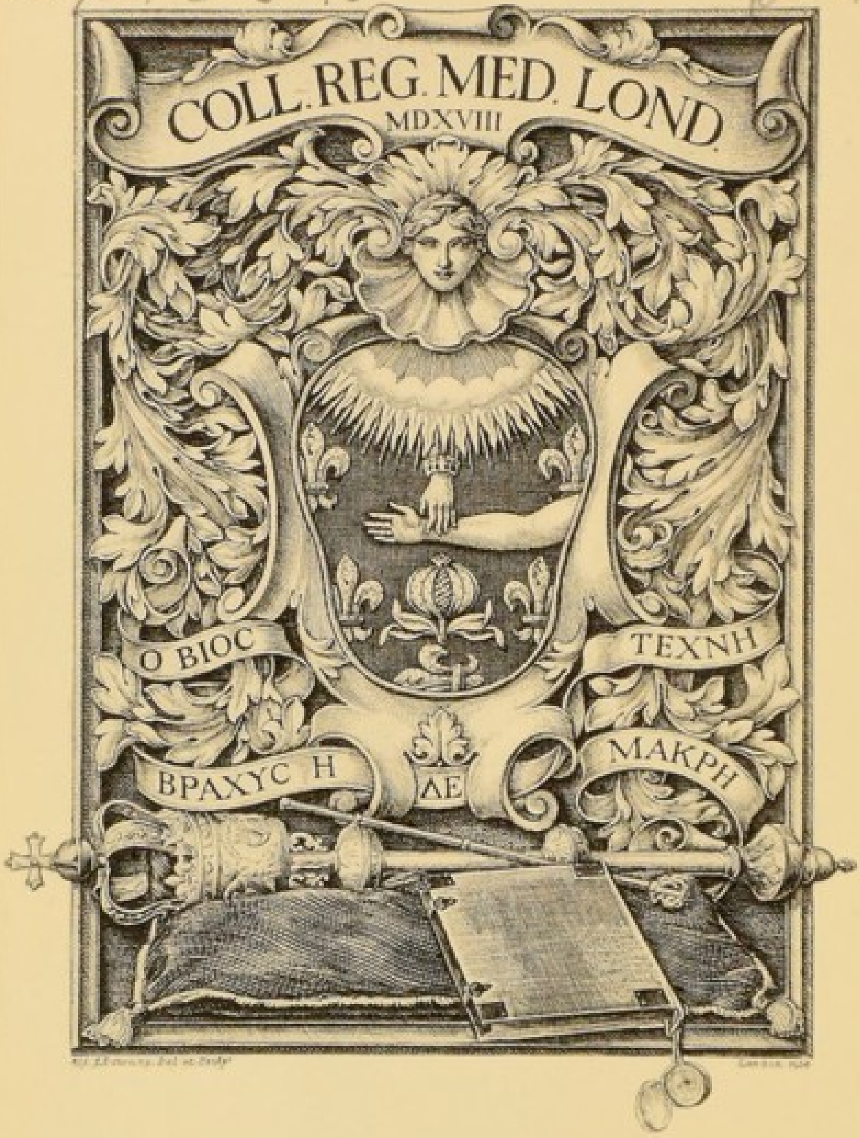
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WORKS

OF THE

CAVENDISH SOCIETY.

FOUNDED 1846.

H. Cavendish

THE HONOURABLE HENRY CAVENDISH,

Born 10th October 1731   Died 24th February 1810.

(From a Drawing by Alexander in the Print Room of the British Museum)

John Weale.

THE

LIFE

OF

THE HON[BLE] HENRY CAVENDISH,

INCLUDING

ABSTRACTS OF HIS MORE IMPORTANT SCIENTIFIC PAPERS,

AND A

CRITICAL INQUIRY INTO THE CLAIMS OF ALL THE ALLEGED DISCOVERERS OF THE COMPOSITION OF WATER.

BY

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PREFACE.

