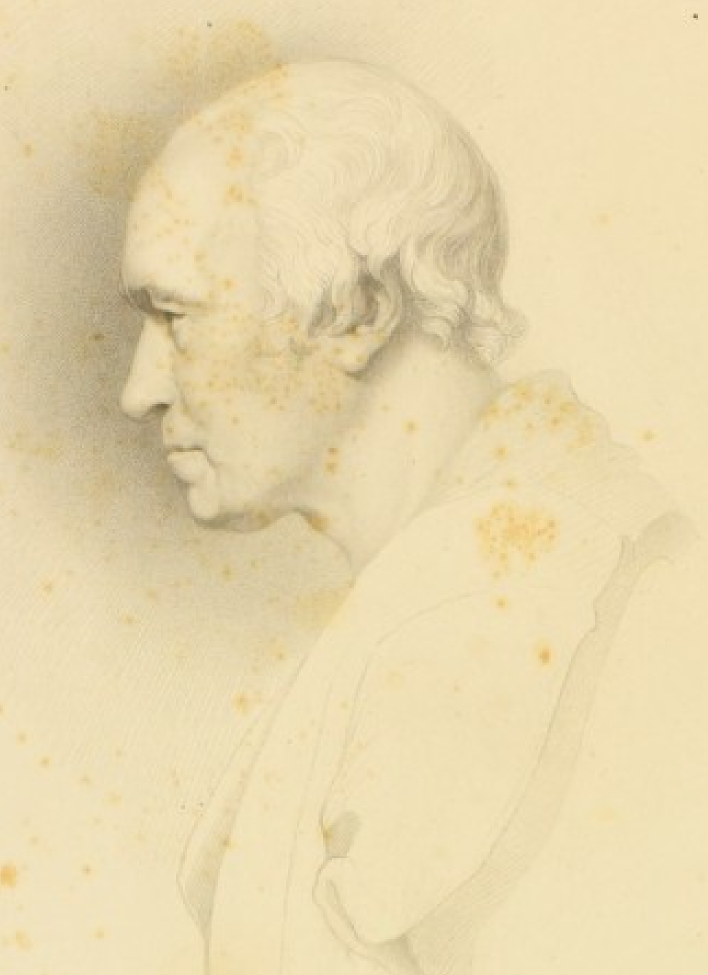
THE DISCOVERY

OF

THE COMPOSITION OF WATER.

EDINBURGH : PRINTED BY T. CONSTABLE, PRINTER TO HER MAJESTY.



E. Finden, sculpt.

James Watt

CORRESPONDENCE

OF THE LATE

JAMES WATT

ON HIS DISCOVERY OF THE THEORY OF

THE COMPOSITION OF WATER.

WITH A LETTER FROM HIS SON.

EDITED

WITH INTRODUCTORY REMARKS AND AN APPENDIX

BY JAMES PATRICK MUIRHEAD, ESQ.

F. R. S. E.

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LETTER

FROM

JAMES WATT, ESQ. TO THE EDITOR.

ASTON HALL, *February* 5, 1846.

MY DEAR SIR,

YOU have satisfied me that the time has now arrived for the publication of the documents in my possession, relative to my Father's discovery of the Theory of the Composition of Water.

After the testimony borne by M. Arago in his Eloge, and by Lord Brougham in his Historical Note appended to it, I deemed such publication not necessary, and certainly not urgent. My opinion was in no degree affected by the weak declamation of the Rev. W. Vernon Harcourt, at the meeting of the British Association at Birmingham in 1839, which, shortly afterwards, met with just exposure and rebuke in the Notes to your Translation of the Eloge,* (page 114), and was treated as it deserved by MM. Arago and Dumas, in the Memoirs of the Institute of France. The Diary of Mr. Cavendish, subsequently

* London. John Murray, 1839.

printed by the same Reverend gentleman, appeared to me too obviously inconclusive to call for any comment; although it has since received one from a far abler pen than mine.*

It had, however, always been my intention, when retirement from business and active pursuits should permit the requisite leisure, that such a publication should form an amusement and occupation of my later years; perhaps accompanied by another volume, containing the Specifications of my Father's various Mechanical Patents, which so materially contributed to the development of our national industry and resources; and also a volume of his Reports on subjects of Civil Engineering, which, though now obsolete, would add to the history of that important art, and mark the accuracy and talent of a young self-taught engineer, then fully estimated by his great precursor, Smeaton. Some of the infirmities of age have, however, come upon me more suddenly than I had taken into my account; and now render it difficult for me to peruse written or printed documents. I therefore, willingly and gratefully, resign to your friendly care the editing of my father's correspondence, the originals of which you have minutely examined. As a question of evidence, this falls peculiarly within the sphere of your pursuits, and I am satisfied it could not be placed in better hands.

* See Lord Brougham's Lives of Men of Letters and Science, vol. i. p. 400.

That correspondence commences about the close of the year 1782, and is continued throughout 1783, and part of 1784. Although I was at that period too young and uninformed to be able to appreciate the whole merit of his discovery, I well recollect his conversations with his philosophical friends, and the sentiments he expressed in regard to it. He early directed my attention, both at home and abroad, to Natural Philosophy and Chemistry; and on my return from the Continent in 1794, when he and Mr. Boulton took me into partnership, together with the late Mr. Robinson Boulton, and my late brother Gregory, I was tolerably versed in the facts and doctrines of the new system of chemistry, which the able writings and generalization of Lavoisier had caused to be commonly received. The old nomenclature was supplanted by the new, although Dr. Priestley, who had just retired to the United States, as well as Mr. Keir and some others, formed brilliant exceptions to the universality of its adoption. When the theory of the composition of water was spoken of in the presence of my father, he calmly but uniformly sustained his claim to its discovery; and once, on my hinting that it was passed over by some writers, and not correctly given by others, he observed, that having done all that he and his friends considered requisite to place it upon record, by the note affixed to his paper of 26th November 1783, in the Philosophical Transactions, the accuracy of which had never been questioned, *he should leave posterity to decide.* The important, though vague testimony of Dr., afterwards Sir Charles Blagden, was published in Crell's *Chemische Annalen* for 1786; consequently in the lifetime of Cavendish and Lavoisier; and was never contradicted, nor in any way impeached. When I met with it, I shewed it to my father; who, although he no longer felt any warm interest in the question, was amused by the skill of the narrator. After becoming in 1785 a Fellow of the Royal Society, he formed the personal acquaintance of Mr. Cavendish, and lived upon good terms with him. I well remember his introducing me, at one of the meetings of the Society, to that highly-gifted and singular man.

When upon my father's death, in August 1819, I became possessed of his papers, I found copies of all his letters taken by his copying machine, arranged in volumes, and carefully preserved; and the letters of his correspondents relating to this subject, tied up together, along with the press copies of his letters to Dr. Priestley of 26th April 1783, and to Mr. De Luc of 26th November 1783; and I was gratified to find that the documents he had left contained proofs so ample, satisfactory, and conclusive.

I then shewed the whole to my friend and neighbour, the late Mr. John Corrie, the President of the Philosophical Society of Birmingham, whose literary and scientific attainments are well known, and highly estimated; and who strongly expressed an opinion concurrent with my own. In the summer of 1820, having occasion to visit Scotland in the performance of some of my duties as my father's executor, I, on my passage through Manchester, consulted the late Dr. William Henry, whose knowledge of the history and practice of chemistry is undisputed; referring him to all the printed authorities, and acquainting him generally with the corroborative proofs in my father's correspondence. The former were sufficient to convince him, as appeared from a letter which I received from him at Edinburgh; where I had mentioned the subject to Dr. Hope, and Dr. (now Sir David) Brewster, whose opinions, however, differed from my own, and from those of Mr. Corrie and Dr. Henry.*

But a farther examination entirely confirmed my own conviction of my father's priority; and I was restrained from giving to the public at that time the whole of the documents now first printed, only by the constant avocations of the business of which I had then assumed the management, and by my own dislike to appear as an author.

In the year 1823, on being applied to by Mr. Macvey Napier, as editor of the Encyclopædia Britannica,

* Dr. Henry afterwards, in the years 1835 and 1836, called upon me and inspected the original correspondence, which had the natural effect of strengthening the opinion he had formed and expressed in 1820; and upon the latter occasion he mentioned his intention of writing a history of Chemistry, in which he said he should do justice to my father's claims to the priority.

for a short life of my father, in the Supplement then publishing, I inserted in the memoir the following statement, which, from the whole of the facts, since ascertained, not having been known to me, was necessarily somewhat imperfect.

"Chemical studies engaged much of his attention "during his busiest time; and at the very period "when he was most engaged in perfecting his rotative "engines, and in managing a business become con-"siderable, and, from its novelty, requiring close at-"tention, he entered deeply into the investigations "then in progress relative to the constitution and "properties of the different gases. Early in 1783, "he was led, by the experiments of his friend and "neighbour, Dr. Priestley, to the important conclu-"sion, that water is a compound of dephlogisticated "and inflammable airs (as they were then called,) "deprived of their latent or elementary heat, and "he was the first to make known this theory. This "was done in a letter to Dr. Priestley, dated the 26th "April 1783, in which he states the Doctor's ex-"periments to have come in aid of some prior notions "of his own, and supports his conclusions by original "experiments. That letter Dr. Priestley received in "London, and, after shewing it to several members "of the Royal Society, he delivered it to Sir Joseph "Banks, with a request that it might be read at some "of the public meetings of the Society; but before "that could be complied with, Mr. Watt. having heard "of some new experiments made by Dr. Priestley, "begged that the reading might be delayed. Those "new experiments soon afterwards proved to have "been delusive; and Mr. Watt sent a revised edition "of his letter to Mr. De Luc, on the 26th November "of the same year, which was not read to the Society "until the 29th April 1784, and appears in the Phi-"losophical Transactions for that year, under the title "of 'Thoughts on the Constituent Parts of Water "'and of Dephlogisticated Air, with an Account of "'some Experiments on that Subject.' In the in-"terim, on the 15th January 1784, a paper by Mr. "Cavendish had been read, containing his 'Experi-"'ments on the Combustion of the Dephlogisticated "'and Inflammable Airs,' and drawing the same in-"ference as Mr. Watt, with this difference only, that "he did not admit elementary heat into his expla-"nation. He refers in it to his knowledge of Mr. "Watt's paper, and states his own experiments to "have been made in 1781, and mentioned to Dr. "Priestley; but he does not say at what period he "formed his conclusions: he only mentions that a "friend of his had, in the summer of 1783, given M. "Lavoisier some account of his experiments, as well "as of the conclusion drawn from them. It is quite "certain that Mr. Watt had never heard of them; "and Dr. Blagden has stated, that he mentioned at "Paris the opinions of both the English philosophers, "which were not admitted without hesitation, nor

" until the French chemists had satisfied themselves " by experiments of their own."

To this was appended a note to the following effect :—" There is a confusion of dates in the ac- " counts of this affair. Mr. Watt's letter to Mr. De " Luc, in the Philosophical Transactions, appears " dated 26th November 1784, which is evidently an " error of the press. Mr. Cavendish, in his letter, " read 15th January 1784, speaks of Mr. Watt's " paper as 'lately read before the Society,' whereas " the paper itself purports to have been read on the " 29th April 1784. This we cannot explain."

What was then unintelligible to me has since been explained by Lord Brougham's discovery, that the passage citing my father's paper, had been interpolated by Dr. Blagden at a period subsequent to that at which Mr. Cavendish's paper was read. It cannot escape observation, that it is the only passage in that paper in which Mr. Watt's name is even mentioned. It is now, also, well known that another extraordinary error of the press was committed, in the numerous separate copies of his paper circulated by Mr. Cavendish, in which that paper was said to have been " read at the Royal Society, January 15, 1783 ;" it having been in fact read there January 15, 1784.

On the 18th of June 1824, a public meeting was held at Freemason's Hall, for the purpose of erecting a monument as a tribute of national gratitude to my father; at which many of the most distinguished statesmen of the day attended, and the Earl of Liverpool, who presided, announced that the King had graciously commanded him to put down his Majesty's name as heading the subscription. A Committee having been appointed, of which Mr. Charles Hampden Turner, the attached and zealous friend both of my father and myself, was chairman, the execution of the colossal statue, now erected in Westminster Abbey, was confided to the late Sir Francis Chantrey, and an inscription for it was written by Lord Brougham. In September of the same year, Sir Humphry Davy paid me a visit, and remained with me a few days. I then showed him the Life I had written for the Encyclopædia Britannica, of which the editor had sent me some detached copies. I directed his attention to what is there said on my father's claim to the discovery of the theory of the composition of water; but the facts stated appeared to be new to him, or, if known at all before, to have been forgotten, or not to have been considered. I mentioned my desire to do justice, and inquired if he knew of any papers left by Mr. Cavendish, from which the date of his *conclusions* might be ascertained; but he was ignorant of the existence of any such papers. I then laid before him the press copies of my father's letters, and the original ones of his correspondents, which he read over with much interest, and appeared exceedingly struck with their contents. He expressed concern at the effect which their publication must produce, (a concern not unnaturally proceeding from his known attachment to Mr. Cavendish,) and he did not then, or at our subsequent meeting in 1826, endeavour to lessen their force, or to call in question the deductions resulting from their perusal. In the last conversation I had with him here on the subject, he said he thought that my father's theory, admitting the latent heat, would prove correct.

Year after year of a life of business had passed away, without my finding leisure to resume the subject, when, in May 1833, I received notice from M. Arago of his having been directed, as Perpetual Secretary of the Academy of Sciences at Paris, to write an Eloge of my father, and he requested some details of his life. These were given ; and, in the autumn of 1834, M. Arago paid me a visit, in order to collect further materials, and to make himself acquainted with the scenes of my father's later life. He afterwards extended his journey to the earlier ones in Scotland.

Finding, upon conversing with M. Arago, that he had studied and made himself master of my father's improvements on the steam-engine, I inquired whether he had also paid attention to the origin of the theory of the composition of water. He answered in the affirmative, and said he had satisfied himself, by a perusal of the published documents, of my father's right to the priority. I then showed him the press copies of my father's letters, and the originals of those of his correspondents, which put the seal on his conviction, and he requested permission to make use of them in his intended memoir, urging that, in justice to my father's memory, and as a matter of history, I ought not to withhold them. In consequence, I arranged them in chronological order for his use, accompanied by such brief explanations and remarks as occurred to me.

His Eloge was read to the Institute on the 8th December 1834, and although some parts of the personal history were subsequently corrected and added to, the portion relative to the composition of water experienced no alteration.

In the summer of 1834, I called the attention of Lord Brougham, who was then Lord Chancellor, and had undertaken to write the inscription for the monument in Westminster Abbey, to the Memoirs in the Philosophical Transactions, and the papers I had collected and transcribed, with a request that he would examine them with the discrimination of a lawyer, and the impartiality of a judge. After having given them his attentive perusal, he suggested the propriety of an inquiry whether Mr. Cavendish had left any papers, as these might throw light on the precise period when his conclusions were formed. His Lordship wrote to the Duke of Devonshire, as representative of Mr. Cavendish, and received for reply, that all Mr. Cavendish's papers were in the hands of Mr. Hudson, who was arranging them for publication ; and His Grace most handsomely gave me permission to inspect them. I, however, felt it a matter of delicacy to become a witness in a cause, where I must, as the representative of my father, be considered a party; and I requested those two very competent and unexceptionable gentlemen, Mr. Charles Hatchett and Mr. W. T. Brande, the former of whom had been a friend both of Mr. Cavendish and of my father, to undertake the examination, which they both promised to do. Mr. Hatchett reported to me that he had found nothing whatever to indicate the period when Mr. Cavendish's conclusion was formed. Mr. Brande further carefully searched the books of the Royal Society, and expressed his opinion that the records which he there found were "satisfactory as to the "priority of Mr. Watt's claims; in short, leave no-"thing further to be said against them."

Lord Brougham also suggested an examination of the original papers preserved in the archives of the Royal Society, which he undertook himself: he then discovered the interpolations in the Memoir of Mr. Cavendish, in the hand-writing of Sir C. Blagden, with which, from frequent correspondence with him, he was himself familiar; and thus threw light on what was before unintelligible. At his Lordship's request I afterwards accompanied him to Somerset House, and saw the documents confirming his statement.

M. Arago's Eloge is published in the Memoirs of the Institute, and in the *Annuaire du Bureau des*

Longitudes for 1839, accompanied by the paper of Lord Brougham, with Notes, which I added, at his Lordship's request, and which he desired to be printed along with it. The reader will find all these in the Appendix to your translation.

To those who may wish to form a just appreciation of the circumstances in which this correspondence took place, and of the merit that attaches to my father for the discovery it records, I beg to state, in the words of the great master of the English tongue, that "it was written, not in the soft obscuri-"ties of retirement, or under the shelter of academick "bowers; but amidst inconvenience and distraction, "in sickness and in sorrow."

About the beginning of the year, when the correspondence commences, he had returned from planning and superintending the erection of his steam engines, during a long sojourn in Cornwall, where he had been much harassed by attempts to pirate his improvements; and he was, through the greater part of the subsequent period, laboriously engaged in making out drawings and descriptions for the long specifications of his three great patents for mechanical improvements and inventions, taken out in the years 1781, 1782, and 1784, besides giving the constant attention necessary to the concerns of a nascent manufactory, and himself writing volumes of other letters on business, which alone would have furnished full employment even to an industrious intellect.

His mind had been greatly affected by his unavoidable absence from the death-bed of his aged father; and during the greater part of the time, I well remember seeing him suffer under most acute sick headaches, sitting by the fire-side for hours together, with his head leaning on his elbow, and scarcely able to give utterance to his thoughts.* It was unquestionably the busiest, as well as the most anxious, period of his life, and fraught with the most important results. I need not attempt to do justice to them, for time has sanctioned the judgment of his contemporaries, who had done it already.

The principals and witnesses whose names appear in the correspondence have long departed this life. M. Lavoisier in 1794, Dr. Black in 1799, Dr. Priestley in 1804, Mr. Cavendish in 1810, Mr. Kirwan in 1812, Mr. De Luc in 1817, Mr. Watt in 1819, Dr. Blagden and Sir Joseph Banks in 1820. The historical facts must therefore now be sought for in the contemporary memoirs, published by themselves or others, and in the documents they have left. Inquiry was made of Dr. Priestley's son (since dead) as to his father's papers in 1783-4. He supposed them to have been burned at the time of the Birmingham riots in 1791, which was confirmed by a search he caused to be

* To show the state of his own feelings, there are inserted in the Correspondence extracts from his letters to his confidential friend and brother-in-law, Mr. Hamilton, of date 3d January and 18th February, 1783.

made in America. My father's letters and papers, and the letters of his friends, which, as already mentioned, have fortunately been preserved, are still in my possession, and all that seemed material are copied in this publication. They are authenticated beyond the reach of doubt, to which, as you have inspected and perused them all, and collated the originals with the copies furnished to M. Arago and Lord Brougham, you can now add your own testimony.

Should their publication produce any unpleasant sensation in the minds of the friends of Mr. Cavendish, they will, I trust, do me the justice to admit, that it has been neither hastily nor prematurely brought forward, and that it would now be a dereliction of duty not to produce evidence so creditable to my father, both as a philosopher and as a man. Let me also hope that the Rev. Mr. Harcourt, and other gentlemen who may be placed in a similar elevation, may thus receive a caution, how they abuse functions, the exercise of which is expected to combine talents for historical research with scientific attainments, and impartiality of judgment with competency of knowledge. Mr. Harcourt may plead that he had not seen this correspondence. I think it appears equally probable that he has little examined the published documents, from which Lord Brougham principally draws his conclusions, and which alone were sufficient in the first place to satisfy M. Arago.

These remarks are called for by the late desperate attempt in the Quarterly Review, a Journal generally most respectable, to gain for Mr. Harcourt a scientific reputation, (undeserved, so far as I know,) by fulsome panegyric, misrepresentation of facts, additional blunders, and reckless assertions. Such of these as concern M. Arago and Lord Brougham may safely be left to the retribution that awaits them. To those which concern myself I shall not condescend to reply otherwise than by the above narrative, and the annexed documents.

Having thus accomplished what I feel to be peculiarly incumbent on myself, I must now confide to you the superintendence and editorship of the publication, accompanied by such further narrative, remarks, and illustrations, as may appear to you to be necessary; and I entertain a full conviction that the publication, when completed, will form a permanent record of my father's merit in that great discovery, as well as place the claims of others in their just light.

Believe me,

My dear Sir,

Truly yours,

JAMES WATT.

INTRODUCTORY REMARKS

BY

THE EDITOR.

THE admiration which the discoveries and inventions of the late James Watt won from the greatest masters of intellectual power, could be surpassed only by the readiness with which men acknowledged his singular modesty, benevolence, and worth. From many, that welcome commendation came early in his career ; by others, it was bestowed when " time and " reflection had contributed to enhance their esti- " mate of Mr. Watt's extraordinary merits."* But by few indeed was it tardily offered—from none coldly or reluctantly extorted ;—and when his useful and blameless life came at last to a close, all deplored the loss of one of the greatest benefactors that had ever blessed his country and the world.

How large a tribute of national gratitude was due to genius and industry which had long been so laborious, and had at last become so triumphant, the greatest statesmen, philosophers, and orators of Britain

* Mr. C. H. Turner's Preface to the Report of the Speeches delivered at Freemasons' Hall, 18th June 1824. See Translation of Arago's Eloge, p. 183.

have proudly and eloquently told. The variety of their sentiments, the opposition of their politics, the diversity of their paths—all were disregarded in the endeavour to do honour to merits of which they showed themselves justly sensible.

The fame thus liberally accorded, must not be considered as having been gained solely by the combination of rare virtues, with those creative powers which first discovered, and then supplied, the capacity for improvement in the steam-engine. Mr. Watt's principal inventions connected with that machine, with all their prodigious results, were founded on the attentive observation of great philosophical truths; and the economy of fuel, increase of productive power, and saving of animal labour, which gradually ensued, all originated in the sagacious and careful thought with which he investigated the nature and properties of heat. Other very material improvements in the construction of the engine were effected by changes in the mode of communicating, directing, or regulating the force generated; and by the efforts of a mind prolific of mechanical expedients, and perfectly conversant with practical details, the double engine, the beautiful parallel motion, the crank, the sun and planet wheels, the application of the governor, the float, the indicator, the smokeless furnace, and many other ingenious devices, were no less successfully executed than they had been felicitously conceived.

But the surprising powers of Mr. Watt's intellect were not limited to one set of subjects, nor was he, in his course of invention, content to travel only by one path, however arduous or untrodden. He appeared to roam at large over every field of science and learning, exploring them all; and to be confined by nothing less expanded than the horizon of his own enlarged views. The systems, which he first brought into effective operation, of heating by steam, and bleaching by chlorine, are instances of his numerous contributions to the practical arts and comforts of his country; his extensive reading and acquaintance with languages, his accurate study of both the principles and practice of some of the more difficult parts of law, his knowledge, which "overflowed on all subjects,"* were such as to astonish the most gifted and energetic students of literature; while the press for copying letters, the machine for reducing and copying statuary, the musical instruments† which, though without a natural ear for music, he skilfully constructed—even his neat drawings and faultless calligraphy—still exist, to prove how fertile in resources, how universal in acquirements, how thoughtful even in its amusements, was his patient and industrious mind.

The department of physical science with which, next to mechanics, he may be said to have been at one time most familiar, and which long continued in some measure to occupy his leisure hours, was Chemistry. With what success he studied it, we know from the testimony of the most eminent among his contemporaries who directed their attention especially

* Sir Walter Scott, in his Preface to "the Monastery."

† Of these, we know of at least four kinds; an organ, æolian harp, guitar, and flute. The organ was constructed by Mr. Watt for his friend, Dr. Black, and presented to him.

to that subject, and many of whom were his frequent correspondents. " He was equally distinguished," said the late illustrious President of the Royal Society, Sir Humphry Davy, "as a natural philosopher and a " chemist, and his inventions demonstrate his *pro-* " *found knowledge* of those sciences."* The numerous experiments which he made with a view to the attainment of the great principles of which he was in search, are further commended by the same accomplished and able judge, as difficult, delicate, and refined.

It is stated in the Memoirs of his friend and neighbour, the celebrated botanist Dr. Withering, that " in " his estimation, Mr. Watt's abilities and acquire- " ments placed him next, if not superior, to New- " ton ;"† a judgment dictated, no doubt, by the kind partiality of a friend, but shewing the estimation in which Mr. Watt's talents were held by an able and discerning man of science. How intently he watched the phenomena, how deeply he penetrated into the causes of chemical action, might be conceived from his friend Robison's description of him as " a philoso- " pher in the most exalted sense of the word, who " never could be satisfied with a conjectural know- " ledge of any subject, and who grudged no labour nor " study to acquire certainty in his researches."‡ The highest merit certainly attaches to his chemical discoveries, and deep interest must be felt by all who

* Speech in 1824.—Translation of Arago's Eloge, p. 191.

† Tracts and Memoir of Dr. Withering, by his Son, 1822. Vol. i. p. 46.

‡ Preface to Black's Lectures.

attend to the history of their origin and progress, from the fact that he was in this, as in almost every other part of learning, self-taught. He has himself, on one of the very few occasions on which he ever made public any of his writings through the medium of the press, (almost all the others being only communications to the Royal Society, which were ordered to be printed,) taken pains to correct the statements of Professor Robison on this point. That gentleman, in dedicating to him his edition of Dr. Black's Lectures, called him Dr. Black's pupil, declared that he had attended two courses of his lectures, and even alluded to his professing to owe his improvements on the steam-engine to the instructions he had received from that eminent teacher. This, however, is altogether erroneous; and Mr. Watt has lamented* that the necessary avocations of his business at that time prevented his attending either Dr. Black's or any other lectures. But he repeatedly acknowledged the information and pleasure he derived from the conversation of that enlightened philosopher, as well as from the friendship of such men as Robert Simson and Dr. Dick, both distinguished cultivators of kindred branches of natural knowledge.

It was not till 1774 that he left his residence in Glasgow, the scene of his early studies and struggles, where his merits had been recognised and fostered by patrons of deserved eminence and the most kindly feelings, and where he had first conceived those felici-

* See his Preface to his edition of Dr. Robison's Articles, Steam and Steam-Engine.

tous ideas which afterwards became so honourably and inseparably associated with his name. In establishing himself at Soho,* he retained his habits of intimate correspondence with Dr. Black, who had then, for more than twenty years, made known his discovery of carbonic acid gas, and for at least sixteen had annually explained his theory of latent heat in his lectures, in which, also, for the first time, he developed the doctrine of the capacities of bodies for heat, (or that of specific heat ;) and who, after spending ten years of academical labour in the University of Glasgow, had, in 1766, accepted that chair in Edinburgh, which for thirty years longer he continued to render famous.†

In a work, the object of which is to cause justice to be done to Mr. Watt's claims to a great chemical discovery, we have much pleasure in being able, on indisputable authority, to attribute the public announcement of his illustrious friend's theory of latent heat to a period considerably earlier than has been

* The celebrated manufactory situated within a mile or two of Birmingham.

† Dr. Ferguson, as quoted by Robison in his Preface to the Lectures, and repeated, among many others, by Lord Brougham, says that Dr. Black died on the 26th November 1799. But we have now before us Dr. Black's last letter to Mr. Watt, which was written on the 2d December of that year ; which is indorsed by Mr. Watt, " *his last* " *letter*," and in which he mentions that he had been slightly unwell, but was then better. In fact, on the 11th December, Professor Robison wrote to Mr. Watt, that his much respected friend had died on the Friday preceding, viz. the 6th December. Ferguson also says, that he died in the seventy-first year of his age ; but he really died in his seventy-second year, for in a letter to Mr. Watt of 8th April 1798, he writes " I have now finished my seventieth year."

named, even by Dr. Black's zealous admirer and pupil, Lord Brougham. His Lordship says that Dr. Black meditated on that theory, investigated it by experiment, and taught it in his lectures, at least as early as 1763. But the following extract from his letter to Mr. Watt, of 15th May 1780, furnishes information more precise, and which, as assigning with certainty a much earlier date to so admirable a discovery, cannot fail to interest the scientific world. " I began," says the Doctor, " to give the doctrine of latent heat " in my lectures at Glasgow, in the winter 1757-58, " which I believe was the first winter of my lecturing " there, or, if I did not give it that winter, I certainly " gave it in the 1758-59, and I have delivered it " every year since that time in my winter lectures, " which I continued to give at Glasgow until winter " 1766-67, when I began to lecture in Edinburgh."

In the same letter he mentions by name many distinguished foreigners, as well as natives of this country, who had attended some of the earliest courses of his lectures, and had then heard his explanations of that remarkable theory; adding, that about 1760-61, or soon after, he read a paper on the subject, in the Philosophical or University Club at Glasgow, and thus concluding :—" I could bring a " multitude of other evidences to prove the early date " of my doctrines on this subject." We need hardly observe, that none who are duly aware of the modesty and carelessness of fame, the scrupulous veracity, and exact observation of facts, which distinguished that truly learned and excellent person, can imagine any other kind of evidence more convincing than his own testimony. After the publication of so decisive a record, further exposure of the attempts which have of late been made to rob Dr. Black of his great and well-earned glory is wholly superfluous.*

Priestley, who in the year 1774, had effected by far the most remarkable and brilliant of his numerous discoveries, (that, viz. of oxygen gas,) came in 1780 to Birmingham; where he afterwards usually resided, till driven away from that place in 1791, by the violence of a riotous mob, under the influence of religious and political exasperation. During the whole of his stay in that neighbourhood, which has been well described as at that period "a region of rare "talents," he was on terms of habitual and friendly intercourse with Mr. Watt, frequently conversing with him on those scientific subjects which were of the greatest interest to them both; and we find him publicly acknowledging the pleasure he derived from such congenial society.†

It is impossible to conceive a more complete contrast than was presented by the mode of philosophising adopted by Black and Priestley respectively. The one, calm and reflective, conducted his experiments often with such simple apparatus as came

* Preposterous pretensions have also been, by insinuation, set up for Cavendish to the discovery of the same theory; pretensions which are quite unfounded. See p. 30 of the Birmingham Address of the Rev. W. V. Harcourt; in whom Mr. Cavendish has certainly found a most injudicious defender. We can duly respect Mr. Cavendish's fame, and praise his chemical skill; but we cannot undertake to save him from his friends, nor to approve of their indiscriminate and unreasonable eulogies.

