche ; pourquoi les tuyaux [88] des poèles, quand il fait froid, condensent une si grande quantité d'eau, dont une partie sort des tuyaux et tache les murailles : on n'attribueroit plus la violence de la détonation de la poudre à canon au dégagement des fluides élastiques qu'elle contient, mais à la vaporisation de l'eau produite par l'inflammation, &c. Mais cette hypothèse comporte

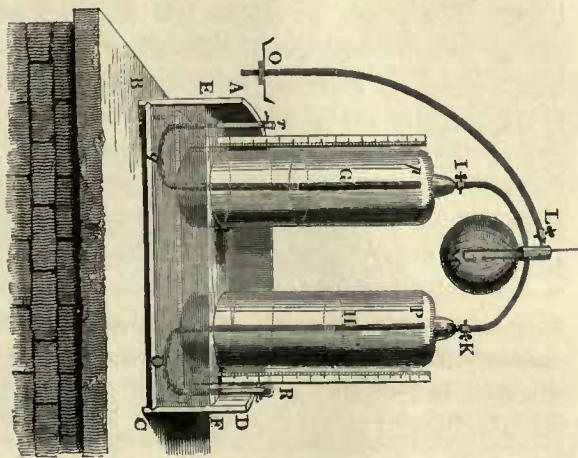
une difficulté qui, dans l'état actuel de nos connaissances, est difficile à résoudre.

En effet, il est confirmé par une foule d'observations que le mélange du gaz inflammable et du gaz déphlogistique n'a besoin, pour s'enflammer, que d'une simple élévation de température, et que cette température dépend de la nature du gaz inflammable, de la dose du gaz déphlogistique, et des densités de ces deux fluides. On éteint une bougie en approchant de sa flamme un corps très-froid, de même qu'on la ralume, lorsqu'on vient de l'éteindre, en approchant de sa mèche un corps très-chaud : le vent même n'éteint la bougie que parce qu'il abaisse trop la température de la vapeur inflammable qui s'élève de la mèche. Les huiles bouillantes s'enflamment par leur propre température et sans avoir besoin du contact d'un corps dans l'état d'ignition. Actuellement, si les deux gaz ne sont autre chose que les dissolutions de deux substances différentes dans le fluide de feu, et si dans l'inflammation ces deux dissolutions se précipitent l'une l'autre, en sorte que les deux bases, en abandonnant le feu qui les dissolvoit, se combinent pour produire de l'eau, il arrive donc qu'en élevant la température, c'est-à-dire qu'en introduisant du feu dans le mélange des deux gaz, ou pour mieux dire encore, qu'en augmentant la dose du dissolvant, on diminue l'adhérence qu'il avoit pour ses bases, ce qui est absolument contraire à ce qu'on observe dans toutes les opérations analogues de la Chimie.

Il nous manque donc encore beaucoup de lumières sur cet objet, mais nous avons droit de les attendre, et du temps, et du concours des travaux des Physiciens.



*Fig. 1.*



*Fig. 2.*



## No. VII.

## EXTRACT FROM THE TRANSLATION OF M. ARAGO'S HISTORICAL ELOGE OF JAMES WATT,\* RELATIVE TO THE DISCOVERY OF THE COMPOSITION OF WATER.

IF Watt had produced during the whole of his long career, nothing but the steam-engine with a separate condenser, the expansive engine, and the parallel motion, he would still occupy one of the first places among the small number of men whose life forms an epoch in the annals of the world. But I hold his name to be also illustriously united to the greatest and most prolific invention of modern chemistry ; I mean, the discovery of the composition of water. My assertion may appear rash, for, in the numerous works which professedly treat of this principal topic in the history of science, Watt has been forgotten.† But, notwithstanding this, I hope that

\* London, John Murray, 1839.

† This is not quite correct, for, in point of fact, Mr. Watt's claims are set forth in an article on WATER, in the third edition of the Encyclopædia Britannica, published in 1797. This, from the great circulation of the work in which it appears, could hardly fail to be pretty generally known. There is a very short account of the matter in Murray's Chemistry, ed. 1806, vol. ii. p. 158 ; and an imperfect one in Nicholson's Chemical Dictionary, first edit. 1795, article WATER ; and in Thomson's Chemistry, second edit. 1804, vol. i. p. 577 ; in all of which, however, the merit of the discovery is more or less attributed

you will have the goodness to follow, without prejudice, what I have to say ; that you will not suffer yourselves to be debarred from making any inquiry, by authorities which, after all, are not so numerous as they are commonly supposed to be ; that you will not refuse to observe how few are the authors who now-a-days derive their information from original sources ; how toilsome a labour they find it to disturb the dust of libraries ; how convenient, on the other hand, it seems to them to live on other men's learning, and to make the composition of a book nothing better than a mere business of editorship. The commission with which the confidence you repose in me has thought proper to entrust me, appeared to me deserving of more serious attention ; I have carefully collated numerous printed papers ; the whole of a voluminous authentic correspondence still in MS. ; and if, after the lapse of fifty years, I am going to

to Mr. Watt ; (although Thomson, in his Life of Cavendish, in vol. i. of the "Annals of Philosophy," does not show even that degree of correct information on the subject contained in his Chemistry.)

But, so far as the French Chemists are concerned, M. Arago's statement is literally true. Fourcroy, in his voluminous work, "Système des Connaissances Chimiques," published in 1801, appears studiously to have avoided the very mention of Mr. Watt's name, although he could not but be acquainted with his paper in the Philosophical Transactions for 1784, and had met with him at Paris in 1787, in the society of his friends, Berthollet, La Place, Monge, and Lavoisier, by all of whom Mr. Watt's merits were appreciated. Cuvier, probably misled by this authority, gives the discovery to Cavendish and Monge, at p. 57 of his "Rapport Historique sur les Progrès des Sciences Naturelles," which was presented to Napoleon by the Institute in 1808, as well as in his Eloge of Fourcroy, read 1811, and of Cavendish, read 1812.—TR.

vindicate for James Watt an honour which has been, on too slight grounds, accorded to one of his most illustrious countrymen, it is because I deem it expedient to show, that in the bosom of learned societies, truth is sooner or later brought to light ; and, that where discoveries are concerned, there never can be any prescription.

The four pretended elements, [or simple substances], fire, air, earth, and water, of which the various combinations were said to give birth to all known bodies, are one of the many legacies bequeathed by that brilliant philosophy, which, for ages, dazzled and bewildered the noblest intellects. Van Helmont was the first to shake, though feebly, one of the principles of this ancient theory, by calling the attention of chemists to several permanent elastic fluids, several airs, which he called *gases*, and the properties of which were different from those of common air—of the element air. The experiments of Boyle and Hooke, gave rise to still more serious doubts ; they rendered it certain that common air, which is necessary for respiration and combustion, undergoes, in these two operations, remarkable changes ; such a change of properties as involves the notion of composition. The numerous observations of Hales ; the successive discoveries of carbonic acid by Black ; of hydrogen by Cavendish ; of nitric acid, oxygen, muriatic acid, sulphuric acid, and ammonia, by Priestley, finally caused the old belief in a single elemental air, to be classed among the random and almost invariably false notions, which owe their birth to those who have the audacity to think that they are called on to explain

the course of nature, by way, not of discovery, but of divination.

Amid so many remarkable discoveries, water had always retained its character of an element. The year 1776 was, at last, signalized by one of those observations which ought to lead to the overthrow of this general belief. It must be confessed, that from the same year are also to be dated those curious efforts which chemists long continued to make, to refuse to admit the natural consequences of their experiments. The observation to which I allude was made by Macquer.

This judicious chemist, having applied a saucer of white porcelain to the flame of hydrogen gas, which was quietly burning at the mouth of a bottle, observed that this flame was accompanied by no smoke, properly so called, and that it deposited no soot ; that part of the saucer which the flame *licked*, became covered with little drops, quite perceptible, of a liquid like water, and which, on analysis, proved to be pure water. Here, unquestionably, was a singular result. Observe well, that it was in the middle of the flame, in that part of the saucer which it *licked*, to use Macquer's expression, that the little drops of water were deposited ! This chemist, however, did not stop to inquire into this fact ; he felt no astonishment at that which is really astonishing in it ; he merely mentions it without comment ; he does not see that he has just laid his finger on a great discovery.