WHEN the Council of the Cavendish Society did me the honour to ask me to write the life of the great philosopher with whom the Society had associated itself by its name, I willingly undertook the task. During the enforced leisure of a long illness, I commenced, in 1842, to collect materials for a projected work on the lives of the Chemists of Great Britain, in which Cavendish should occupy a prominent place; and I had made some progress in my task when the Cavendish Society was founded. Although an original member of that association, I had no share in determining the selection of the name by which it is distinguished, nor was Cavendish an object of greater interest to me than the other great philosophers of our country, whose lives I proposed to write. When, however, at the call of the Society, I laid aside the more general undertaking in which I was engaged, and turned my attention solely to the works and character of the Honourable Henry Cavendish, circumstances had occurred which gave him an importance in the eyes of the lettered public, such as no other chemist at the time possessed. He prosecuted zealously and successfully so many branches of knowledge, that the students of nearly all the physical sciences may consider him as an illustrious brother; nor have I any wish to assert that Chemistry is entitled to claim him as peculiarly hers. It so happens, however, that his memory has been specially honoured by chemists, among whom Sir Humphry Davy, Faraday, and Thomas Thomson have been foremost. And it is also the case, that his chemical essays have furnished to some the occasion for a denial of his intellectual capacity and of his moral worth, which has to a great extent thrown his defence into the hands of the chemists. I have written this volume as a student of chemistry; and having only a limited space at my disposal, I have dwelt less upon Cavendish's purely physical researches, than I should have done had I been free to expatiate upon his merits as a natural philosopher. His physical researches, however, especially those on electricity and on the density of the earth, have not been overlooked in the succeeding pages; and the value of these memoirs is so fully appreciated by men of science, that they do not demand special criticism. I have given prominence, accordingly, to his discoveries in chemistry, and in the science of heat, but especially to the former. It has been impossible to do otherwise. Within a very recent period, Cavendish has been the occasion of the keenest controversy that has interested chemists for a long time, and much of this volume is occupied with its discussion. The controversy turns upon the question, Who discovered the composition of water,—Cavendish, Watt, or Lavoisier? and it has been prosecuted at greatest length in reference to the rival claims of the English philosophers. The points in debate are not merely questions of priority, and of relative intellectual merit, but also of morality; for charges of plagiarism, and of unfair dealing towards each other, have been brought against the rivals, nor have their friends and acquaintances escaped reproach, including the entire Royal Society at one period of its existence. Cavendish, in truth, has during the last ten years been the object of attack or of defence to a much larger number of writers of great eminence, belonging to different professions, than any one could have anticipated would interest themselves in the reputed author of a solitary discovery made eighty years ago. I have undertaken, accordingly, a delicate and difficult task, in writing a work which compels me to pass under review the judgments of men of such note in science and letters as Arago, Dumas, Brougham, Brewster, Jeffrey, Harcourt, Whewell, and Peacock, at whose feet I have been accustomed to sit as a humble disciple. I may be allowed, therefore, to explain briefly in what spirit I have undertaken my task.

The volume consists essentially of three distinct portions. The first, a biographical narrative; the second, abstracts of scientific papers; the third, a criticism of the asserted merits of all the claimants of the discovery of the composition of water. This critical inquiry has, for convenience of reference, been printed immediately after the abstracts of the chemical papers, but those upon heat throw light upon it also. In the abstracts there is nothing polemical. It is otherwise with portions of the biography, and with the critical inquiry.

It was open to me to write as a partizan, as an advocate, or as an historian. I have chosen the last character as the only befitting one. I do not pretend to bear witness to my own impartiality, of which others must be the judges, but I can at least testify to the spirit in which I have sought to write; and candid readers, I think, will acquit me of partizanship. The conclusions to which I have come in reference to Cavendish's priority and merits as a discoverer, and his integrity as a man, are such that I can rank myself amongst his most hearty admirers and defenders. Had I written, however, *only* as his advocate, I should have left much unnoticed which I have recorded. Thus I have been at pains to point out the defects of his theories, as well as their excellences, and to indicate the merits of his rivals, as well as their faults. The reputation of Lavoisier, and of Watt, is as sacred a thing in my eyes as that of Cavendish; and I should be the first to regret if the tone of this work should seem at variance with the catholic spirit of esteem for all great philosophers, which is an essential element of vitality in associations like the Cavendish Society. Whilst thus, however, I have endeavoured to be impartial, and to make the biography a faithful sketch, not a eulogy, I have deemed it an essential part of my duty as a biographer to vindicate the moral character of Cavendish from even the shadow of suspicion. It has been impossible to do this, without censuring those who have called his good name in question. If in uttering censure I have forgotten what is due to great authorities in literature and in science, even when they are in error, I shall deserve and bow to reproof; but if I have only reluctantly fulfilled an imperative though invidious duty, and have justified my censures by showing that they are deserved, I shall hope to be vindicated at the hands of my readers.