† Philosophical Transactions, 1783, p. 416.

readiest to his hand, but always with studied neatness, accuracy, and success; carefully watching every step of the well-considered process, and deducing, with all the force of exact demonstration, either the overthrow of some long-settled belief, or the description of a new substance, or the establishment on solid foundations of a theory altogether unsuspected by any other inquirer; his conclusions being as much distinguished for their originality, beauty, and usefulness, as any thing to be found in the whole history of inductive research. The other, with warm zeal and untiring perseverance, but with little idea of order, and an imperfect acquaintance with the true first principles of science, contrived experiments of infinite number and variety, observed them with lively interest, and often with a just perception; and minutely recorded the smallest particulars, which in their progress he noticed, if not always for his own advantage, yet certainly for the great benefit of others. But to the higher objects of philosophical inquiry and generalisation, he was little accustomed to apply the many great and luminous truths which he was the first to make known; and in more than one instance he even plunged deep into error, which some of his contemporaries, neither better informed on other points, nor gifted with superior powers of observation, were able to avoid. It is curious to find his well-known candour thus expressing his own views of the manner in which scientific research ought to be conducted, at a period nearly twenty years after he had received the Copley medal for his inquiries into several kinds of air, and had, almost at the same time, completed his grand and undisputed discovery of oxygen gas :—

"I do not think it at all degrading to the business "of experimental philosophy, to compare it, as I often "do, to the diversion of *hunting*, where it sometimes "happens that those who have beat the ground the "most, and are consequently the best acquainted with "it, weary themselves without starting any game, "when it may fall in the way of a mere passenger; so "that there is but little room for boasting in the most "successful termination of the chase."* His metaphor reminds us of the jocose observation, said to have been addressed by Sir Isaac Newton to Dr. Barrow, who complained that he had occupied all the ground of new discovery :—"Beat the bushes: there is still "plenty of game to be raised."† But the proceedings of the other two great experimental inquirers whom we have named, were nothing like this; and we may perhaps question the propriety of applying language which conveys the idea of something vague and even fortuitous, to that system which Bacon first illustriously taught, and which Black and Watt so worthily exemplified; by which the present age has been guided to very many of the more remote and occult parts of nature, with the same certainty and safety, with which the compass has directed the course of navigation to the discovery of new regions of the globe.

It cannot, however, be said that Priestley either derived small amusement from his quest of the game

* See the Preface to his Abridgement of the "Experiments on "Air," in three vols. 1790, p. 21.

† Works of Sir Humphry Davy, edited by his Brother, vol. vii. p. 124.

to which he alludes, or failed of brilliant success in that exciting chase, which he followed with enthusiastic ardour. It is equally true that he greatly contributed to its popularity with others. But, though he could not fairly be called uncertain in his aim, he occasionally abandoned the main pursuit to follow some deceptive appearance in another track; and had often to submit, which he always did with perfect frankness and good-nature, to see his competitors triumph where he himself had failed. No more apposite or memorable instance of the truth of these remarks could be found, than in the discovery of which we are about to recount the history; where he stedfastly opposed a theory which was in great measure founded on one of his own experiments, but in which, even after it had received the most ample confirmation from the results of further inquiry, and had been adopted by nearly all the most eminent chemists of the day, he never could be induced to believe.*

Before proceeding to the history, as it appears in the following correspondence, of the manner in which Mr. Watt was more immediately led to form and state in writing, his conclusions respecting the composition of water, which had previously always been looked upon as an *element* or simple substance, it is proper that we should shortly relate the steps which had been taken, before the year 1783, towards a more accurate knowledge of its real nature. If this must of necessity lead us to recapitulate some of the informa-

* Among the latest of his publications was "The Doctrine of "Phlogiston Established, and that of the Decomposition of Water "Refuted." Northumberland, 1800.

tion, which has already been laid before the public by the learned labours of M. Arago and Lord Brougham, we shall at least gain the advantage of being able to present at one view, and with brevity, several particulars which have been hitherto a good deal dispersed, and are on that account not easy of reference.

The first observation of the moisture which is formed when inflammable air or hydrogen gas is burnt in common air, was made by M. Macquer, an excellent French physician and chemist, whose good sense and judicious experiments rendered great service to science, at a time when few minds had as yet shaken off any of the fetters of the old philosophy. In that edition of his *Dictionnaire de Chimie* which was published in 1778, and of which his translator, Mr. Keir, says, that it had been much esteemed, and had perhaps contributed more to the diffusion of chemical knowledge than any other book, (and which, as well as its author, was always spoken of by Dr. Black with the greatest respect,) he details, under the article Inflammable Gas, many experiments on its combustion, which were made in 1776-7, and in which he was assisted by M. Sigaud de Lafond. "I assured "myself also," he says, "by placing a saucer of white "porcelain in the flame of inflammable gas burning "tranquilly at the orifice of a bottle, that the flame is "not accompanied by any fuliginous smoke; for that "part of the saucer which the flame licked, remained "perfectly white; it was only moistened by small "drops of a liquor as clear as water, and which, in fact, "appeared to us to be only pure water."* The pheno-

* Dictionnaire de Chymie, tom. ii., p. 314; ed. Neuchatel, 1789.

menon was certainly a remarkable one, and its observation appears now, as it did to Lavoisier in 1783,* to have nearly approximated to a most interesting inquiry, which might, indeed, have ended in the discovery afterwards so famous. But Macquer drew no conclusion from it, takes no further notice of it, and seems not even to have hazarded a speculation on its cause.

He also mentions the combustion of mixtures both of inflammable gas and common air, and of inflammable gas and dephlogisticated air or oxygen gas; and describes the explosion by which it was in both cases attended; that being, however, very much more violent in the latter case than in the former. He seems to have fired the airs in glass vessels, but although on one occasion he speaks of having done so in close vessels, it is evident from his further account of the experiment, that the vessel employed had a narrow aperture, to which a lighted match was applied.

Volta, in a letter dated 10th December 1776, which is printed in Dr. Priestley's third volume,† says, that he then fired inflammable air by the simple electric spark.

The next considerable step in the progress towards the grand discovery, was made by an English chemist and philosophical lecturer, Mr. Warltire, whose mode of conducting his experiments on the combustion of gases was highly creditable to his ingenuity. He fired

* Lavoisier, Mémoires de l'Académie for 1781, printed in 1784, p. 469.

† Priestley's Experiments on Air, &c., 1781, vol. iii. p. 381.

a mixture of common and inflammable airs in a close metal flask or globe, by the electric spark ; and, his object being to ascertain " whether heat was heavy " or not," he says, " I always accurately balanced the " flask of common air, then found the difference of " weight after the inflammable air had been intro- " duced, that I might be certain I 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." Mr. Warltire adds, that in his experiments of this sort, he always found a small loss of weight, but not constantly the same ; the vessel held three wine pints, and weighed fourteen ounces, and the average loss which he thought he detected, was only two grains.

These experiments are detailed in a letter dated Birmingham, 18th April 1781, which was addressed to Dr. Priestley, and published by him in the appendix to the second volume of his " Experiments and " Observations relating to various branches of Natural " Philosophy ; with a continuation of the Observations " on Air ;" printed at Birmingham in 1781.* From the

* Mr. Warltire's letter is given by Dr. Priestley as follows :—

" *A letter from Mr. John Warltire, Lecturer in Natural Philosophy, on " the firing of inflammable air in close vessels.*

" BIRMINGHAM, 18*th April* 1781.

" SIR,—I had long entertained an opinion that it might be de- " termined whether heat is heavy or not, by firing inflammable air, " mixed with common air, and applying them to a nice balance ; but " as I conceived the danger of passing the electric spark through so " combustible a mixture in a close vessel to be greater than it is, I same letter it appears, that Priestley was the first to fire air in a close *glass* vessel, and to observe a deposit of water; but that Warltire, on repeating the same

" was deterred from making the experiment, till, being encouraged " by you, I procured a copper ball, or flask, which holds three wine " pints, the weight 14 oz., with a screw stopper adapted to it, and " began with small quantities of inflammable and large quantities of " common air, which were fired without the least danger.

" I then increased the bulk of the inflammable air to half that of " the common air, which, when fired, made the flask very warm to " my hand ; and every time I applied a long glass tube, fastened to " the pipe of a pair of bellows, to blow the phlogisticated air out of " the flask, I observed a smoke escape along with it. I also fired the " air when the flask was under water, and did not observe anything " escape when I perceived the heat against my hand with which I " kept the ball from rising. When the stopper was unscrewed, the " external air always rushed into the vessel containing the phlogis-" ticated air with some violence.

" The method I usually practise to mix the airs in any proportion, " is accurately to fill a measure with inflammable air, and rest it in " a tub, with its rim barely under water, hanging over the edge of a " shelf, so far as to admit one leg of an inverted syphon, the other " leg being closed, but afterwards opened, and the copper flask in-" verted upon it, but closed with its stopper when the measure of air " has been plunged under water, to force it out through the syphon. " I have sometimes exhausted the common air to admit the inflam-" mable air into the flask, but I do not find that that circumstance " produces any difference in the result of the main experiment.

" My next object was to adjust the balance in such a manner as " that I could always be certain to weigh to less than a grain when " it was loaded with the flask and its counterpoise, and I con-" stantly examined it at the beginning and end of every experiment. " The apparatus being adjusted, I proceeded to make the experiment " I had in view, and always accurately balanced the flask of common " air, then found the difference of weight after the inflammable air " was introduced, that I might be certain I 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 experiment, obtained the same result. "I have fired "air in *glass* vessels," says Mr. Warltire, "since I saw "you 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." Dr. Priestley adds, that Mr. Warltire, "the moment he saw the moisture "on the inside of the close glass vessel in which I "afterwards fired the inflammable air, said that it con-"firmed an opinion he had long entertained, viz., that

"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 about two grains.

"I have fired air in *glass* vessels since I saw you 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.

"If you think these experiments worth communicating to your "philosophical acquaintance, it may be depended upon that the cir-"cumstances appeared to me as I have represented them, whatever "they may be found to prove.

"I am, with great esteem,

"Your humble servant,

"JOHN WARLTIRE."

On this letter Dr. Priestley makes the following remarks:—

"The preceding article, though coming too late to be printed to-"gether with the rest of the volume, and to be noticed in the con-"tents of it, I have thought proper to insert on account of the re-"markable facts it exhibits.

"Dr. Withering and myself were present when the mixture of "common air and inflammable air was fired repeatedly in the close "copper vessel, and we observed that, notwithstanding all the pre-"cautions we could think of, the vessel certainly weighed less after "the explosion than it had done before. I do not think, however, "that so very bold an opinion as that of the latent heat of bodies "common air deposits its moisture when phlogisti-"cated;" both inquirers being evidently impressed with the belief that the dew was nothing else than the mechanical deposit of the moisture dispersed in common air.

It is remarkable enough, as an instance of the confusion which the least inattention must introduce into the history of such discoveries, and of the consequent importance of exact accuracy as to all their most mi-

"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. With me it was a mere random experiment, made to "entertain a few philosophical friends, who had formed themselves "into a private society, of which they had done me the honour to "make me a member.

"After we had fired the mixture of *common* and inflammable air, "we did the same with *dephlogisticated* and inflammable air; and "though, in this case, the light was much more intense, and the heat "much greater, the explosion was not so violent, but that a glass "tube about an inch in diameter, and not exceeding one tenth of an "inch in thickness, bore it without injury. Nor shall we wonder at "this, when we consider that the expansion of air by heat does not "go beyond four or five times its bulk. It is evident, however, from "this experiment, that little is to be expected from the firing of inflammable air in comparison with the effects of gunpowder; besides, that after firing of inflammable air, there is a great diminution of the bulk of air, whereas in the firing of gunpowder there "is a production of air."—PRIESTLEY's *Experiments and Observations*, &c. Birmingham, 1781. Vol. ii. p. * 395.

nute particulars, that Mr. Watt inadvertently stated* that he believed Mr. Cavendish was the first who observed the dewy deposit ; thereby assigning to him too much merit in place of too little. Mr. Cavendish† expressly states Mr. Warltire to have observed it. Mr. Warltire‡ states Dr. Priestley to have observed it ; while, ultimately, the mere observation of the moisture must be referred to Macquer, who also first ascertained it to be pure water.§ But this point may be said to have excited no controversy, which has been limited to the question, who first explained the real cause of the formation of the water, by drawing and stating the conclusion that water is composed of two gases, which unite in the process of their combustion, or explosion. To that question, accordingly, we shall now confine our attention, and see who was in point of fact the first to make public that theory, after having formed it altogether independently of the ideas of others.

On the publication of Dr. Priestley's work in 1781, Mr. Cavendish proceeded in July of that year, and at subsequent times, to examine Mr. Warltire's experiment, (the object of which, it will be remembered, was to determine whether heat was ponderable,) fre-

* See his Note, Phil. Trans. for 1784, p. 332.—It is proper, however, to observe, that the note is not in Mr. Watt's original draft, nor in the press copy of the letter in his own writing, sent to Mr. De Luc, of 26th November 1783 ; but is added at the bottom in pencil, in his own hand.

† Phil. Trans. 1784, pp. 126, 127.

‡ In his letter, cited above.

§ Dictionnaire de Chymie ; Mémoires de l'Académie for 1781, p. 489 ; Arago, Eloge of Watt, p. 98 ; ante, p. xxviii.

quently repeating it, with changes in some parts of the apparatus, and in the mode of preparation of the airs employed. He fired mixtures both of common and inflammable air, and of inflammable and dephlogisticated air, varying the proportions of each ; and, as was to be expected, not uniformly obtaining quite the same results. For, although he always observed, as Priestley and Warltire had done before him, that a dew was deposited, or, as he calls it, *condensed*, on the sides of the vessel in which the airs were fired, and though he applied more accurate measurement to the airs, and some tests to the "liquor condensed," he sometimes observed a slight loss of weight, sometimes none at all. In one instance, he found that "the weight seemed to be diminished two-tenths on "firing, and one-tenth more on standing."*

Mr. Cavendish's journal, or collection of laboratory notes, in which the details of all these experiments were entered, has been preserved among his papers. The whole of those papers were accurately examined, his Grace the Duke of Devonshire having granted permission, for the purpose of ascertaining whether any of them contained anything indicative of the dates of Mr. Cavendish's *conclusions*, respecting the theory of the formation of water by the combustion of hydrogen and oxygen gases; but Mr. Charles Hatchett "could not find anything in them which refer-"red to any date connected with the time when Mr. "Cavendish probably first conceived his theory;"†

* MS. Journal.

† Letter to the present Mr. James Watt, 16th April 1835.

and another gentleman, Mr. Hudson, in whose hands the papers had been placed by the Duke of Devonshire, and who minutely investigated them with every wish to discover some support to the claims which had been put forth on behalf of Mr. Cavendish, said, "I "do not find in these journals of the experiments any-"thing more than the simple statement of the facts, "without any casual mention of theoretical opinions."* This material fact has since been placed beyond the possibility of doubt, by the publication of the journal in question; in the whole course of which Mr. Cavendish does not make a single inquiry into the cause of the appearance of the water, nor indicate the most remote suspicion of its real origin; never using any expressions which could imply an union of the two airs, or which are inconsistent with the notion which Warltire and Priestley had entertained, of a mere mechanical deposit of the water. We are fully borne out in this assertion by the opinion of Lord Brougham, who says, "I must add, having read the full publica-"tion 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

* Letter to Mr. Hatchett, 15th April 1835. In the continuation of his letter, Mr. Hudson *supposes* that there could be "no doubt" of Mr. Cavendish having then also formed his theory. We should suppose so too:—*if the theory had then occurred to him.* That is THE important step; of which there is not a particle of evidence. *After the theory had been stated by Mr. Watt,* it may to Mr. Hudson appear to have been easy. The story of Columbus and the egg is exactly in point.

† The Reverend Gentleman who, with a curious infelicity for his own purpose, gave to the public the journal in question. "*Amicus "Cavendish, sed magis amica veritas!*"

" diaries, and notes of his experiments, not an intima-
" tion occurs of the composition of water having been
" inferred by him from those experiments earlier
" than Mr. Watt's paper of spring 1783."*

This fact further receives great confirmation from all that Mr. Cavendish has himself stated on the subject. His Paper, in which his conclusions are contained, was not read to the Royal Society till the 15th of January 1784; and although in July 1784, when the Philosophical Transactions for that year were printed, he said that his experiments (made in 1781,) had been mentioned to Dr. Priestley, he does not name the precise time, nor even the year, when the experiments were so communicated. He does not say that any conclusion was, along with them, mentioned or even hinted at. He does not even say at what time he himself first drew any conclusion on the matter. But in a continuation of the same passage he says, " during the last summer, [1783] also, a friend of " mine gave some account of them to M. Lavoisier, as " well as of the conclusion drawn from them, that de- " phlogisticated air is only water deprived of phlogis- " ton." This passage was not contained in Mr. Cavendish's paper, as originally written, presented, and read to the Society; and it was afterwards added, not in Mr. Cavendish's handwriting, but in that of Dr. (afterwards Sir Charles) Blagden, who was the friend referred to; but being printed in the body of the paper, without any explanation as to its separate authorship, and, of course with the knowledge and

* Lives of Men of Letters and Science, vol. i. p. 401.

approval of Mr. Cavendish, that gentleman is to be held as making the statement contained in it, and the whole passage must be taken as part of his Paper.

And (what is a most material proof of Mr. Cavendish never having made any communication of the theory) Dr. Priestley, who, in his Paper dated 21st April 1783, and read 26th June of the same year, alludes to one experiment of Mr. Cavendish as being known to him, says not a word of any theory which that gentleman had founded upon it; but, on the contrary, was in evident ignorance of any conclusion such as that which Mr. Cavendish, nearly a year later, communicated to the Royal Society. "It is clear," says Lavoisier,* "that Dr. Priestley has formed water "without suspecting it." It will presently be seen that his first intelligence of any idea being entertained that water is a compound body, came from Mr. Watt, and was received by him not only with surprise, as being entirely novel, but also with incredulity, as being quite erroneous. The real state of the case is very well explained by him in his Paper, read 24th February 1785, and printed in the Philosophical Transactions for that year, where he says, "Mr. Watt "concluded from some experiments of which I gave "an account to the Society, and also from some ob- "servations of his own, that water consists of dephlo- "gisticated and inflammable air, in which Mr. Caven- "dish and M. Lavoisier *concur* with him."†

There is thus no statement put on record by Mr. Cavendish, so far as we have yet gone, of his conclu-

* Mémoires de l'Académie for 1781, p. 479.

† Phil. Trans. for 1785, p. 280.

sions having been either drawn by himself, or made known to a single human being, previous to the summer of 1783; while the only intimation to be derived from the printed papers in the Philosophical Transactions, of his having drawn his conclusions at even so early a period, is contained in the above passage, which was written by Blagden, interpolated after the paper had been read in January 1784, and then adopted by Cavendish.

It is, further, apparent from the very title of his Paper, "*Experiments on Air*," that the composition of water was not the principal object to which Mr. Cavendish's attention had been directed. In this respect, his Paper presents an obvious contrast to that of Mr. Watt, which bears the much more unequivocal title of "Thoughts *on the Constituent Parts* "*of Water*, and of Dephlogisticated Air;" and of which the great object is to maintain that doctrine of the composition of water which is distinctly stated in its outset.

Moreover, some of the expressions used by Mr. Cavendish in further treating of the subject, are marked by no small ambiguity, and even inconsistency; for his theory is thus expressed in his own Paper:—"From what has been said there seems the "utmost reason to think, that dephlogisticated air is "only water deprived of its phlogiston, and that in-"flammable air, as was before said, is either phlogisti-"cated water, or else pure phlogiston; but in all pro-"bability the former." Now, besides the strange supposition as to inflammable air being phlogisticated water, which shows that Mr. Cavendish had then no very clear ideas on the subject of water being composed of oxygen and hydrogen, it is evident that he here omits entirely the consideration of latent heat; an omission which he even attempts to justify in one of the passages interpolated by Blagden.* But it is well known to every one acquainted with the first principles of chemical science—even as it was taught in the days of Black—and it was indisputably familiar to Mr. Watt, that no aëriform fluid can be converted into a liquid, nor any liquid into a solid, without the evolution of heat, previously latent. This essential part of the process, Mr. Cavendish's theory does not embrace. But without it, no theory on the subject can be complete.

It will presently be seen, that Mr. Watt's theory took fully into account this most important principle, without which, no conversion from the aëriform to the liquid state can possibly take place; and without which, therefore, Mr. Cavendish's theory was quite inadequate to explain the facts observed.

We have the authority of one of the best informed practical and theoretical chemists of this country, for declaring that "ideas exactly similar to those of Mr. "Watt are entertained by the most distinguished phi- "losophers of the present day." "Dr. Black," says Professor Graham of University College, "made it "appear probable, that metals owe their malleability "and ductility to a quantity of latent heat combined "with them."† And the learned Professor carries the same doctrine further; where, in referring to change

* Phil. Trans., p. 140.

† Elements of Chemistry, p. 42.

in the physical condition, and crystalline configuration of bodies, without any alteration in their ponderable constituents, he says, "The loss of heat observed "will afford all the explanation necessary, if heat be "admitted as a constituent of bodies, equally essential "as their ponderable elements."* This may serve as another illustration of the masterly grasp of Mr. Watt's comprehensive mind, which could so early foresee all that subsequent inquiry has fully confirmed.

M. Lavoisier, in his celebrated Memoir, admits that a partial communication was made by Blagden, to him and some other members of the French Academy, when, on the 24th of June 1783, along with M. La Place, he tried the experiment which they reported to the Academy on the following day. "He "informed us," says Lavoisier, "that Mr. Cavendish "had already attempted to burn inflammable air in "close vessels, and that he had obtained a very sensi-"ble quantity of water." He thus confines the communication within very narrow limits; for neither the experiment nor the result, as thus reported, was any thing more than had been effected by Warltire and Priestley. Evidently he did not intend to admit that he knew of any *conclusion* as to the real origin of the water having been drawn by Cavendish; for in a subsequent part of the same memoir, he takes to his coadjutor and himself the credit of drawing such conclusion:—"we did not hesitate to conclude from it, "that water is not a simple substance, and that it is "composed, weight for weight, of inflammable air, and

* Elements of Chemistry, p. 154.

" of vital air." He adds also, that they were then ignorant, and did not learn for some days, that M. Monge was occupied on the same subject.

It may be observed in passing, that as compared with Lavoisier and Cavendish, sufficient justice does not appear to have been done by writers on this subject, to the valuable labours of Monge. It is true, that when we consider the whole contents of his Paper, which includes some deductions both hesitating and obscure, and even, so far as we can judge, incorrect; and recollect the comparatively late period at which it was first given to the world, in the Memoirs of the Academy, we find it impossible, without showing an undue excess of favour to his memory, to rank him, in respect either of the precision, or of the early date of his conclusions, along with any of the other three great philosophers who have been candidates in either country, for the credit of the discovery. But his experiments, performed in the laboratory of the School at Mézières, were on a great scale ; and are admitted by Lavoisier and Meusnier,* to have been conducted with a very exact apparatus, and the most scrupulous attention. They are described in his Paper in the Memoirs of the Academy for 1783, printed in 1786 ; it is not stated when that Paper was read, but a note mentions that they were made in June and July, and repeated in October 1783, in ignorance of those of Cavendish in England, which were on a smaller scale, and of those of Lavoisier and La Place at Paris, which were made

* Mémoires de l'Académie for 1781, pp. 269, 270.

with an apparatus not fitted to attain so great exactness. Lavoisier and Monge thus declare their mutual ignorance of each other's proceedings: but Monge has never been accused, and may safely be acquitted, while the other has been frequently, and with too much justice, convicted, of concealing previous knowledge of other men's proceedings, in order to increase the estimated amount of his own merits.

The want of any date for either the authorship or the reading of M. Monge's paper, between the end of the year 1783, in which his experiments were made, and that part of 1786 in which it was printed, leaves us in doubt as to how far he may have profited by the lights which were during that interval thrown upon the subject. Certainly his words, as there given, are very similar to those of Mr. Watt's letter of April 1783, hereafter to be particularly noticed. "It follows," says Monge, "from this experiment, that when "we detonate inflammable gas and dephlogisticated "gas, each considered as pure, we obtain no other result than pure water, the matter of heat, and that "of light." But his conclusions, as further explained in the same paper, are less clear and decided than Mr. Watt's, or than those of Lavoisier and Cavendish; for he hesitates whether to consider water as not a simple substance, or fire as a compound one, and is encumbered with the uncertainty of an alternative theory;—either of different substances being held in solution by the fluid of fire considered as a common solvent, and combining to produce water; or else, of the two gases being solutions of water in different elastic fluids, which quit the water they held in solution, in order to combine and form the fluid of fire and light, which escapes through the sides of the vessel in which the detonation takes place.

Lavoisier's paper having been in part read in November 1783, was afterwards published with additions, which are not specifically distinguished from the original memoir, but are said to refer to the labour undertaken in common with M. Meusnier relative to the same subject. The volume in which it appears was printed in 1784, and is known in the series of the *Mémoires de l'Académie* as that for 1781. It arrived in this country after Mr. Cavendish's paper had been read on 15th January 1784, but before it was printed in July of that year; and it is alluded to in another addition to Mr. Cavendish's paper, which was unquestionably made after its arrival in England, and in which the theory of the composition of water is more clearly stated than it had been by him previous to the enunciation and exposition of it by the enlightened French chemist.* A point of internal evidence that seems to fix within very narrow bounds the period at which that volume of the French Memoirs was printed, is, that Lavoisier therein speaks of Blagden as "*aujourd'hui Secrétaire de la Société* "*Royale de Londres;*" an office to which he was not appointed till the 5th of May 1784.

Now, there can be little doubt, that the passage already cited, in which Blagden, in his own hand, but in Cavendish's name, detailed his communication to Lavoisier, was written to supply the imperfect ad-

* Phil. Trans. pp. 150-153.

mission of the French author, and to prevent those inferences as to priority of the theory, which otherwise might have been drawn from it, in favour of Lavoisier. Considering the object thus manifestly in view, here, if anywhere, we ought to look for an explicit statement of the earliest date at which Mr. Cavendish's theory could be said to have been formed, which, at that time, there was no difficulty in ascertaining, and there could have been little in establishing; and we are fairly entitled to hold, that the earliest date consistent with the fact would be assigned, if not by the author of the paper, at least by his zealous and assiduous friend who is so much mixed up with the transaction. All this we say, on the supposition, that the question as to priority had arisen merely between Lavoisier and Cavendish: for that is the whole length that our statement has as yet gone. We shall presently see whether other circumstances had not in the meantime arisen, which called still more loudly for that full, clear, and precise declaration which was to have been expected; and which was absolutely indispensable, in order to authenticate for the theory which Mr. Cavendish stated to the Royal Society on the 15th January 1784, an earlier date than its publication on that day could ensure.

Mr. Watt, in whose neighbourhood Dr. Priestley says he had "the happiness to be situated," and with whom, as has been mentioned, he was on habits of friendship and frequent intercourse, had, previous to 1783, for many years entertained an opinion that air was a modification of water; and that, if steam could be made red-hot, so that all its latent heat should be converted into sensible heat, either the steam would be converted into permanent air, or some other change would take place in its constitution. So early as 13th December 1782, he talks of processes "by which," he says, "I now believe air is generated from water;" using the expression, "if this process contains no de-"ception, here is an effectual account of many phe-"nomena, *and one element dismissed from the list.*"*

Being thus, even at that time, prepared to expect that water was, in some way or other, convertible into air, he directed his attention to Dr. Priestley's experiment, which he thus accurately relates: "He "puts dry dephlogisticated air and dry inflammable "air into a close vessel, and kindles them by electricity. "No air remains, at least if the two were pure, but he "finds on the sides of the vessel a quantity of water "equal in weight to the air employed."† In less than a month after he thus mentions his knowledge of that experiment, we find him writing to Dr. Black that he "believes he has found out the cause of the

* Mr. Watt to Mr. De Luc, 13th December 1782. As frequent reference will be made to the correspondence of Mr. Watt, printed in a subsequent part of this volume, we are happy to be able to record the very perfect condition in which, on a minute inspection, we find that correspondence to have been preserved; and which, fortunately, leaves nothing to be regretted on the score of mutilation or destruction. The copies of Mr. Watt's own letters, taken by his copying-machine, are still in excellent preservation; and, although in several of them the ink has become somewhat pale, it is nowhere so faint as to be illegible. They had been carefully pasted by the late Mr. Watt, in the order of their dates, into a large folio volume, in which they still remain.

† To his brother-in-law, Mr. Gilbert Hamilton, 26th March 1783.

"conversion of water into air;"* and giving the very words in which, both on that day, and a few days later, he stated his conclusions in the letter to Dr. Priestley, which he desired might be read to the Royal Society. The same conclusions are given in other letters written nearly at the same time; but nowhere are they more clearly, briefly, or forcibly stated, than in that to Mr. Gilbert Hamilton of the 22d of April, where, after a short enumeration of FACTS, beginning with the result of Dr. Priestley's experiment, follow these DEDUCTIONS.

"*Pure inflammable air is phlogiston itself.*

"*Dephlogisticated air is water deprived of its phlo-*" *giston, and united to latent heat.*

"*Water is dephlogisticated air deprived of part of its*" *latent heat, and united to a large dose of phlogiston.*"

In writing to Mr. De Luc, four days afterwards, "These," says Mr. Watt, "seem bold propositions, "but I think they follow from the present state of the "experiments; and if I were at leisure to write a book "on the subject, I think I could prove that no experi-"ment hitherto made contradicts them, and that the "greater number of experiments affirm them."† To others of his correspondents he announced his theory in similar terms. To Mr. Smeaton, writing that he has "attempted to demolish two of the most ancient ele-"ments—air and water;"‡ and to Mr. Fry, giving particular directions for the production of water and of [dephlogisticated] air, concluding thus:—"The ingre-"dients of water are pure air and phlogiston, united

* 21st April 1783. † 26th April 1783. ‡ 27th April 1783.

"in a state of ignition, and deprived of much elemen-" tary heat."* It will be remembered, that in the letter to Mr. Hamilton he had shown his belief to be, that pure inflammable air and phlogiston were exactly synonymous; and it is very remarkable, that the proportions of the two gases which he directs to be fired, viz., of pure air one part, and of inflammable air two parts, by measure, are exactly those which chemists of the present day would employ.

It appears from the letter to Dr. Black of the 21st of April, that Mr. Watt had, on that day, written his letter to Dr. Priestley, to be read by him to the Royal Society; but on the 26th he informs Mr. De Luc, that having observed some inaccuracies of style in that letter, he had removed them, and would send the Doctor a corrected copy in a day or two, which he accordingly did on the 28th; the corrected letter, (the same that was afterwards embodied verbatim in the letter to Mr. De Luc, printed in the Philosophical Transactions,) being dated 26th April, and containing, almost at its very commencement, the following passages:—

"The same ingenious philosopher mixed together "certain proportions of pure dry dephlogisticated air "and of pure dry inflammable air in a strong glass "vessel, closely shut, and then set them on fire by "means of the electric spark. The first effect was "the appearance of red heat or inflammation in the "airs, which was soon followed by the glass vessel "becoming hot. The heat gradually pervaded the

* 28th April 1783.

" 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 temperature 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 $\frac{1}{200}$th " part of its whole contents ; and this small residuum " may safely be concluded to have been occasioned " by some impurity in one or both kinds of air. The " moisture adhering to the glass, after these deflagra- " tions, 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 " whole quantity of sooty-like matter was too small " to be an object of consideration, particularly as it " did not occur in all the experiments.