Is, then, genius in the sciences of observation to be reduced to the faculty of saying apropos, *Why?*

In the physical world there are some volcanoes

which have made only a single eruption. In like manner, in the intellectual world also, there are men, who, after a flash of genius, disappear entirely from the history of science. Such a one was Warltire, of whom the chronological order of dates leads me to mention a truly remarkable experiment. In the beginning of the year 1781, this philosopher imagined that an electric spark could not pass through certain gaseous mixtures, without causing some change in them. An idea so novel, which was not then suggested by any analogy, and of which such happy use has since been made, ought, I think, to have led all the historiographers of science not willingly to omit to make honourable mention of its author. Warltire was deceived as to the real nature of the changes which the electrical matter produced. Luckily for himself, he foresaw that they would be attended by an explosion, and it was for this reason that he first made the experiment with a metal vessel, in which he had enclosed some common air and hydrogen gas.\*

Cavendish very soon repeated Warltire's experiment. The certain date of his labours, (by *certain* I mean any date which can be proved by an authentic record, a memoir read to a Society, or a printed

\* M. Arago has omitted to state, that Mr. Warltire, in his letter dated Birmingham, 17th April, 1781, after relating his own experiments in the metal globe, goes on to say, "I have fired air in glass vessels since I saw you [Dr. Priestley] venture to do it, and have observed, as you did, that though the glass was clean and dry before, yet, after firing the air, it became dewy, and was lined with a sooty substance." This proves Dr. Priestley to have first made the experiment in glass vessels, as well as to have first noticed the dewy deposit.—TR.

paper,) is not later than the month of April, 1783 ; since Priestley quotes Cavendish's observations in a paper dated the 21st of the month. From this quotation we gain only one other piece of information ; viz. that Cavendish had obtained water by exploding a mixture of oxygen with hydrogen ; a result already established by Warltire.

In his paper of the month of April [1783,] Priestley added one important circumstance to those which followed from the experiments of his predecessors ; he proved that the weight of the water which is deposited on the sides of the vessel at the instant of the explosion of the oxygen and hydrogen, is the sum of the weights of these two gases.

Watt, to whom Priestley communicated this important result, with the penetration of a superior mind, instantly saw in it a proof that water is not a simple substance.

" Let us now consider," he wrote to his illustrious friend, " what obviously happens in the case of the " deflagration of the inflammable and dephlogisticat- " ed air. These two kinds of air unite with violence, " they become red-hot, and, upon cooling, totally dis- " appear. When the vessel is cooled, a quantity of " water is found in it equal to the weight of the air " employed. This water is then the only remaining " product of the process, and *water, light, and heat,* " are all the products.

" *Are we not then authorized to conclude, that water " is composed of dephlogisticated air and phlogiston, " deprived of part of their latent or elementary heat ; " that dephlogisticated or pure air is composed of*

"water deprived of its phlogiston, and united to elementary heat and light ; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye ; and if light be only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat?"\*

This passage, so clear, so precise, so methodical, is extracted from a letter of Watt, dated 26th April 1783. The letter was communicated by Priestley to several of the London philosophers, and without delay transmitted to Sir Joseph Banks, President of the Royal Society, for the purpose of being read at one of the meetings of that learned body. Circumstances which I suppress, because they do not affect the present inquiry, delayed this reading for a year, but the letter remained in the archives of the Society. It figures in the 74th volume of the Philosophical Transactions, with its true date of 26th April 1783. It is there to be found contained in a letter from Watt to De Luc, dated 26th November 1783 ; and is distinguished by inverted commas, added by the Secretary of the Royal Society.†

\* In that chemical nomenclature which was most commonly employed when Mr. Watt wrote the above letter, hydrogen gas was called inflammable air, or phlogiston ; and oxygen gas, dephlogisticated air. For a more full account of the use of those terms, (which we here explain merely in order to prevent any misapprehension on the part of the reader,) we refer to M. Arago's note on the subject, given in Lord Brougham's paper in the Appendix.—Tr.

† See Phil. Trans. vol. LXXIV. p. 330, and particularly Mr. Watt's

I ask no pardon for this profusion of detail ; you will see that nothing but the most minute comparison of dates can set the truth in its full light, and that there is here question of one of those discoveries, which do the greatest honour to the human intellect.

Among the pretenders to this prolific discovery, we are now to see the two greatest chemists of whom France and England boast. The names of Lavoisier and Cavendish will at once occur to the minds of all.

The date of the public reading of the memoir, in which Lavoisier gave an account of his experiments, and developed his views as to the formation of water by the combustion of oxygen and hydrogen, is two months later than that of Watt's letter, preserved in the archives of the Royal Society of London, which has been already noticed.

The celebrated paper by Cavendish, entitled, "Experiments on Air,"\* is still more recent ; it was

note at the foot of that page. The note is as follows :—" This letter  
" Dr. Priestley received at London ; and, after showing it to several  
" Members of the Royal Society, he delivered it to Sir Joseph Banks,  
" the President, with a request that it might be read at some of the  
" public meetings of the Society ; but before that could be complied  
" with, the author, having heard of Dr. Priestley's new experiments,  
" begged that the meeting might be delayed. The letter, therefore,  
" was reserved until the 22d of April last, [1784,] when, at the author's  
" request, it was read before the Society. It has been judged unnec-  
" cessary to print that letter, as the essential parts of it are repeated,  
" almost *verbatim*, in this letter to Mr. De Luc; but, to authenticate  
" the date of the author's ideas, the parts of it which are contained  
" in the present letter are marked with double commas."—Tr.

\* See Phil. Trans. vol. LXXIV. p. 119.—Tr.

read on the 15th of January, 1784. You might with reason be astonished that facts so well authenticated as these, could ever have become the subject of an animated polemical controversy, did I not hasten to call your attention to a circumstance of which I have not as yet spoken. Lavoisier declared in positive terms, that Blagden, the Secretary to the Royal Society of London, was present at his first experiments of the 24th June, 1783, and, "that he informed him "that Cavendish, having already tried" (at London) "to burn inflammable air in close vessels, had obtained a very perceptible quantity of water, [une "quantité d'eau très sensible.]"<sup>\*</sup>

Cavendish also, in his paper, repeated the communication which Blagden made to Lavoisier. According to his account, it entered into greater detail than the French chemist acknowledged. He said that it embraced the conclusions to which the experiments led; that is to say, the theory of the composition of water.

Blagden, himself made a party in the cause, wrote, in Crell's Journal,<sup>†</sup> in 1786, a confirmation of Cavendish's assertion.

If we are to believe him, the experiments of the

\* Lavoisier's Memoir, in which these words occur, appears at p. 472 of that volume of the "Mémoires de l'Académie Royale des Sciences," which is entitled, "pour l'année 1781," but which was not printed till the year 1784. The paper was read on the 11th of November, 1783.—Tr.

† Entitled, "Chemische Annalen für die Freunde der Naturlehre, "Arzneygelehrtheit, Haushaltungskunst, und Manufacturen: von D. "Lorenz Crell," etc. etc. Helmstädt u. Leipzig, 1786. 8vo.—Tr.

Parisian Academician were nothing more than a mere verification of those of the English chemist. He declares that he told Lavoisier, that the water obtained at London had a weight precisely equal to the sum of the weights of the two gases which were exploded. "Lavoisier," adds Blagden in conclusion, "*has told the truth, but not the whole truth.*"

A reproach such as this is severe ; but, even were it well-founded, should I not greatly diminish its force if I were to show, that, with the exception of Watt, all those whose names figure in this narrative, were, in a greater or less degree, exposed to it ?

Priestley records in detail, and as his own, experiments which prove, that the water produced by the combustion of a mixture of hydrogen and oxygen, has a weight exactly equal to that of the two gases which are burned. Cavendish, some time after, claims this result for himself, and insinuates that he had communicated it verbally to the chemist of Birmingham.

Cavendish draws from this equality of weight the conclusion that water is not a simple substance. In the outset, he makes no mention of a paper deposited in the archives of the Royal Society, in which Watt developed the same theory. It is true, that when his paper went to the press, the name of Watt was not forgotten ; but it was not in the archives that the work of the illustrious engineer had been seen ; the author declares that he became acquainted with it in a paper "lately read before this [the Royal] Society."\*

\* See the Phil. Trans. vol. LXXIII. p. 140.—Tr.

Yet it is now perfectly established, that this reading took place several months after that of the paper in which Cavendish speaks of it.

At his first entrance on this grave inquiry, Blagden states his firm resolution to clear up every thing, and, in every thing, to be precisely accurate. In fact, he does not shrink from any accusation, nor from the citation of any date, so long as there is question of ensuring to his patron and friend, Cavendish, the priority over the French chemist. As soon as he begins to speak of his two countrymen, his explanations become vague and obscure. "In the spring [Frühjahr,"] he says, "of 1783, Mr. Cavendish communicated to me and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us that out of them he must draw the conclusion, that dephlogisticated air" (or oxygen) "was nothing else than water deprived of its phlogiston," (that is to say, deprived of its hydrogen,) "and vice versa, that water was dephlogisticated air united with phlogiston. *About the same time*, [um dieselbe Zeit,] the news was brought to London, that Mr. Watt of Birmingham had been induced by some observations, to form [fassen] a similar opinion."\*

That expression, "*about the same time*," cannot be, to use Blagden's own words, "*the whole truth.*" "*About the same time*" proves nothing; questions as

\* See Blagden's paper in Crell's Journal, vol. i. 1786, p. 59. It is, on many accounts, a very remarkable one.—Tr.

to priority may depend on weeks, on days, on hours, on minutes. To be precisely accurate, as he had promised to be, it was indispensable that he should say, whether the verbal communication, made by Cavendish to several members of the Royal Society, preceded or followed the arrival in London of the news of Watt's labours. Can it be supposed that Blagden would not have explained so very important a circumstance, if he could have brought forward an authentic date favourable to his friend ?