I count it a great advantage that I had studied all the earlier portion of the literature of what may be succinctly styled the Water Controversy, before I had any temptation to take a side in the dispute. I also congratulate myself on having been compelled to look at Cavendish's discoveries and character from two exactly opposite points of view. In 1845, Mr. Muirhead, the able editor of "the Correspondence of the late James Watt on his discovery of the theory of the Composition of Water," was introduced to me, and by that gentleman, who is the most zealous of Watt's defenders, and the most unhesitating of Cavendish's assailants, I had everything that could be said in favour of Watt urged upon me in the strongest terms. The publication, also, of the *Watt Correspondence* in 1846, led to my obtaining the friendship of the late lamented Lord Jeffrey. He had known and esteemed Watt, and he welcomed the publication of the *Watt Correspondence*, as furnishing a becoming occasion for exalting the honour of his old friend. Before his Lordship published his judgment on the rival claims of Cavendish and Watt in the *Edinburgh Review* for 1848, I had many conversations with him on the subject. Chemistry was a science in which he had always taken great interest, and it continued to the last to engage his attention. With his estimate of the relative merits of Cavendish and Watt I could not concur, and he listened to my earnest defence of the former with all the frank courtesy and love of fair dealing which so eminently characterized him. Against Cavendish he entertained no animosity or prejudice, and he was most willing to praise him; but he thought that

Watt had been wronged, and he was solicitous to see him righted, so that he pressed me with all the arguments which he perceived might be urged in favour of his great client, whose case he has so skilfully and eloquently pleaded. He did not even refuse to discuss (I may say to debate) contested points with me, and I defended Cavendish in the strongest terms which courtesy sanctioned.

My zeal in Cavendish's cause made no difference in Lord Jeffrey's kindly dealings towards me, and he was the first in whose hands I purposed to place this volume, in which many of his conclusions are called in question.

Having thus had the claims of Cavendish's English rival brought before me in the amplest way, I have been secured against under-estimating what may be said in favour of Watt. Lord Jeffrey's article, indeed, is by much the ablest defence of Watt that has appeared.

After Lord Jeffrey's decease, the Rev. William Vernon Harcourt, the ablest of Cavendish's defenders, most kindly put himself in communication with me, and furnished me with his estimate of the position in which Cavendish's claims were placed by the publications in favour of Watt, which had appeared since 1846. I cannot concur in all Mr. Harcourt's conclusions, but I am indebted to him for many valuable suggestions, and for much assistance. In particular I owe to him an introduction to the Earl of Burlington, who placed at my disposal the whole of Cavendish's papers in his possession, and obtained for me much information concerning his illustrious ancestor's personal history. The papers on Electricity which Cavendish left behind him, are at present in the hands of that accomplished Electrician, Sir W. Snow Harris, who, in the kindest manner, drew up for me an abstract of them, accompanied by a commentary. It is matter of great regret to me, that I have not been able to print either the abstract or the commentary in this volume; but I trust that they will yet be made public.

I have thus had access to many unpublished documents, which are fitted to throw light on Cavendish's merits and his personality, and I have largely availed myself of them. For the opinions expressed in this work, I alone am responsible. I have accepted and solicited information and assistance from every party known to me, willing or likely to furnish aid; but as it was manifestly impossible for a single writer to represent the diversified opinions of all the members of a large society upon a contested question, I requested the Council of the Cavendish Society to allow me to write in my own name. No one accordingly but myself is committed to the conclusions contained in this volume.

It remains for me to express my obligation to the many scientific men who have assisted me in this work. To Robert Brown, Esq., of the British Museum, I have been indebted for interesting particulars concerning Cavendish and Blagden, and I am under similar obligations to Dr. Thomas Thomson, of Glasgow. M. François Delessert, of Paris, also, has made me his debtor for much valuable information. From R. H. Blagden Hale, Esq., of Cottles, I have received various interesting papers, which have been of very great service to me. To Dr. Percy, and Dr. James Russell, of Birmingham, I am under great obligations for their good offices in procuring for me the loan of a number of important unpublished letters of Dr. Priestley's, which have been confided to my care by his grand-daughter, Miss Finch. Francis Wedgwood, Esq., of Barlaston, has allowed me access to the papers of his celebrated ancestor, Josiah Wedgwood; and through Professor Graham, I have obtained from Mr. Hudson, the Secretary of the Royal Agricultural Society of England, several of the very few extant letters of Cavendish. To Mr. Redwood, also I am much indebted.