" Let us now consider what obviously happens in the " case of the deflagration of the inflammable and de- " phlogisticated air. These two kinds of air unite with " violence; they become red-hot, and upon cooling " totally disappear. When the vessel is cooled a quan- " tity of water is found in it equal to the weight of " the air employed. The water is then the only re- " maining product of the process, and *water*, *light*, " and *heat*, are all the products.

" *Are we not, then, authorised 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 ther-* "*mometer or to the eye; and if light be only a modifi-* "*cation 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.*"*

In enclosing it, Mr. Watt adds, "As to myself, the "more I consider what I have said, I am the more "satisfied with it, as I find none of the facts repug- "nant."

Thus was announced, for the first time, and with as much confidence as its eminent author thought it became any philosophical inquirer to feel, when prosecuting his researches into new parts of science, one of the most wonderful discoveries that are recorded in its annals; of startling novelty, of admirable simplicity, leading to consequences of an importance and grandeur perhaps unparalleled, except by those which have attended other exertions of the same inventive mind; or by those which, emanating from a kindred intellect, have immortalized the name of Newton. It has been justly termed the commencement of a new era, the dawn of a new day, in physical inquiry,—the real foundation of the new system of chemistry. The language in which this new and

* See the same passages, printed in the Philosophical Transactions for 1784, pp. 331–333.

astonishing truth was expressed, though plain and perfectly unpretending, is so clear, precise, and just, that Mr. Cavendish—accomplished chemist and perspicuous writer as he was—could vary scarcely a single word of it, and that not for the better, when nine months later he made it public as his own: while M. Lavoisier, when *he too*, after it had been explained to him by Blagden, "*invented it himself, " and read a paper on the subject to the Royal Aca- " demy of Sciences,*"* altered only the terms which Mr. Watt had employed to express the two gases, viz. dephlogisticated air and inflammable air, or phlogiston, for their equivalents in his new nomenclature, viz. oxygen and hydrogen; their equivalents, that is to say, *in the sense in which Mr. Watt had used them.*

"This letter," as is stated in Mr. Watt's Note published in the Philosophical Transactions, "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."†

Had that been then done as requested, there cannot be a doubt in the mind of any one at all fitted to form an impartial opinion on the subject, that all possibility of controversy as to priority in the discovery must have been effectually prevented. It is true, that, judging from what actually occurred, it is difficult to say, even in that case, what use might

* Mr. Watt to Mr. Fry, 15th May 1784.

† Philosophical Transactions, 1784, p. 330.—Note.

have been made of the private perusal with which "several members of the Royal Society" were favoured. Lavoisier in France might even then have displayed that culpable want of a due acknowledgment of the aid he derived from others, which is so frequently to be deplored in the long series of his most interesting, able, and elegant memoirs. Cavendish in England might still have failed to exemplify that generous liberality, which ought to have noticed with eulogy, or, at least, to have named with exact justice, a philosophical discoverer who had thus preceded him in the same path. But both of those illustrious chemists would, at all events, have been in that case peremptorily debarred from openly taking credit for either priority or novelty in the announcement of their theory; and it would have been still harder for Cavendish or his friend even to have pretended—as for Lavoisier it is absolutely impossible to establish—a right to the claim of independent originality.

But, as it happened, the public reading which had been so requested by Mr. Watt did not take place at that time. "Before that could be complied with," the note continues, "the author, having heard of Dr. "Priestley's new experiments, begged that the reading "might be delayed." The delay was, in some small measure, unfortunate for the scientific renown of Mr. Watt; because competitors thereafter stepped in, and sought to appropriate that discovery of which the world had not yet heard, and which, at that time, must have been by all allowed to be honestly, solely, and honourably his own. But the misfortune is infinitely increased if we consider it as having, with some writers, led to doubts, seriously affecting the reputation of those competitors; as adding to the reproach which one of them had, to the sorrow of science, already justly incurred in similar matters; and as leaving on the fame of the other what must at least be termed a shade of suspicion.

The new experiments alluded to in the note, Priestley had announced in these terms :—" Behold with " surprise and indignation the figure of an apparatus " that has utterly ruined your beautiful hypothesis,"* giving a rough sketch with his pen of the apparatus employed. But Mr. Watt immediately and unhesitatingly replied, " 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 experiment. * * * I maintain my " hypothesis until it shall be shewn that the water " formed after the explosion of the pure and inflam- " mable airs, has some other origin."† So to Mr. De Luc :—" I do not see Dr. Priestley's experiment " in the same light that he does. It does not dis- " prove my theory. * * My assertion was simply, " that air," [*i. e.* dephlogisticated air, or oxygen, which was also commonly called vital air, pure air, or simply *air*,] " was water deprived of its phlogiston, " and united to heat, which I grounded on the

* Dr. Priestley to Mr. Watt, 29th April 1783.

† Mr. Watt to Dr. Priestley, 2d May 1783.

"decomposition of air by inflammation with inflam-"mable air, the residuum, or product of which, is "only water and heat."* Even when writing to Dr. Black that he had withdrawn his paper, he adds, "I "have not given up my theory."†

But he did withdraw, or rather reserve the public reading of his paper, till he should further examine the new experiments which were said to be hostile to the doctrine which it unfolded; and also, as he adds with his usual modesty, because he was "informed that that "theory was considered too bold, and not sufficiently "supported by facts."‡ "Mr. Watt then wished," as it is more fully expressed in a work published shortly afterwards, "that the letter should not be "read at the public meeting of the Society, because "he learned that his theory was thought too bold, "or that a substance such as water, till then con-"sidered as of the nature of an *element*, was there "placed in the class of *compounds*."‖ But the letter itself, after being read by many members, remained in the custody of the President till the day when it was read to the Society, 22d April 1784, as is well ascertained from Mr. Watt's letter to Blagden of 27th May 1784.

On the upright and unsuspecting philosopher, whose diffidence of his own admirable judgment, and "re-"spect for the opinions of others where he thought "they might merit it," had led him thus to delay what

* To Mr. De Luc, 18th May 1783.

† To Dr. Black, 23d June 1783.

‡ Mr. Watt to Sir Joseph Banks, 12th April 1784.

‖ De Luc, Météorologie, tom. ii. p. 216. 1786.

he calls "the first attempt he had made to lay any "thing before the public," a new and unpleasant light was destined soon to break. But in the meantime, having by additional experiments still further satisfied himself of the correctness of his theory, in which he had never been able to detect error, and the truth of which he now held to be abundantly confirmed, he proceeded, towards the end of November, tranquilly to occupy himself in preparing a more full statement of it, to be sent to his friend De Luc, for the purpose of being read to the Royal Society. By the 1st of December, however, we find that he had received accounts of an occurrence which appeared to stand much in need of explanation; and which, after that had been obtained, proved in some respects little to the credit of those concerned. "M. Lavoisier," he writes, "has read a memoir opening a theory very "similar to mine on the composition of water; in-"deed, so similar, that I cannot help suspecting he "has heard of the theory I ventured to form on that "subject, as I know that some notice of it was sent "to France."*

To this conjecture, Mr. Kirwan was able, in his reply, to add the most positive assurance. "M. "Lavoisier," he writes, "certainly learned your theory "from Dr. Blagden, who first had it from Mr. Ca-"vendish, and afterwards from your letter to Dr. "Priestley, which he heard read, and explained the "whole minutely to M. Lavoisier last July." [June.]†

* To Mr Kirwan, 1st December 1783.

† Mr. Kirwan to Mr. Watt, 13th December 1783.

The letter was, of course, well known to Dr. Priestley, who received it, perused it, and at once occupied himself in answering it, and to Sir Joseph Banks, in whose hands it long remained. But that it was also read by many other members of the Royal Society, though not then at a public meeting of the body, there cannot be any manner of doubt. For we have not only the direct statement of Mr. Watt to that effect, published in the Philosophical Transactions in 1784, under the direct superintendence of Dr. Blagden, and repeated by Mr. De Luc in 1786,* but we have Blagden admitting his own knowledge of the paper, both in the statement which he says he made to Lavoisier in June, and in his letter which Crell printed in 1786, of which we shall presently have much more to say. Mr. Kirwan's letter completes the demonstration of Blagden having acquired a minute knowledge of the paper, some time at least before he went to Paris, which was not later than the beginning of June.† It also appears very probable, (as it was clearly meant by Kirwan, and understood by Mr. Watt), that the first account of Mr. Watt's theory which Blagden ever received, he had from Cavendish. For the words are, "Lavoisier learned *your theory* "from Dr. Blagden, who first had *it* from Mr. Caven-"dish, and afterwards from your letter to Dr. Priest-"ley, which he heard read." The theory there spoken of is not said to have been one which had been formed by Cavendish, or which merely bore some

* Météorologie, vol. ii. p. 216.

† We know, from a private letter of Blagden's, that on the 11th of June he had been in Paris for several days.

resemblance, whether general or close, to that of Mr. Watt; it is Mr. Watt's own theory alone that is spoken of—the same that Blagden more minutely studied when he read the paper in which it was explained, but which he first appears to have heard of from Cavendish's report. Such is the only natural and obvious sense of Blagden's words, as reported by Kirwan; and, though it is by no means essential to our argument to insist upon it, they are almost incapable of any other interpretation. We are, however, perfectly justified in asserting that *two* such theories, so novel and strange as to be then deemed incredible, could scarcely have come to any man of science, or even any pretender to scientific knowledge, first from one discoverer and then from another, both within the same month—perhaps on the same day, without eliciting some observation on so marvellous a coincidence,—some further explanation—some particular inquiry, as to the time and manner of the theory being announced, or formed, by each discoverer respectively. Still more strongly does this remark apply, from the circumstance of Blagden being well acquainted with Cavendish's proceedings. If the theories had then been distinct, but if Mr. Watt's so much resembled another previously formed, as to be spoken of and treated as the same, would Blagden have had no wonder to express, no disappointment to feel, at his patron having been both rivalled in the formation of it, and certainly anticipated in the announcement? Would he have had no explanation to offer—no priority to attempt to sustain — no originality to claim for Mr. Cavendish, even if that gentleman was unwilling to do so for himself ? We repeat it :—the only theory alluded to here, is, so far as appears, that which Mr. Watt conceived, and which he alone had as yet committed to writing. Such was evidently Mr. Watt's own view of the meaning of Mr. Kirwan's communication ; and we are, however unwillingly, compelled to admit that the first part of the great engineer's reflections on the tidings sent by Kirwan may have been applicable in other quarters than that to which he then directed it. "You see," he says, "from "the above, that it is possible for a philosopher to "be disingenuous. For M. Lavoisier had heard of "my theory before he formed his, or before he tried "the experiment of burning dephlogisticated and "inflammable airs together, and saw the product was "water."*

Mr. De Luc having gone to Paris in December, 1783, and there passed the month of January, 1784, returned to England in February, when his letters to Mr. Watt were resumed. In the meantime, on the 15th January, Mr. Cavendish had read to the Royal Society the first part of his celebrated "Experiments "on Air," of which the second part was read on the 2d of June, 1785. In one of Mr. De Luc's letters, dated 1st March, 1784, he mentions that he had heard some particulars of the paper which Mr. Cavendish had read, but nothing concerning *the conclusions* stated in it as to the composition of water appears to have been then reported to him. The imperfect

* Mr. Watt to Mr. De Luc, 30th December, 1783.

account which he thus received came from Dr. Blagden. As the paper, however, was said to have included a thorough examination of the combustion of the two airs, he requested Mr. Cavendish's permission to see it, which was granted.

The consternation into which he was thrown on perusing it for the first time is well depicted in the close of the same letter :—" Being at this point of " my letter, I have received Mr. Cavendish's paper, " and have read it ! ! . . . . Expect something " that will astonish you as soon as I can write to you. " . . . Meanwhile, tell no one. . . . *In* " *short, he expounds and proves your system, word for* " *word, and makes no mention whatever of you.*"

The fact, however surprising, and whatever inferences may be drawn from it, was literally true. In the whole of that paper, as Mr. De Luc saw it, and as it had been read at the Royal Society, the learned chemist who had so carefully prepared it, had never once named James Watt, whose theory on the same subject had become " known to all the active mem-" bers" of the same Royal Society for nearly nine months ; had been announced and confirmed at Paris for nearly seven months, and was confessedly all the while minutely familiar to Blagden, the chosen friend and constant companion of Cavendish, professing to be engaged in the same pursuits with him, and who certainly was, as De Luc has elsewhere said, " in-" formed of all his experiments, as well as of those " of Dr. Priestley, and of the ideas of Mr. Watt."

Mr. De Luc, in his letter of the 1st March, had promised an analsisy of Cavendish's paper, and on the same day began a long transcript of its principal parts, which he finished on the 4th March, and sent to Mr. Watt in a letter, which showed that, on a further examination, his amazement had not subsided. Having endeavoured, in some degree, to defend Lavoisier and La Place from the charge of *le Plagiat*, he says— "But that which is, on the other hand, per-"fectly clear, precise, astonishing, is the memoir of Mr. "Cavendish. *Your own terms, in your letter of April "to Dr. Priestley, given as something new, by some one "who must have known that letter, which was known to "all the active members of the Royal Society—to Dr. "Blagden above all,* (for he said he had spoken of it to "Messrs. Lavoisier and La Place,) who well knew Mr. "Cavendish's memoir, both before it was read to the "Royal Society, and at its reading, and who conversed "with me about it, as I told you in my last—me, whom "he knows to be your zealous friend." After strongly recommending caution, De Luc says—"It is yet pos-"sible that Mr. Cavendish does not think he is pillaging "you, however probable it is that he does so;" giving as his reasons for desiring to entertain so charitable a hope, that Cavendish had not objected to let him peruse his paper, and also the character which both Cavendish and Blagden had previously maintained. The force of the first of these considerations is much diminished, when we remember, that the paper in question had already been made public to a great extent by being read at the Royal Society, and was, besides, soon to be printed in the Philosophical Transactions: so that there could be no possibility of keeping it secret, had that been desired.

And the character of Mr. Cavendish was clearly no excuse for the entire suppression of Mr. Watt's name in his paper; a defect which was afterwards, in Blagden's interpolation, most inadequately remedied; and which must ever remain a reproach both to Cavendish and to his companion Blagden, whose early and intimate knowledge of Mr. Watt's letter to Priestley has been so completely proved.

In the very delicate and disagreeable circumstances which had thus occurred, Mr. De Luc suggested two modes of proceeding; the one, to suffer in silence the injustice which he could not but feel had been done, in which case he engaged to print the letters to Dr. Priestley and himself, with their dates, in a work he was then preparing; the other, to make the matter more public, by requesting Sir Joseph Banks to cause both the letters to be read to the Royal Society. In recommending the former, the too discreet philosopher used these words :—"I should almost advise it, "considering that, in your position of drawing from "your discoveries practical consequences for your fortune, you must avoid making yourself *des jaloux*."

He had yet to learn the full extent of the manly virtue of his friend; who, while he declined to make any attack upon Mr. Cavendish, admitting, perhaps with a somewhat extravagant liberality, that it was "barely possible" that he might not have heard of his theory, still spoke in a strain of honest indignation of the plagiarism which he felt there was too much room to believe had been effected, of the wound which his scientific fame had been made to suffer, and of the hardship of being thus anticipated in the first attempt he had made to lay anything before the public. " As to what you say," he wrote, " about making " myself *des jaloux*, 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 contempt " for the modicum of reputation which would result " from such a theory, from a conviction in my own " mind that I was their superior, or from an indolence " that makes it more easy for me to bear wrongs, " than to seek redress. In point of interest, so far as " connected with money, that would be no bar : for " though I am dependent on the favour of the public, " I am not on Mr. C. or his friends, and could despise " the united power of the illustrious house of Cavendish, as Mr. Fox calls them."*

What followed may be very briefly told : " 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 ;"† and it is said by

* Mr. Watt to Mr. De Luc, 6th March 1784.

† Note by the present Mr. James Watt, added to Lord Brougham's Historical Note.—See Translation of Arago's Eloge, p. 164.

Sir Joseph Banks, that " both appeared to meet with " great approbation from large meetings of Fellows." *

Both of the letters were ordered by the Committee of Papers to be printed, and it was arranged that the best form in which that could be done, in order to avoid repetition, was by incorporating the first with the second, which was accordingly the plan adopted ; " but," as the note in the Philosophical Transactions bears, " 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."

Blagden became Secretary of the Royal Society on the 5th of May 1784 ; and to him, in virtue of his office, was entrusted the superintendence of the printing of Mr. Watt's paper. In his letters on that subject, he appeared perfectly willing to attend with care to the publication ; and in one of them offered, should Mr. Watt desire it, to send him the proof-sheets for correction. Mr. Watt, residing at a distance from town, declined his offer ; a resolution which he had afterwards reason to regret ; for the consequence has been, that in his paper, as it stands in the Philosophical Transactions, there is a very inexcusable *error of the press.* The date of the letter to Mr. De Luc, which we have just seen was 26th November 1783, is there given as 26th November 1784. It is true that the date of the *reading* of the paper is rightly given, and therefore that error might not always mislead ; but, considering all that had previously occurred, it was of great importance that every date

* Sir Joseph Banks to Mr. Watt, 11th May 1784.

establishing Mr. Watt's priority should be accurately printed, and what we shall in this instance call carelessness, cannot well be freed from blame.

But this is not all. Of Mr. Cavendish's Paper there were a number of separate copies thrown off, which were widely circulated throughout Europe by himself and his friends, before the seventy-fourth volume of the Philosophical Transactions, in which it was to be contained, made its appearance. These also, it is presumed, had been printed under the superintendence of Dr. Blagden, and of Mr. Cavendish. They all bear on their title page, that Mr. Cavendish's paper was "read at the Royal Society, January 15, 1783." Moreover, the true date, 1784, which is placed at the head of that paper as it stands in the Philosophical Transactions, is not given at all in those separate copies.

It is said by Mr. Harcourt, that in one instance, more than a year afterwards, (when the error had already been propagated in most of the scientific Journals of the Continent, and when also the Philosophical Transactions with the true date of the reading of the Paper had come into circulation,) Mr. Cavendish desired that it might be corrected.* We have no desire to take from him the credit of having done so in that instance. But the error continued long after-

* The above is the only new fact which that reverend gentleman, among all his petty cavillings and prolix sophistry, has disclosed; excepting, indeed, the additional and very important evidence which his publication of the Diary affords, of Mr. Cavendish's conclusions not having been drawn till after those of Mr. Watt had been made known.

wards to have its natural, unjust effect. For Cuvier, writing at the distance of four and twenty years from the circulation of the erroneous date, has distinctly said, "The experiment of Mr. Cavendish dates from 1781; "the reading of his Memoir, from January 1783;" and gives Cavendish the precedence over Lavoisier in their respective published memoirs, making the latter superior only in having discarded the hypothesis of phlogiston.* In his Eloge of Cavendish,† it is true, he alters 1783 to 1784, observing that three years had been occupied "in establishing that great pheno-"menon;" but still his readers are left without the means of knowing which of the two dates is the right one. Numerous as are Cuvier's errors on such points, yet his illustrious name, and the charms of the diction in which he clothes the history of philosophy and philosophic men, have led him to be cited by many as a safe authority; and Mr. Harcourt, who, as will presently be seen, himself practises such inaccuracies with a fatal facility, seems to think lightly of their effect. But this only the more deeply impresses us with the sacred obligation of scrupulously recording matters of fact in subjects of controversy, and makes us more sensible of the inestimable value of rigid accuracy.

Every one must admit, that after the series of events which we have now detailed—after the zealous attempts to establish priority which had been made by two of the three great claimants for

* Rapport Historique, 1808, p. 57.

† Mémoires de l'Académie, for 1811, p. cxxxiii; and, in the separate edition of Cuvier's Eloges Historiques, tome ii. p. 87.

the honour of the discovery, and the public statement which had been put on record by the third, (which, being uncontradicted, might be deemed decisive,) it was, truly, most unfortunate that any thing should occur, which could give to any of the proceedings, even in appearance, a character not altogether consistent with justice. It was at least a piece of most singular negligence, on the part of the Secretary to the Royal Society who superintended the printing, that those Papers should have been circulated with a double error in their dates ; that the tendency, if not the effect, of both the errors should have been, to take the priority from Watt, and to give it to Cavendish ; and that of all the errors which the printer might have committed, he should have happened to select precisely those which were best fitted to effect that object. When M. Arago exclaimed, after mentioning the same circumstance, " God forbid that I should, by these re-" marks, intend to cast any imputation on the literary " probity of those illustrious philosophers ; they only " prove that, on the subject of discoveries, the strict-" est justice is all that can be expected from a rival, " or a competitor, however high his reputation may " already be,"* we must confess that he well deserves to receive credit, for restraining within the bounds of those moderate words, the expression of a strong and just indignation.

An additional argument certainly arises from the remarkable fact, that Cavendish appears never to have

* Eloge of Watt, p. 106.

made any observation on Mr. Watt's chronological note, when it was printed with his Paper by the Royal Society ; nor ever to have confessed his knowledge of the real time at which Mr. Watt made either his first or his second communication, or of that at which he thus knew that his conclusions were drawn. But we have not yet done with either the history of the discovery, or the share which Dr. Blagden took in it as an auxiliary and historian. Finding that Lavoisier still maintained some claim, and seeing from the note appended to Mr. Watt's Paper, and from the total want of any statement as to the chronology of Cavendish's conclusions, that Mr. Watt stood distinctly recorded as the first discoverer, notwithstanding the inexplicable awkwardness of the typographical errors, he thought proper to write the letter to Crell, printed two years later in his Journal, which is given at full length at p. 71 of this volume. Blagden there says :—

" I can certainly give you the best account of the " little dispute about the first discoverer of the arti- " ficial generation of water, as I was the principal " instrument through which the first news of the disco- " very that had been already made was communicated " to M. Lavoisier. The following is a short statement " of the history. *In the spring* of 1783, Mr. Caven- " dish 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 dephlogis- " ticated air was nothing else than water deprived

" of its phlogiston, and, *vice versâ*, that water was " dephlogisticated air united with phlogiston. *About* " *the same time* the news was brought to London that " Mr. Watt of Birmingham had been induced, by " some observations, to form a similar opinion. Soon " after this I went to Paris, and in the company of " M. 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. * * * But those con- " clusions opened the way to M. Lavoisier's present " theory. * * * He was induced to institute " such experiments solely by the accounts he received " from me, and of our English experiments, and he " really discovered nothing but what had before been " pointed out to him to have been previously made " out and demonstrated in England."

Now, before examining the history which this letter gives of the discovery, it is to be observed that it professes to have been written in order to give *the best account* of the dispute about *the first discoverer*. And from the relations in which Blagden had always stood to Cavendish, and the obligations which he owed him, he cannot be suspected of under-stating any claims which he might have been able to establish for that gentleman to the possession of so great an honour.

Bearing this in mind, and taking the statement as we find it, an extraordinary fact which meets us at the outset is, that it does not contain any distinct allegation of Cavendish having been *the first discoverer;* although it does positively assert that he was prior to Lavoisier, and appears to aim at having it understood that he was prior also to Mr. Watt. Even the time at which Cavendish is reported to have communicated to his friends of the Royal Society his experiments and their results, and "showed that "out of them he must draw the conclusion," is only noted in the most general way, as "in the Spring of "1783." But we know that Mr. Watt's conclusions, on the other hand, were actually formed, reduced to writing, (which Cavendish's confessedly were not), and known to many members of the Royal Society, also "in the Spring of 1783;" and Blagden, though he was well aware of all these circumstances, and professes to give "the best account," and was naturally desirous of gaining the credit of the priority for his patron, does not even state that *Cavendish's verbal communication* preceded his knowledge of *Mr. Watt's written conclusions*.

But further, no time has ever yet been stated, either by Cavendish or Blagden, at which the former really drew his conclusions; which are thus never heard of as having been even imagined by him till "the Spring of 1783;" and in the absence of all such assertion by either of those gentlemen, or by any one else who was acquainted with the circumstances, it is impossible, in common fairness to the other parties concerned, to attribute his conclusions to an earlier period than that which, however vaguely, is so assigned to them.

Again, if Mr. Cavendish, at the time of making his communication to his friends, was ignorant of Mr. Watt's conclusions, of which, even according to Blagden, "the news was brought to London about

" the same time," why does not Blagden, in his claim of priority, make any assertion to that effect? Would he not have done so if he could, and is it not a perfectly fair inference from the fact of his not having done so, that he knew he could not?

If, on the contrary, Cavendish was then in the knowledge of Mr. Watt's conclusions, why did he not, in order to assert any claim for himself, not only to priority, but even to originality, mention in his verbal communication to his friends, that he had drawn his own conclusions, or rather, had seen " that he must draw them"—for that is the more circuitous way in which Blagden puts it—before he had heard of those of Mr. Watt, and independent of them?

Failing any statement of the time—not during which he had been occupied with his experiments, for that proves nothing—but at which he had first drawn the particular conclusion from them, that " dephlogis-" ticated air is in reality nothing but dephlogisticated " water, or water deprived of its phlogiston,"—he could claim no priority, except as against a discoverer, the date of whose discovery could be proved to be subsequent to that communication to his friends, the members of the Royal Society. But only a vague approximation being attempted to the date of his communication, and no better or earlier one being even suggested as that of his conclusions—(and that, too, in " the best account" that could be given of his claims, published in his own lifetime, and written by one who well knew the necessity there was for the greatest possible minuteness and precision of chronological record)—and no later period being assigned as that of his knowledge of Mr. Watt's conclusions, the inference is both just and inevitable, that neither Cavendish, nor Blagden on his behalf, could establish any priority as against Mr. Watt.

It was comparatively an easy matter to assert it for one or both of the English philosophers as against Lavoisier, for that chemist, on his own shewing, could not claim even for his *experiment*, an earlier date than the 24th of June 1783; and, had his been the only competition which Cavendish had to apprehend, "the Spring" might have been held a sufficient anticipation; when taken in connexion with what Blagden states, and Lavoisier partially admits, to have passed at Paris. Still, even in that case, Blagden's way of speaking must have appeared to all accurate inquirers very negligent, very unsuitable to the nicety of the subject, and very unfit for the purposes of careful scientific history.

But when the question concerns the conclusions of Mr. Watt, which had been stated not verbally, nor at an uncertain date, nor only to his own private and particular friends, but in writing, on the 21st and subsequent days of April, to many members and the President of the Royal Society; and which, before this letter of Blagden's was written, had been printed in the Philosophical Transactions, under Blagden's immediate eye and sole superintendence, with a note emphatically and fully "authenticating the date of "the author's ideas"—it would be utterly absurd to found any claim, or even any argument in support of a claim, on an expression so indeterminate as that of "*the Spring*."

Was it early in the Spring, or late in the Spring? Was it in February, or in March, or in April? We apprehend that neither Mr. Cavendish nor Dr. Blagden would have thanked us for the supposition, that it might possibly have been *in May*. But "questions "as to priority," says M. Arago, "may depend"—not only on years, and on seasons, and on months—but "on weeks, on days, on hours, on minutes." In what week, on what day of the month, was the important disclosure made by Mr. Cavendish? To bring the matter to a short issue;—was it not after a certain letter, of date the 26th of April 1783, had been received by Dr. Priestley at London, shewn to "several "members of the Royal Society," nay, *read and minutely studied by Dr. Blagden*, (for that is proved by his own admission to Kirwan,) and then delivered to Sir Joseph Banks the President?

Blagden could not, surely, have so soon forgotten all the circumstances which attended so important a communication; he must at least have remembered whether, when it came from Mr. Cavendish, it was no longer graced with the freshness and interest of novelty; and whether it was not an echo of something else which had come to London and his ears "*about the same time*." Of two theories so nearly identical, he surely could have recollected, without much difficult reflection, which he had heard first; the memory which was so retentive as to the proceedings at Paris, where Lavoisier was concerned, could not well have been oblivious as to the occurrences in London, where Mr. Watt's communication excited so much attention, had been intimately known to Blagden himself, had gained most honourable applause from many learned persons, and stood recorded in the books of the Royal Society as the first announcement of the discovery of the compound nature of water. When Mr. Watt's conclusions were first made known, and that to all the active members of the Royal Society, they laughed at them, says De Luc, as at the explanation of the golden tooth; so great was their wonder, so strong their disbelief. But Mr. Cavendish's friends are not said by Blagden to have testified any surprise, or any incredulity; yet "the "conclusions," as Lord Brougham has truly said, "are "identical," with the single difference as to heat, in which respect the discoveries of modern chemists have shewn that Mr. Watt's had greatly the advantage. But the novelty was gone, and the disbelieving wonder had ceased. When Blagden says only, that both communications were made in "the Spring," and "about the same time," he claims for his patron no priority; he is content to insinuate for him only a very questionable sort of independence in the discovery; nay more,—for that is the result to which the evidence brings it,—he can for Mr. Cavendish, as against Mr. Watt, neither claim priority, nor establish independence.

In Mr. Cavendish's paper as first written, and as read on the 15th January 1784, he made no mention whatever of Mr. Watt's theory. Yet it appears from this letter to Crell, that Blagden was not uninformed at a much earlier period, (viz. the Spring of 1783,) of Mr. Watt having formed "an opinion" similar to that of Cavendish; he confesses that "the news was brought "to London" in the same spring; that he knew it, at latest, before June; and he authorised Kirwan to tell Mr. Watt that he had even heard his paper read; the Philosophical Transactions bear that it was known from Mr. Watt's own letter, to many members of the Royal Society; De Luc says it was known to all the active members, and to Dr. Blagden especially, who had full acquaintance with Mr. Cavendish's paper, both before it was read, and at its reading; and, lastly, it is highly probable that Blagden first heard of Mr. Watt's theory from Cavendish himself,—at least Mr. Watt evidently so interpreted Kirwan's letter. Blagden certainly nowhere asserts that Cavendish was not aware of it. Neither does Cavendish himself. Why, then, did he suppress, so far as depended on him, all notice of the theory which had thus been formed elsewhere, and of which he well knew the vast importance,—which was then many months old,—and to which his own was so wonderfully conformed as to be justly termed, "*its proof and exposition, word for* "*word?*" Why did he so readily grasp at the undivided merit of the discovery, but never once name the discoverer who had been treading, as even he must have admitted, with no unequal steps, and, as it was very soon proved, even in advance of himself, in the same path?

But, in the next place, when—after Mr. Watt's paper had been read to the Royal Society—Blagden added a passage, which was adopted and printed as his own by Cavendish, and therein mentioned both the name and the theory of Mr. Watt, why did neither of the two coadjutors say a single word to enlighten the scientific world on the dates at which the two theories respectively were formed? Or, was this unaccountable desideratum supplied when at a later period, Blagden undertook to give his "best account" of the matter? On the contrary, although he declares Lavoisier to have known of the conclusions of both Watt and Cavendish, and, therefore, to have been posterior to both, he is still satisfied with trying loosely to couple those two together, as having arrived at the discovery somewhere "about the same time." "Those conclusions," says Blagden, "opened the way to M. Lavoisier's pre-"sent theory;" and he thus informs us who was, of three, the last discoverer. Why does he not, in "the best account" of "the little dispute," venture to state the knowledge, which we well know he must have possessed, as to which of the other two was *the first discoverer?* That was the only point which he professed to settle; that is the only one which he leaves altogether untouched. His "best "account" is indeed a miserably bad one, alike for himself and his friend; and of his phrase "*about* "*the same time*," it has been happily observed,* in the case of another philosopher, that it was used "with a convenient degree of ambiguity, just suffi-"cient for self-defence, should he be charged with "unfair appropriation."