To complete the *imbroglio*, the foremen, the compositors, and printers of the *Philosophical Transactions*, also took part in it. Some dates in them were typographically wrong. In the detached copies of his paper, which Cavendish distributed to various learned men, I observe a mistake of one whole year. By a sad fatality—for it is a real misfortune to give rise, unintentionally, to annoying and unmerited suspicions—not one of those numerous errors of the press was favourable to Watt ! God forbid that I should, by these remarks, intend to cast any imputation on the literary probity of those illustrious philosophers, whose names I have mentioned ; they only prove, that, on the subject of discoveries, the strictest justice is all that can be expected from a rival, or a competitor, however high his reputation may already be. Cavendish would hardly listen to his men of business, when they came to consult him as to the investment of his twenty-five or thirty millions [francs] ;\* you can now judge, whether he felt the

\* The circumstance here alluded to, is thus recorded by Dr. Tho-

same indifference about experiments. It would not, then, be too much to require, that, following the example of Judges in matters of civil law, the historiographers of science should never admit as probative, any titles to property, but such as are in writing ;— perhaps I should even say, but such as are registered.—Then, but not till then, would cease those contentions, continually recurring, which are usually fed by national vanity ; then, in the history of chemistry, the name of Watt would reassume that lofty position, which of right belongs to it.

The settlement of a question of priority, when it turns, as in the above instance, on the most careful examination of printed memoirs, and the most minute comparison of dates, assumes the character of a very demonstration. Yet, I do not consider, that this entitles me to an exemption from taking a rapid review

mas Thomson, in his Life of Cavendish :—" In consequence of the " habits of economy which he had acquired, it was not in his power " to spend the greater part of his annual income. This occasioned " a yearly increase to his capital, till at last it accumulated so much, " without any care on his part, that at the period of his death he left " behind him nearly £1,300,000, and was the greatest proprietor in " the Bank of England. On one occasion, his money in the hands " of his bankers accumulated to the amount of £70,000; these " gentlemen, thinking it improper to keep so large a sum in their " hands, sent one of the partners to wait upon him, in order to learn " how he wanted it disposed of. This gentleman was admitted, and, " after employing the necessary precautions to a man of Mr. Caven- " dish's peculiar disposition, stated the circumstance, and begged to " know whether it would not be proper to lay out the money. Mr. " Cavendish dryly answered, ' You may lay it out if you please,' " and left the room."—*Thomson's Annals of Philosophy*, vol. i. p.

5.—TR.

of various difficulties, to which very able minds appear to have attached some importance.

How is it possible, I have heard it said, that in the midst of a vast vortex of business engagements ; with his time taken up by a host of law-suits ; every day obliged to provide, by new contrivances, for the difficulties of a manufacture yet in its infancy, Watt could have found leisure to follow, step by step, the progress of chemistry—to make new experiments—to propose explanations, of which the greatest masters of the science had never thought ?

To this difficulty, I will give a short, but conclusive reply. I have in my hands a copy of an active correspondence, relating chiefly to subjects of chemistry, which Watt maintained, beginning in 1782, and continued in 1783 and 1784, with Priestley, Black, De Luc, Smeaton the engineer, Gilbert Hamilton of Glasgow, and Fry of Bristol.

The next objection is more plausible ; it is founded in a deep knowledge of human nature.

The discovery of the composition of water, keeping pace with those admirable inventions which we find united in the steam-engine ; can we suppose that Watt would consent with cheerfulness, or at least without expressing his dissatisfaction, to see himself stripped of the honour which it ought for ever to reflect on his name ?

This reasoning has the fault of being wholly without foundation. Watt never renounced the share which by right belonged to him, in the discovery of the composition of water. He caused his paper to be printed, with scrupulous care, in the Philosophical

Transactions. A detailed note, authentically established the date of the giving in of the different paragraphs of that paper. What more could or ought a philosopher of the character of Watt to have done, but wait with patience for the day of justice ? Yet, an awkwardness of De Luc had nearly roused our fellow-member from the forbearance natural to him. The Genevese philosopher, after having apprised the illustrious engineer of the unaccountable omission of his name in the first copy of Cavendish's paper ; after having characterised this omission in terms which regard for such high reputations prevents me repeating, wrote to his friend—"I should almost advise you, "considering your position, to draw from your discoveries practical results for your fortune ; you "should be cautious how you excite jealousy."\*

Words such as these, wounded the high soul of Watt. "As to what you say about making myself "*des jaloux*," wrote he, "that idea would weigh little ; "for were I convinced I had had foul play, if I did "not assert my right, it would either be from a "tempt of the modicum of reputation which could "result from such a theory ; from the conviction in "my own mind that I was their superior, or from "an indolence that makes it easier to me to bear "wrongs, than to seek redress. In point of interest, in so far as connected with money, that "would be no bar ; for, though I am dependent on

\* The words in the original letter are these :—"Je le vous conseillerai presque, attendu votre position, de tirer de vos découvertes des conséquences pratiques pour votre fortune. Il vous faut éviter de vous faire des jaloux."—Tr.

"the favour of the public, I am not on Mr. Cavendish  
"or his friends."

Can it be thought that I have attached too much importance to the theory which Watt devised, to account for the experiments of Priestley? Surely not. Those who would refuse to this theory the applause which it deserves, because it now appears to follow necessarily from the facts, forget, that the greatest discoveries of the human intellect, have been most remarkable for their simplicity. What did Newton himself do, when, repeating an experiment known fifteen centuries before his time, he discovered the composition of white light? Of that experiment he gave an explanation so perfectly natural, that it appears impossible at this day to find any other. "All "that is drawn," says he, "by whatever process, out "of a ray of white light, was contained in it in its "compound state. The glass prism has no creative "power. If the parallel and infinitely delicate ray "of solar light, which falls on its first face, passes out "by the second divergently, and with a perceptible "magnitude, it is because the glass separates that, "which in the white ray, was, by its nature, unequally "refrangible." These words are nothing else, than the literal translation of the well-known experiment of the prismatic solar spectrum. Yet this explanation had escaped an Aristotle, a Descartes, a Robert Hooke!

Let us, without leaving the subject, proceed to arguments which come still more directly to the point. The theory which Watt formed of the composition of water, reaches London. If in the opinion of that

time it is considered as simple, as self-evident, as it now appears to us, the Council of the Royal Society would not fail to adopt it. But it was not so ; its strangeness even caused the truth of the experiments of Priestley to be doubted. "People even go so far," says De Luc, "as to *laugh at it*, as at *the explanation of the golden tooth.*"\*

\* The history of this egregious imposition is given at length by Daniel Sennertus, a physician of Wittemberg, to whom it was communicated by D. Michael Doringius, who again had received it from Daniel Bucretius of Vratislau. It is copied from Sennertus by the learned Dr. Antony van Dale, in his second Dissertation, "de Oracleculis Ethnicorum," (pp. 474, 475, edit. Amst. 1683,) and is thence adopted by Fontenelle; who, in somewhat abridging the particulars of the story, has not failed to adorn it with the graces of his wit. We quote it in his words :—" En 1593, le bruit courut que les dents " estant tombées à un enfant de Silésie, âgé de sept ans, il luy eu " estoit venu une d'or, à la place d'une de ses grosses dents. Horstius, " Professeur en Médecine dans l'Université de Helmstad, écrivit en " 1595 l'Histoire de cette dent, et prétendit qu'elle estoit en partie " naturelle, en partie miraculeuse, et qu'elle avoit été envoyée de " Dieu à cet Enfant pour consoler les Chrétiens affligez par les Turcs. " Figurez-vous quelle consolation, et quel rapport de cette dent aux " Chrestiens, ny aux Tures ! En la mesme année, afin que cette " dent d'or ne manquast pas d'Historiens, Rullandus en écrit encore " l'Histoire. Deux ans après, Ingolsteterus, autre Scavant, écrit " contre le sentiment que Rullandus avoit de la dent d'or, et Rul- " landus fait aussitost une belle et docte Réplique. Un autre grand " Homme, nommé Libavius, ramasse tout ce qui avoit été dit de la " dent, et y ajoute son sentiment particulier. Il ne manquoit autre " chose à tant de beaux Ouvrages, sinon qu'il fust vray que la dent " estoit d'or. Quand un Orfevre l'eut examinée, il se trouva que " c'estoit une feüille d'or appliquée à la dent avec beaucoup d'adresse ; " mais on commença par faire des Liures, et puis on consulta l'Or- " fevre." " In 1593, the rumour spread, that the teeth of a child, " seven years old, in Silesia, having fallen out, a golden one had

A theory, the formation of which presented no difficulty, would assuredly have been disdained by Cavendish. Now recollect with what eagerness Blagden, under the influence of this talented man, claimed the priority for him in opposition to Lavoisier.

