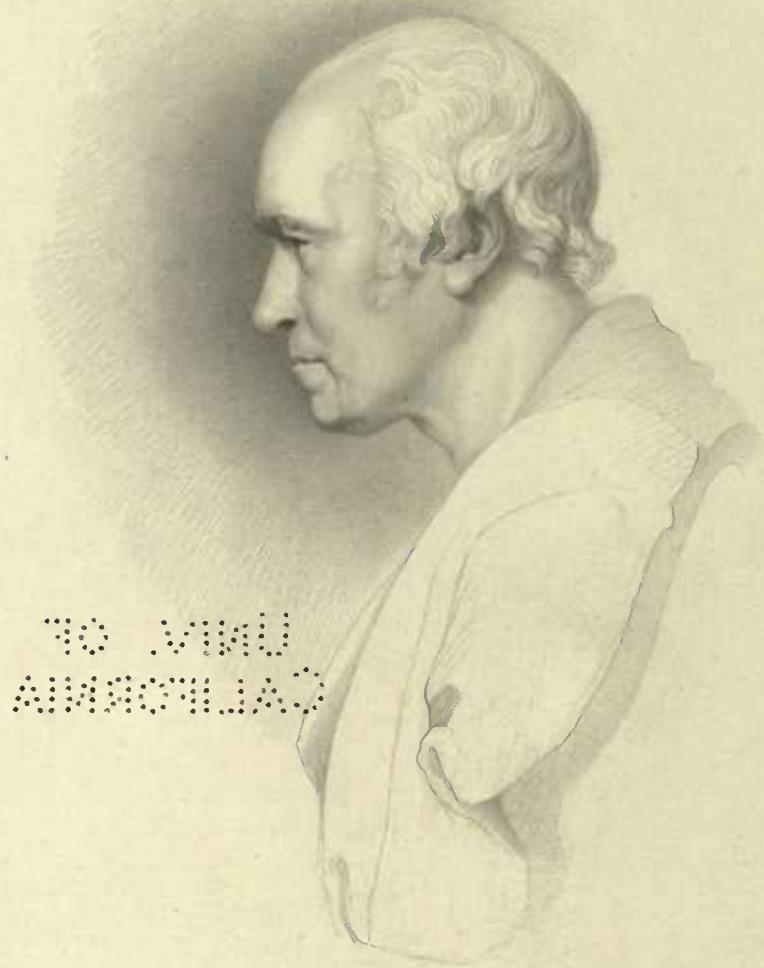


**THE DISCOVERY
OF
THE COMPOSITION OF WATER.**

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James Watt

LITERATURE OF
CALIFORNIA.

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.

LONDON: JOHN MURRAY.

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ДО КОМПАНИЙ

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МЕРКИ МО. ЗОВІДНОГО ДІЛУ

ДІЛУ ВІДПОВІДЬ ТОГО, ЧО ПРОДУКТИ

ОДНОГЛАСНОГО ПРИЧІСТЯ БЫЛИ

ПРИДІЛЕНІ



ДІЛУ ВІДПОВІДЬ ТОГО, ЧО ПРОДУКТИ
ОДНОГЛАСНОГО ПРИЧІСТЯ БЫЛИ
ПРИДІЛЕНІ

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Linen of
Cavendish

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 states-

men 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 con-

cern 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 Chaneellor, 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 diserimination 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 Gracee 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 nothing 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 obscurities 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 estimate 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 Elogie, 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 ap-

peared 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 *profound 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 acquirements placed him next, if not superior, to Newton;"† 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 philosopher in the most exalted sense of the word, who never could be satisfied with a conjectural knowledge 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 Elogie, 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, 18th 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 siphon, 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 siphon.
" 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 phlogistized ;” 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 Lue, 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 referred 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 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

* 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 intimation 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 dephlogisticated air is only water deprived of phlogiston.” 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 sup- position 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 aërisome 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 aërisome 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 philosophers 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 solu-

tion, 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 Mémoirs 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 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."*

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 elementary 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 repugnant."