Such is the whole state of the case for Cavendish;

* By Lord Brougham, of Lavoisier, in the Life of Dr. Black.—Lives of Men of Letters and Science, vol. i., p. 329.

utterly deficient in any real claim to priority, even on the statement of his own friends,—let us rather say, of the only friend who has attempted to give testimony, solitary, partial, and obscure, in his favour. On the supposition of any claim of priority at all on the part of Cavendish, it is certainly very singular, and must be held to be very decisive, that in a cause of so much interest to science, which not a little concerned the renown of both the parties, and nearly touches the honour of one, that claim should never have been put forth, except by a kind of uncertain implication. It is, further, very unfortunate, that the penury of evidence in support of that imperfectly implied claim, should have been able to furnish nothing more satisfactory, than the feeble and ambiguous explanations of Blagden. We should like to know who were those "other members of "the Royal Society, Mr. Cavendish's particular "friends," who, Blagden states, participated with him in the private verbal communication; but of whom we hear neither the names, the number, nor any thing more than those few words, of the most distant and general allusion? When Mr. Cavendish read his paper, he did not hint at their existence, nor at the occurrence with which they were afterwards said to have been connected; he passed them over in silence when he published it, though both Blagden and he had then shewn their sense of the advantage to be gained from any claim which they could establish, and did not hesitate to make it as against Lavoisier. Yet they did not omit, in the same paper, to mention the communication of the "experiments," (though not of the much more important *conclusions*,) to Dr. Priestley particularly by name. That in the same paper they should not have named one of the several persons, who are said to have been informed of *both* the experiments and the conclusions, is, to say the least of it, a notable piece of inconsistency. But that Blagden in his letter to Crell should not have named one such individual besides himself, while no one but himself has ever admitted having received such a communication, is really, under all the circumstances, quite inexplicable.

There is one other point, on which, however, we touch unwillingly and briefly, because it is of a delicate nature, and we have no desire, nor, indeed, occasion, to draw from it any conclusion. For, as has been fully shewn, Dr. Blagden's statements, even if perfectly correct, cannot be said to contradict Mr. Watt's priority. But it certainly ought not to be kept altogether out of sight, in estimating the value of any testimony given by Dr. Blagden on behalf of Mr. Cavendish, that he received from that distinguished chemist, both a considerable annuity for a great part of his life, and afterwards a legacy of fifteen thousand pounds.* Lord Brougham says that Blagden's legacy was generally understood to have fallen far short of his ample expectations.†

In the Memoir of Mr. Watt, which was published in 1824, in the sixth edition of the Encyclopædia

* Mr. Cavendish's latter will was made 18th February 1804, and commences with the bequest to Sir C. Blagden. It was proved 5th March 1810.

† Lives of Men of Letters and Science, vol. i., p. 446.

Britannica, his just claims to the priority of the theory of the composition of water, as well as the other particulars of his life, are concisely but comprehensively detailed. It is there related, that he did not escape the common lot of eminent men; that of meeting with pirates of his inventions, and detractors from his merits. But it is added, "the latter indeed were "few, and their efforts transitory." And in a striking and exact delineation of his character, which came from the pen of Lord Jeffrey, written little more than a week after Mr. Watt's death, it was with singular truth observed, that "all men of learning and "science were his cordial friends; and such was the "influence of his mild character, and perfect fairness "and liberality, even upon the pretenders to these "accomplishments, that he lived to disarm even envy "itself, and died, we verily believe, without a single "enemy." The inscription on his monument in Westminster Abbey records the grateful sense entertained of his services by the Monarch, his Ministers, the Nobles, and the Commoners of this realm. The eloquence of M. Arago, Perpetual Secretary to the French Academy of Sciences, has still more widely spread the fame of his illustrious fellow-member; and we have the pride and happiness of knowing that, till very recently indeed, no one dared to intrude a dissentient opinion on the general voice; nor to decry, in the smallest particular, that reputation to which the greatest names in every nation have done reverent honour.

But, on occasion of the British Association for the advancement of science meeting at Birmingham in 1839, the Rev. W. Vernon Harcourt took advantage of the privilege of his temporary office as president, to assail M. Arago in public, on account of his Eloge of Mr. Watt ; accusing him of incorrectness in his statement of facts, and of unfairness in his inferences, on this subject.* Selecting for the object of his attack an absent foreigner, but then, and for years afterwards, carefully avoiding all allusion to Lord Brougham, who had so materially confirmed the arguments of M. Arago's able and brilliant composition, the Rev. gentleman was not long kept in ignorance of the sentiments which his ill-timed, and worse executed performance excited in the minds of those who witnessed it, or heard of it ; when, as was eloquently said,—" injustice was done to the genius " of Mr. Watt, before crowds who knew and who loved " him—within the walls of a city which that genius " had enriched—within the very sound of those " mighty establishments to which he had given life " and being—and in sight of the hallowed fane where " moulder his earthly remains."†

The sophistical reasoning,—nay worse, the unfounded assertions,—in which Mr. Harcourt had freely indulged before a popular audience, were readily exposed at the time in more than one publication. To the brief and somewhat contemptuous notice, which the Perpetual Secretary to the Academy of Sciences bestowed upon them, at a public meeting of that most learned body, was added the emphatic corro-

* See Report of that Meeting, published in 1840.

† Sir David Brewster, in the Edinburgh Review, Vol. LXX, p. 496.

boration of M. J. Dumas; who stated, that after having attentively examined the reasoning of his fellow-member, after having also scrupulously studied the correspondence preserved at Aston Hall,* he adopted "completely and in all its parts," the history which M. Arago had written of the discovery of the composition of water; and that his opinions upon that point were so decided, that he desired his declaration to be inserted in the Compte Rendu of the meeting.†

Than M. Dumas, a more competent judge of such a question could not possibly be imagined; for while he has shewn, in common with M. Arago, complete impartiality, in deciding against the claims of his much distinguished and lamented countryman Lavoisier, he happens to be also very intimately conversant with every part of the subject itself. The details of a prolonged series of most laborious and skilful experiments, whereby he was enabled to correct the errors into which MM. Berzelius and Dulong had been led, and for the first time to establish with minute precision, the exact proportion in which oxygen and hydrogen combine to form water, are to be found in his valuable *Mémoires de Chimie*;‡ and well deserve the attentive perusal of all those who can appreciate the merit of ingenuity, perseverance, and accuracy, in matters

* The original letters were submitted to his perusal by Mr. Watt, as they had before been to M. Arago.

† See the observations of MM. Arago and Dumas, printed at p. 260 of this volume.

‡ "Mémoires de Chimie, par M. J. Dumas, Membre de l' Institut." Paris, 1843, p. 395.

demanding the most difficult, protracted, and refined investigation.

Besides employing the argument arising from the reputation of Mr. Cavendish, which does not really affect the question of priority in the discovery, if established by other evidence, Mr. Harcourt made two assertions with the view of impugning M. Arago's accuracy. He said first, that Priestley "con-"stantly maintained" that he had never found the weight of the water, produced in his experiment, equal to that of the gases exploded ; and secondly, that an undue license had been used, in substituting the term *hydrogen* for *phlogiston*, as used by Mr. Watt.

The first of these assertions might well be termed by M. Arago "inconceivable," when it is remembered that in Priestley's own paper he says,—"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 fil-"tering 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 *I always found,* "*as near as I could judge, the weight of the decom-*"*posed air in the moisture* acquired by the paper."* In the very first pages of Mr. Watt's paper "on the "constituent parts of water," (which it would thus appear Mr. Harcourt has never even looked into,) in describing Dr. Priestley's experiment, it is said,—"These two kinds of air unite with violence, they "become red hot, and, upon cooling, totally disap-

* Phil. Trans., 1783, p. 427.

"pear. When the vessel is cooled, *a quantity of* "*water is found in it equal to the weight of the air* "*employed.*"* So in the Correspondence now printed, "he finds on the side of the vessel *a quantity of* "*water equal in weight to the air employed.*" And again, "No residuum, except a small quantity of "*water equal to their weight.*" So also, "you will find "*the water*, (*equal in weight to the air*,) adhering to "the sides of the vessel." The circumstance of the equality of weight was indeed one of the facts on which Mr. Watt repeatedly states that he founded his deductions.

With our three last quotations, Mr. Harcourt could not have been acquainted; although they may now serve to warn him not to make rash assertions on subjects on which his knowledge is so limited. But the two former, he ought to have known well; and when we observe him telling how "Priestley collected the fluid "by wiping the inside of the glass with filtering "paper," and yet concealing the fact of the equality of weight, (which is mentioned in the very same page and sentence of the same paper,) and referring only to Priestley's inaccurate recollections of the matter seven years afterwards, long after Mr. Watt's theory had been formed, published, and firmly established, we must confess that the epithet which the forbearance of M. Arago bestowed on the subterfuge of the Canon of York, seems rather unreasonably mild.

The substitution of the term *hydrogen* for *phlogis-*

* Phil. Trans. 1784, p. 333.

ton, had been so amply explained by M. Arago in the note on that subject which accompanied Lord Brougham's Historical Note,* that it might have been supposed no fair objection could have been raised by any one; even by the most injudicious and ill-informed partisan of Mr. Cavendish. M. Arago was also at the pains to produce a letter from Dr. Priestley to M. Lavoisier, dated 10th July 1782, in which he says he has made "some experiments with inflam-"mable air, that seem to prove *that it is the same* "*thing that has been called phlogiston.*" Dr. Priestley, in relating, in his paper of 1785, the theory which Mr. Watt had formed, says that he "concluded, &c., "that water consists of dephlogisticated *and inflam-* "*mable air.*" But further, Mr. Harcourt's professed difficulty might have been removed, if he had chosen to profit by Mr. Watt's own note, (which, if he did not read, he at least ought to have read, and might have been supposed to have considered, because it is given both in the Philosophical Transactions and in Lord Brougham's Historical Note,) viz.: "Pre-"vious to Dr. Priestley's making these experiments, "Mr. Kirwan had proved, by very ingenious de-"ductions from other facts, that *inflammable air was,* "*in all probability, the real phlogiston in an aerial* "*form. These arguments were perfectly convincing* "*to me.*"†

So in Mr. Watt's paper we find these expressions:—"It was reasonable to conclude, that *inflammable air* "*must be the pure phlogiston, or the matter which*

* Eloge of Watt, p. 167

† Phil. Trans. 1784, p. 331.

"*reduced the calces to metals;*"——"*the inflammable* "*air being supposed to be wholly phlogiston;*"——"*in-* "*flammable air or phlogiston;*"——"it is worthy of "inquiry whether the greater part of the heat let "loose was not contained in *the phlogiston or in-* "*flammable air,*"* &c. &c. So in writing to Dr. Black on the 21st of April 1783,—the very day on which his letter to Dr. Priestley was first written, although the second edition, read a year afterwards at the Royal Society, was written on the 26th of the same month—he says, "therefore *inflammable air is* "*the thing called phlogiston.*" So to Mr. Hamilton, on the 22d of April, the first of the three deductions he states is, "*pure inflammable air is phlogiston it-* "*self.*" Above all, in the same letter to Dr. Black, as if to exclude all possibility of any cavil being raised, on the ground of the language in which his theory is expressed, he further states his conclusion to be, "that water is composed of dephlogisticated *and in-* "*flammable air.*" Nothing can be more clear—nothing more demonstrative—than this; no words can more justly explain the doctrine which they convey, nor more completely refute any such reasoning, if reasoning it can be called, as that of which we have now exposed the unfairness and fallacy. We take the liberty of assuming, and little demonstration will be needed to convince most readers, that Mr. Watt both understood and could explain his own meaning quite as well as Mr. Harcourt can do it for him.

Neither is the objection, thus groundlessly stated

* Phil. Trans. pp. 349, 350, 352.

and frivolously persisted in by Mr. Harcourt, original with that very inaccurate gentleman, nor has it now been for the first time effectually answered. For, nearly half a century ago, a far abler pen than his thus wrote : " We have said that the theory of " Mr. Watt is now demonstrated to be true. To this " assertion, an objection may be raised from the language in which he states his theory ; for he explains " it by using the word ' phlogiston,' a word which is " now exploded from philosophy as the name of an " imaginary substance. *But it is sufficient to reply,* " *that Mr. Watt uses the word phlogiston as synonymous with inflammable air.*"*

It is evident that the term *hydrogen*, derived from the Greek word for water, and designating one of its constituents, could not have been invented till after the composition of that fluid had been ascertained. Lavoisier himself, the inventor of the term, did not use it till a later period ; and he expressly says, in the beginning of his paper, " The inflammable air " which I understand when I mention it in this " Memoir, is that which is obtained, either from the " decomposition of water by iron alone, or from iron " and zinc dissolved in vitriolic and marine acids ; " and, as it appears proved that in all cases that air " comes originally from water, I shall call it, when it " presents itself in the aëriform state, *aqueous inflammable air* ; and when it is engaged in any combination, *aqueous inflammable principle.*" That passage is one of those additions to the paper, which

* Article WATER, Encyc. Brit. 1797.

are said not to have been made till after November 1783; for it contains an allusion to the experiments made with M. Meusnier, which had not been performed at that date, but were described in the Memoir read at Easter 1784.

But in what respect was Cavendish superior to Mr. Watt on this point? Even in 1784 he used neither the term hydrogen at all, nor uniformly the term inflammable air; for his conclusion is in that year thus stated:—"There seems the utmost reason to "think that dephlogisticated air is only water de-"prived of its *phlogiston*, and that inflammable air is "either phlogisticated water or else pure phlogiston; "*but in all probability the former*,"—a conclusion infinitely more dim and distant from the truth than those which we have just cited from Mr. Watt's paper and letters. Such also is the language in which the rest of Mr. Cavendish's paper, on this subject, is couched; and even with all the additional lights supplied by Watt and Lavoisier to guide him, it is undeniable that his conclusions are at least as much embarrassed and disguised as those of either of the others: while M. Arago, that equal justice might be done to all parties, used exactly the same substitution in speaking of Cavendish's labours; thus making them, as well as those of Mr. Watt, more intelligible to those accustomed only to the modern nomenclature.

In November 1783, it is true, Mr. Watt rather thought that inflammable air contained a small quantity of water and much elementary heat. Mr. Cavendish also, in 1784, "thought it more probable that "inflammable air is water united to phlogiston."* Now, in regard to this supposition of Cavendish, we have a word to say to Mr. Harcourt, who has chosen to publish this observation :—"That Watt derived "from Cavendish his views on this subject, is evident "from the parenthetical introduction of his altered "opinion, that inflammable gas was not pure phlo- "giston, but a combination of phlogiston and water, " * * * *after the publication of Cavendish's* "*theory.*"† Mr. H. thus considers the exact resemblance of the two suppositions as to the possible nature of inflammable air, to be so great, that one of those two inquirers must have "derived it" from the other. Let us, then, just remind him, that Mr. Watt's "supposition" on this point was written on the 26th November, 1783, and was in April thereafter read, *unaltered*, at the Royal Society. Cavendish's paper was read on the 15th January, 1784, and was neither seen nor heard of by Mr. Watt till March 1784. Therefore, Mr. Watt wrote that passage, and the whole of his paper, *not after, but months before* Cavendish's statement of the same supposition, contained in the same words, was made known to any one ; and Mr. Cavendish's "candid friend" will see, that in the over-warmth of his zeal, not according to knowledge, he has made rather an awkward mistake ;—a mistake which, if any weight at all had been due to his reasoning, would have compromised the reputation of his client.

* Phil. Trans., 1784, p. 137.

† Address to the Birmingham Meeting, p. 12, *note*.

Not to weary the patience of the reader by correcting all the errors of the same kind to which Mr. Harcourt is prone, we shall only select one other instance. He says, "Priestley's paper was "printed in March 1783; and therefore Caven-"dish's communication of his 'conclusive' expe-"riments was anterior to Watt's speculations in "April, as well as to Lavoisier's experiments in "June of the same year."* Moreover, he coolly tells Lord Brougham, that his Lordship need only have referred to the "volume of the Transactions which "Cavendish quotes, to have found the 'epoch' which "was wanted." Yet any man may see, on turning to that volume of the Philosophical Transactions† to which Lord Brougham has been so rashly referred, that the paper in question, so far from having been printed in March 1783, *was not even read till the 26th of June of that year;* and thus, in place of its communication being anterior to the formation of Mr. Watt's theory, or the performance of Lavoisier's experiment, *it was posterior to both.*‡

* Phil. Mag. Feb. 1846, p. 116.

† Phil. Trans. for 1783, p. 398.

‡ Since the above was written, Mr. H. has substituted for his blunder as to March, the following sentence,—"Priestley addressed "his paper to the Royal Society on the 21st April 1783; and "therefore, Cavendish's communication of his experiments to him, "twice alluded to in that paper, must have been antecedent to the "speculations founded upon it, which Watt tells us he addressed "to Priestley on the 26th of the same month, as well as antecedent "to Lavoisier's experiments in June."

It has been clearly shewn, that Mr. Watt's theory was formed neither upon any communication from Cavendish, nor upon Priestley's

Lastly, Mr. H. asserts, that Cavendish's mere experiments, apart from the formation of any theory, "involved the notion, and established the fact," of the composition of water. So in some sense did Priestley's—so did Warltire's; nay, on the same principles, it might be hard to withhold the merit of priority from Macquer and Sigaud de Lafond, who produced water by the combustion of gases, and ascertained it to be pure. It may be true that Macquer's data, so far as he has recorded them, were scarcely sufficient to have led him readily to form a just opinion on the subject. But Priestley and Warltire, in their experiments of 1781, came very much nearer the last experimental step afterwards arrived at by Cavendish: the loss of weight which Warltire detected after the combustion was almost imperceptible, and was at once to be accounted for by the least imperfection in his apparatus. Yet they both confidently attributed the formation of the dew to the mere deposition of suspended moisture.

paper;—that his paper was first written on the very same day as Priestley's, viz. the 21st of April;—and that he was in complete ignorance of all Cavendish's proceedings, till the memorable eclaircissement in 1784. Yet the reverend gentleman, who seeks for truth with no better care, caution, nor success, than this, presumes at the same time to call Mr. Watt's admirable discovery mere "erroneous "speculation." If it be so, we are of course bound to admire the superior acuteness of Mr. Vernon Harcourt; as much as to deplore the blindness of Mr. Cavendish, who, not anticipating the objections of his self-constituted defender, was content to promulgate a theory identical with that of Mr. Watt in all particulars but one, in which it was confessedly inferior. Contrasting Mr. Watt's "erroneous "speculation" with Mr. Harcourt's specimens of wisdom, "*Errare* "*mehercle malo cum Platone.*"

So late as 1784, Meusnier and Lavoisier, in the commencement of their Memoir on the decomposition of water,* remark, that "there have nevertheless been doubts raised on that entire reduction of "two aëriform fluids into water; and, notwithstanding the precautions taken by M. Lavoisier, to ensure, as much as possible, precision in so delicate "an experiment; notwithstanding the conformity of "the result obtained nearly at the same time by M. "Monge, in the laboratory of the school of Mézières, "with a very exact apparatus and the most scrupulous attention, some persons have believed, that the "water which proceeds from that operation may be "attributed to humidity held in solution by the airs, "and deprived of support at the moment of their "combustion." Such was, then, the experience of MM. Meusnier and Lavoisier, who, it will not be denied, had the best means of ascertaining the impression really made on the scientific world by those experiments, which, to their own minds, had brought conviction of the truth of the theory of the composition of water. And in that Memoir, read as late as the 21st of April 1784, when the conclusions of Watt, and the able reasoning of Lavoisier in his first paper, and of Cavendish, and the confirmatory observations of La Place, and Meusnier, and Monge, had all become well known, those two distinguished philosophers thus found it needful to begin anew their argument, by that positive and particular statement of the opposition which was made to the theory, or at

* Mémoires de l'Académie for 1781, printed in 1784.

least of the difficulties which, with some, stood in the way of its reception.

In the same year, Mr. Kirwan appears to have thought that he ventured far in admitting himself to be "nearly convinced," that, when the two gases are fired, "water is really produced."* The example of caution, which had been set by so many sage experimentalists, was further illustrated in the case of Dr. Black, who, in his correspondence with Mr. Watt, only remarks of the steps immediately preceding his discovery, that they appeared to him "very surprising;" and, in 1790, thus wrote to Lavoisier:—"I long experienced a great aversion to "the new system, which represented as erroneous "that which I had regarded as a sound doctrine; "nevertheless, that aversion, which was caused by "the power of habit alone, has gradually diminished, "yielding to the clearness of your demonstrations, "and the solidity of your plan."†

Nay, the most conspicuous instance of the same truth, (at least in France, for it would be hard to point out a more signal one than Priestley), is to be found in the case of M. Monge himself. He, as has been shown,‡ was perfectly aware of the *result* of the combustion of the two gases; having performed the experiment on a greater scale, and obtained its product in a larger quantity than was done by any other at so early a date; and yet he appears, at

* Phil. Trans. for 1784, p. 167.

† Annales de Chimie, viii. p. 227.

‡ See above, p. xlii.

a period as late as 1786, when his paper was printed, to have entertained very uncertain notions as to the nature of the change which was operated, and very great doubts as to the theory, which is now so idly represented to have been obvious to any one, who performed the experiments on which *it might have been founded.* After enumerating the various deductions which he thought possible, "either consequence," says M. Monge, "is equally "extraordinary; and we could not decide between "them without experiments of another sort." And he concludes, "we have, then, need of much further "light on this subject; but we are entitled to ex-"pect it, both from time, and from the concourse of "the labours of physical enquirers." The hesitation in yielding his assent to the new doctrine, which Monge thus philosophically, but perhaps even too cautiously expresses, is as great, as the incredulity of Priestley was persevering.

In 1789, also, six years after the discovery had been made, Berthollet found occasion to write no less than fifty pages, (printed in the *Annales de Chimie* for that year,) in confutation of some of the arguments then maintained against it; chiefly of those of Mr. Keir,* whose acuteness and ability were unquestioned, and to the extent of whose learning, Berthollet does all justice. Yet Mr. Keir's opposition was both zealous and obstinate. Even Berthollet, the author of the paper, and a chemist equally judicious and original, professes himself, at that time, only

* In the Article NITRIC ACID, in his Dictionary of Chemistry.

a recent proselyte to the doctrine which he had adopted; with great candour admitting, that he had resisted it "longer than perhaps befitted a philoso-"phy, which should rise above those secret motives "which keep us bound down to our own opinions."*

The experience of every observant student of chemistry, who beholds for the first time the wonderful experiment in which water is formed, will serve to convince him that at that period such hesitation, or even denial, was not so unnatural, as to have been at all uncommon, or very discreditable to the acumen of those who entertained it. Even if any chemist of the present day, looking at Mr. Cavendish's experiments with the great additional light which the improved state of our knowledge now affords, should find it difficult to suppose, that the mere facts observed, and results obtained, should not at once have received the interpretation which was afterwards put upon them,—let him reflect whether great weight is not also due to these considerations; viz.—That if Mr. Cavendish had formed his theory in 1781, he most probably would have mentioned it, or alluded to it, before 1784, or even 1783; or, at least, that when he did make it known, he would have named the earliest date at which he could say that he had formed it. If the probability of either, or both, of these things be admitted, it must also be admitted, as a consequence from the facts as ascertained, that he probably did not form his theory,—as he is not even pretended to have stated it,—previous to "the Spring of 1783."

* Ann. de Chimie, iii. p. 114.

Besides, if the theory could not be separated from the experiments, but was necessarily involved in them, so as to have been apparent to any chemical philosopher, or even any common observer, who was informed of them; and if it be true, as stated by Blagden in Mr. Cavendish's paper, that "all the "experiments were made in 1781, and mentioned to "Dr. Priestley," how came *Dr. Priestley* not to see in them the conclusion represented to be so unavoidable? Yet we know, that even in 1783, he viewed Mr. Watt's theory, which was so nearly identical with that afterwards promulgated by Cavendish, as entirely novel.

It is thus quite impossible to say, that the experiments necessarily imply the conclusions; or to consider the right explanation of that most remarkable phenomenon as having been included in the mere observation of the fact. To argue the reverse, as Mr. Harcourt has done, is to betray an ignorance of the writings of the many eminent philosophers who doubted, and even denied the true theory, after it had received what modern chemists may consider irresistible confirmation. Cavendish appears, from his own diary of experiments, as well as from all the statements of himself and his friend Blagden, never to have expressed even a suspicion of the theory of the composition of water, till the date of Mr. Watt's paper of April 1783; when "the notion," and "the fact," were both alike, for the first time, made generally known. It is quite incredible that he could have made so surprising a discovery, and satisfied himself of its truth, and then thrown it aside for years, without even stating, at any time, in any way, or to any individual, that he had done so: especially when a "little dispute," as Blagden calls it, arose as to the priority; and assertions were distinctly made on the other side, which, uncontradicted as they have been, certainly place Mr. Cavendish second in order of time. Yet such is the absurd result at which Mr. Harcourt struggles—laboriously but vainly struggles—to arrive.

After all that we have said, it might appear, if not a bitter satire on the arguments which we have now been occupied in examining, at least a somewhat malicious excess of courtesy towards their author, were we to express any very high respect for either the abilities, the learning, or the discretion with which they have been employed. We shall not so err; for of Mr. Harcourt we must confess, as a very able writer has done, with far less reason, of Priestley,—"We have read over carefully all his "papers concerning the conversion of water into air, "but cannot help saying, that we went along with "the bewildered author weary and fatigued; his ex-"periments," (in the case of Mr. H. we might substitute "his assertions,") "are very often made at ran-"dom, almost always founded on false principles, "and seldom lead to anything but doubt and per-"plexity."* We wish, indeed, that they never led to anything worse; but we have another charge, at least as grave, to bring against the reverend gentleman, of either incompetency, deficiency in research, or

* Water, Encycl. Brit. 1797.

want of ordinary caution: a charge for which we regret that he should ever have given occasion.

He has—not in the address which he read to the British Association, but in a postscript which he added to it, and which was not published till nearly a year later—thought proper to make the following assertion:—"Though I have not had the advantage "of studying the unpublished MSS. of Watt, I know "that they were submitted to the inspection of the "late Dr. Henry, with whose reputation as a pneu-"matic chemist M. Dumas is well acquainted; and "whose knowledge, acuteness, and candour, were "such as eminently qualified him to judge in such "a question; and I learned from Dr. Henry, that these "MSS. produced no change in his opinion as to Ca-"vendish's title to be considered the first discoverer "of the composition of water."* Now, the late Dr. Henry is the only witness summoned by Mr. Harcourt as acquainted with the MSS. of Watt; the declaration thus put into his mouth was, as the place it occupies evidently shows, intended to cancel the opposite testimony of M. Dumas, one of the most distinguished, accurate, and philosophic chemists, of whose enlightened labours the world has reaped the advantage and acknowledged the value; and, from the silence of the grave, Mr. Harcourt seems not to have feared to receive contradiction.

The present Mr. James Watt has preserved, and we have seen Dr. Henry's original letter of 8th June 1820; in which, under his own hand, his opinion at that time is thus stated:—

* Mr. Harcourt's Postscript to his Address, p. 26.

"I have made use of the very first moments of "leisure that have occurred to me since you were "here, to look attentively over the papers of Mr. "Cavendish and your father, and the other documents "which you pointed out to my notice.

"*There is no room for doubt as to your father's "priority.*

"*It is established beyond all dispute, by a compa- "rison of dates, that your father was the first to in- "terpret rightly the important experiments showing "the synthesis of water.*

"*I should say that your father was the first who "had the sagacity to draw the right conclusion from "the experiment of Dr. Priestley, and to take that view "of the constitution of water, which, to this time, con- "tinues to be received by philosophers as the true one.*"

The entire letter, written *before* Dr. Henry had read the correspondence now published, is given below.* It seems by it, that Dr. Henry *absolutely excludes* Mr. Cavendish as a *discoverer*. For he rightly

* *Letter from the late Dr. Henry of Manchester to James Watt, Esq., Aston Hall.*

MANCHESTER, 8*th June* 1820.

"MY DEAR SIR,—I have made use of the very first moments of "leisure that have occurred to me since you were here, to look "attentively over the papers of Mr. Cavendish and your father, and "the other documents which you pointed out to my notice.

"It does not appear that Mr. Warltire has a claim to any share "in the discovery of the composition of water. His sole object was "to ascertain, by firing dephlogisticated and inflammable airs in a "close vessel, accurately weighed before and after the experiment, "whether heat be ponderable or not. The results which he ob- "tained indicated a small loss of weight, but these must have been "rendered erroneous by some defect of his apparatus, which, being attributes to Priestley the making the experiment, with the important observations of the deposit of water, and of the equality of weight; and to Cavendish merely the praise of performing the one and repeating the other with precision. He assigns to Mr. Watt *the whole merit of the discovery of the theory.* As to the distinction which Dr. Henry seems to have been then inclined to make, between *the discovery of the composition*, and *the discovery of the theory of the composition*, of water, because Mr. Watt drew his conclusions from an experiment of Dr. Priestley's, we leave it to others to say

" of copper, prevented him from observing the production of mois-
" ture, subsequently remarked by Dr. Priestley, when the process was
" repeated with the substitution of a vessel of glass. Dr. Priestley,
" also, first remarked the almost entire condensation of the two gases,
" and the correspondence of their weight with that of the water
" formed. 'This water,' your father observes, (Phil. Trans., Vol.
" lxxiv., p. 333,) '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;' and then im-
" mediately follows the conclusion, 'that water is composed of de-
" 'phlogisticated air and phlogiston,' (a term then used as synony-
" mous with hydrogen gas, which had just come to be considered as
" pure phlogiston,) 'deprived of part of their latent or elementary
" 'heat.' This just inference from the facts is distinctly ascribed to
" your father by Mr. Cavendish himself, (same vol., p. 140,) and there
" is, therefore, no room for any doubt as to your father's priority.
" The subject was next prosecuted by Mr. Cavendish, with that ad-
" mirable sagacity and precision for which he is so justly celebrated,
" and it was not till after his experiments that those alluded to by
" your father, (p. 333,) as made at Paris on large quantities of the
" two airs, appear to have been performed.

" It is, therefore, established beyond all dispute, by a comparison
" of dates, that your father was the first to interpret rightly the im-
" portant experiments showing the synthesis of water. But as the

how any share of the credit of the discovery can possibly attach to Dr. Priestley ; who, though he made that experiment, and thus unconsciously furnished the facts on which Mr. Watt's reasoning was in great measure founded, *uniformly denied the whole doctrine of the composition of water, and was never persuaded to believe in it.* It is evident that Mr. Cavendish also might well have performed the experiment, without drawing the conclusion.