Priestley, on whom was to redound a considerable

" come in the place of one of the large teeth. Horstius, Professor  
" of Medicine in the University of Helmstad, wrote, in 1595, the  
" History of this tooth; and pretended that it was partly natural,  
" partly miraculous, and that it had been sent from God to this child,  
" to console the Christians oppressed by the Turks. Fancy what  
" consolation, or what concern this tooth could be to the Christians  
" or to the Turks! In the same year, in order that this golden tooth  
" might not want historians, Rullandus wrote a second history of it.  
" Two years after, Ingolsteterus, another philosopher, wrote against  
" the theory which Rullandus had about the golden tooth, and Rul-  
" landus forthwith makes a fine and learned Reply. Another great  
" man, named Libavius, collects all that had been said of the tooth,  
" and adds his own theory. Nothing else was wanting to all those  
" fine books, except that it should be true that the tooth was of gold.  
" On a goldsmith examining it, he found, that it was a leaf of gold  
" applied to the tooth with much address; but they began by mak-  
" ing books, and then they consulted the goldsmith." Fontenelle,  
Hist. des Oracles, p. 22, édit. d'Amst. 1719. The Treatise of Horstius  
referred to, is appended to that addition of his book, " de Natura,  
" Differentiis, et Causis eorum qui Dormientes Ambulant," which was  
printed at Leipzig in 1595. A work which seems to have escaped  
the notice of both Van Dale and Fontenelle, is the " Tractatus de  
" dente aureo," of Dr. Duncan Liddel, a native of Scotland, Professor  
of Mathematics and of Medicine in the same University with Horstius.  
It was printed at Hamburg in 1628.

Sennertus ends his narrative with this apposite moral:—" Quae  
" historia omnes naturae scrutatores meritò monere debet, ne causas  
" rei, et TO ΔΙΟΤΙ prius quacrant, quàm TO OTI sit manifestum, et  
" de re ipsà planè constet."—Tr.

portion of the honour belonging to the discovery of Watt ; Priestley, whose affectionate regard for the great engineer admits of no question, wrote to him, on the 29th of April 1783, “Behold with surprise “and indignation, the figure\* of an apparatus that “has utterly ruined your beautiful hypothesis.”†

In short, a hypothesis which was laughed at by the Royal Society ; which made Cavendish break through his habitual reserve ; which Priestley, laying aside all self-love, set himself to overturn, deserves to be recorded in the history of science as a great discovery, whatever we might at the present day be led to think of it, from knowledge now become common.‡

\* In this letter, Priestley has made a rough sketch, with his pen, of the apparatus which he employed in the experiments to which he here alludes.—Tr.

† Mr. Watt, in his reply to the above letter, uses these forcible expressions :—“I deny that your experiment ruins my hypothesis. “It is not founded on so brittle a basis as an earthen retort, nor on “its converting water into air. I founded it on the other facts, and “was obliged to stretch it a good deal before it would fit this expe-“riment. \* \* I maintain my hypothesis, until it shall be shewn “that the water, formed after the explosion of the pure and inflam-“mable air, has some other origin.”—Tr.

‡ Lord Brougham was present at the public meeting, at which, in the name of the Academy of Sciences, I paid this tribute of gratitude and admiration to the memory of Watt. On returning to England, his Lordship collected valuable documents, and studied afresh the historical question to which I have assigned so large a space, with all that superiority of discernment which is habitual to him, and that acuteness, in some sort judicial, which might have been expected from one who was Lord Chancellor of Great Britain. I owe it to a considerate kindness, of which I feel the full value, that I am enabled to make known the result, hitherto unpublished, of the

labour of my illustrious fellow-member. It will be found appended to this Eloge.—M. ARAGO.

It is not without feelings of regret, that we find ourselves here called upon to refer to a speech, delivered by the Rev. W. Vernon Harcourt, from the chair which he temporarily occupied, as President of the British Scientific Association, lately assembled at Birmingham. But we have been informed by some of the audience, that the address was read from a written paper; and the manner in which it has since been elaborately reported, and extensively circulated in newspapers, does not permit us altogether to overlook it.

After a feeble, and almost reluctant admission of the merits of Mr. Watt, as an inventor and engineer, Mr. Harcourt proceeded to accuse M. Arago of error and misrepresentation, in having called in question what Mr. H. is pleased to term the long-established claims of Mr. Cavendish to the discovery above mentioned. As M. Arago neither was present at the meeting, nor had any friend there acquainted with the subject, or prepared to defend him, we can say little for the courtesy and liberality which prompted this public attack on an absent foreigner; more especially as, in the report so elaborately drawn up, Mr. H. has avoided all allusion to the Historical Note by Lord Brougham, whose opinion on the subject is as decided as that of M. Arago. The latter needs no aid of ours for his vindication, should he consider the provocation deserving of his notice. It will occur to every one, that when we see the Secretary of the French Academy of Sciences, (who, from his place, as well as his personal character, must be exempted from all suspicion of indifference to the intellectual glory of his nation,) abandoning the claim of priority for his most ingenious and ill-fated countryman, Lavoisier, he may be allowed to be well qualified to form an impartial estimate of the respective claims of two Englishmen, known to him only by their writings, acts, and reputation.

To Mr. H.'s main argument, founded on the character and reputation of Mr. Cavendish, we take leave to reply, that while we entertain the highest opinion of his merits as an experimentalist and philosopher, this can never blind us to the *facts*, so clearly detailed, and established on such conclusive evidence, by M. Arago and Lord Brougham. And we beg leave to inform Mr. H., that the later and more matured opinion of Sir Humphry Davy on this question, dif-

ferred little from that of every other competent judge who has examined it.

We can lay no stress on what is said of the diffidence of Mr. Cavendish. For, although we were aware of his personal shyness and retired habits, we never heard of his betraying any distrust of his scientific attainments, or any unconsciousness of their value; which alone could have any bearing on a question like the present; and when we see a deduction attempted to be forced from *his* alleged want of ambition and indifference to fame, we are called upon to observe, that it would have been but justice to have stated how much more eminently those qualities appeared in the man from whose merits Mr. H. is here labouring to detract. The unassuming modesty of Mr. Watt's character was conspicuous in every action of his life; it has been recognised by the most eminent men of his age; and was never more signally displayed, than in his conduct throughout this very affair, as most correctly stated by M. Arago.

The difficulty which Mr. H. professes to feel in supposing, that Mr. Watt, by *phlogiston*, meant inflammable air or *hydrogen* gas, would have been removed if he had attended to Mr. Watt's own note, (given both in the Phil. Trans. and in Lord Brougham's Historical Note,) which is to this effect:—“*Prievous to Dr. Priestley's making these experiments, Mr. Kirwan had proved, by very ingenious deductions “from other facts, that inflammable air was, in all probability, the real “phlogiston in an aërial form. These arguments were perfectly con-“vincing to me.*”

We look in vain for any other argument by which Mr. Harcourt attempts to support his rash hypothesis. No evidence whatever is produced to disprove any fact brought forward by M. Arago; and, not daring to grapple with the priority of publication, placed upon record by Mr. Watt's note in the Philosophical Transactions, which was never contradicted or called in question by Mr. Cavendish, or his friends, he expends himself in tedious sophistical declamation on the merits of the respective explanations of their theories, given by the three great candidates for the discovery. We shall for the present leave him to the possession of his opinion—“alone,” we believe, “in his glory!” But, since his TASTE led him to select, for the scene of his diatribe, a town justly proud of Mr. Watt's long residence near and connexion with it, he can hardly be surprised at our informing him that, *there* at least, his ill-advised oration has left no impression so strong, as that of general DISGUST.—TR.

## No. VIII.

HISTORICAL NOTE ON THE DISCOVERY OF THE THEORY  
OF THE COMPOSITION OF WATER. BY THE RIGHT  
HON. HENRY LORD BROUHAM, F.R.S., AND MEMBER  
OF THE NATIONAL INSTITUTE OF FRANCE.

THERE can be no doubt whatever, that the experiment of Mr. Warltire, related in Dr. Priestley's 5th volume,\* gave rise to this inquiry, at least in Eng-

\* Mr. Warltire's letter is dated Birmingham, 18th April, 1781, and was published by Dr. Priestley in the Appendix to the 2d Vol. of his "Experiments and Observations relating to various branches of Natural Philosophy; with a continuation of the Observations on "Air,"—forming, in fact, the 5th volume of his "Experiments and "Observations on different kinds of Air;" printed at Birmingham in 1781.

Mr. Warltire's first experiments were made in a copper ball or flask, which held three wine pints, the weight 14 oz.; and his object was to determine "whether heat is heavy or not." After stating his mode of mixing the airs, and of adjusting the balance, he says, he "always accurately balanced the flask of common air, then found the difference of weight after the inflammable air was introduced, that "he might be certain he had confined the proper proportion of each. "The electric spark having passed through them, the flask became "hot, and was cooled by exposing it to the common air of the room: "it was then hung up again to the balance, and a loss of weight was "always found, but not constantly the same; upon an average it was "two grains."