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 Academy 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 inflammable 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 disprove 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 inflammable 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 considered 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 "respect 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. Caven-dish 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 members” 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, “ informed 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 analysis 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, perfectly 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 possible 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 for- “ tune, 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 Caven- " dish, 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 Elogie, 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 remarks, intend to cast any imputation on the literary probity of those illustrious philosophers ; 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,”* 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

* Elogie 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 versa*, 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 claimis 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 “dephlogisticated 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 recollect ed, 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 sufficient 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 deery, 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 serupulously 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 "constantly 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 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 *I always found*, *as near as I could judge, the weight of the decomposed 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 inflammable 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 inflammable 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.: "Previous to Dr. Priestley's making these experiments, "Mr. Kirwan had proved, by very ingenious deductions from other facts, that *inflammable air was, in all probability, the real phlogiston in an aerial form.* These arguments were perfectly convincing "to me."†

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*

* Elogie of Watt, p. 167

† Phil. Trans. 1784, p. 331.

“reduced the calces to metals;”—“the inflammable air being supposed to be wholly phlogiston;”—“inflammable air or phlogiston;”—“it is worthy of inquiry whether the greater part of the heat let loose was not contained in the phlogiston or inflammable 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 inflammable 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 lan-“ guage 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 in-*“ *flammable air* ; and when it is engaged in any com-“ bination, *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 Mémoire 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 undeniably 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 Cavendish’s communication of his ‘conclusive’ experiments 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 éclaircissement 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 aërifluid 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 philosophy, 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, with-

out 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 comparison of dates, that your father was the first to interpret 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, continues 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, 8th 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 obtained 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 moisture, 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 immediately follows the conclusion, 'that water is composed of dephlogisticated air and phlogiston,' (a term then used as synonymous 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 admirable 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 important 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 thoroughly 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 experiments, without any knowledge of Mr. Watt's earlier 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 adjudication 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 documentary evidence you submitted to him, impressed with a clear conviction that Mr. Watt was the first to interpret justly the experiment of the synthetic formation of water, and must be regarded as the discoverer 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 retraction, 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 convincing"—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 posi-
tively 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 Phi-
losophical Transactions, that before it was so delivered
to the President, it had been shown to several mem-
bers of the Royal Society. And as Blagden’s letter
to Crell, in which he distinctly admits his own know-
ledge 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 empha-
tic 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 sin-
gularly 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 en-
tirely 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 respec-
tive theories. That this purpose was in great mea-
sure 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 discoverer 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 ascertained ; 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 scientific document connected with this controversy ;"—

* Edin. Review, vol. iii. p. 11. ‡ Vol. ii. p. 158 ; vol. ii. p. 111.

† Vol. i. p. 577 ; vol. ii. p. 109. § 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 phlogiston in an aerial 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 phlogiston 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 composed 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 composed 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 implying 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 constitute 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 commonly 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 uncontested, placed on record in the registers of the most learned body in the kingdom, and uncontra-dicted during the lives of any of the parties,—while it is by no means our wish to lessen the high repu-

tation 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 exercised a careful and scrupulous self-denial, and was

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

" anxious not to appear, even by accident, as appropriating 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 prizes 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 beneficial 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.’ ”

S U M M A R Y

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.

JOURNAL OF
CALIFORNIA.

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 discovery, 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 plea-

sure 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 materi-

als, 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 1763.

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,

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 retraction, 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 its 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 experi-

ments 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 mak-

ing 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.

C

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 $\frac{1}{10}$ 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.

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

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

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

4thly, 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.

5thly, 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 re-

pugnant. 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 atmospheric.

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

inbibed 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, tremplant 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 : *2dly*, 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.

* * * *

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, Lianeourt ?

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.

* * * *

EXTRACT—MR. KIRWAN TO MR. WATT.

London, 29th Nov. 1783.

* * * *

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 éclaircissements aux endroits où d'autres physiciens, non chymistes pratiques, pourroient être arrêtés 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 $\frac{1}{5}$ th of its weight of alkali, and $\frac{1}{6}$ 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.

* * * * *

Londres, le 9^e 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 marquez 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 déphlogistique 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 1^{er}. 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 connaissez. 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 1^{er} 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, (puisque il dit en avoir parlé à MM. Lavoisier et De La Place), qui a eu pleine connaissance 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 terrain ; 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 ; com-

ment 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 done, 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 écrivîtes 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 plaît, 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 parlcz, s'il vous plaît, ni de l'un, ni de l'autre. Seulement vous pouvez bien dire au Dr. Priestley, en lui demandant les circonstances ci-dessus, 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.

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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
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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.

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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 paroitra, 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.

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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.

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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 Trans-

actions 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.

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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 apprise 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 Lue 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.

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