Mr. Watt, on the other hand, as we have already shown, announced first in order of time, and with the utmost clearness, the real nature of the composition

" experiment leading to this doctrine originated not with him but " with Dr. Priestley, I am not sure whether it would not be too com- " prehensive a claim to assert for your father, 'the discovery of the " 'composition of water,' to which extent, if I recollect rightly, your " method of stating it goes ; for this would imply that the facts were " discovered by him, and not merely that he had reasoned correctly " on the facts of another person. I should, therefore, rather say that " your father was the first who had the sagacity to draw the right con- " clusion from the experiment of Dr. Priestley, and to take that view " of the constitution of water, which, to this time, continues to be " received by philosophers as the true one—or something to that " effect. In the case of your father, there is such a firm foundation, " in discoveries most beneficial to mankind, for a great and imperish- " able fame, that it is perhaps better to claim less rather than more " than his due—a sentiment which has evidently influenced the " general tone of the memoir which you were kind enough to show " me, and of which I expressed to you very warm and very sincere " approbation.

" I hope that I shall again have the pleasure of seeing you, and " for a longer time, as you pass southwards ; and in the meantime, I " remain,

" My dear Sir, yours very faithfully,

(Signed) " William Henry."

of water, and the proportion in which the two gases combine to form it. In the words of Lord Brougham's note on his " Natural Theology," published in 1835, " Dr. Priestley drew no conclusion of the least value " from his experiments. But Mr. Watt, after tho- " roughly weighing them, by careful comparison with " other facts, arrived at the opinion that they proved " the composition of water. This may justly be said " to have been the discovery of that great truth in " chemical science. I have examined the evidence, " and am convinced that he was the first discoverer, " in point of time ; although," his Lordship then continued, " it is very possible that Mr. Cavendish may " have arrived at the same truth from his own expe- " riments, without any knowledge of Mr. Watt's *ear-* " *lier* process of reasoning."

The present Mr. Watt's statement of the opinion which the late Dr. Henry expressed to him *after* having carefully read the Correspondence, is given in a note to his letter to the Editor, at page v. of this volume. But that nothing may be wanting to complete the information which on this point we are anxious to supply to Mr. Harcourt, we beg next to give some passages of a letter, in which his own name is placed in juxtaposition with the same opinion which he has so utterly distorted. It is from Dr. William Charles Henry, whose learned accomplishments still worthily adorn that name, which the well-known merits of his father and grandfather have so long endeared to science.

" Mr. Vernon Harcourt, I observe, in the news- " paper record of his opening speech at Birmingham,

" has challenged the accuracy of M. Arago's adju-
" dication of your father's and Cavendish's claims to
" the discovery of the composition of water. * * *
" *My father, I distinctly remember, came last from*
" *a visit to you, after a full examination of the docu-*
" *mentary evidence you submitted to him, impressed*
" *with a clear conviction that Mr. Watt was the first*
" *to interpret justly the experiment of the synthetic for-*
" *mation of water, and must be regarded as the disco-*
" *verer of the true theory of its composition.*"*

We do not envy Mr. Harcourt the position in which these letters place him. For, little acquainted with the rules of evidence as he appears to be, he cannot deny that he is bound by the testimony of his own witness. We do not, of course, undertake to say what interpretation he may have put on any private conversation he may have had with the late Dr. Henry; nor can we pretend to explain how far any portion of his statement may be attributable to a defect of memory. Any misconception which he may have entertained as to Dr. Henry's latest and real opinion on this subject, we have now, we presume, effectually removed. But that opinion, even if it had been such as it was thus erroneously represented, could not have disproved any of the indisputable facts which stand on record, and are now open to the inspection of every one. The question has become one of evidence much more than of chemistry; and we cannot but remember that if, in a Court of Justice, any one were detected attributing to a deceased witness a declaration the very reverse

* Dr. William Charles Henry to Mr. James Watt, 4th Jan. 1840.

of what that witness was proved, by better evidence, really to have said, he would learn a sharp and salutary lesson, by losing alike his credit and his cause. The general nature of the contents of the first of these letters was distinctly stated by Lord Brougham, in an addition to his Historical Note, more than a year ago.* Mr. Harcourt, although by him Dr. Henry's respected name was first dragged into this controversy, and though he has since made a further publication on the subject,† has allowed that statement to remain unconfuted, without offering one word of retractation, explanation, or apology.

From the specimens we have given of his arguments and accuracy, an estimate may readily be formed of the credit due to his unsupported assertions. A writer in the Quarterly Review, who has ventured to rate them at more than they are worth, has done so with flattery so manifest, that it cannot be very palatable even to its object ;‡ and *he* might well seek shelter under the cover of anonymous authorship, who could, in the face of all evidence to the contrary, rank the Rev. Mr. H. among *the greatest men of science of the day;* or describe his performance at Birmingham as "remarkable"—"a singularly "elaborate analysis"—"eloquent and forcible"— "thorough knowledge of the subject in dispute"— "argument clear and powerful"—"powerful and con- "vincing"—and much more to the same purpose!

The fallacies of his reasoning are singularly con-

* Lives of Men of Letters and Science, vol. i. p. 401.

† Philosophical Magazine for 1846.

‡ See the Quarterly Review for December 1845, p. 105.

formed to those of the model which he thus humbly, but most unwisely, proposes to himself for imitation. Both, for their own purposes, keep entirely out of view Blagden's letter to Crell, and his interpolations in Cavendish's Paper ; both deny his knowledge, or even the possibility of his knowledge, of Mr. Watt's Paper, though Blagden himself, as we have seen, less cautiously admits it ; both studiously seek to confound Cavendish's *experiments*, which no one doubts may have been made in 1781, with his *conclusions*, which, there is as little doubt, were never publicly stated till the summer of 1783. And, in the art of unfounded assertion, the Quarterly Reviewer has not fallen far behind that Reverend author, to whom he offers such extravagant adulation.

He has said, among many other things equally incorrect and absurd, that Cavendish had from the first adopted the conclusion, that hydrogen or inflammable air was the real phlogiston of the popular theory ;—that Mr. Watt's theory totally failed in its application to facts ;—and that the paper in which it is contained is the only one which Mr. Watt ever published. Now, we have seen, that Cavendish thought it " much " more likely that inflammable air is water united to " phlogiston, than pure phlogiston ;"—Mr. Watt's theory, though formed under many disadvantages, and especially under that great one of being the FIRST theory formed on the subject, was not only quite as good as Cavendish's, but far surpassed it in completeness, by his introduction of the consideration of heat ; —and in the very same volume of the Philosophical Transactions in which Mr. Watt's paper appears, the

Reviewer would have seen, if he had ever looked into it, or even glanced at its list of contents, two other papers by the same author.*

The critic in question has even carried his want of caution, or defiance of accuracy, not to say his wilful contempt of truth, so far, as to hazard assertions like these, thrice repeated within two pages and a half: "There is no reason to believe that the contents of "this letter were made known to Mr. Cavendish, to "Dr. Blagden, or to any other person."—"Mr. Watt's "letter was not deposited in the archives of the Royal "Society, so as to be accessible to its members."—"Mr. Watt's paper was not deposited in the archives; "it was accessible neither to Mr. Cavendish nor to "Dr. Blagden, and its existence was probably alto-"gether unknown to them." Now, we not only positively know, from the letter to Blagden already cited, of 27th May 1784, that after Mr. Watt's letter had been given by Dr. Priestley to Sir Joseph Banks, it remained in his custody till the day it was read; but it is further particularly stated in the note in the Philosophical Transactions, that before it was so delivered to the President, it had been shown to several members of the Royal Society. And as Blagden's letter to Crell, in which he distinctly admits his own knowledge of the doctrines contained in that paper, and says that he told Lavoisier of them in June 1783, is cited both by M. Arago and by Lord Brougham, even the Reviewer's ignorance, great as on all this subject it unquestionably is, cannot be admitted as any excuse for scandalous misrepresentations.

* One read 6th May 1784, the other 27th May 1784.

After all that has now been said, it can hardly be thought necessary that we should gravely answer the ridiculous assertion, that Mr. Watt did not in his lifetime put forward a distinct claim to the honour which was justly his due ; especially because that assertion has been made only by such writers as the Rev. Mr. Harcourt and his Reviewer. But it may be proper, as it is easy, to refute the further mis-statement, that Cavendish "was universally regarded, and "has continued to be regarded as the sole author of "this great discovery ;" and that "it was only in "later times that attempts have been made to upset "this unanimous decision in his favour, when there "are no living witnesses to the impression which pre- "vailed among his contemporaries."*

Mr. Watt's note in the Philosophical Transactions, which most effectually declares his priority, was never contradicted nor called in question by Cavendish, or any of his friends ; to all of whom—and especially, as we have seen, to Dr. Blagden—it was well known, being printed in the same volume with both of the papers.† Having, by that note, done all that became so high-minded a man and so true a philosopher, he could well afford to despise any portion of fame that might have been gained by more elaborate or less

* Quarterly Review, for December 1845, p. 137.

† It deserves to be mentioned, that in the Abridgment of the Papers in the Philosophical Transactions, prepared by Hutton, Shaw, and Pearson, Mr. Watt's important note is, very improperly, omitted. This may account for Cavendish having received the credit of the priority, with some of those who on subjects of scientific interest do not consult original authorities.

worthy means. Well might he have used the words, as he always exemplified the sentiment, by which one of the most eminent of his admirers, in another country, has added grace and dignity to his own memorable labours. "Though the opinions," says M. Dumas, "to which my researches have conducted me, "might have given room for more than one discussion, "I shall be pardoned for having deemed myself above "those vain polemics. The moments which I rescue "from them are devoted to ascending by experiment "to the sources themselves of truth; and I trust that "they are thus more usefully employed for the in-"terests of that science, to which I have consecrated "my life."*

Mr. Watt's constant occupation in pursuits which he was obliged to prefer even to his chemical studies, as still more essential to the advantage both of himself and his country, together with his "contempt for "the modicum of fame which would result from "such a discovery,"—nay, even the indolence of which he frequently speaks as constitutional, but of which the great works he accomplished certainly exhibit no trace;—above all, his extreme modesty, and absolute detestation not only of appearing in any way to celebrate his own praises, but even of being compelled to listen to them;—all combined to prevent his taking other steps for ensuring credit to himself, than were absolutely essential for placing his priority upon record. He had very nearly, in his own person, formed an illustration of the words of one of

* Preface to Mémoires de Chimie, Paris 1843.

his letters to Dr. Black*—" all this you bring on " yourself by not publishing your discoveries." And the foresight of the same observant and sagacious friend led him to write to Mr. Watt in these emphatic words ;—" Were you to be the first publisher of " your discoveries, *you would do it in such a cold and " modest manner, that blockheads would conclude there " was nothing in it, and rogues would afterwards, by " making trifling variations, vamp off the greater part " of it as their own, and assume the whole merit to " themselves.*"† A remarkable prediction—most singularly verified !

Further, we have shewn that the doubly erroneous dates which were inserted in the Papers printed under Blagden's immediate superintendence, as Secretary to the Royal Society in 1784, were calculated entirely to mislead the world as to Mr. Watt being first, and Cavendish last, in the discovery ; or, at least, could not fail to produce much confusion and uncertainty, as to the relative priority of their respective theories. That this purpose was in great measure effected as regards some chemical authors, is proved by the inconsistencies of various works which touch on the point ; and a practice unquestionably prevailed with many writers in this country, (some of whom did little more than copy from the others,) of speaking loosely of " Mr. Cavendish's discovery," just as in France the same thing was done in regard to Lavoisier, La Place, Monge, and Meusnier.

But to that rule there have also been many excep-

* Sept. 25, 1783. † Dr. Black to Mr. Watt, 13th Feb. 1783.

tions. Thus Nicholson, in his preface to the translation of Fourcroy, published in 1788, says, " Mr. " Watt has therefore a claim to the merit of a disco- " verer with regard to the composition of water, and " has the advantage of priority in the discovery of its " decomposition."* The same statement is repeated in his Chemical Dictionary, in 1795;† although in both places Mr. Cavendish also is called a discoverer. In the excellent article on Water, in the third edition of the Encyclopædia Britannica, published in 1797, it is distinctly said,—" with respect to Mr. Watt, we " think it appears that he was the first person who " formed the true theory." In the translation of the fifth edition of Fourcroy, published, with numerous valuable notes, by the late Dr. John Thomson of Edinburgh, the very learned translator has supplied the undue omission of his author;—" It is but justice," he says, " to add, that the same inference had been made " by Mr. Watt, and communicated by him in a letter " to Dr. Priestley, dated April 26, 1783. See Phil. " Trans. vol. lxxiv. p. 330."‡ Lord Brougham, writing in the Edinburgh Review in 1803, ably stated for the first time the opinion to which his early studies had led him, and which the additional inquiries of nearly half a century have so materially confirmed, viz. that " some ingenious men, particularly Mr. Watt, " reasoning from all these facts, concluded that this " fluid is a compound of the two airs, deprived, by " their union, of a considerable portion of their latent

* Vol. i. p. 14. † P. 1020.

‡ Thomson's Fourcroy, vol. i. p. 240, 1798.

"heat; the quantity, viz. which is necessary for "maintaining the elastic state."* In Dr. Thomas Thomson's Chemistry, 1804, 1807,† and Murray's Chemistry, 1806, 1819,‡ while the independence of Mr. Cavendish is maintained, the priority is assigned to Mr. Watt. Dr. Dalton, in his "New System of "Chemical Philosophy," in 1810,§ says, that "the "composition and decomposition of water were ascer-"tained; the former by Watt and Cavendish, and "the latter by Lavoisier and Meusnier." In his History of the Royal Society also, published in 1812, Dr. Thomas Thomson says, after having mentioned Cavendish's paper, "Mr. Watt had previously drawn "the same conclusion from the experiments of Dr. "Priestley and Mr. Warltire."||

All of these statements excepting the last, were made during the life of Cavendish, who died in 1810; and the whole of them were made in the lifetime of Watt, who died, as is well known, in 1819; and also in that of Blagden, who died in the following year.

The story told by the Reviewer is, therefore, curiously inconsistent with fact. Yet that writer does not hesitate to apply to so admirable an example of sagacious generalisation as Mr. Watt's theory, the epithets "unprofitable and worthless;"—to declare that "it is most probable that neither M. Arago nor "Lord Brougham have ever read any original scien-"tific document connected with this controversy;"—

* Edin. Review, vol. iii. p. 11.

† Vol. i. p. 577; vol. ii. p. 109.

‡ Vol. ii. p. 158; vol. ii. p. 111.

§ Part II., p. 210.

|| P. 471.

and that a statement drawn up by Mr. James Watt, "the son of the great engineer, is not perfectly cor-"rect in the general outline of its facts, and is sin-"gularly partial and unjust in the conclusions which "it deduces from them."

We give the Reviewer full credit for being unable to appreciate the merits either of Mr. Watt's theory, or of any of his other discoveries, or of any part of his exalted character. M. Arago and Lord Brougham, we need hardly say, have shown a familiarity with every original document connected with the subject, in which their ill-informed and unscrupulous critic does certainly not participate, and of which he is, therefore, no competent judge. And the present Mr. James Watt may justly claim the possession of a quality, by which his revered father was so eminently distinguished,—but which, by the Reviewer, seems to be utterly abhorred,—that, namely, of giving to every man his own, and of rigidly abstaining from overstating any claim to any kind or measure of merit. We might safely appeal to the internal evidence of the present Mr. Watt's Letter, prefatory to these pages, as confirming both the substance and spirit of all he had previously written;—a letter which, we think, no one can peruse without feeling satisfied that, in discharging a duty incumbent upon him, and vindicating the fair claims and fame of his Father, he has confined himself as closely as the nature of the subject admitted, to a mere narrative of facts, based upon undeniable documents, leaving the conclusions to others. In accordance with Mr. Watt's known feelings, we may safely dispense with further refutation of the Reviewer's unwarranted aspersion.

One would suppose that a critic, who has ventured to assail the best informed and most able writers on the subject of which he professes to treat, and affects dogmatically to decide on rival claims to a great discovery, might at least have had the decency to prepare for such an undertaking by careful study, if he did not endeavour after the attainment of ordinary candour. But we have, with more pains than may be thought needful, thus exposed some of the practices of that writer, because we think it of importance to show how unworthy they are of the high respectability of the Journal into which they have been incautiously admitted; and because it is right that the public should know, what reliance can be placed on such a piece of criticism, in which professions of sincerity and pretensions to learning are as extravagantly made as they are unblushingly belied ;—extravagantly urged on the critic's own behalf, and unblushingly denied to others.

To go no further than one of his fatal exposures, he states that no claim was preferred in the lifetime of Mr. Watt, and that among his contemporaries there was an unanimous decision in favour of Mr. Cavendish; while we have, by quotations from no fewer than nine works published in Britain, to say nothing of Mr. Watt's own Note in the Philosophical Transactions, and Blagden's Letter in Crell's Journal, and Sir Humphry Davy's Lecture,* presently to be cited,—

* We do not take into account the German writers, such as Gren, and others of established repute, who might have been added to the

proved his statement to be untrue. Here, then, is a positive and most material assertion of this reckless mis-stater of facts, at once exposed, and shown to be the most gross misrepresentation, or the most crass ignorance, in no less than twelve different references; and those not difficult, doubtful, or obscure, but contained in books easy, common, and usually consulted by all who make any pretensions to an acquaintance with chemistry, or with the history of any of its doctrines. We are far from objecting to the exercise of the due license of criticism, and readily admit its beneficial influence on literature; but in proportion to the desire we feel that the stream should be sacredly preserved in all purity and usefulness, is the detestation with which we witness any pollution of its channel.

In conclusion, we may observe, that Mr. Harcourt and his anonymous encomiast are known to us only by their respective performances, which we have had occasion pointedly to censure. Their obvious want of careful research—their assumed knowledge and real ignorance of the subject—their egregious and

list. Neither have we added citations from the periodical literature of the end of the last century. But there is one journal which we may notice, because it was edited by Dr. Maty, the Secretary to the Royal Society, at the time that Mr. Watt's paper was first laid before that body. There it is said, in a full review of the paper, that "the direct investigation of the properties of a new thing, or its "relations to other things, requires that exertion of industry and "abilities which men mean to praise, if they mean anything, when "they speak of inventors. *Among these we do not scruple to place Mr. "Watt, as far as relates to the paper before us.*"—MATY's *Review* for 1785. Vol. vii., p. 106.

repeated mis-statements—have been paraded before the public, till the dignity of science, and the interests of truth, alike demand the refutation and reprobation of such conspicuous error. They cannot but feel, that our serious accusations *have been fully borne out;* nor can they disprove the blame which we have shown *justly attaches to their writings.* Let them now learn, that the question is one, which no retort of vain or virulent words can affect. They can point out no inaccuracies in *our statements of fact—our dates—our references—or*, we believe we might safely add, *our conclusions.*

The learned and philosophical chemist of Sweden, Berzelius, in 1841, on a deliberate review of the works then published on this subject, has, without hesitation, assigned to Mr. Watt that merit and priority of date, which so many other learned men have with justice attributed to him: saying that it is clear that he arrived at his conclusions eight months earlier than Cavendish, who could scarcely have been ignorant of them when he wrote his paper; and only expressing a doubt as to whether he used the term phlogiston as synonymous with inflammable air, and whether he did not amend his views on the publication of those of Lavoisier.* We have adduced incontestable proof, in no less than eight distinct passages from Mr. Watt's own writings, besides those cited from Priestley and others on the same point, of his having considered phlogiston and inflammable air to be identical; and all those were

* Berzelius, "Jahres-Bericht über die Fortschritte der physischen "Wissenschaften," II. Heft. pp. 43-51. Tübingen: 1841.

written previous to his knowledge of Lavoisier having *even entered upon the subject.* Mr. Watt's note, given above at p. lxxix, further shows that he was "perfectly "convinced" of inflammable air being "the real phlo-"giston in an aërial form," even previous to Dr. Priestley making his experiments.

As Berzelius further expressly says, that if we translate the quotation from Mr. Watt's paper into the language of the anti-phlogistic chemistry, (*i. e.* if we translate the word phlogiston into inflammable air, and dephlogisticated air into oxygen gas,) his conclusion is indisputable, we cannot but feel that any censure he bestows on M. Arago for making that translation, which the facts so fully warrant, is wholly undeserved. For, however various may have been the meanings attached to the word "*phlogiston,*" by other chemists of the phlogistic school, we have shown that there can be no mistake as to what *Mr. Watt* meant by it, when he formed his famous conclusions. Both in his paper on the constituent parts of water, and in his correspondence now published, he repeatedly uses "phlogiston" and "inflammable air" *as convertible terms;* and *that,* not by implication merely, but in the most direct and distinct language, in which his belief could be stated. Not content with declaring his conviction that "*pure inflammable air is phlogis-*"*ton itself,*" and reiterating the same doctrine in almost innumerable instances, he has, in his letter to Dr. Black of 21st April 1783, as we have already noticed, stated his conclusion to be, "that water is com-"posed of dephlogisticated *and inflammable air.*" Now, it is certain, that no doubts have ever been raised as to what is intended by "dephlogisticated "air;" by which term, all admit, is unquestionably meant *oxygen gas.* Nor has any one ever disputed, that by "inflammable air" is meant *hydrogen gas.* Therefore, when Mr. Watt says "*that water is com-* "*posed of dephlogisticated and inflammable air,*" he states the true doctrine of the composition of that fluid, not only with quite as much accuracy and clearness as was afterwards done by Cavendish and Lavoisier, but so as to meet and annihilate the objection to which we have now adverted. Nothing, indeed, can be more absolutely free from obscurity, than the doctrine as so expressed.

Sir Humphry Davy's opinion on the matter having been referred to, may, with propriety, here be noticed. In his Elements of Chemical Philosophy* he slightly alludes, (as many others have done in the same loose way of speaking,) to Mr. Cavendish's two discoveries of the composition of water and of nitric acid. But in one of his lectures, supposed to have been written about 1806, the more particular account he gives is, that in 1781, "Mr. Cavendish, in a process conceived "with his usual sagacity, and executed with his usual "precision, showed that when common air and hy-"drogen were exploded together, in the proportion "of two and a half to one, the product was pure "water, which exactly corresponded in weight to the "gas consumed. And Mr. Watt, reasoning on this "experiment, formed the conclusion that water con-"sisted of pure and inflammable air, deprived of the

* Vol. iv. p. 30, of the edition of his collected works, published by his brother, Dr. John Davy.

"greatest portion of their latent heat." Now, the experiments on which Mr. Watt reasoned were, as has been seen, not Cavendish's, but Priestley's. But the great and important distinction is clearly drawn, between Mr. Cavendish's mere observation of a fact, and the explanation of it by the theory which Mr. Watt formed.

We must really protest against the interpretation put upon the above account by our amiable and excellent friend, Dr. Davy; who cannot have understood his brother's meaning, when he paraphrased it in such words as these :—"*the fact of the discovery implying* "*the inference* is assigned to Mr. Cavendish, *the happy* "*inference requiring to be confirmed to constitute a dis-* "*covery,* is assigned to Mr. Watt." Sir Humphry Davy does not say a word of any "discovery imply-"ing an inference" having been made by Cavendish, nor of any inference at all having been drawn by him. Neither does he say, that Mr. Watt's reasoning and conclusion "required to be confirmed, to con-"stitute a discovery." Dr. Davy's commentary strongly reminds us of what we once heard asserted, viz.—*that he might have been supposed to know something of his late brother's opinions, if he had not taken the pains to show the world that he did not.* With a quiet disregard alike of the difficulties of the case, and of the evidence which helps to remove them, which cannot well be surpassed, he goes on to say—"Mr. Cavendish, in 1781, made the experiments "showing that water is the true product of the com-"bustion of oxygen and hydrogen ; *and drew the in-* "*ference* that water is composed of oxygen and hy-

"drogen." Of course, this *pro ratione voluntas* mode of proceeding would reduce the whole inquiry to the greatest possible simplicity ;—the only disadvantage of it being, that it can be used, at the same time, with equal justice, and equal success, on both sides of any given question.*

Sir David Brewster, in an article in the Edinburgh Review,† in which he reviewed the Eloge of M. Arago, first of all stated that "chemists of our own "and foreign countries had, by acts of omission, de-"prived Mr. Watt of a merit to which he is clearly "entitled," and then, "established," as he says, "on "the authority of printed documents, the priority of "Mr. Watt's hypothesis, to the experiments and de-"ductions of Cavendish ;" and "obtained," as it is repeated, "for Mr. Watt's hypothesis a decided "priority, or, to use Lord Brougham's words, showed "that he was the first to reduce the theory of com-"position to writing." He then went on to attempt to lessen the merit of the priority, so established by himself to his own satisfaction ; borrowing his principal support from the modest expressions which Mr. Watt himself used in the matter.

The caution with which Mr. Watt thought it proper to speak, he has himself well described in his Paper,‡ as "the diffidence which ought to accompany "every attempt to account for the phenomena of na-

* Sir Humphry Davy's latest and best informed opinion has been given at pp. ix. x. of this volume.

† In January 1840. The article has been publicly acknowledged by Sir David.

‡ Phil. Trans., 1784, p. 357.

"ture, on other principles, than those which are com-"monly received by philosophers in general." We have yet to learn that any inquirer into the causes of phenomena previously unexplained, could with propriety either recommend or adopt a greater degree of boldness in assertion, respecting subjects of which the difficulty could be considered as at all analogous. And we may suppose, that when Sir David Brewster shall be satisfied, by the perusal of the correspondence now first published, that Mr. Watt's theory was with him much more than mere conjecture or bare hypothesis,—that he was never shaken in his confidence in it, and positively refused to doubt, much more to abandon it, even after examining the experiments on which Priestley denied it—he may modify his opinion to one more consistent with the facts, and more liberal of praise to *the first discoverer*.

Had Mr. Watt's statement as to the date of his conclusions ever been called in question, or had he, like Mr. Cavendish, left no precise chronological statement at all;—had we been now forced to collect from other quarters, and for the first time, the facts on both sides of a disputed question, and to decide the cause according to the preponderance of such secondary evidence,—a chief consideration might have been, the peculiarities of character and disposition of the two principal parties. Even as matters now stand, with a priority of publication really incontestable, placed on record in the registers of the most learned body in the kingdom, and uncontradicted during the lives of any of the parties,—while it is by no means our wish to lessen the high reputation which Mr. Cavendish maintained, (however much that may have been exaggerated by the indiscriminate eulogy of Cuvier and others)—we may be forgiven if we dwell with pride on some characteristics of Mr. Watt, in which he was surpassed by no man, and could certainly have been equalled by few; which are not without a very important and obvious bearing on a question like the present.

The Earl of Liverpool, when Prime Minister of England, after publicly declaring that on his personal knowledge he could aver, that a more amiable and excellent man in all the relations of life never existed, amply enlarged on the simplicity of his character, the absence in him of every thing like presumption and ostentation, and his unwillingness to obtrude himself not only upon the great and powerful, but even on those branches of the scientific world to which he more immediately belonged.* An orator and statesman still more distinguished, after mentioning that he had the happiness of knowing Mr. Watt for many years, in the intercourse of private life, said that those who were admitted to his society would readily allow, that any thing more pure, more candid, more simple, more scrupulously loving of justice, than the whole habits of his life and conversation proved him to be, was never known :—" There was one quality, which " most honourably distinguished him from too many " inventors, and was worthy of all imitation—he was " not only entirely free from jealousy, but he exer- " cised a careful and scrupulous self-denial, and was

* Speeches at Freemason's Hall, 18th June 1824. Translation of Arago's Eloge, p. 189.

"anxious not to appear, even by accident, as appro-
"priating to himself that which he thought belonged
"to others. * * The only jealousy I have known
"him to betray, was with respect to others; in the
"nice adjustment he was fond of giving to the claims
"of inventors. Justly prizing scientific discovery
"above all other possessions, he deemed the title to
"it so sacred, that you might hear him arguing by
"the hour to settle disputed rights; and if you ever
"perceived his temper ruffled, it was when one man's
"invention was claimed by, or given to another; or
"when a clumsy adulation pressed upon himself that
"which he knew to be not his own."*

It is no derogation from his excellence, that he was at the same time not unconscious of "just pride, "founded on great talents and great services; that "pride, which the most exalted and most worthy can "justly indulge."† But his exemplary mind borrowed an additional grace from his habitual restraint of all such emotions; and we shall never forget the noble animation with which one of our most gifted and venerable Poets,‡ after having pointedly censured the unhappy passion for notoriety by which he conceived that some scientific men of the present day were too much actuated, fervently exclaimed—"It was not so, "that NEWTON made *his* discoveries, the grandest ever "known; nor that WATT made *his*, the most bene-"ficial to mankind:—I look upon him, considering

* Lord Brougham's Speech, printed with the Translation of Arago's Eloge, pp. 216-218.

† Sir R. Peel, in the House of Commons, 23d January 1846.

‡ Mr. Wordsworth, in September 1840.

"both the magnitude and the universality of his genius, as perhaps the most extraordinary man that "this country ever produced; he never sought display, but was content to work in that quietness and "humility, both of spirit and of outward circumstances, in which alone all that is truly great and "good was ever done."

Such is his enviable reputation as a man;—such his fame as a philosopher. And it is interesting in a high degree to remark, that for him, who had so fully subdued to the use of man the gigantic power of STEAM, it was also reserved to unfold its compound nature and elemental principles: as if on this subject there were to be *nothing which his researches did not touch—nothing which they touched that they did not adorn.*

That to his thoughtful sagacity is due the glory of having first made that remarkable step in the progress of science, cannot admit of a reasonable doubt. Had Mr. Watt's discovery of the theory of the composition of water been, like very many of his inventions, directly available for the increase of his own wealth, and, as such, protected by a patent, most certainly no case has been made out, on the part of Mr. Cavendish, of such public use, or prior invention, as could have invalidated that patent. But, is honour to be meted out with a less liberal hand, or guarded with less jealous care, than those pecuniary rewards, which the true philosopher does not covet, and which few men would with equal ardour desire? Are learned Societies, or the individual followers and friends of Science, to be guided by less exact principles of justice, in their award of praise to *a first inventor*, than those impartial Tribunals where, in similar cases, but with other interests at stake, the great improver of the steam-engine found his rights vindicated, and his inventions sacredly protected, by the strong arm of the Law ?

> " Vilius argentum est auro, virtutibus aurum.
> " O cives, cives ! quaerenda pecunia primum est,
> " Virtus post nummos ?"*

The result of the evidence on the whole case, as far as Mr. Watt's priority is concerned, we shall briefly express in these propositions, which certainly do not assume more than we have already proved ; and of which every one who has been accustomed to the exactness of legal inquiries into matters of disputed discovery, will acknowledge the force.

First, that Mr. Watt formed the original idea in his own mind, and thus was A DISCOVERER of the true theory of the composition of water.