He goes on to say, "I have fired air in glass vessels since I saw you

land ; Mr. Cavendish expressly refers to it, as having set him upon making his experiments.—(Phil. Trans. 1784, p. 126.) The experiment of Mr. Warltire consisted in firing, by electricity, a mixture of inflammable and common air in a close vessel, and two things were said to be observed ; *first*, a sensible loss of weight ; *second*, a dewy deposit on the sides of the vessel.

Mr. Watt, in a note to p. 332 of his paper, Phil. Trans. 1784, inadvertently states, that the dewy deposit was first observed by Mr. Cavendish ; but Mr. Cavendish himself, p. 127, expressly states Mr. Warltire to have observed it, and cites Dr. Priestley's 5th volume.

Mr. Cavendish himself could find no loss of weight,

" (Dr. Priestley) venture to do it, and I have observed, *as you did*,  
" that, though the glass was clean and dry before, yet, after firing  
" the air, it became dewy, and was lined with a sooty substance."

As you are upon a nice balanceing of claims, ought not Dr. Priestley to have the credit of first noticing the dew ?

In some remarks which follow, by Dr. Priestley, he confirms the loss of weight, and adds, " I do not think, however, that so very bold an opinion as that of the latent heat of bodies contributing to their weight, should be received without more experiments, and made upon a still larger scale. If it be confirmed, it will no doubt be thought to be a fact of a very remarkable nature, and will do the greatest honour to the sagacity of Mr. Warltire. I must add, that the moment he saw the moisture on the inside of the close glass vessel in which I afterwards fired the inflammable air, he said, that it confirmed an opinion he had long entertained, viz. that common air deposits its moisture when it is phlogisticated."

It seems evident, that neither Mr. Warltire, nor Dr. Priestley, attributed the dew to any thing else than a mechanical deposit of the moisture suspended in common air.—[NOTE BY MR. JAMES WATT.]

and he says, that Dr. Priestley had also tried the experiment, and found none. But Mr. Cavendish found there was always a dewy deposit, without any sooty matter. The result of many trials was, that common air and inflammable air being burnt together, in the proportion of 1000 measures of the former to 423 of the latter, “about one-fifth of the common air, and nearly all the inflammable air, lose their elasticity, and *are condensed into the dew which lines the glass.*” He examined the dew, and found it to be pure water. He therefore concludes, that “almost all the inflammable air, and about one-sixth of the common air, are turned into pure water.”

Mr. Cavendish then burned, in the same way, dephlogisticated and inflammable airs, (oxygen and hydrogen gases,) and the deposit was always more or less acidulous, accordingly as the air burnt with the inflammable air was more or less phlogisticated. The acid was found to be nitrous. Mr. Cavendish states, that “almost the whole of the inflammable and de-phlogisticated air *is converted into pure water.*” And, again, that “if these airs could be obtained “perfectly pure, the whole would be condensed.” And he accounts for common air and inflammable air, when burnt together, not producing acid, by supposing that the heat produced is not sufficient. He then says that these experiments, with the exception of what relates to the acid, were made in the summer of 1781, and mentioned to Dr. Priestley ; and adds, that “a friend of his, (Mr. Cavendish’s,) last summer” (that is, 1783,) “gave some account of them to Mr. Lavoisier, as well as of the conclusion drawn from

" them, that dephlogisticated air is only water de-  
" prived of its phlogiston ; but, at that time, so far  
" was Mr. Lavoisier from thinking any such opinion  
" warranted, that till he was prevailed upon to repeat  
" the experiment himself, he found some difficulty in  
" believing that nearly the whole of the two airs could  
" be converted into water." The friend is known  
to have been Dr., afterwards Sir Charles Blagden ;  
and it is a remarkable circumstance, that this passage  
of Mr. Cavendish's paper appears not to have been  
in it when originally presented to the Royal Society ;  
for the paper is apparently in Mr. Cavendish's hand,  
and the paragraph, pp. 134, 135, is not found in it, but  
is added to it, and directed to be inserted in that  
place. It is, moreover, not in Mr. Cavendish's hand,  
but in Sir Charles Blagden's ; and, indeed, the latter  
must have given him the information as to Mr. La-  
voisier, with whom it is not said that Mr. Cavendish  
had any correspondence. The paper itself was read  
15th January 1784. The volume was published  
about six months afterwards.

Mr. Lavoisier's memoir (in the *Mém. de l'Académie des Sciences* for 1781,) had been read partly in November and December 1783, and additions were afterwards made to it. It was published in 1784. It contained Mr. Lavoisier's account of his experiments in June 1783, at which, he says, Sir Charles Blagden was present ; and it states that he told Mr. Lavoisier of Mr. Cavendish having "already burnt inflammable "air in close vessels, and obtained a very sensible "quantity of water." But he, Mr. Lavoisier, says nothing of Sir Charles Blagden having also mention-

ed Mr. Cavendish's conclusion from the experiment. He expressly states, that the weight of the water was equal to that of the two airs burnt, unless the heat and light which escape are ponderable, which he holds them not to be. His account, therefore, is not reconcilable with Sir Charles Blagden's, and the latter was most probably written as a contradiction of it, after Mr. Cavendish's paper had been read, and when the Mémoires of the Académie were received in this country. These Mémoires were published in 1784, and could not, certainly, have arrived, when Mr. Cavendish's paper was written, nor when it was read to the Royal Society.

But it is further to be remarked, that this passage of Mr. Cavendish's paper in Sir Charles Blagden's handwriting, only mentions the experiments having been communicated to Dr. Priestley; "they were made," says the passage, "in 1781, and communicated to Dr. Priestley;" it is not said when, nor is it said that "the conclusions drawn from them," and which Sir Charles Blagden says he communicated to Mr. Lavoisier in summer 1783, were ever communicated to Dr. Priestley; and Dr. Priestley, in his paper, (referred to in Mr. Cavendish's,) which was read June 1783, and written before April of that year, says nothing of Mr. Cavendish's theory, though he mentions his experiment.

Several propositions then are proved by this statement.

*First,* That Mr. Cavendish, in his paper, read 15th January 1784, relates the capital experiment of burning oxygen and hydrogen gases in a close vessel,

and finding pure water to be the produce of the combustion.

*Secondly,* That, in the same paper, he drew from this experiment the conclusion, that the two gases were converted or turned into water.

*Thirdly,* That Sir Charles Blagden inserted in the same paper, with Mr. Cavendish's consent, a statement that the experiment had first been made by Mr. Cavendish in summer 1781, and mentioned to Dr. Priestley, though it is not said when, nor is it said that any conclusion was mentioned to Dr. Priestley, nor is it said at what time Mr. Cavendish first drew that conclusion. *A most material omission.*

*Fourthly,* That in the addition made to the paper by Sir Charles Blagden, the conclusion of Mr. Cavendish is stated to be, that oxygen gas is water deprived of phlogiston ; this addition having been made after Mr. Lavoisier's memoir arrived in England.

It may further be observed, that in another addition to the paper, which is in Mr. Cavendish's handwriting, and which was certainly made after Mr. Lavoisier's memoir had arrived, Mr. Cavendish for the first time distinctly states, as upon Mr. Lavoisier's hypothesis, that water consists of hydrogen united to oxygen gas. There is no substantial difference, perhaps, between this and the conclusion stated to have been drawn by Mr. Cavendish himself, that oxygen gas is water deprived of phlogiston, supposing phlogiston to be synonymous with hydrogen ; but the former proposition is certainly the more distinct and unequivocal of the two : and it is to be observed that

Mr. Cavendish, in the original part of the paper, *i. e.* the part read January 1784, before the arrival of Lavoisier's, considers it more just to hold inflammable air to be phlogisticated water than pure phlogiston, (p. 140.)

We are now to see what Mr. Watt did ; and the dates here become very material. It appears that he wrote a letter to Dr. Priestley on 26th April 1783, in which he reasons on the experiment of burning the two gases in a close vessel, and draws the conclusion, "that water is composed of dephlogisticated "air and phlogiston, deprived of part of their latent "heat."\* The letter was received by Dr. Priestley and delivered to Sir Joseph Banks, with a request that it might be read to the Royal Society ; but Mr. Watt afterwards desired this to be delayed, in order that he might examine some new experiments of Dr. Priestley, so that it was not read until the 22d April 1784. In the interval between the delivery of this letter to Dr. Priestley, and the reading of it, Mr. Watt had addressed another letter to Mr. De Luc, dated

\* It may with certainty be concluded from Mr. Watt's private and unpublished letters, of which the copies taken by his copying-machine, then recently invented, are preserved, that his theory of the composition of water was already formed in December 1782, and probably much earlier. Dr. Priestley, in his paper of 21st April 1783, p. 416, states, that Mr. Watt, prior to his (the Doctor's) experiments, had entertained the idea of the possibility of the conversion of water or steam into permanent air. And Mr. Watt himself, in his paper, Phil. Trans. p. 335, asserts, that for many years he had entertained the opinion that air was a modification of water, and he enters at some length into the facts and reasoning upon which that deduction was founded.—[NOTE BY MR. JAMES WATT.]