Secondly, that being a discoverer, he was also THE FIRST PUBLISHER of that true theory.

Thirdly, that being both a discoverer, and also the first publisher, he must therefore be held to be " THE " TRUE AND FIRST INVENTOR THEREOF."†

* Hor. Epist. I. i. 52.

† See Godson on Patents, pp. 27-30. The term " Inventor" is, of course, here used in the legal sense, of " one that has found out " something new."

END OF THE INTRODUCTORY REMARKS.

SUMMARY

OF

THE HISTORY OF THE PROGRESS TOWARDS THE DISCOVERY, AND OF THE DISCOVERY ITSELF.

1776.

Volta fires inflammable air by the electric spark.

1776-77.

Macquer explodes mixtures of inflammable and common airs, and of inflammable and dephlogisticated airs, (but not by the electric spark,) in glass vessels, not close. He makes his observation of the moisture formed when inflammable air is burned in common air, and of that moisture being pure water.

1778.

Macquer publishes his observations.

1781.

Before the 18th of April, Mr. Warltire, being encouraged by Dr. Priestley, fires, by the electric spark, a mixture of common and inflammable air in a close metal flask, weighing the vessel before and after the explosion, observing the dewy deposit, and finding only a very trifling loss of weight.

Dr. Priestley fires mixtures of common and inflammable airs, and of inflammable and dephlogisticated airs, in a close *glass* vessel, and observes a deposit of water on the sides of the vessel.

Mr. Warltire repeats Dr. Priestley's experiment in the close glass vessel, and confirms his observation of the dewy deposit.

In July, after the publication of Dr. Priestley's and Mr. Warltire's experiments, Mr. Cavendish repeats them.

No conclusion as to the real origin of the water, published by Mr. Cavendish; nor communicated to any individual, nor contained in the Journal and Notes of his experiments; nor alleged by himself, nor by any one else, to have been then drawn by him.

1782.

13th December.—Mr. Watt, in writing to Mr. De Luc and Dr. Black, mentions an opinion which he had held for many years, that air was a modification of water; and that if all the latent heat of steam could be turned into sensible heat, the constitution of the steam would be essentially changed, and it would become air.

1783.

" Dr. Priestley having put dry dephlogisticated air " and dry inflammable air into a close [glass] vessel, " and kindled them by the electric spark, finds on the

" sides of the vessel a quantity of water equal in " weight to the air employed."

26th March.—Mr. Watt mentions as new to him, that experiment of Dr. Priestley's.

21st April.—Mr. Watt states in his letters, both to Dr. Priestley and to Dr. Black, his conclusions, viz. : " that water is composed of dephlogisticated and in- " flammable air, or phlogiston, deprived of part of " their latent heat ; and that dephlogisticated or pure " air is composed of water deprived of its phlogiston, " and united to heat and light." He requests his letter to Dr. Priestley to be read to the Royal Society.

26th April.—Mr. Watt having re-written his letter of the 21st, sends it to Dr. Priestley, who receives it in London—shows it to several members of the Royal Society,—*among whom was Mr. Cavendish's intimate friend, Dr. Blagden*,—and then delivers it to Sir Joseph Banks the President, for the purpose of being publicly read to the Society.

Prior to the 23d of June, Mr. Watt requests the public reading of his paper to be delayed till he should examine new experiments, said by Dr. Priestley to contradict his theory.

24th June.—MM. Lavoisier and La Place perform their experiment at Paris, at which Blagden is present. They are informed, as Lavoisier says, of Mr. Cavendish having burned the two airs and obtained water ;—as Blagden says, of the conclusions of Watt and Cavendish—(this being the first time that any conclusion of Mr. Cavendish on the subject is referred to by any one.)

25th June.—MM. Lavoisier and La Place give an account of their experiment to the Academy of Sciences, and Lavoisier states the conclusion as to the compound nature of water, to have been drawn by La Place and himself.

June and July.—M. Monge performs his experiments at Mézières; and repeats them in October.

Martinmas.—M. Lavoisier reads to the Academy of Sciences his memoir on the composition of water.

26th November.—Mr. Watt being fully satisfied of the correctness of his theory, and hearing that Lavoisier was passing it off as his own, repeats it in his letter to Mr. De Luc, which he requests may be read to the Royal Society.

No conclusion published, nor known to have been committed to writing, nor alleged (excepting by Dr. Blagden,) to have been drawn by Mr. Cavendish.

1784.

15th January.—In his paper read to the Royal Society this day, Mr. Cavendish, *for the first time*, states publicly in writing, and in his own person, his conclusions as to the compound nature of water; coinciding generally with those of Mr. Watt, but omitting the consideration of latent heat, as well as the mention of Mr. Watt's name.

March.—Mr. Watt, finding that in Mr. Cavendish's paper his own theory had been fully explained and proved, and his name excluded, expresses his indignation, and takes immediate steps for having his own letters, of 26th April and 26th November 1783, read at the Royal Society, with their true dates.

21st April.—MM. Meusnier and Lavoisier read to the Academy of Sciences their memoir on the decomposition of water, which is printed in the same year.

22d April.—Mr. Watt's first letter, which had till now remained in the custody of the President, is, according to his request, read at the Royal Society.

29th April.—His second letter is also read. Both letters are ordered to be printed in the Philosophical Transactions.

5th May.—Dr. Blagden is appointed Secretary to the Royal Society, and is entrusted with the superintendence of the printing of both of Mr. Watt's letters, to be embodied in one paper, with marks distinguishing each from the other.

June ?—M. Lavoisier's memoir is printed with additions.

July.—Mr. Watt's paper is printed, under the *sole* superintendence of Dr. Blagden, *and with the erroneous date of* 1784 *instead of* 1783. Mr. Cavendish's paper is printed ;—the separate copies, *with the erroneous date of* 1783 *instead of* 1784 ; and the paper itself containing two interpolations, made by Dr. Blagden some months after it had been read to the Society. In one of these, Mr. Watt's name is *for the first time* mentioned as if by Mr. Cavendish, and his theory alluded to as his own.

1786.

The paper of M. Monge is printed ; no date being mentioned at which it had been read.

END OF THE SUMMARY.

EXTRACTS

FROM

MR. WATT'S CORRESPONDENCE RESPECTING THE THEORY OF THE COMPOSITION OF WATER.

EXTRACTS

FROM

MR. WATT'S PRIVATE CORRESPONDENCE RESPECTING HIS DISCOVERY OF THE THEORY OF THE COMPOSITION OF WATER, &c.

DR. PRIESTLEY TO MR. WATT.

Fairhill, Birmingham, 8th Dec. 1782.

I HAVE the pleasure to inform you that I readily convert water into a permanent air, by first combining it with quicklime, and then exposing it to a red heat. This, I believe, agrees with your idea on the subject. I have not, though, much merit, as I had only random expectations from exposing volatile substances in general to a red heat, when combined with other substances, in imitation of the method of converting the acids into air, when combined with the calces of metals, or with alkaline bodies. When I have the pleasure of seeing you, I will inform you what kind of air I get, and what quantity, &c.

Yours sincerely,

JOSEPH PRIESTLEY.

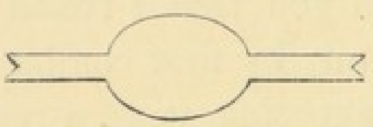
EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 13th Dec. 1782.

* * * *

Dr. Priestley has made a most surprising discocovery, which seems to confirm my theory of water's undergoing some very remarkable change at the point where all its latent heat would be changed into sensible heat, which must follow from the diminution of the latent heat, as the sensible heat increases, probably at or near 1200° of Fahrenheit.

The Doctor took a quantity of very caustic quicklime (calx viva) from which he had driven all the fixed air by means of violent heat ; he poured upon this quicklime one ounce of water, and put the lime after it had absorbed the water into an earthen retort, and subjected it to a strong heat. He placed a



balloon between the retort and the receiver. On the application of heat, air began to come over, and continued to do so until he got a quantity equal in weight to the ounce of water, viz. 800 oz. measures. The balloon remained quite cold, and was perfectly dry, without any appearance of moisture.

The air so produced contained a little fixed air, but the greatest part of it was nearly of the nature of atmospheric air, only somewhat more phlogisticated.

I have observed several other processes by which I now believe air is generated from water, some of which I shall mention to you when I have the pleasure of seeing you. If this process contains no deception, here is an effectual account of many phenomena, and one element dismissed from the list.

With the greatest regard and esteem, I remain, Dear Sir, your obliged friend,

JAMES WATT.

EXTRACT—MR. WATT TO DR. BLACK.

Birmingham, 13th Dec. 1782.

Mr. De Luc was here lately, and told me that he was now writing something on heat, and on the nature of elastic fluids, and begged I would explain to him some of my experiments and theories of that fluid, which I complied with in part, but could not do so without first explaining your theories of latent heat, of which he wanted to know more than I could tell him, or chose to do without your consent.

He is a man of great modesty and most engaging manners; is a great admirer of you from what he has heard of your discoveries; thinks you have been ill-used by Dr. Crawford and other people who have endeavoured to rob you of the merit of your discoveries, and wishes to be made able to do you justice; as he will take upon himself the trouble of being the editor of whatever you please to communicate, either as received directly from yourself, or through me. If, therefore, you should chuse to communicate any thing, I think you may depend on his doing you justice, in publishing as yours whatever you claim.

If it is not agreeable to you to furnish any materials, I shall only explain to him more fully your doctrine of the latent heat of steam; but, in doing that, I know not how to avoid mixing what may have been the suggestions of my own mind, with what I have learned from you, which I would [not] wish to do, as my suggestions may do your theory no honour.

What I mean to tell him that I think my own, is, —the trying the experiment on the latent heat in vacuo, and finding it to be greater than under the pressure of the atmosphere;—the experiments to ascertain the different degrees of heat at which water boils under different pressures;—the expansion which steam in its perfect state receives from heat;—and the experiments on the bulk of water when converted into steam; together with a theory which I have devised, which accounts for the boiling heat of water not following a geometrical progression; and shewing that, as steam parts with its latent heat as it acquires sensible heat, or is more compressed, that when it arrives at a certain point it will have no latent heat, and may, under proper compression, be an elastic fluid nearly as specifically heavy as water; at which point I conceive it will again change its state and become something else than steam or water. My opinion has been that it would then become air; which many things had led me to conclude, and which is confirmed by an experiment which Dr. Priestley made the other day, in his usual way of groping about. As he had succeeded in turning the acids into air by heat only, he wanted to try what water would become in like circumstances. He under-saturated some very caustic lime with an ounce of water, and subjected it to a white heat, in an earthen retort. He fixed a balloon between the receiver and the retort. No water or *moisture* came over, but a quantity of air, equal in weight to the water, viz., 800 oz. measures, a very small part of which was fixed air, and the rest of the nature of atmospheric air, but rather more phlogisticated. He has repeated the experiment with the same results.

Mr. Keir also presents his compliments to you. He is going to publish a new edition of his Dictionary, and makes the same request that Mr. De Luc does, as he must now say something on the subject of heat, which he formerly declined, hoping you would have done it yourself. He wishes to have his information from the fountain-head, and to give to Cæsar the things that are Cæsar's. In relation to those things which I look upon as my own, if you think my title to any of them bad, I will cheerfully resign it if you claim it ; and shall at all events own that I have built my house on the foundation of your theory of latent heat, and that I owe a just way of thinking on these subjects to you.

Mr. De Luc will be here again about the middle of February ; and I wish, as soon as proves convenient, that you would give me a few hints how you would have me act in the matter, as I have it much at heart to do what would prove most agreeable to you in it.

It will also give me great pleasure to hear of your health, and also of that of all my good friends with you, to whom I beg to be remembered. My own health is, as it used to be, none of the best, and I think my vexations increase faster than my wealth. —I remain, dear Doctor, most affectionately yours,

JAMES WATT.

DR. PRIESTLEY TO MR. WATT.

Fairhill, 26th Dec. 1782.

I have the pleasure to inform you that I now convert water into air without combining it with lime or any thing else, with less than a boiling heat, in the greatest quantity and with the least possible trouble or expense. The air is of the purity of that of the atmosphere, and, I think, without any mixture of fixed air.

The method will surprise you more than the effect, but that I may give you the pleasure of speculating on the subject, I shall defer the communication of the hocus pocus of it, till you give me the pleasure of your company at Fairhill.

I have other curious things to shew you.

Yours sincerely,

JOSEPH PRIESTLEY.

EXTRACT—MR. WATT TO MR. HAMILTON.

Birmingham, January 3, 1783.

My spirits have been so much affected by one thing and another, and my headaches have been so frequent and of such long continuance, that there have scarcely been two days in the week, this long time, that I have been tolerably well ; and even at those times my head stupid and confused. This, united to the necessity of writing such letters of business as required immediate answers, and contriving many things which were to be contrived, has made me put off from day to day everything I could. As you know the keenness of my sensibility, you can conceive how much these various accidents have affected me. * * This is the first day of a clear head I have had this fortnight; I dare not strain it too much. * * *

DR. BLACK TO MR. WATT.

Edinburgh, 30th January 1783.

My Dear Watt,—There is nothing I meet with now, that gives me so much pleasure as your letters, excepting those parts of them in which you mention your health and your vexations; when I come to these I exclaim, "Good God, why cannot I find the "philosopher's stone, that I may be enabled to relieve "my friends from their diseases and their distresses!"

But though I feel a painful sympathy with you on such occasions, I wish to hear everything that relates to you, and I would beg of you to write to me more particularly on this very subject, were I not sensible that it would give you a great deal of trouble to explain such matters to me, and in the busy restless state of your mind, to add to your trouble would be unpardonable; as I am persuaded that nothing would conduce so much to your relief and better health, than relaxation, and ease, and amusement. You may, however, give me a few lines when you have any new experiment or discoveries, such as you mention, to communicate ; early knowledge of these things being of some consequence to me. I have thought upon your conversation with Mr. De Luc and am very much flattered by his opinion of me, as I have a very high opinion of his genius and abilities ; nor have I the smallest doubt of his candour, or any suspicion that he would fail to do me ample justice were he to be the editor of what I have done on the subject of heat.* But I assure you I have already prepared a part of that subject for publication, and that I am resolved next summer to prepare the rest, and give it to the world such as it is. This is my fixed resolu-

* As there was afterwards a good deal of discussion in regard to this very point, (on which, however, we must decline entering,) it is but just to the memory of Mr. De Luc to relate the manner in which that was terminated. Professor Robison, in his edition of Dr. Black's Lectures, openly accused Mr. De Luc of having published Dr. Black's discovery of latent heat without due acknowledgment, and even of his having claimed it as entirely his own. That accusation was fully noticed and commented on in the Edinburgh Review for October 1803 ; the Reviewer at the same time expressing a wish, that some friend of the Genevese philosopher would step forward, to clear him from so foul a charge.

Mr. De Luc was at that time on the Continent, and long remained ignorant of the attack which had been made upon him. It is much to be lamented, that he was deprived of the opportunity of receiving a full retractation, by the death of Dr. Robison ; which occurred some months previous to his return to England. But in April 1805, Mr. De Luc addressed a very full explanation of the whole matter to the conductors of the Edinburgh Review, which they published in that Journal for July of the same year ; and "in which," said they, "we think he exculpates himself completely from the imputation "which was rather rashly thrown upon him in Dr. Robison's edition "of Dr. Black's Lectures, and repeated by us in our review of that "publication."—Edinburgh Review, vol. vi. p. 501.—Ed.

tion, and I am sorry that it is inconsistent with the friendly offer of Mr. De Luc.

It gives me also particular concern that I cannot gratify Mr. Keir in this matter, to whom I reckon myself under great obligation. But, perhaps, the inconvenience to both of these gentlemen will not be great, even if they should choose to see what I have to say before they publish. It will delay their publications only some months, or, at most, one year, supposing that they were nearly ready at present.

As for what you have done on these subjects, you have certainly a right to communicate it to the public in what manner you please ; but I think you ought to do it in such a manner as to derive from it some profit, as well as reputation ; and if you choose to make it a part of my publication, I shall certainly think myself bound to give you a share of what I make by it, proportioned to the number of pages which it fills ; and I shall willingly either receive it from you in your own composition, or express it myself as well as I can ; in which case it will be necessary that I pay you a visit, or that we have a meeting somehow or other.

Having thus answered the principal part of your letter, I can only, for the present, return you my thanks for the rest, which contains very curious matter, and some of it appearing to me very surprising ; but I have no time to spare just now ; adieu, then, and present my best compliments to Mrs. Watt.

I am, my dear Friend,

Yours most affectionately,

JOSEPH BLACK.

EXTRACT—MR. DE LUC TO MR. WATT.

Londres, le 31 Jan^er. 1783.

J'ai commencé pendant mon court séjour à Paris, ce que j'ai à cœur de faire ; c'est qu'on vous connoisse comme vous le méritez. Je me suis donc beaucoup entretenu de vous, et de vos expériences et inventions ; et ayant reconnu qu'il importe de publier promptement quelque chose sur les *fluides élastiques complexes*, soit composés de substances purement graves, et de fluides subtils, j'ai tout arrangé pour la production d'une première partie expérimentale sur cet objet, dans lequel je désire extrêmement de faire entrer le récit des expériences que vous voulez bien faire en ma présence. * * *

Les chimistes de Paris, s'occupent beaucoup aujourd'hui de la chaleur, et des modifications de ces transmissions ; des grands mathématiciens se joignent à eux ; car la théorie de ces transmissions ou communications donnent lieu à de fort beaux problèmes. MM. Lavoisier et De La Place entr' autres sont en grand travail et publieront. Enfin il est certain, qu'on commence à fouiller vivement dans les vraies bases de la Physique ; ainsi je vous prie, mon cher Monsieur, de vous prêter à y coopérer, car vous y pouvez beaucoup.

* * Je finis donc, en vous assurant, qu' on ne peut être avec plus de considération que je le suis, mon cher Monsieur,

Votre très humble et très obéissant serviteur.

J. A. De Luc.

EXTRACT—MR. WATT TO DR. BLACK.

Birmingham, 3d February 1783.

[Mr. Watt, in the early part of this letter, refers to his preceding one of 13th December, and states that he has received no answer, and goes on to say,] —which makes me fear that I have been too presuming in my request. I hope, however, that you will impute it to the desire I have to set your fame in the light it merits, and which, I think, you have neglected too long. For my own part, I have little ambition or desire to publish any of the few experiments I have made; but I find myself so set upon by many of my friends to do it, that I cannot longer resist their importunities; though neither my health nor leisure enable me to repeat the experiments with the necessary attention. One thing prompts me more than any other, which is, that we have been so beset with plagiaries, that if I had not a very good memory of my doing it, their impudent assertions would lead me to doubt whether I was the author of any improvements on the steam-engine; and the ill-will of those we have most essentially served, whether such improvements have not been highly prejudicial to the commonwealth.

* * * *

Mr. De Luc writes his book in French, and publishes it at Paris; and as he is an author who will be read by all men of philosophical learning there, I look upon it as a good opportunity.

* * * *

Dr. Priestley has been going on with his experiments on turning water into air, and has discovered many facts which seem in some degree contradictory to each other. He finds the mixture of quicklime and water heated in a glass vessel gives no air, only water ; but that water alone, put into the stone-ware retort, gives air in great quantities, even the eighth part of its weight. That olive oil, or oil of turpentine, in that earthen retort, produces very pure inflammable air. That water being put into a gun-barrel, and distilled over slowly, gives no air ; but on being confined by a cock, and let out by puffs, it produces much air ; which agrees with my theory, and also coincides with what I have observed in steam-engines. In some cases I have seen the tenth of the bulk of the water, of air extricated or made from it.—Hoping to hear from you soon,

I am, &c.

Most affectionately yours,

James Watt.

EXTRACT—MR. WATT TO MR. DE LUC.

3d February 1783.

* * * *

I have written to Dr. Black to try if he would favour us with any communication, but have received no answer yet ; and fear that, as he is now in the middle of his course of lectures, he will use that as a cover to his *inertia*. I thank you most sincerely for the pains you have taken at Paris in my behalf, and wish to be able to prove deserving of it.

* * * *

DR. BLACK TO MR. WATT.

Edinburgh, 13th February 1783.

My Dear Watt,—I received yours of the 3d instant, and, by observing the dates, I see that you would receive my answer to your former, two or three days after you wrote it.

In my last I acquainted you that it is my fixed resolution to publish next summer. At present, I am so much occupied with the busiest part of my course and other matters, that I cannot do any thing in that business. What you tell me in your last gives me a different notion of Mr. De Luc's intention from that I had formed before. I had imagined that he meant to publish in England, and in the English language. His intention to publish in France, and in the French language, makes a considerable difference; and if it was in my power to sit down just now and give him an *esquisse* of what I have done, and mean soon to publish, on heat, I should do it with pleasure; and I think it is very proper for you to give him a short account of your discoveries and speculations, and particularly to assert, clearly and fully, your sole right to the honour of the improvements on the steam-engine. And there is one advantage which will attend this method of publication. Mr. De Luc will naturally mention your discoveries with a proper degree of esteem for their value and ingenuity; whereas, were you to be the first publisher of them yourself, you would do it in such a cold and modest manner, that blockheads would conclude there was nothing in it, and rogues would afterwards, by making trifling variations, vamp off the greater part of it as their own, and assume the whole merit to themselves. I am greatly obliged to you for your philosophical news, and I assure you, that the friends you mention here remember you always with the greatest affection and esteem.

* * * *

Farewell, my dear friend, and believe me most affectionately yours,

JOSEPH BLACK.

EXTRACT—MR. WATT TO MR. GILBERT HAMILTON.

18th February, 1783.

* * * *

Dr. Priestley finds that when he confines the steam of water in a gun-barrel, and lets it out at intervals, it produces air, but does not if suffered to distil without pressure. He finds that in a copper tube, water treated in the same way produces very little or any air, and has never been able to produce it in glass vessels. While any water remains in the gun-barrel, the air is about the goodness of atmospheric air ; but as soon as all the water is distilled, there comes the common inflammable air.

As to my own health, it is as usual ; headaches frequent, listlessness, confusion of head, and inactivity constant, or nearly so. * *

I remain, dear Sir, yours affectionately,

JAMES WATT.

NOTE LEFT BY DR. PRIESTLEY AT MR. WATT'S HOUSE.

March, 1783.

Dr. Priestley has called to inform Mr. Watt, that by an improvement in his process, he now gets readily 500 ounce measures of air, quite as good as that of the atmosphere, from an ounce of water. He also collects the water that escapes through the pores of the retort, and finds that the weight of this and of the air together, are very nearly the weight of the original water. The water so collected serves for making fresh air, as well as fresh water.

EXTRACT—MR. WATT TO MR. GILBERT HAMILTON.

26th March, 1783.

* * * *

Dr. Priestley makes fixed air from dephlogisticated and inflammable air, in the following manner. He takes merc. precip. ruber. which yields only dephlogisticated air; and iron, which yields only inflammable air, and heats them together. They produce only fixed air. He puts dry dephlogisticated air and dry inflammable air into a close vessel, and kindles them by electricity. No air remains, at least if the two were pure; but he finds on the side of the vessel a quantity of water, equal in weight to the air employed.—Yours affectionately,

JAMES WATT.

EXTRACT—MR. WATT TO MR. DE LUC.

11th April, 1783.

* * * *

I have the pleasure of informing you that Dr.

Priestley, who goes to London soon, has made some more discoveries on the production of air from water.

* * * *

EXTRACT—MR. WATT TO DR. BLACK.

21st April, 1783.

[In the early part of this letter Mr. Watt acknowledges the receipt of two letters from Dr. Black, of 30th January and 13th February 1783; which have been already given. Mr. Watt again urges him to publish his discoveries. He states that Mr. De Luc had been staying for ten days with him, making experiments on latent heat; the result of which was, that the sum of the latent and sensible heat was always equal. He then continues:—]

I have not yet begun to put my sentiments into writing. I shall consider it a great honour, to have the little I have been able to add to your doctrines, published along with them. As to any share of the profit, it would be a shame for me to think of selling your doctrines, which I learnt from you; and all I can do in that way will be but a small recompense for the many obligations you have laid me under. It will give me great pleasure to see you here, and I hope you will put your proposal in practice; but let me know the time you can come, that I may be disengaged as much as possible from worldly concerns. Dr. Priestley has made many more experiments on the conversion of water into air, and I believe I have found out the cause of it; which I have put in the form of a letter to him, which will be read at the Royal Society, with his paper on the subject. It is briefly this:—1st, By reducing metals in inflammable air, he finds they absorb it, and that the residuum of ten ounces out of the hundred is still the same sort of inflammable air; therefore inflammable air is the thing called phlogiston. 2dly, When quite dry pure inflammable air, and quite dry pure dephlogisticated air, are fired by the electric spark in a close glass vessel, he finds, after the vessel is cold, a quantity of water adhering to the vessel, equal, or very nearly equal, to the weight of the whole air; and when he opens the vessel under water, or mercury, it is filled within $\frac{1}{200}$ part of its whole contents, which remainder is phlogisticated air, probably contained as an impurity in the other airs. 3dly, When he exposes to heat porous earthen retorts, previously soaked in water, or makes steam pass slowly through a red-hot tobacco pipe, the water or steam is converted into air, either entirely or in great part, according as the process is conducted. This conversion does not take place when the water is contained in metalline or glass vessels, and only in a small degree when the water is imbibed by clay inclosed in a glass vessel; and the conversion goes on much less rapidly when the earthen vessel is immersed in heated quicksilver.

In the deflagration of the inflammable and dephlogisticated airs, the airs unite with violence,—become red hot,—and, on cooling, totally disappear. The only fixed matter which remains, is *water;* and *water, light,* and *heat,* are all the products. Are we not then authorized to conclude, that water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat; and that dephlogisticated, or pure air, is composed of water deprived of its phlogiston, and united to heat and light; and if light be only a modification of heat, or a component part of phlogiston, then pure air consists of water deprived of its phlogiston and of latent heat ?

[Some farther explanations of the phenomena follow here, which it does not appear necessary to extract, as they are more fully developed in the letter to Dr. Priestley of 26th April 1783, printed in the Philosophical Transactions.]

EXTRACT—MR. WATT TO MR. GILBERT HAMILTON.

Birmingham, 22d April 1783.

* * * *

Dr. Priestley has made many discoveries lately in relation to the conversion of water into air; and I have from them made out what water is made of, and what air is made of; which theory I have given him in a letter to be read at the Royal Society, along with the accounts of his discoveries. It is briefly as follows :—

Facts.—1*st,* Pure dry dephlogisticated air and pure dry inflammable air fired together, leave no residuum, except a small quantity of water equal to their weight.

2*d,* Pure inflammable air reduces calces of metals, and is absorbed by them. The residuum, after nine-tenths was absorbed, was still inflammable air.

3*d,* All substances which produce inflammable air, are substances which contain some water firmly united to them, and have some principle which is known to attract phlogiston strongly. (Example—nitre, alum, gypsum, calces of metals, &c.)

4th, Porous earthen vessels imbibed with water, and slowly heated, produce air, if the process is well performed, equal in weight to the water.

Deductions.—Pure inflammable air is phlogiston itself.

Dephlogisticated air is water deprived of its phlogiston, and united to latent heat.

Water is dephlogisticated air deprived of part of its latent heat, and united to a large dose of phlogiston. The acid of the neutral salts take the phlogiston of the water, and convert it into something else; and the fire gives the latent heat.

* * * *

[Mr. Watt's letter to Dr. Priestley, dated 26th April 1783, gives the statement of his theory; to be read at the Royal Society, at the same time as Dr. Priestley's paper, containing the experiments upon which that theory was in great measure founded. Dr. Priestley's paper was addressed by him to Sir Joseph Banks on the 21st April 1783, and was read on the 26th June 1783. Dr. Priestley went to London about the former period, and had Mr. Watt's paper sent to him there; as appears from the following letter.]

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 26th April 1783.

* * * *

I fancy that before you receive this, you will have seen Dr. Priestley, and heard the account of his new discoveries in the air way, and of my attempt to give a reason or theory for the conversion of water into air. Lest you should not have seen him, I shall just mention what I attempt to prove from his experiments.

1*st*, That dephlogisticated air is composed of water deprived of its phlogiston, and united to latent or elementary heat and light.

2*dly*, That water is composed of pure air, deprived of a great part of its latent heat, and united to phlogiston.

3*dly*, That nitre and other salts attract the phlogiston from water; and, by the assistance of heat, convert it into air.

4*thly*, That clay vessels attract the phlogiston from water, and transmit it from particle to particle, until it comes to the outside, where they give it to the external air.

5*thly*, That air attracts phlogiston from clay, partially from the acid of nitre, and perfectly from vitriolic acid.

These seem bold propositions, but I think they follow from the present state of the experiments; and, if I were at leisure to write a book on the subject, I think I could prove that no experiment hitherto made contradicts them, and that the greater number of experiments affirm them. Since the Doctor's departure, I have observed some inaccuracies of style which I wish to correct—(if the Society should do me the honour to publish it)—and also some ambiguity concerning the decomposition of nitrous air, which I have removed, and shall send him a corrected copy in a day or two.

* * * *

EXTRACT—MR. WATT TO MR. SMEATON.

27th April 1783.

* * * *

By the help of Dr. Priestley's experiments, I have attempted to demolish two of the most ancient elements (air and water); a third, (fire), has been destroyed for some time, but in return we have made two or three more. For particulars I refer you to a letter of mine to Dr. Priestley, which he was to do me the honour to read to the Royal Society.

* * * *

MR. WATT TO DR. PRIESTLEY IN LONDON.

Birmingham, 28th April 1783.

DEAR SIR,—Having discovered some inaccuracies in language, and some inconsistencies in the theoretical essay I sent you, I have made out another copy, which I shall be obliged to you to put in the place of that formerly sent you, and to return the former to me when you return here. Dr. Withering has read it, and approves of it. I have also shewn it to Mr. Keir, who thinks it ingenious, but adheres to his former opinion, that some acid enters into the composition of air; which theory I cannot make to account for the phenomena in question. As to myself, the more I consider what I have said, I am the more satisfied with it, as I find none of the facts repugnant. I shall be glad to hear from you at your convenience, and remain, dear Sir, yours sincerely,

JAMES WATT.

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

28th April, 1783.

* * * *

Dr. Priestley, as you observe, converts water into air, and air into water, and I have found out the reason of all these wonders, and also what air is made of, and what water is made of; for they are not simple elements.—I have written a paper on the subject, and sent it with Dr. Priestley's to the Royal Society. It is too long to give you even an abstract of it, but if you will forgive me the reasoning, I will add the receipt below for making both these elements.

To make Water.—

R. Of pure air and of phlogiston Q.S., or if you wish to be very exact, of pure air one part, of phlogiston, in a fluid form, two parts, by measure. Put them into a strong glass vessel, which admits of being shut quite close; mix them, fire them with the electric spark; they will explode, and throw out their elementary heat. Give that time to escape, and you will find the water, (equal in weight to the air), adhering to the sides of the vessel. Keep it in a phial close corked for use.

To make Air.—

Take pure water Q. V., deprive it of its phlogiston by any practicable method, add elementary heat Q.S. and distil. You will obtain pure air, to be preserved as above.

The ingredients of air are water deprived of its phlogiston, and united to much elementary heat ; and the ingredients of water are pure air and phlogiston, united in a state of ignition, and deprived of much elementary heat.

Now, I have given you somewhat to ruminate upon, and my head aches much. I remain,

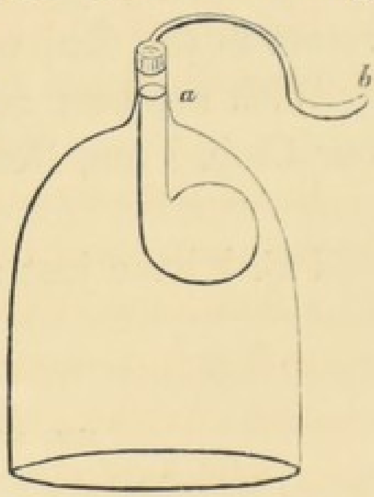
Dear Sir, your obliged friend,

JAMES WATT.

DR. PRIESTLEY TO MR. WATT.

London, 29th April 1783.

DEAR SIR,—Behold with surprise and with indignation the figure of an apparatus that has utterly ruined your beautiful hypothesis, and has rendered some weeks of my labour in working, thinking, and writing, almost useless.



In order to ascertain the effect of heating the moist clay in an earthen retort, on the *external air*, I put the retort within a glass receiver, standing in a basin of water, and with good luting made the juncture air-tight at *a*. Then throwing the heat of Mr. Parker's excellent lens upon the bulb, within the receiver, air was collected very copiously at *b*, and the water ascended within the receiver. This looked like a phlogistication of the internal air ; but the process went on till more than three-quarters of the internal air disappeared, and I believe it would all have gone farther, if the water had not almost covered the bulb of the retort.

The process then stopping, I found I had got about as much air as was missing in the receiver It was, however, a little better than the air of the atmosphere, and the remainder of the air within the receiver was a little worse, but only a mere trifle. It is, therefore, a new hydraulic engine, but on what principle it acts, I know not. It is more within your province than mine. You must convene the Club,* and give me your joint opinion.

Before this experiment I had fully satisfied Mr. Kirwan of the reality of the conversion. He, and many others, saw the simple experiment (with the retort in the fire) with astonishment.

With my best respects to Mrs. Watt, and also all our Club, I am, Dear Sir, yours sincerely,

J. PRIESTLEY.

P.S. I have just received yours.

MR. WATT TO DR. PRIESTLEY.

Birmingham, 2d May 1783.

DEAR SIR,—I received yours of the 29th to-day. 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

* The Lunar Society ; so called because the members met every month at the full of the moon. See Translation of Arago's Eloge, p. 93.—ED.

it on the other facts, and was obliged to stretch it a good deal before it would fit this experiment.

I am not, however, quite clear that even this new experiment overturns any thing; (not even that great law of Nature, which says, that all fluids fly from the side on which they are most pressed, towards that where they find least resistance.) I say, perhaps, (but I say it feebly) the air of the receiver was changed into fixed air, and absorbed by the water in the receiver. Let it be tried what happens when the solar receiver is filled with dephlogisticated air—what happens when filled with fixed air—and what with phlogisticated. Will you find these different species unchanged in the second receiver? But if, after all, this should account for the production of common air from water, where did the dephlogisticated air come from, which was produced by spirit of nitre and by vitriolic acid passing through the red-hot tobacco pipe, or the inflammable air produced from spirit of wine, and oils, or the air from the volatile alkali? Some of these, or indeed any of them, could not be got in such quantities from the atmosphere.

I maintain my hypothesis, until it shall be shewn that the water, found after the explosion of pure and inflammable air, has some other origin; nor shall I believe that air is a child of acids, or rather a modification of them, until such acids can be found after the decomposition of it. I have many experiments to propose to you to help to bring out the truth, which I think is certainly to be got at, and a fair analysis made of the two fluids. *Quære*, does the imbibed water remain in the solar receiver, or is it impelled into the other in the form of steam ?

I have read Scheele since I saw you, and found several things to confirm my hypothesis.

I shall take the first opportunity to communicate your letter to the Club, but in the meantime hope to be furnished with some more facts from you, with any philosophical news the town produces at present.

I remain, Dear Sir,

Yours sincerely,

James Watt.

MR. DE LUC TO MR. WATT.

Londres, le 8 Mai 1783.

Bien obligé, mon cher Monsieur, de votre bonne lettre du 26me Avril. Elle m'a fait grand plaisir par le succès de vos machines ;* et elle m'en auroit fait beaucoup par vos idées chimiques, si le Dr. Priestley lui-même ne croyoit pas avoir renversé, d'un seul coup, toutes ses expériences précédentes par une nouvelle, du moins quant à la conclusion qu'il *faisoit de l'Air*. "*We are undone*," me dit il, en entrant un matin dans ma chambre. Et là dessus il m'expliqua, ce que vous savez déjà sans doute, qu'ayant lutté sa cornue de terre en haut d'un récipient plein d'air, trempant dans le mercure, et ayant fait tomber sur elle le foyer de la lentille de Mr. Parker, il avoit vu *l'eau* sortir de sa cornue en dehors, et couler le long

* Rotative Steam-Engines.—Ed.

des parois du récipient, et en même tems le mercure y monter ; preuve que l'air passoit de quelque manière dans la cornue, et alloit par son col dans le Vase ou il plongeoit. Qu'au lieu de l'air atmosphérique, il avoit mis de l'air inflammable autour de sa cornue dans le récipient ; que l'eau étoit venue prendre la place de cet air, qu'il avoit recueilli pur par le col de la cornue.

Je vous marque toujours cela, mon cher Monsieur, en cas que le Dr. n'eut pas pu en écrire à Birmingham, et pour que vous y réfléchissiez de votre côté.

* * * *

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 18th May 1783.

Your kind letter of the 8th I received last Sunday, and would have answered sooner, but have been demolished for a whole week by a fever and sore throat, from both which I am now recovered.

I do not see Dr. Priestley's new experiment in the same light that he does. It does not disprove my theory; it only shows that that experiment does not require it, or rather does not admit the application of it. My assertion was simply, *that air was water deprived of its phlogiston, and united to heat;*—which I grounded on the decomposition of air by inflammation with inflammable air, the residuum, or product of which, is only water and heat : 2*dly*, on the facts, that in all cases wherein dephlogisticated air is obtained by distillation, some one of the principles has a great attraction for phlogiston, and that water is also contained as another constituent part of these substances.

The water remaining after inflammation is not in the least acid, which must be the case if the air was formed of the acid part of the substances. In most of the experiments, the substances from which the air was detached become phlogisticated, the metallic calces are reduced, and the vitriolic acid is converted into vitriolic acid air, which is known to be one of the combinations of that acid with phlogiston.

When you calcine metals in pure air, water is always produced. There are many other facts which coincide in furnishing similar proofs.

* * * *

EXTRACT—MR. WATT TO DR. BLACK.

Birmingham, 23d June 1783.

* * * *

I wrote you last month,* giving you an account of some curious experiments of Dr. Priestley's, and a theory I had formed to account for the production of dephlogisticated air ; which I supposed to be water deprived of phlogiston, and united to heat, and mentioning that I had written a short paper on this subject, to be presented to the Royal Society. Since that time I have not had the pleasure of hearing from you.

I have withdrawn my paper from the Royal Society, on account of an ugly experiment the said Dr. Priestley tried at my desire, and which renders the

* Mr. Watt alludes to his letter of 21st April 1783.—Ed.

theory useless in so far as relates to the change of water into air by means of porous earthen vessels. [Mr. Watt here enters into the details of the experiment, for which see Dr. Priestley's letter of 29th April, p. 25.] I have not given up my theory, though neither it, nor any other known one will account for this experiment.

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EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 26th June 1783.

*   *   *   *

Dr. Priestley, by using very pure nitre, has obtained 787 ounce measures of dephlogisticated air from two ounces of nitre, measure of the test with two equal measures of nitrous air, 1.25. I have examined the residuum which he sent me of a former distillation of nitre, and found that the greatest part, say four-fifths of the acid, still remained united to the alkali; but that part of it was highly phlogisticated, and could be separated in the form of nitrous acid, by the muriatic acid, or even by vinegar, neither of which would have acted upon nitre in its common state. I have distilled the nitre of magnesia, and also the calcareous nitre; and I have obtained again, as near as I could determine, all the acid, besides a quantity of pure air. The acid in these cases comes over highly phlogisticated, however much it might be freed from that principle beforehand.

*   *   *   *

EXTRACT—MR. WATT TO DR. BLACK.

Birmingham, 25th September 1783.

Dear Doctor,—I have long expected the pleasure of a letter from you, but have had none, except a few lines by the Marquis de Biancourt.* Mr. de Luc, who is here, desires his compliments to you, and has sent along with this, MM. Lavoisier and La Place's Mémoire upon heat, which is a very well written paper, though not free from objections. It is a present to you from the authors; who, I think, might have done you the justice to have mentioned your name in it; but this, and much more, you bring on yourself by not publishing your discoveries. I think, so far as I can see into the matter, that Dr. Irvine's doctrines, and Dr. Crawford's, of capacity, will fall to the ground, and your original theory of latent, or essential heat, be established.

* * * *

[Mr. de Luc paid a visit to Mr. Watt in September, October, and November, 1783,—and Mr. Watt appears then to have determined to send a revised copy of his memoir, through him, to the Royal Society. On the 25th November he writes to Mr. de Luc that he was then engaged upon it; and on the 30th November he writes, that he had sent it the day before.]

EXTRACT—MR. WATT TO MR. KIRWAN IN LONDON.

Birmingham, 26th Nov. 1783.

* * * *

I have lately tried some farther experiments on

* Quære, Liancourt ?

dephlogisticated air. I took 1 oz. pure nitre, and distilled from it, in a coated flint glass retort, 50 oz. measures of air. The air was received in 50 oz. of water, which became slightly acid. The air smelt of phlogisticated nitrous acid,—and I could not free it from the smell by washing. The residuum was alkaline, but on being dissolved in the receiving water, the mixture was nearly neutral, and became perfectly so, by the addition of 10 grains of a dilute nitrous acid—105 grains of which contained the acid of 60 grains of nitre—consequently the 10 grains contained about 2 grains of real nitrous acid, by your experiments. There was therefore 34 oz. measures* of air produced, and only 2 grains of acid lost. I attribute part of the loss to the pungent gas mixed with the air, and part to some of the alkali of the glass, which was set free by its solution in the nitre. I could not determine the loss of weight in the nitre and retort, because some of the coating stuck too fast to be got off, particularly as the retort cracked into a hundred pieces in cooling. This is the fourth experiment which has given nearly the same results; but I shall go on with some variations.

The experiments of yours which I was comparing, were those on the quantity of phlogiston in fixed air. You make it 14 per cent., and MM. Lavoisier and La Place, 9 per cent. I am now completing my paper on those subjects, at least as far as my present facts permit. I shall send it to Mr. De Luc when

* It will be seen from Mr. Watt's next letter to Mr. Kirwan, that 34 oz. measures were here a mistake for 34 grains' weight.—Ed.

done, when I shall be obliged to you to read it. I have discovered a more accurate test of alkalis and acids than Litmus; of which I shall send you some if it continues to please me.

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EXTRACT—MR. KIRWAN TO MR. WATT.

London, 29th Nov. 1783.

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As to your experiment on the decomposition of nitre I shall make some remarks. 1st, From an oz. of nitre you obtained only 50 oz. measures of air, equal to 94.75 cubic inches. But Dr. Priestley obtained, from the same quantity of nitre, from 393 to 406 oz. measures. How is this to be explained? It is probable he operated in an earthen, as you have done in a glass retort. The greater part of the nitre was therefore undecomposed in your experiment, and in effect your 50 oz. measures, if consisting of pure dephlogisticated air, weighed but 39,795 grains. You cannot be sure that the whole of the alkalized part of their residuum was saturated by 2 grains of real nitrous acid, because part of the alkali united to the silex of the glass, and, consequently, you cannot infer that the dephlogisticated air should, according to me, proceed from 2 grains of nitrous acid; but if, indeed, you had obtained after the saturation 1 oz. of pure crystallized nitre, then you might be sure of the inference; but this, I believe, will not happen. That dephlogisticated air contains a large proportion of water, I do not deny. Why you infer that 2 grains of real acid afford 34 oz. measures* of air, I do not understand. As Mr. Lavoisier does not acknowledge the existence of phlogiston, I pray you tell me why you infer, that he says fixed air contains 9 per cent. of it? It is probable he says something tantamount; but, as I am now busied about mineralogy, I do not recollect where. I shall be much obliged to you for informing me of your new test of acids and alkalis; and am, with great esteem, &c.,

R. KIRWAN.

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, Nov. 30, 1783.

* * * *

I was at Dr. Priestley's last night. He thinks, as I do, that Mr. Lavoisier, having heard some imperfect account of the paper I wrote in the Spring, has run away with the idea, and made up a memoir hastily, without any satisfactory proofs. How that may be, I cannot take on me to say; but if you will read the 47th and 48th pages of Mr. De La Place's and his Memoir on Heat, you will be convinced that they had no such ideas then, as they speak clearly of the nitrous acid being converted into air. I, therefore, put the query to you of the propriety of sending my letter to pass through their hands to be printed; for even if this theory is Mr. Lavoisier's own, I am vain enough to think that he may get some hints from my letter, which may enable him to make experiments, and to

* Mr. Watt has here written on Mr. Kirwan's letter, "This is a mistake."—ED.

improve his theory, and produce a memoir to the Academy before my letter can be printed, which may be so much superior as to eclipse my poor performance, and sink it into utter oblivion; nay, worse, I may be condemned as a plagiary, for I certainly cannot be heard in opposition to an Academician and a Financier. * * * But, after all, I may be doing Mr. Lavoisier injustice. * * * * I see it, on the one hand, so difficult to satisfy those nice chemists, and, on the other hand, so difficult to be allowed even the honour of the discovery, that I am nearly discouraged, either from publishing at all, or trying any more experiments; as it seems to be losing my labour and procuring myself disquiet. * * *

EXTRACT—MR. WATT TO MR. KIRWAN.

Birmingham, 1st Dec. 1783.

I would not delay a minute to answer such part of your objections as I can. 1*mo*, I only took from the ounce of nitre 50 ounce measures of air, in order to prevent the action of nitre on the retort, which would have been sufficient to destroy it, had I used more heat; as it was, that action was very trifling. I have no reason to think that my nitre would have yielded less air than Dr. P.'s, if the vessel could have retained it, and the heat had been raised to the same degree; the greater part of the nitre was, therefore, undecomposed. 2*do*, I allow that the alkali of the nitre did act upon the glass; but, as glass is composed of alkali as well as earth, and the earthy matter was precipitated, I rather suppose that the nitre was made more alkaline, by the addition of the alkali of that part of the glass which it decomposed. *3tio*, I did not attempt the obtaining the nitre in a crystallized form, because the quantity of water was large, and the nitrous acid, or at least part of it, was a little phlogisticated, and would have left the alkali during the evaporation. I shall, however, attempt it the next experiment I try. *4thly*, All the inference I draw from your experiment is, that the acid of 5.7 grains nitre is about two grains, and that quantity of acid is all that was wanting to saturate the nitre of my experiment, in which 34 grains weight of dephlogisticated air was produced. If I wrote 34 ounce measures, it was a mistake; I meant 34 grains; which is the weight I make the dephlogisticated air at the specific gravity of $\frac{1}{700}$ of that of water; whether that specific gravity is right or not I cannot say, as I have never weighed it. Whether M. Lavoisier acknowledges phlogiston yet, I cannot say; but in the 45th page of his and M. La Place's memoir on heat, they say that 3.3167 ounces of dephlogisticated air, formed 3.6715 of fixed air; so that to 9 parts of dephlogisticated air, was added 1 part of a certain principle furnished by the charcoal, which was the basis of fixed air. Now, I infer that this principle was no other than phlogiston. By an attentive perusal of the same passage, you will find there is $\frac{2}{3}$ of an ounce of charcoal, of which they give no account. What became of it? For only $\frac{1}{3}$ ounce entered into the composition of the fixed air, and 1 ounce was consumed.

Mr. Lavoisier has read a memoir, opening a theory very similar to mine, on the composition of water; indeed, so similar, that I cannot help suspecting he has heard of the theory I ventured to form on that subject, as I know that some notice of it was sent to France. He does not seem, however, to have been more fortunate in his proofs of it than I have been.

* * * *

EXTRACT—MR. DE LUC TO MR. WATT.

Windsor, 7th Dec. 1783.

* * * *

J'ai reçu tout à la fois votre mémoire en deux paquets, et la lettre qui l'a suivi.

* * * *

Je ne puis pas encore vous dire précisément ce que je pense des détails ; le language chimique ne m'étant pas bien familier. J'en jugerai mieux en traduisant; et, chemin faisant, je noterai les questions que je voudrois vous faire, pour ajouter quelques petits éclaircissemens aux endroits où d'autres physiciens, non chymistes pratiques, pourroient être arrétés comme moi. Mais, quant à l'ensemble, j'ose vous donner courage. Il y a un ensemble de faits, si beaux, si concluans, qui me plaisent tant,—oui tant,—que si votre système n'est pas absolument la vérité, il en est bien près; et c'est beaucoup, dans un moment comme celui-ci. J'ose espérer qu'entre nous deux nous mettrons les têtes en travail.

* * * *

EXTRACT—MR. KIRWAN TO MR. WATT.

London, 13th Dec. 1783.

* * * *

I am still of opinion that much of the alkali remains with the silex of the glass, as you know that flint glass contains only $\frac{1}{13}$th of its weight of alkali, and $\frac{6}{10}$ths of its weight of silex, which is capable of combining with much more alkali. I readily allow that the acid of 5.7 grains of nitre is only about two grains, but surely 34 grains of dephlogisticated air cannot proceed from 5.7 of nitre.

Mr. Lavoisier certainly learned your theory from Dr. Blagden, who first had it from Mr. Cavendish, and afterwards from your letter to Dr. Priestley, which he heard read, and explained the whole minutely to Mr. Lavoisier last July.* This he authorized me to tell you. As for Mr. Lavoisier's conversion of dephlogisticated air into fixed, by charcoal, it is too inaccurate to rely on. He does not tell us how good his dephlogisticated air was, nor does he take notice that charcoal itself contains in general much fixed air.

I am much obliged to you for your test liquor, and shall send for it immediately. I am, Sir, with great esteem, &c. &c.,

R. Kirwan.

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 30th Dec. 1783.

* * * *

I should have written to you before now, on the

* A mistake of Mr. Kirwan's for June.—Ed.

subject of dephlogisticated air, but, though I have tried several very laborious experiments, I have not obtained any thing more satisfactory than what I have already sent you; and think the matter, in so far as relates to its production from nitre, still extremely uncertain, and I have great doubts of the propriety of publishing any more than what is interwoven in your letters to M. De La Place. The following is an extract from a letter from Mr. Kirwan to me. [Here follows a copy of that part of Mr. Kirwan's letter of 13th December, given above, p. 38, commencing "Mr. "Lavoisier," and ending "fixed air."]

You see from the above, that it is possible for a philosopher to be disingenuous. For Mr. Lavoisier had heard of my theory before he formed his, or before he tried the experiment of burning dephlogisticated and inflammable air together, and saw the product was water. As to the proofs he pretends to give of his hypothesis, I am pretty certain they are not facts. He has, therefore, run away with a thing he does not understand. I will not imitate him in that; for if another experiment or two I mean to try do not give more certainty, I think it will be better to content myself with opening the theory, without adducing any controvertible experiments.

* * * *

EXTRACT—MR. DE LUC TO MR. WATT.

Londres, le 9e Févr., 1784.

* * * *

Je me persuade, que cette doctrine des capacités, prises pour unique cause des phénomènes de chaleur produite ou perdue, est une chimère, fondée sur des illusions.

Il en est bien autrement de votre système, mon cher ami; car au contraire, plus j'y réfléchis, plus je me persuade que vous avez trouvé la vérité, et qu'il ne faut que du tems et de la patience, pour le déterminer plus sûrement, et lever les objections. Prenez donc courage, je vous en prie; ne vous laissez pas dégoûter par les difficultés. Si vous ne trouvez pas encore des faits décisifs, aucun fait ne vous est contraire; et ce qui semble d'abord ne pas répondre à vos idées, dans les expériences que vous avez faites, peut s'expliquer de bien des manières.

*   *   *   *

Malgré ce que vous marque Mr. Kirwan, je ne saurois accuser MM. Lavoisier et La Place de vous avoir copié; non seulement parcequ'ils ne parlent point comme vous, mais parcequ'en fait, ce qu'ils disent aujourd'hui, Mr. De La Place me l'a écrit dans le mois de Juin.—Voici d'abord ce qu'ils me disoient dans une lettre du 28me; "nous avons répété, "ces jours derniers, Mr. Lavoisier et moi, devant "Mr. Blagden et plusieurs autres personnes, l'expé-"rience de Mr. Cavendish sur la conversion en eau des "airs déphlogistiqué et inflammable, par leur com-"bustion; avec cette différence, que nous les avons "fait brûler sans le secours de l'étincelle électrique, "en faisant concourir deux courants, l'un de l'air pur, "l'autre de l'air inflammable. Nous avons obtenu "de cette manière plus de $2\frac{1}{2}$ gros d'eau pure, ou "au moins qui n'avoit aucun caractère d'acidité, et "qui étoit insipide au goût; mais nous ne savons

" pas encore, si cette quantité d'eau représente le
" poids des airs consumés ; c'est une expérience à
" recommencer avec toute l'attention possible, et qui
" me paroit de la plus grande importance."

Vos *queries* sur un objet aussi obscur, et en même tems si important, auront le caractère de celles de NEWTON.

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 22d Feby. 1784.

* * * *

I must still differ from you in regard to Mr. Lavoisier's knowledge of my theory before he even made his experiments ; because, according to Mr. La Place's letter to you, Dr. Blagden was present when those gentlemen tried the experiment; and, as Dr. Blagden had not only heard of my theory, but had read with attention the paper which I drew up for the Royal Society, it was certainly natural for him to mention it ; and I can easily conceive Mr. Kirwan, or Dr. Blagden himself, writing, or saying, July for June. Of this matter you can easily satisfy yourself from Dr. Blagden. The matter is not, however, of much importance, though it somewhat takes off from the new gloss of my idea, and may with many lose me the honour of it, if it can convey any, —which I somewhat doubt of.

EXTRACT—MR. DE LUC TO MR. WATT.

Londres, le 1er. Mars, 1784.

* * * *

Mr. Cavendish a fait lire un long mémoire à la

Société Royale, où il traite à fond le sujet *de la combustion des deux airs,* par des expériences et des raisonnemens. Il est fort contraire à la doctrine des capacités ; ainsi il ne soutient surement pas ce système. Mais il est contraire aussi, à celle du *feu latent* à notre manière, parcequ'il ne conçoit la *chaleur,* que comme un mouvement dans les particules propres des corps, &c., doctrine que vous connoissez. Dans ce mémoire il nie la formation d'aucun *air fixe* dans la combustion, et soutient que celui qu'on trouve après la combustion, est sorti des substances combustibles. Le Dr. Blagden, son ami, de qui je tiens ces détails, étoit de cette opinion, malgré un autre mémoire de Mr. Kirwan, lu aussi déjà à la Société Royale, dans lequel il réfute cette partie du mémoire de Mr. Cavendish. * * * Je ne vais guère à la Société Royale, ainsi je n'ai pas ouï la lecture de ces deux mémoires ; mais j'ai demandé à Mr. Cavendish la permission de voir le sien, et je compte de voir les deux dans quelques jours ; après quoi je vous écrirai. * * * Etant ici de ma lettre, jai reçu le mémoire de Mr. Cavendish, et je l'ai lu ! ! . . . . Attendez-vous à quelque chose qui vous étonnera dès que je pourrai vous écrire. Mais ce ne pourra être que dans quelques jours ; car j'aurai beaucoup de travail à faire pour vous rendre compte de ce que j'ai lû et que je relirai. . . . . En attendant ne dites rien à personne. . . . . Je vous quitte pour y travailler ; *sans façon.*

J. A. D. L.

En bref, on expose et prouve votre système, mot pour mot, et on ne dit rien *de vous.*

EXTRACT—MR. DE LUC TO MR. WATT.

Londres, commencé le 1er Mars, terminé le 4 do. 1784.

Dans ma lettre qui va à la poste pour vous, mon cher Monsieur, je n'ai laissé en arrière qu'un article de votre dernière, et c'est celui qui regarde *le Plagiat.* Je ne puis point non plus être d'accord avec vous, sur ce que MM. Lavoisier et De La Place vous ont copié. Je conviens qu'ils le pouvoient, parceque le Dr. Blagden étoit à Paris lorsque Mr. De La Place m'écrivit la lettre dont je vous ai fait mention. Mais, je le répète, ce qu'il dit dans cette lettre, et ce qu'ils ont dit de plus dans leur mémoire postérieur; n'est point de tout votre système ; ce n'est absolument que l'expression du fait tout pur ; ainsi n'en prenez absolument aucun souci.

Mais ce qui est tout autrement clair, précis, étonnant, est le mémoire de Mr. Cavendish. Vos *propres termes,* dans votre lettre d'Avril au Dr. Priestley, donné pour quelque chose de *nouveau,* par quelqu'un qui doit connoître cette lettre, connue de tous les membres actifs de la Société Royale : du Dr. Blagden surtout, (puisqu'il dit en avoir parlé à MM. Lavoisier et De La Place), qui a eu pleine connoissance du mémoire de Mr. Cavendish avant qu'il fut lû à la Société Royale, et à sa lecture ; et qui m'en a entretenu, comme je vous le disois dans ma précédente,—moi qu'il sait être votre ami zélé. Mais gardons tout cela entre vous et moi. Nous sommes trop occupés, l'un et l'autre, pour avoir des tracasseries, et par conséquent pour entamer rien de polémique, ni de bouche, ni par écrit. Je vous réponds *d'assurer votre date;* cela me convient mieux à moi, comme *tiers,* qu'a vous-même : et pour vous, tout comme pour moi, je le ferai sans heurter personne de front ; ce seront les conséquences des faits simples, qui feront justice.

L'essentiel donc, est que vous preniez courage, sur votre propre terrein ; et je crois que le mémoire de Mr. Cavendish contribuera à vous en donner. Ce mémoire ayant été lue à la Société Royale, et étant sans doute destiné à l'impression, je ne me fais aucun scrupule de l'extraire pour vous et pour moi. J'écrirai en François ce qui ne sera qu'extrait, et en Anglois ce qui sera copié littéralement.

[Here follows a long transcript, with remarks on sundry parts of Mr. Cavendish's memoir.]

Tel est, mon cher Monsieur, l'essentiel de ce mémoire, dans lequel le fond de votre système se trouve en propres termes, quoiqu'il y manque l'addition du FEU. Maintenant, réfléchissons entre nous sur ce singulier évènement, pour ne prendre aucune résolution à la légère.

Il est encore possible que Mr. Cavendish ne croit pas vous piller, quelque probable qu'il soit qu'il le fait. Son caractère semble plaider d'abord pour la première opinion ; et voici copie d'un billet de sa part, en réponse au mien, qui semble fortifier cette idée. " Mr. " Cavendish, &c. . . . Saw Mr. Planta yester- " day, and informed him that he had no objection to " his lending the paper to Mr. de Luc, and is glad " to hear that he is preparing a work on these sub- " jects."

C'est par le Dr. Blagden qu'il sait cela ; le Dr. Blagden sait ma liaison intime avec vous ; comment l'un et l'autre, s'ils pensoient seulement à vous, en exposant ce système, verroient ils tranquillement ce mémoire passer sitôt entre mes mains ?

L'explication la plus naturelle que je puisse donner de ce paradoxe, est, que lors de votre lettre au Dr. Priestley en Avril dernier, comme elle étoit destinée à expliquer un fait prétendû, dont l'équivoque venoit d'être trouvée, on n'y fit pas attention. Mais que quelque idée vague peut en être resté dans l'esprit de Mr. Cavendish, qui ensuite aura germé et produit ce mémoire. Alors donc, il est encore plus certain, que MM. Lavoisier et De La Place ne vous ont pas pillé ; et que tout ce que le Dr. Blagden a pu leur dire là-dessus, est les procédés pour la combustion des deux airs, et l'eau qui en résultoit, sans parler de votre système. Car s'il connoissoit réellement votre système, il faudroit supposer et à lui, et à Mr. Cavendish, un caractère que personne de ma connoissance ne leur suppose.

Maintenant que faut il faire ? Il va bien sans dire, que dans mon ouvrage je ferai l'histoire de votre découverte, avec sa *date* et celles de vos autres lettres sur ce sujet ; et, si vous vous contentez de cela, je n'ai pas de doute, que vous n'ayez toute la gloire de l'invention sans autre appareil. Je vous le conseillerois presque ; vu, que dans votre position, de tirer de vos découvertes des conséquences pratiques pour votre fortune, il faut éviter de vous faire des jaloux.

Si toute fois vous vouliez que cette affaire s'éclaircit plutôt, je crois que le plus court seroit que je remisse de votre part une lettre au Chevalier Banks, par laquelle vous lui diriez, qu'apprenant que la Société

Royale est occupée des expériences sur *l'air*, vous le priez, s'il le juge à propos, d'y faire lire deux lettres ; l'une que vous écrivites au Dr. Priestley à telle date, et l'autre à moi à telle date, (celle que je dois traduire) comme ayant beaucoup de rapport au sujet traité. Je ne crois pas qu'il peut refuser cela ; et personne n'auroit à s'en plaindre.

Soit que vous preniez ce dernier parti, ou le premier, sachez, s'il vous plait, du Dr. Priestley, si votre lettre d'Avril fut lue à la société *assemblée*, ou de qui au moins elle fut connue. Je sais qu'elle fut connue; et qu'on en rit, au cause de la circonstance que *vous expliquiez la dent d'or ;* et que je dis alors, *rira bien qui rira le dernier*.

J'ai le mémoire de Mr. Kirwan. Il est fort intéressant, comme vous pensez bien ; et il n'y a rien contre *nous ;* même il est pour nous : je vous en enverrai un extrait, comme de celui de Mr. Cavendish ; mais ne parlez, s'il vous plait, ni de l'un, ni de l'autre. Seulement vous pouvez bien dire au Dr. Priestley, en lui demandant les circonstances cidessus, que les deux mémoires ont été lu, et leur sujet général. Peut-être lui-même en sait-il quelque chose, et vous en parlera-t-il le premier.

* * * *

MR. WATT TO MR. DE LUC.

Birmingham, 6th March, 1784.