26th November 1783,\* with many further observations and reasonings, but almost the whole of the original letter is preserved in this, and is distinguished by inverted commas. One of the passages thus marked, is that which has the important conclusion above mentioned ; and that letter is stated, in the subsequent one, to have been communicated to several members of the Royal Society at the time of its reaching Dr. Priestley, viz. April 1783.

\* The letter was addressed to Mr. J. A. De Luc, the well-known Genevese philosopher, then a Fellow of the Royal Society, and Reader to Queen Charlotte. He was the friend of Mr. Watt, who did not then belong to the Society. Mr. De Luc, following the motions of the Court, was not always in London, and seldom attended the meetings of the Royal Society. He was not present when Mr. Cavendish's paper of 15th January 1784, was read ; but, hearing of it from Dr. Blagden, he obtained a loan of it from Mr. Cavendish, and writes to Mr. Watt on the 1st March following, to apprise him of it, adding that he has perused it, and promising an analysis. In the postscript he states, "In short, they expound and prove your system, word for "word, and say nothing of you." The promised analysis is given in another letter of the 4th of the same month. Mr. Watt replies on the 6th, with all the feelings which a conviction he had been ill-treated was calculated to inspire, and makes use of those vivid expressions which M. Arago has quoted ; he states his intention of being in London in the ensuing week, and his opinion, that the reading of his letter to the Royal Society will be the proper step to be taken. He accordingly went there, waited upon the President of the Royal Society, Sir Joseph Banks, was received with all the courtesy and just feeling which distinguished that most honourable man ; and it was settled that both the letter to Dr. Priestley of 26th April 1783, and that to Mr. De Luc of 26th November 1783, should be successively read. The former was done on the 22d, and the latter on the 29th April 1784.—[NOTE BY MR. JAMES WATT.]

In Mr. Cavendish's paper as at first read, no allusion is to be found to Mr. Watt's theory. But in an addition made also in Sir C. Blagden's hand, after Mr. Watt's paper had been read, there is a reference to that theory, (*Phil. Trans.* 1784, p. 140,) and Mr. Cavendish's reasons are given for not encumbering his theory with that part of Mr. Watt's which regards the evolution of latent heat. It is thus left somewhat doubtful, whether Mr. Cavendish had ever seen the letter of April 1783, or whether he had seen only the paper (of 26th November 1783) of which that letter formed a part, and which was read 29th April 1784. That the first letter was for some time (two months, as appears from the papers of Mr. Watt,) in the hands of Sir Joseph Banks, and other members of the Society, during the preceding spring, is certain, from the statements in the note to p. 330 ; and that Sir Charles Blagden, the Secretary, should not have seen it, seems impossible ; for Sir Joseph Banks must have delivered it to him at the time when it was intended to be read at one of the Society's meetings, (*Phil. Trans.* p. 330, Note,) and, as the letter itself remains among the Society's Records, in the same volume with the paper into which the greater part of it was introduced, it must have been in the custody of Sir C. Blagden. It is equally difficult to suppose, that the person who wrote the remarkable passage already referred to, respecting Mr. Cavendish's conclusions having been communicated to Mr. Lavoisier in the summer of 1783, (that is, in June,) should not have mentioned to Mr. Cavendish that Mr. Watt had drawn the same

conclusion in the spring of 1783, (that is, in April at the latest.) For the conclusions are identical, with the single difference, that Mr. Cavendish calls dephlogisticated air, water deprived of its phlogiston, and Mr. Watt says, that water is composed of dephlogisticated air and phlogiston.

We may remark, there is the same uncertainty or vagueness introduced into Mr. Watt's theory, which we before observed in Mr. Cavendish's, by the use of the term Phlogiston, without exactly defining it.\* Mr. Cavendish leaves it uncertain, whether or not he meant by phlogiston simply inflammable air, and he inclines rather to call inflammable air, water united to phlogiston. Mr. Watt says expressly, even in his later paper, (of November 1783,) and in a passage not to be found in the letter of April 1783, that he thinks that inflammable air contains a small quantity of water, and much elementary heat. It must be admitted that such expressions as these on the part of both of those great men, betoken a certain hesitation respecting the theory of the composition of water. If they had ever formed to themselves the idea, that water is a compound of the two gases deprived of their latent heat—that is, of the two gases—with the same distinctiveness which marks Mr. Lavoisier's

\* Mr. Watt, in a note to his paper of 26th November 1783, p. 331, observes, " previous to Dr. Priestley's making these experiments, " Mr. Kirwan had proved, by very ingenious deductions from other " facts, that inflammable air was, in all probability, the real phlogis- " ton in an aerial form. These arguments were perfectly convinc- " ing to me, but it seems proper to rest that part of the argument on " direct experiment."—[NOTE BY MR. JAMES WATT.]

statement of the theory, such obscurity and uncertainty would have been avoided.\*

Several further propositions may now be stated, as the result of the facts regarding Mr. Watt.

*First,* That there is no evidence of any person

\* Mr. Watt, in his letter of 26th April 1783, thus expresses his theory and conclusions, (Phil. Trans. p. 333 :)—“ Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These two kinds of air unite with violence, they become red hot, and, upon cooling, totally disappear. When the vessel is cooled, a quantity of water is found in it, equal to the weight of the air employed. This water is then the only remaining product of the process, and *water, light, and heat, are all the products;*” (unless, he adds in the paper of November, there be some other matter set free, which escapes our senses.) “ Are we not then authorized to conclude, that *water is composed of dephlogisticated air and phlogiston, deprived of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston, and united to elementary heat and light; that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston, and united to elementary heat?*”

Is not this as clear, precise, and intelligible, as the conclusions of Mr. Lavoisier?—[NOTE BY MR. JAMES WATT.]

The obscurity with which Lord Brougham charges the theoretical conceptions of Watt and Cavendish, does not appear to me well-founded. In 1784, the preparation of two permanent and very dissimilar gases was known. Some called these gases, pure air, and inflammable air; others, dephlogisticated air and phlogiston; and lastly, others, oxygen and hydrogen. By combining dephlogisticated air and phlogiston, water was produced equal in weight to that of the two gases. Water thenceforward was no longer a simple body, but a compound of dephlogisticated air and of phlogiston. The chemist who drew that conclusion might have erroneous ideas as to the intimate nature of phlogiston, without that throwing any uncertainty upon the

having reduced the theory of composition to writing, in a shape which now remains, so early as Mr. Watt.

*Secondly,* That he states the theory, both in April and November 1783, in language somewhat more distinctly referring to composition, than Mr. Cavendish does in 1784, and that his reference to the evolution of latent heat renders it more distinct than Mr. Cavendish's.

*Thirdly,* That there is no proof, nor even any assertion, of Mr. Cavendish's theory (what Sir C. Blagden calls his conclusion) having been communicated

merit of his first discovery. Even at this day, have we *mathematically demonstrated* that hydrogen (or phlogiston) is an elementary body; or that it is not, as Watt and Cavendish supposed at the time, the combination of a radical and of a little water?—[NOTE BY M. ARAGO.]

It should be borne in mind that the new chemical nomenclature was not proposed to the Academy of Sciences by the Messrs. De Morveau, Lavoisier, Berthollet, and de Fourcroy, until 1787, accompanied by introductory memoirs by M. Lavoisier and M. De Morveau.

Lavoisier himself had suggested the use of the term *acidifying principle, or oxygen*, in 1778, for the basis of pure or dephlogisticated air; and he used it in subsequent memoirs in 1780 and 1782; but it was not until the decomposition of water was discovered in 1783 and 1784, that he fully adopted it. Berthollet, perhaps the most philosophical chemist of France, did not become a convert to this nomenclature until 1785, nor did De Morveau and Fourcroy, according to the statement of the latter, fully enter into it until the end of 1786. As far as we recollect, it was first legitimated, if we may use the expression, in Lavoisier's System of Chemistry in 1789. It is surely, then, wrong to expect that Mr. Watt, in expounding his theory in 1783, should use a phraseology not generally sanctioned in France until four years later, not admitted by Black, Priestley, Kirwan, and other great English chemists, until a still more recent period, and by some of them never recognised at all.—[NOTE BY MR. JAMES WATT.]

to Dr. Priestley before Mr. Watt stated his theory in 1783, still less of Mr. Watt having heard of it, while his whole letter shows that he never had been aware of it, either from Dr. Priestley, or from any other quarter.

*Fourthly,* That Mr. Watt's theory was well known among the members of the Society, some months before Mr. Cavendish's statement appears to have been reduced into writing, and eight months before it was presented to the Society. We may, indeed, go farther, and affirm, as another deduction from the facts and dates, that, as far as the evidence goes, there is proof of Mr. Watt having first drawn the conclusion, at least that no proof exists of any one having drawn it so early as he is proved to have done.