Dear Sir,—You have laid me under a debt which I cannot repay, at least at present. I mean I cannot pay your two long and kind letters in like coin ;

and, perhaps, may never pay them at all. I mean, however, to be in London next week, where your demands on my person shall be answered, and to which time I must refer particulars, having much business as disagreeable, but of another nature than the plagiarism of Mr. C., pressing hard upon me. On the slight glance I have been able to give your extract of the paper, I think his theory very different from mine; which of the two is the right I cannot say; his is more likely to be so, as he has made many more experiments, and, consequently, has more facts to argue upon.

I by no means wish to make any illiberal attack on Mr. C. It is *barely* possible he may have heard nothing of my theory; but, as the Frenchman said when he found a man in bed with his wife, "*I suspect something.*"

As to what you say of making myself "des "jaloux," 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 contempt 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 favour of the public, I am not on Mr. C. or his friends; and could despise the united power of *the illustrious house of Cavendish*, as Mr. Fox calls them.

You may, perhaps, be surprised to find so much pride in my character. It does not seem very compatible with the diffidence that attends my conduct in general. I am diffident, because I am seldom certain that I am in the right, and because I pay respect to the opinions of others, where I think they may merit it. At present, *je me sens un peu blessé;* it seems hard, that in the first attempt I have made to lay any thing before the public, I should be thus anticipated. It will make me cautious how I take the trouble of preparing any thing for them another time.

I defer coming to any resolution till I see you; but, at present, I think reading the letters at the Royal Society to be the proper step. I ask your pardon for the egotism of this letter, and remain,

Most truly yours,

JAMES WATT.

[Mr. Watt at this time, or in the following week, went to London, and saw Sir Joseph Banks. All that can be collected of what passed must be deduced from the following letters.]

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 4th April, 1784.

* * * *

Sir Joseph Banks called on me in London, on Monday or Tuesday, and left a note, asking me to have my letters on Air read to the Society, and promising to take care there should be no mistake. In the very civil manner in which he has requested it I cannot avoid complying with it, if they can be published in the volume now in the press, or at least the first of them, with some proper notes which I shall transmit. I, however, leave the affair wholly to you, and beg you would call and settle it with him. If you give the first letter to be read immediately, please alter the phrase where, speaking of the composition of nitrous air, I say, "I suppose it "to be nitrous acid super-saturated with phlogiston," to nitrous acid *not fully saturated* with phlogiston.

I shall with first possible convenience make the necessary alterations on the second letter, so as to make it follow the first properly, and add some explanatory notes concerning the processes; still retaining the original form of a letter to you.

* * * *

EXTRACT—MR. DE LUC TO MR. WATT.

Londres, le 10me Avril, 1784.

* * * *

J'ai vu le Chevalier Banks au sujet du billet qu'il avoit laissé pour vous : il ne m'a pas paru qu'il attachât *pour lui* aucun intérêt à la lecture de ces lettres, mais seulement qu'il les feroit surement lire si vous le désirez, disant positivement *que cela dépend de vous*. Quant à la condition de les insérer dans le premier volume qui paroîtra, vous savez que cela dépend du Comité, et non pas de lui. Ainsi, faites exactement ce que vous jugerez à propos, et parlez lui en, en lui envoyant ce que vous avez dessein de lui envoyer sur *le Test*. Mais si vous souhaitez que ces lettres soient lues, envoyez moi d'avance la nouvelle édition de celle que vous m'aviez écrite *le* 26. *Novem-*

bre, en y mettant la même date ; afin que la traduction que j'en ferai, soit d'accord avec ce qui sera lu à la société. J'ai corrigé la phrase dans la lettre au Dr. Priestley.

* * * *

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, April 12th, 1784.

* * * *

In relation to Sir Joseph Banks, he wants the paper to be read, not, as you observe, because he is attached to me, but because he feels as a slight put upon the Society the withdrawing it ; and perhaps thinks his own honour a little called in question, which I do not wish him to think, as he has always behaved in a friendly manner towards us.

For my own part, I would rather that the matter had been left to take its course in your publication ; but, after the reading of this paper of Mr. Cavendish's, and being civilly requested to publish in the same channel, I think it would savour a little of resentment and cowardice to decline it any farther.

I know very well that the insertion depends on the Committee, but he can influence them ; and if he does not, there is nothing lost. I have still my remedy. At all events, I shall certainly send the letter to yourself through your own hands, and, I assure you, I should have been much better pleased that you had been the President and members of the Society who should publish it ; but circumstances compel me to give it to the other, and I hope it will answer your end as well, after they have had their will of it.

* * * *

MR. WATT TO SIR JOSEPH BANKS.

Birmingham, 12th April, 1784.

SIR,—I intended to have done myself the honour of writing to you sooner, but caught cold in my journey home; which, with a quantity of business which had fallen behind in my absence, has prevented me from writing some necessary explanations of the method of conducting the experiments I made last summer, on dephlogisticated air, an account of which is contained in my letter to Mr. De Luc of November 26th, which I intend shall be soon laid before you.

I desired Mr. De Luc to do me the favour to return to you my letter to Dr. Priestley on that subject, begging the favour of you to present it to the Royal Society, and to inform them of my reasons for withdrawing it last year; which were, in the first place, my having attempted in that letter to account for the (apparent) conversion of water into air, by exposing it to heat in porous earthen vessels; which Dr. Priestley soon after discovered to be no real conversion, but an exchange of air for water or steam: and, secondly, my being informed that that theory was considered too bold, and not sufficiently supported by facts. These reasons made me think it prudent to delay the publication, until I should have considered it more maturely, and have made some experiments to determine the truth, or falsehood of it. I have since that time made several experiments, (an account of which you will find in my letter to Mr. De Luc,) and have considered the theory in every view which occurred to me, without being able to find any fallacy in it; and as similar theories have since been, as I am informed, supported by philosophers of first-rate abilities, the second objection seems to be removed. I hope, therefore, that the Royal Society will excuse my troubling them with laying before them my letter to Dr. Priestley unaltered, and also that to Mr. De Luc, which contains some additional reasoning, and an account of some of the experiments I have made; and that they will also excuse the defects of my style, which must naturally be concluded to savour more of the mechanic than of the philosopher.

It will add much to the obligations I have already received from you, if you will, as soon as you judge it proper, present my letter to Dr. Priestley to the Society; and, as soon as I get the postscript to the letter to Mr. De Luc finished, I shall beg the favour of him to send it to you.

Mr. Boulton joins in presenting our respectful compliments to you; and I remain, with much respect and esteem, &c.

James Watt.

SIR JOSEPH BANKS TO MR. WATT.

Soho Square, London, 15th April, 1784.

Dear Sir,—On the receipt of your favor, I wrote immediately to Mr. De Luc, requesting him to deliver to me your letter to Dr. Priestley. If I receive it before next Thursday, (the day on which the Royal Society resume their meetings,) I will certainly present it to them, either at that or their next meeting.

I beg to thank you for your intention of communicating to them your letter to Mr. De Luc, concerning the method you have taken, of conducting your experiments on dephlogisticated air ; and venture, at the same time, to assure you, that the communications you are pleased to make, will ever be welcome to that body, as long as I have the honour to preside over it. The sooner I receive it, the better I shall like it, as I wish to have both your letters appear in the next volume of the Philosophical Transactions.

I beg my best compliments to Mr. Boulton, and that you will believe me, your faithful servant,

JOS. BANKS.

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, April 17th, 1784.

* * * *

I have just now received a letter from Sir Joseph Banks, wherein he says, that in consequence of my last, he had written to you for the letter to Dr. Priestley ; and that if he received it before Thursday next, he would certainly present it to the Society, either that day, or at their next meeting. He also promises to use his endeavours, to have both the letters published in the next volume of the Transactions. I have not been able to finish the postscript, but have added some notes, and have made some alterations on the first and last page of the letter, which I conceived to be necessary in the present circumstances, and to make it more suitable to the place where it is now to appear. The note on the left hand page, relating to Mr. Kirwan, I have left loose, because I am not quite certain what it was he said about inflammable air, and have not the volume of the Transactions wherein he mentions it. I think it is either the last, or the last but one.

If, on examination, you find I am right, leave it as it is; if not, take it away. It is in the same paper wherein he treats of the quantity of phlogiston in fixed and nitrous airs. I should be sorry you should take the trouble of making an entire fresh translation; I see no need for it, and I think you need not publish both the letters. It may suffice if you publish the second, and mark by commas (") the passages which formed part of the first letter, after giving the history of that letter. However, do as you think proper; I am sure you have my reputation in the matter more at heart than I have myself, and it vexes me exceedingly to cause you so much trouble.

I should have sent the postscript, but a headache yesterday disabled me, and to-morrow I must set out for Shropshire, from which I shall not return for a week at least. As soon as I return, I shall finish and send you the postscript, in the form of a letter of the present date. Meanwhile, I shall thank you to forward the new copy of the letter, which I send by to-morrow's coach, to Sir Joseph Banks, as soon as you have made the necessary alterations and additions to the copy you have. I have mentioned to Sir Joseph that there are a few alterations, and where they are, and have told him that you will show him the original letter, if doubts should arise concerning the date of any part of it; but shall be obliged to you, in such case, to take care they do not read or print the wrong copy.

* * * *

MR. WATT TO SIR JOSEPH BANKS.

Birmingham, 17th April, 1784.

Dear Sir,—I have just received your obliging favour of the 15th. I have not been able yet to finish the postscript to my letter of 26th November to Mr. De Luc, and shall be obliged to delay it for a week, as I shall be absent on a journey into Shropshire. I have, however, revised the letter itself, and by this post send a corrected copy to him, which he will deliver to you. The principal alterations I have made are,—the retrenching some superfluous phrases in the first page, and some part at the end of the last page, which was complimentary to MM. Lavoisier and De La Place, to the former of whom I certainly owe nothing. I have also added some notes, on the left hand pages; which, being in my own hand-writing, are sufficiently distinguished. I thought it right to apprize you of these alterations, lest it should be said by anybody, that the letter was fabricated at a later date than it bears. If anything of that kind should be started, Mr. De Luc can produce the original, in my own handwriting, which can be compared with this present copy. Mr. Kirwan also has a copy, which he took from one I lent him when in town.

As I have not been able to finish the postscript in time to add it to the letter, I mean to write it in the form of an explanatory letter, which may follow the other at any date, and it shall be my first care after I return from my journey. Indeed, I should have finished it yesterday, but was seized with an unlucky headache, which prevented me.

I cannot sufficiently thank you for the trouble you take in this matter, and beg you will believe me to remain, with due respect, dear Sir,

Your most obliged humble Servant,
JAMES WATT.

SIR JOSEPH BANKS TO MR. WATT.

Soho Square, London, 23d April, 1784.

DEAR SIR,—Your letter to Dr. Priestley of April 21, 1783, was read to the Royal Society last night. Yours to Mr. De Luc I have received, and shall bring it into reading as soon as I can do it. Probably, on Thursday, May 6th; of which I give you this notice, that you may, if convenient, send me the postscript in time to follow in immediate succession.

A paper of Dr. Withering's was also read, which the Society seemed to approve much. It contained experiments on various kinds of Terra Ponderosa. Dr. Priestley is here, and in good health and spirits. How much the Royal Society, and the world at large, are indebted in point of science to the town of Birmingham, I need not declare, after mentioning *him*. That you are at last induced to make it the conveyance of your discoveries, gives, I frankly confess, no little pleasure to

Your faithful and obedient Servant,
JOS. BANKS.

MR. WATT TO SIR JOSEPH BANKS.

Birmingham, 2d May, 1784.

DEAR SIR,—I received your very obliging information, of my letter to Dr. Priestley having been read before the Royal Society, and have this day sent the sequel of the letter to Mr. De Luc to him, with a desire that he would send it to you as soon as he could.

From my late absences from home, and the necessary attention to business since I returned, it is but a hasty compilation; and, if I had not judged the few things it contained, necessary to be explained to most of those who may be disposed to try experiments on the same subject, and that, therefore, it should attend the former letters, I should not have sent it until I had been able to put it into a better dress. I must, therefore, beg your and the Royal Society's pardon for its defects, and hope your and their excuse for troubling you so much with my ideas on these subjects, —I remain, with great esteem and respect, dear Sir,

Your much obliged and obedient humble Servant,

James Watt.

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 2d May, 1784.

Dear Sir,—I send you enclosed the sequel to my letter on dephlogisticated air; which, after all the delay, is hastily and badly composed. The fact is, that the subject begins now to wear out of my mind, and I have not time to refresh my memory by fresh experiments, as I have had no leisure hours since I saw you. * * * * I am hurried to be in time for the packet, so must conclude with begging you to send the enclosed to Sir Joseph as soon as you can, as he advises me he means to bring forward the other letter to be read on Thursday next.

* * * *

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'authentiquer 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.

* * * *

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

Birmingham, 15th May, 1784.

* * * *

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. Lavoisier'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 experi-
" ments, begged that the reading might be delayed.
" The letter, therefore, remained in the custody of
" the President until —— ; when, at the author's re-
" quest, 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 dephlogisti-
" cated 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.

* * * *

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.

* * * *

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.

* * * *

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, June 6th, 1784.

* * * *

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.

* * * *

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 dephlogisticated 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 avocations, prevented me from sitting close at it, or thinking continuedly 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.

* * * *

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.

* * * *

EXTRACT—MR. WATT TO MR. DE LUC.

Birmingham, 27th June 1786.

* * * *

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.

* * * *

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. *

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 versâ*, 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.

APPENDIX.

APPENDIX.

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 mat-"ter." (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 con-"taining 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 aërial 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 toge-"ther certain proportions of pure dry dephlogisti-"cated 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 fol-"lowed 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 $\frac{1}{200}$dth 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 produced, 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 expul-"sion 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 parti-"cles, 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 opi-"nion, 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 become 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 become 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 bason, 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 phlo- " giston 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 subtile 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 inflam-"mable 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 completely 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 conclude, 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 decomposed by burning phosphorus, melted 68,634 ounces of ice; and as, according to another of their experiments, ice, upon being melted, absorbs 135° of heat, by Fahrenheit's scale, each ounce of air gave out $68{,}634 \times 135° = 9265°{,}590$; that is to say, a quantity 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 $9265\frac{1}{2}°$: a surprising quantity! (It is to be understood 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; consequently we have $29{,}547 \times 135° = 3988°{,}845$, 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 ounces 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 ounces 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 dimen-* "*sions in proportion to the quantity of heat they con-* "*tain;*" 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 SUBSEQUENT 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 deficience 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 deficience 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 evaporation, 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 bason 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 $87\frac{1}{2}$ 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 $87\frac{1}{2}$ 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 inside 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.
	1,241	,686	1,555	,055	,0
1	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 opening 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 Ingenhousz 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 $\frac{1}{17}$th of the dephlogisticated air employed, or $\frac{1}{50}$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 $\frac{1}{30}$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 $\frac{1}{30}$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 converted 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 cornea 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 hypothesis; 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 LAVOISIER.*

Lû le 21 Avril 1784.

DEPUIS qu'on connoît l'expérience dans laquelle un mélange d'air inflammable et d'air déphlogistiqué, 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 constituans. 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 aériformes 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 seroit encore sujette 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 déphlogistiqué 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 aériforme, qui au bout de quelques jours devint assez abondant pour en essayer la combustion, et le fer, calciné alors, annonçoit une absorption d'air déphlogistiqué, 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 petitesse 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 déphlogistiqué 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 déphlogistiqué, tous les phénomènes qui caractérisent 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 démêle dans l'air inflammable ordinaire, celui-ci se rapprochoit infiniment 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 vaisseaux 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 rallentissement 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 déphlogistiqué qu'il devoit enlever à l'éau, 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 déphlogistiqué, 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 déphlogistiqué 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 operations de ce genre, et l'on sait d'ailleurs que ces dernières ne sont plus inperméables 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 cherchames 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 cherchames 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 interposames 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 déphlogistiqué, et formoit ainsi sur un tiers de ligne d'épaisseur une doublure lisse et brillante, sur laquelle le burin ni la lime ne mordoient 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 déphlogistiqué 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 connoissances 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 peser 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 déphlogistiqué. 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 principes, 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, parce 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 déphlogistiqué, 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 calciné 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 concours 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 déphlogistiqué 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 déphlogistiqué, 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 déphlogistiqué, puisqu'il enlève celui-ci au fer, pour le ramener à l'état métallique, nous en conclumes que le charbon étoit à plus forte raison propre à décomposer l'eau, et qu'il devoit brûler sans le concours de l'air, dès qu'on lui appliqueroit 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 déphlogistiqué de l'eau devoit produire de l'air fixe, et l'air inflammable de l'eau devoit ainsi en être mêlé en grande quantité.

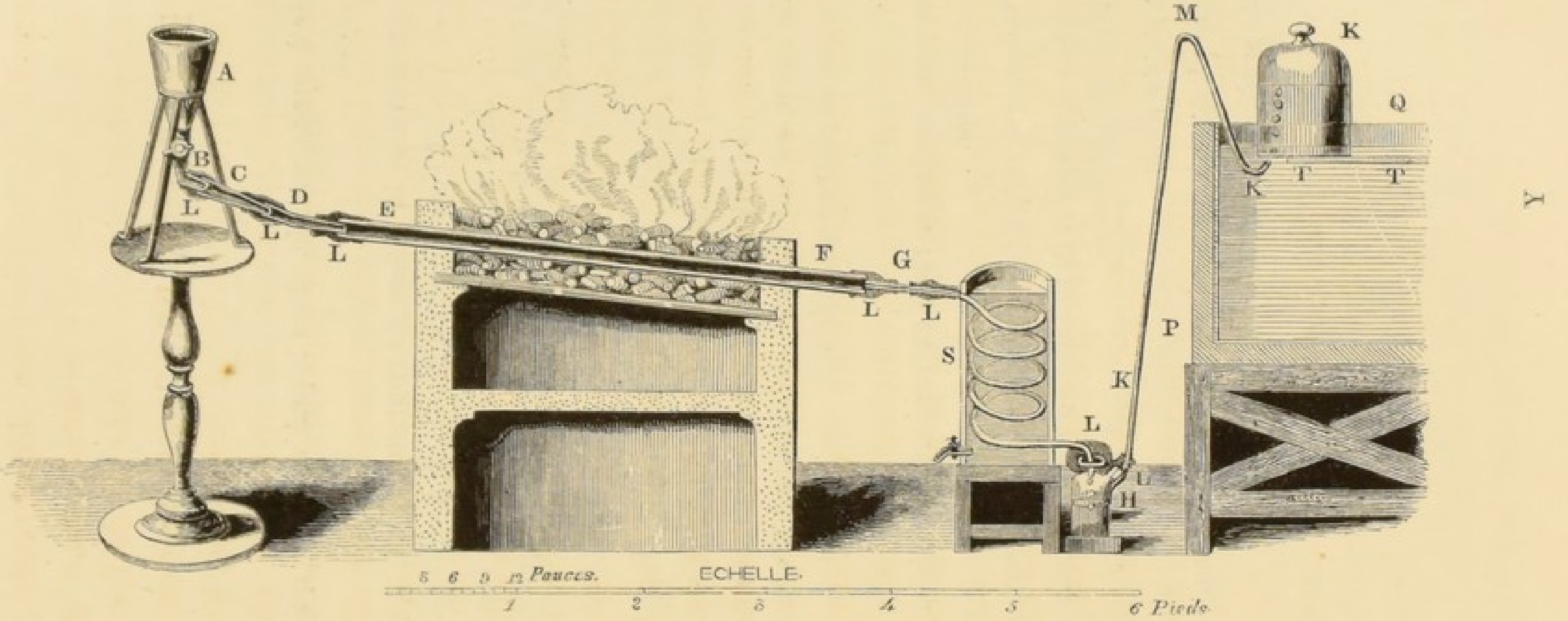
Nous mimes donc dans notre appareil quatre gros et quinze grains de charbon préparé, comme nous l'avons dit plus haut, et nous procédames d'ailleurs comme dans les autres expériences ; celle-ci dissipa deux onces trois gros d'eau, qui avec le charbon composoient un total de près de trois onces, et nous 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 pintes 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 déphlogistiqué 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 élémens 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 annuller, 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, contenoit 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 vîtesse 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]



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EXPLICATION DES FIGURES.

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 SUSCEPTIBLE 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 aériforme, *air inflammable aqueux;* et lorsqu'il sera engagé dans quelque combinaison, *principe inflammable*

* Ce Mémoire a été lû 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 lû 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 élever 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 á 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'etoit 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 connoissance 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 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 louchir 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 bouchames la bouteille avec un bouchon [471] de liége, 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 continuames 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 attribuames à quelques portions d'air commun qui sans doute étoit rentré, éteignit la flamme et mit fin à l'experience.

Nous répétames 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 surprirent 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 longtemps 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, donnoient 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 laissames 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 rougissoit nullement la teinture de tournesol ; elle ne verdissoit pas le sirop de violettes ; elle ne précipitoit 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'écarter 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
Total, . . .	1,00000000

[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
Total, . . .	16	0	0

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
Total, . . .	49240,36

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 eût 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'etoit 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, donnoit 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 expérien-
" ces 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 décom-
" pose donc, et son principe inflammable se développe
" sous forme d'air : il suivoit de-là que si par la com-
" bustion on combinoit de nouveau ce même principe
" avec l'air vital, on reproduiroit l'eau qui s'est dé-
" composée ; 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 ré-
" sulte 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 égale-
" ment 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 permettoient 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 dégageoit 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 oxygine.

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 confirmèrent de plus en plus dans l'opinion où j'étois, que l'eau êtoit un corps composé : voici la manière 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 disparoître 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, disparoît : 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 déphlogistiqué, 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 pneumato-chimique : 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 dûe 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
TOTAL, ...	$58\frac{3}{4}$

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 $\frac{3}{10}$ 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 *minium*, et dont la quantité monte à, . .	1	12,00
TOTAL, ...	4	60,00

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
TOTAL, ...	6	0	0

COMPOSITION DU MINIUM PAR QUINTAL.

	livres.
Plomb, ...	89,9306
Air fixe tout formé, ...	2,4306
Principe oxygine, ...	7,6388
Total, ...	100,0000

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
Total, ...	111,19685

[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
Total, ...	100,000

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 $\frac{2}{8}$.

	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, . .	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 defaut 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 diminutions de poids, n'eût pas cherché a 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'a 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 complétement 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 connoissances sur la décomposition et la recomposition de l'eau, lorsque nous

nous trouvames 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 fumes chargés par l'Académie, d'après les ordres du Roi, pour la perfection des machines aérostatiques, nous conduisoit 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 résolumes 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 pensames 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°. parce que l'affinité du fer pour le principe oxygine, augmente à mesure qu'il est plus échauffé ; 2°. 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 pouces 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, afin 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'extrémite la plus élevée du canon, tandis que l'extrémité inférieure répondoit 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 pneumato-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-cinquièmes 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 éntrée, 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 fournissoit 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 minces roulées, soit en petites barres tournées en élice ; 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 pneumato-chimique ; nous faisions rougir les matières sur lesquelles 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 différentes 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 Sennebier, 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 trés-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 douter 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 Alchimistes 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 INFLAMMABLE 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 déphlogistiqué 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 entroient 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 elastiques, 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 déphlogistiqué 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 répétés 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 dégageât 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 déphlogistiqué avec les dernières de gaz nitreux, j'ai obtenu l'air déphlogistiqué 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 déphlogistiqué 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 I 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'arrêtoit 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 pintes $\frac{27}{48}$ de gaz déphlogistiqué pesoient,	4	13
12 pintes $\frac{38}{48}$ d'air atmosphérique, . .	3	$56\frac{1}{2}$
12 pintes $\frac{41}{48}$ d'air inflammable, . .	0	$39\frac{1}{64}$

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.		
	Grains.	Onces.	Gros.	Grains.
Air déphlogistiqué,	$23\frac{193}{201}$	1	3	67,36
Air atmosphérique,	$21\frac{93}{307}$	1	2	44,03
Air inflammable,	$3\frac{87}{2468}$	0	1	36,86

Pour produire l'inflammation de l'air inflammable et de l'air déphlogistiqué 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 déphlogistiqué, l'autre à l'air inflammable, étoient ouverts par en bas, dans le haut ils communiquoient, par des tuyaux de métal garnis des robinets I et K, à un ballon 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 L, établissoit la communication du ballon à une excellente machine pneumatique 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 déphlogistiqué dans le bocal H, j'ai ouvert les robinets L et 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 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 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 déphlogistiqué, 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 premiére explosion. J'ai laissé entrer une seconde fois un douzième d'air déphlogistiqué, 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 déphlogistiqué, 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 $\frac{91}{144}$ d'air inflammable
Et 74 pintes $\frac{9}{16}$ d'air déphlogistiqué.

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

	onces.	gros.	grains.
Pour l'air inflammable, . .	0	6	10,03
Pour l'air déphlogistiqué, . .	3	0	58,53
TOTAL, . . .	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 trés-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°. 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°. en refroidissant de cette manière le ballon, le fluide qu'il contenoit perdoit sa transparence et présentoit un brouillard très-épais qui disparoissoit 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'excitoit l'inflammation : 3°. dans les commencemens de chaque suite d'explosions les étincelles 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

	onces.	gros.	grains.
difference, ...	3	2	45,1
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
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 étre 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 déphlogistiqué 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 coërcibles ainsi qu'elles le sont dans les fluides [87] 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 déphlogistiqué, 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. Ingenhouz. 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 poëles, 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

2 E

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

En effet, il est confirmé par une foule d'observations que le mélange du gaz inflammable et du gaz déphlogistiqué 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 déphlogistiqué, 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.

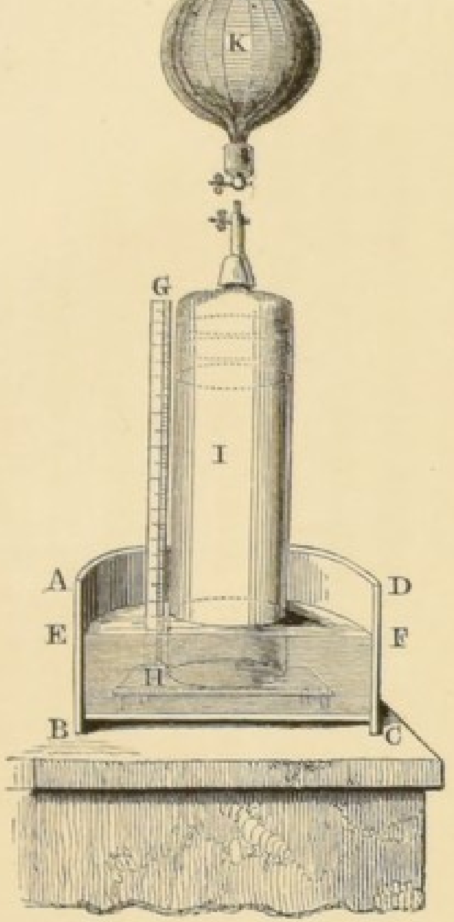

Fig. 1.

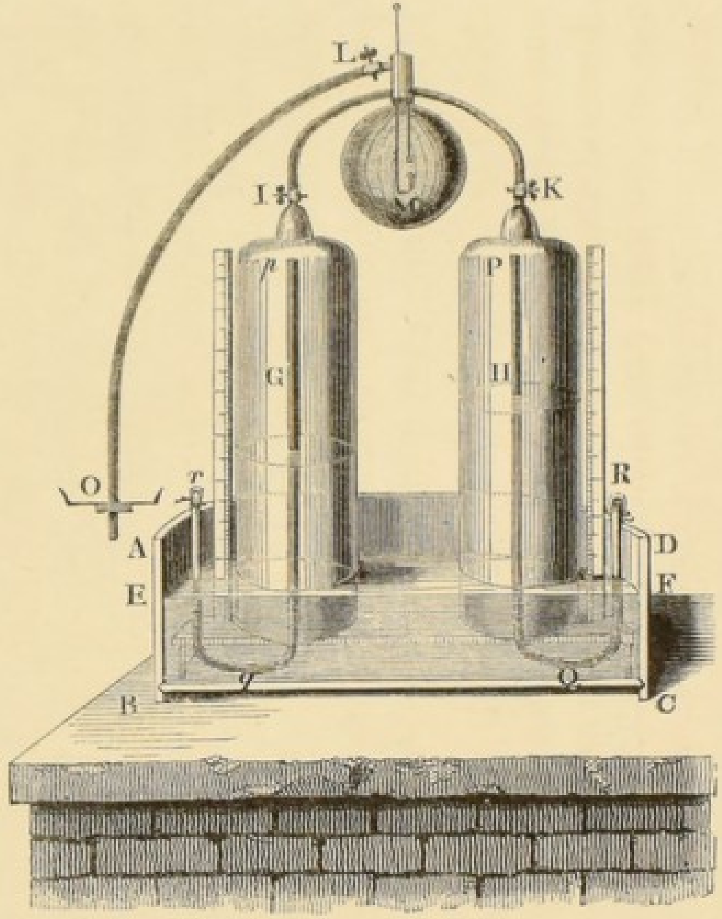

Fig. 2.

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, "Systême 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 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?*"*

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, "Ex- " periments 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 unne- " 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 ob- " tained a very perceptible quantity of water, [une " quantité d'eau très sensible.]"*

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,† 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 com- " municated 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 versâ*, that water was dephlogis- " ticated 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 dis-" coveries 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 con-" 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 in-" terest, 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 conseil-" lerai 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 Ora- " culis 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 en " 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 esté 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 Turcs! 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 Sçavant, 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 Libavïus, ramasse tout ce qui avoit esté dit de la " dent, et y ajoûte 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 ΤΟ ΔΙΟΤΙ prius quaerant, quàm ΤΟ ΟΤΙ sit manifestum, et " de re ipsâ 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, differed 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 BROUGHAM, 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 balancing 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. Lavoisier, 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 mentioned 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. Dė 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 apprize 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 aërial 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 con-" sider 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 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," (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 dephlo-* " *gisticated 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 in-* " *flammable 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.*

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. Arago.]

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'archévèque 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 détona- " tion 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 2e partie des Transactions Philosophiques de 1783 :—

" In order to judge more accurately of the quan- " tity of water so deposited, and to compare it with " the weight of the air decomposed, *I carefully* weigh- " ed 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 pré- " 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 ab- " sorbée 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 á 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 déphlogistiqué* 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 perma-
" nents et très dissemblables. Ces deux gaz, les uns
" les appelaient air pur et air inflammable ; d'autres
" air déphlogistiqué et phlogistique ; d'autres, enfin,
" oxygène et hydrogène. Par la combinaison de l'air
" déphlogistiqué 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 com-
" posa d'air déphlogistiqué et de phlogistique. Le
" chimiste qui tira cette conséquence, pouvait avoir
" de fausses idées sur la nature intime du phlogisti-
" que, sans que cela jetât aucune incertitude sur le
" mérite de sa première découverte. Aujourd'hui
" même a'-t-on *mathématiquement démontré* que l'hy-
" drogène (ou le phlogistique) est un corps élémen-
" taire ; 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 ex-
" periments 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 connoissance, " 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 ar- " rêtées," dit M. Dumas, " que je désire voir ma décla- " ration 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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