*Lastly,* That a reluctance to give up the doctrine of phlogiston, a kind of timidity on the score of that long-established and deeply-rooted opinion, prevented both Mr. Watt and Mr. Cavendish from doing full justice to their own theory; while Mr. Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection and consistency.\*

All three may have made the important step nearly

\* It could scarcely be expected that Mr. Watt, writing and publishing for the first time, amid the distractions of a large manufacturing concern, and of extensive commercial affairs, could compete with the eloquent and practised pen of so great a writer as Lavoisier; but it seems to me, who am certainly no impartial judge, that the summing-up of his theory, (p. 333 of his paper,) here quoted, p. 252, is equally luminous and well expressed as are the conclusions of the illustrious French chemist.—[NOTE BY MR. JAMES WATT.]

at the same time, and unknown to each other ; the step, namely, of concluding from the experiment, that the two gases entered into combination, and that water was the result ; for this, with more or less of distinctness, is the inference which all three drew.

But there is the statement of Sir Charles Blagden, to show that Mr. Lavoisier had heard of Mr. Cavendish's drawing this inference before his (Mr. Lavoisier's) capital experiment was made ;\* and it appears that Mr. Lavoisier, after Sir C. Blagden's statement had been embodied in Mr. Cavendish's paper and made public, never gave any contradiction to it in any of his subsequent memoirs which are to be found in the *Mémoires de l'Académie*, though his own account of that experiment, and of what then passed, is inconsistent with Sir Charles Blagden's statement.†

But there is not any assertion at all, even from Sir C. Blagden, zealous for Mr. Cavendish's priority as he was, that Mr. Watt had ever heard of Mr. Cavendish's theory before he formed his own.

Whether or not Mr. Cavendish had heard of Mr. Watt's theory previous to drawing his conclusions, appears more doubtful. The supposition that he had

\* In the letter which Sir Charles Blagden addressed to Professor Crell, and which appeared in Crell's *Annalen* for 1786, professing to give a detailed history of the discovery, he says expressly, that he had communicated to Lavoisier the conclusions both of Cavendish and Watt. This last name appears in that letter for the first time in the recital of the verbal communications of the Secretary of the Royal Society, and is never mentioned by Lavoisier.—[NOTE BY MR. JAMES WATT.]

† Could Blagden's letter to Crell also have escaped Lavoisier's notice ?—[NOTE BY MR. JAMES WATT.]

so heard, rests on the improbability of his (Sir Charles Blagden's,) and many others knowing what Mr. Watt had done, and not communicating it to Mr. Cavendish, and on the omission of any assertion in Mr. Cavendish's paper, even in the part written by Sir C. Blagden with the view of claiming priority as against Mr. Lavoisier, that Mr. Cavendish had drawn his conclusion before April 1783, although in one of the additions to that paper, reference is made to Mr. Watt's theory.

As great obscurity hangs over the material question at what time Mr. Cavendish first drew the conclusion from his experiment, it may be as well to examine what that great man's habit was in communicating his discoveries to the Royal Society.

A Committee of the Royal Society, with Mr. Gilpin the clerk, made a series of experiments on the formation of nitrous acid, under Mr. Cavendish's direction, and to satisfy those who had doubted his theory of its composition, first given accidentally in the paper of January 1784, and afterwards more fully in another paper, June 1785. Those experiments occupied from the 6th December 1787, to 19th March 1788, and Mr. Cavendish's paper upon them was read 17th April 1788. It was, therefore, written and printed within a month of the experiments being concluded.

Mr. Kirwan answered Mr. Cavendish's paper (of 15th January 1784) on water, in one which was read 5th February 1784, and Mr. Cavendish replied in a paper read 4th March 1784.

Mr. Cavendish's experiments on the density of the

earth, were made from the 5th August 1797, to the 27th May 1798. The paper upon that subject was read 27th June 1798.

The account of the eudiometer was communicated at apparently a greater interval ; at least the only time mentioned in the account of the experiments is the latter half of 1781, and the paper was read January 1783. It is, however, probable from the nature of the subject, that he made further trials during the year 1782.

That Mr. Watt formed his theory during the few months or weeks immediately preceding April 1783, seems probable.\* It is certain that he considered the theory as his own, and makes no reference to any previous communication from any one upon the subject, nor of having ever heard of Mr. Cavendish drawing the same conclusion.

The improbability must also be admitted to be extreme, of Sir Charles Blagden ever having heard of Mr. Cavendish's theory prior to the date of Mr. Watt's letter, and not mentioning that circumstance in the insertion which he made in Mr. Cavendish's paper.

It deserves to be farther mentioned, that Mr. Watt left the correction of the press, and every thing relating to the publishing of his paper, to Sir Charles Blagden. A letter remains from him, to that effect,

\* That the idea existed in his mind previously, is proved by his declarations to Dr. Priestley, cited by the latter ; by his own assertions, p. 335 of his paper; and by the existing copies of his letters in December 1782.—[NOTE BY MR. JAMES WATT.]

written to Sir Charles Blagden, and Mr. Watt never saw the paper until it was printed.\*

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Since M. Arago's learned Eloge was published, with this paper as an Appendix, the Rev. W. Vernon Harcourt has entered into controversy with us both, or, I should rather say, with M. Arago, for he has kindly spared me ; and while I acknowledge my obligations for this courtesy of my reverend, learned, and valued friend, I must express my unqualified admiration of his boldness in singling out for his antagonist my illustrious colleague, rather than the far weaker combatant against whom he might so much more safely have done battle. Whatever might have been his fate had he taken the more prudent course, I must fairly say, (even without waiting until my fellow champion seal our adversary's doom,) that I have seldom seen any two parties more unequally matched, or any disputation in which the victory was so complete. The attack on M. Arago might have passed well enough at a popular meeting at Birmingham, before which it was spoken ; but as a scientific inquirer, it would be a flattery running the risk of seeming ironical to weigh the reverend author against the most eminent philosopher of the day, although upon a question of evidence, (which this really is, as

\* The notes of Mr. James Watt formed part of the manuscript transmitted to me by Lord Brougham ; and it is at the express desire of my illustrious fellow-member, that I have printed them, as a useful commentary upon his essay.—[NOTE BY M. ARAOO.]

well as a scientific discussion,) I might be content to succumb before him. As a strange notion, however, seems to pervade this paper, that everything depends on the character of Mr. Cavendish, it may be as well to repeat the disclaimer already very distinctly made of all intention to cast the slightest doubt upon that great man's perfect good faith in the whole affair ; I never having supposed that he borrowed from Mr. Watt, though M. Arago, Professor Robison,\* and Sir H. Davy, as well as myself, have always been convinced that Mr. Watt had, unknown to him, anticipated his great discovery. It is also said by Mr. Harcourt that the late Dr. Henry having examined Mr. Watt's manuscripts, decided against his priority. I have Dr. H.'s letter before me of June 1820, stating most clearly, most fully, and most directly, the reverse, and deciding in Mr. Watt's favour. I must add, having read the full publication with fac-similes, Mr. Harcourt has now clearly proved one thing, and it is really of some importance. He has made it appear that in all Mr. Cavendish's diaries and notes of his experiments, not an intimation occurs of the composition of water having been inferred by him from those experiments earlier than Mr. Watt's paper of spring 1783.

\* Encyc. Brit., vol. xviii. p. 808. This able and learned article enters at length into the proofs of Mr. Watt's claims, and it was published in 1797, thirteen years before Mr. Cavendish's death.

## No. IX.

EXTRACT FROM THE COMPTES RENDUS HEBDOMADAIRES  
DES SEANCES DE L'ACADEMIE DES SCIENCES.\*

M. ARAGO.—Sur la découverte de la composition de l'eau ; remarques à l'occasion d'une traduction Anglaise de l'éloge historique de feu *M. James Watt*.

M. DUMAS.—Sur les droits de *Watt* à la découverte de la composition de l'eau.

HISTOIRE DE LA CHIMIE.—En présentant à l'Académie, de la part de M. Muirhead, une traduction Anglaise de son *Eloge Historique de Watt*, M. Arago a pensé que, sans préjudice d'une réfutation plus étendue, il ne pouvait pas, vu la circonstance, s'empêcher d'opposer verbalement quelques remarques au discours que prononça l'année dernière, à Birmingham, le fils de l'archevêque d'York, le révérend Vernon Harcourt, président de l'Association britannique. M. Arago examinera en temps et lieu ce qu'il y avait d'insolite, de tronqué, d'inexact dans le langage de M. Harcourt. Devant l'Académie il se contentera de relever les deux principales objections du chanoine d'York.

En écrivant l'histoire de la découverte de la com-

\* 20 Janvier 1840, pp. 109-111.

position de l'eau, M. Arago avait attribué à Priestley cette observation capitale, portant la date du mois d'Avril 1783 ;—“ le poids de l'eau qui se dépose sur “ les parois d'un vase fermé, au moment de la détonation de l'oxygène et de l'hydrogène, est la somme “ des poids de ces deux gaz.” M. Harcourt déclare positivement que “ Priestley n'a jamais trouvé le poids “ de l'eau égal à la somme des poids des deux gaz.” A cette inconcevable assertion, M. Arago oppose textuellement le passage suivant du Mémoire que publia Priestley dans la 2<sup>e</sup> partie des Transactions Philosophiques de 1783 :—

“ In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering paper, and then having wiped with it all the inside of the glass vessel in which the air had been decomposed, weighed it again, and always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper.” (Trans. vol. 73, p. 427. Mémoire daté du 26 Juin 1783.)

La balance de Priestley, nous dit M. Harcourt, n'était pas suffisamment exacte. “ Ai-je donc prévu tendu,” dit M. Arago, “ que l'expérience du chimiste de Birmingham ne méritait pas d'être répétée ?”—“ Je trouvai toujours,” déclare Priestley, “ autant qu'il m'a été possible d'en juger, que le poids des airs combinés était égal à celui de l'humidité absorbée par le papier !” La pesée, plus parfaite, de Cavendish, ne saurait effacer ces paroles. M. Arago les a citées, et il aurait manqué à son devoir en les

laissant de côté. Quant aux incertitudes, ou même, si l'on veut, aux tergiversations qu'on trouve dans des travaux de Priestley postérieurs de *sept* années au Mémoire de 1783, "je n'avais pas à m'en occuper," remarque M. Arago. "En vérité quand j'écrivais "l'histoire d'une découverte dont la date la plus ré-  
"cente est l'année 1784, pouvais-je aller chercher les  
"titres des compétiteurs dans des Mémoires de 1786,  
"de 1788, etc. ? M. Harcourt, je suis peiné d'être  
"forcé de l'en avertir, a raisonné dans cette circon-  
"stance comme un de ses compatriotes, qui voulant  
"me prouver que Papin n'avait pas eu l'idée de la  
"machine à vapeur atmosphérique, au lieu de discuter  
"les passages clairs, catégoriques dont je m'étayais,  
"citait toujours une machine différente à laquelle le  
"physicien de Blois avait aussi songé beaucoup plus  
"tard !"

En traduisant un passage du Mémoire de Watt, M. Arago avait remplacé les mots *air déphlogistique* et *phlogistique* par les termes *oxygène* et *hydrogène* de la nomenclature moderne. Aux yeux de M. Harcourt c'est une faute impardonnable. M. Arago répond par un seul mot : le changement en question a été fait également dans les citations du Mémoire de Cavendish, car l'illustre chimiste se servait, lui aussi, de l'ancien langage. Il n'y a donc nul moyen de supposer que le changement tant critiqué, était suggéré à M. Arago par la pensée mesquine de favoriser Watt aux dépens de Cavendish. En tout cas, le passage suivant, tiré d'une note de M. Arago que M. Vernon Harcourt a dû lire, réduit la question à ses véritables termes :

“ En 1784, on savait préparer deux gaz permanents et très dissemblables. Ces deux gaz, les uns les appelaient air pur et air inflammable ; d'autres air déphlogistique et phlogistique ; d'autres, enfin, oxygène et hydrogène. Par la combinaison de l'air déphlogistique et du phlogistique, on engendra de l'eau ayant un poids égal à celui des deux gaz. L'eau, dès-lors, ne fut plus un corps simple : elle se composa d'air déphlogistique et de phlogistique. Le chimiste qui tira cette conséquence, pouvait avoir de fausses idées sur la nature intime du phlogistique, sans que cela jetât aucune incertitude sur le mérite de sa première découverte. Aujourd'hui même a-t-on mathématiquement démontré que l'hydrogène (ou le phlogistique) est un corps élémentaire ; qu'il n'est pas, comme Watt et Cavendish le crurent un moment, la combinaison d'un radical et d'un peu d'eau ?”

M. Arago n'a substitué le mot *hydrogène* au mot *phlogistique* que pour se rendre plus intelligible à ceux qui connaissent seulement la nomenclature chimique moderne. Afin de montrer, au surplus, qu'en écrivant l'éloge de Watt, il avait parfaitement le droit d'opérer cette substitution, M. Arago a mis sous les yeux de l'Académie une lettre *autographe* de Priestley à Lavoisier, en date du 10 Juillet 1782, une lettre antérieure aux Mémoires en discussion, et dans laquelle le célèbre chimiste de Birmingham s'exprime ainsi :—“ I gave Dr. Franklin an account of some experiments which I have made with *inflammable* air, which he probably [may] have shown you, that seem to prove that it is the same thing that has

" been called *phlogiston*." (" J'ai communiqué au Dr. Franklin la relation de quelques expériences que j'ai faites avec l'air inflammable, [l'hydrogène] dont il vous aura probablement donné connaissance, et qui paraissent prouver que cet air est la même chose que ce qu'on a appelé le phlogistique.")

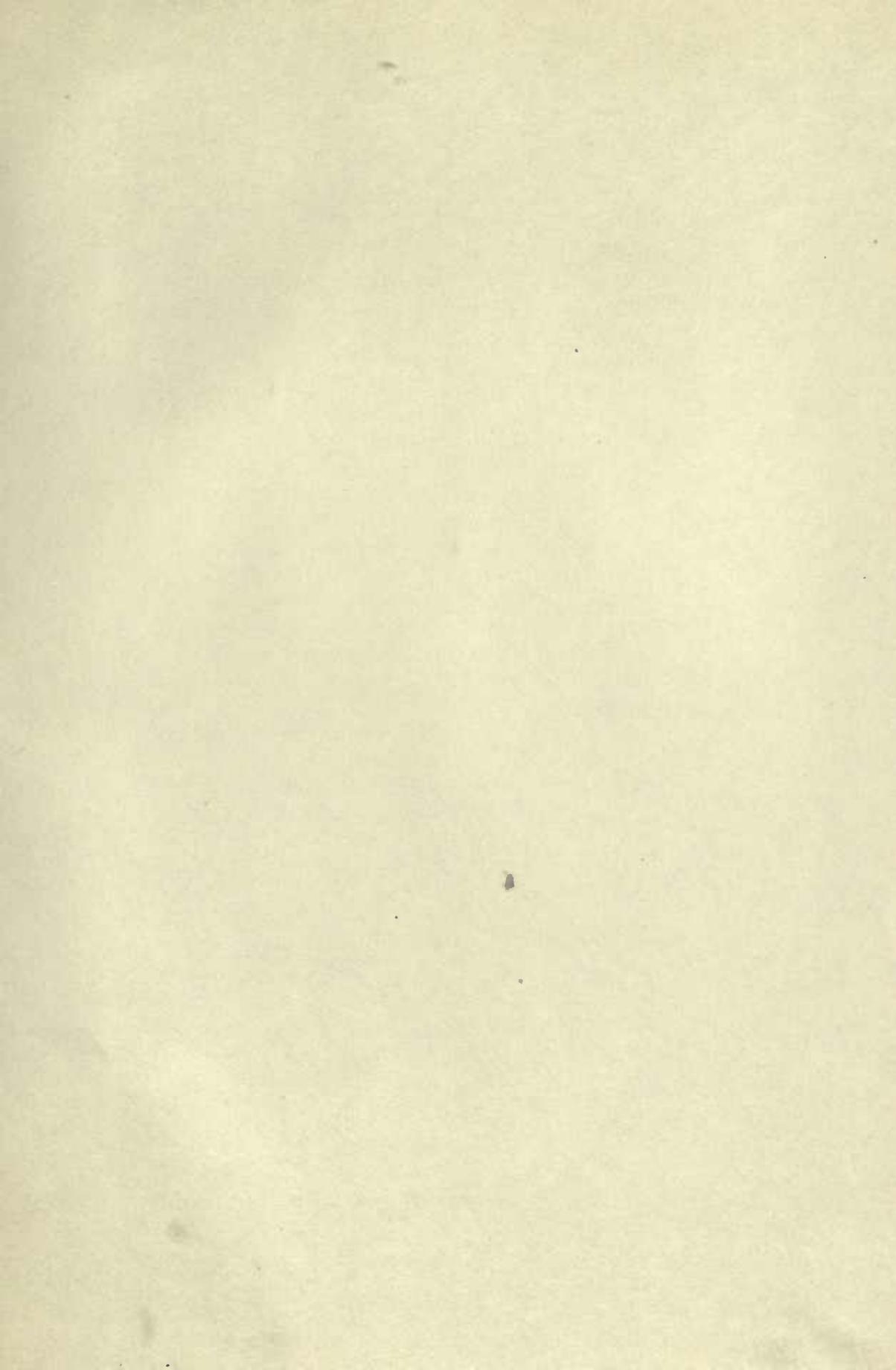
M. DUMAS ajoute à la communication verbale dont nous venons de rendre compte, qu'après avoir examiné attentivement l'argumentation de son confrère ; qu'après avoir fait aussi à Aston-Hall, près de Birmingham, chez M. Watt fils, une étude scrupuleuse de la correspondance de l'illustre ingénieur, il adopte complètement, et dans toutes ses parties, l'histoire que M. Arago a écrite de la découverte de la composition de l'eau. " Mes opinions sur ce point sont tellement arrêtées," dit M. Dumas, " que je désire voir ma déclaration consignée dans le *Compte Rendu* de cette séance."

*Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences*, 20 Janvier, 1840, p. 109, 111.

THE END.

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