Dr. Davy has largely contributed to the materials from which the biography has been written, and so have Professor Brande, Mr. Konig, of the British Museum, W. H. Pepys, Esq., J. G. Children, Esq., and Henry Lawson, Esq., of Bath. I have to thank the Rev. Joseph Romilly, and Frederick Fuller, Esq., of Cambridge, the Rev. C. J. Heathcote, of Upper Clapton, and C. R. Weld, Esq., of the Royal Society, for their ready communication of all the information which their official positions enabled them to furnish in answer to my queries. I have also to acknowledge the liberality with which my friend, the Rev. Dr. Vaughan, of Manchester, has permitted me to make any use I pleased of papers contributed to the *British Quarterly Review*, in which I published, in 1845, a short biographical sketch of Cavendish. To others also I am indebted, but it might savour of parade to enumerate the names of each.

Once for all I would say, that from every one to whom I have applied for information, I have received it, and that much that is most valuable in this volume has been unsolicitedly sent to me. I have reserved for special thanks Charles Tomlinson, Esq., of London, to whose untiring zeal, skilful investigation, and cordial unflagging co-operation, I am indebted for the larger part of the materials from which the last chapter of the Biography has been compiled. To him I am also indebted for the striking and hitherto unknown portrait which graces this volume, and he has done me the favour to read the proofs of this work, besides assisting me in every other way in which either he or I perceived that he could be of service.

I have lastly to notice that since the publication of the *Watt Correspondence*, in 1846, the only lengthened notices which have appeared in reference to Cavendish, have been Sir David Brewster's Article in the *North British Review* for 1847, and Lord Jeffrey's Paper in the *Edinburgh Review* for 1848. Both of these writers pronounce against Cavendish, and refer to the *Watt Correspondence* as decisive of the merits of Watt; but I think it will appear from the following pages, that the admirers of Cavendish have every reason to congratulate themselves on the publication of the "Correspondence;" and for my own part I believe that it furnishes the most decisive evidence in favour of Cavendish, and as such I have constantly quoted from it.

24, Brown Square, Edinburgh,
March, 1851.

CONTENTS.

LIFE OF CAVENDISH.

CHAPTER I.

GENEALOGY OF THE CAVENDISHES. EARLY HISTORY OF THE HON. HENRY CAVENDISH.

THE great majority of the distinguished Chemists of Great Britain have sprung from the middle or lower ranks of the people, but two of the most famous of them, the Honourable Robert Boyle, and the Honourable Henry Cavendish, were men of illustrious lineage, and Cavendish was much the more high born of the two.*

No one could well be more indifferent, than Henry Cavendish was, to the external advantages which birth and fortune gave him, yet few of those who set the greatest value on these, could boast of a descent such as his.

His family traced their pedigree, by unbroken and unquestionable links, to Sir John Cavendish, Lord Chief Justice of the King's Bench in the reign of Edward III; and, according to learned genealogists, they could go much further back, and derive their descent from a Norman family, famous in the days of the Conquest. Cavendish could thus look back, across eight centuries, to the founder of his family, and through the long interval which elapsed between the first appearance of his ancestors in England, and his own days, could point to his prede-

* The blood of these families has mingled within a recent period. Lady Charlotte, the daughter and heir of Richard Boyle, Earl of Burlington and Cork, was married to William, fourth Duke of Devonshire, and was mother of William, the fifth duke, as well as of Lord George Henry Cavendish and others.—(Collins' *Peerage*, 4th edition, vol. i., p. 333.) The Earl of Burlington is thus by descent both a Cavendish and a Boyle, and has a pedigree on which a lover of science must look back with peculiar pleasure.

cessors as famous in the history of their country, and as connected by intermarriages with the most illustrious houses of the kingdom, not excepting the royal families of England and Scotland.*

Some doubts have been expressed by Sir Egerton Brydges, in his edition of Collins' *Peerage*, as to the descent of Lord Chief Justice Cavendish, from the Norman Gernons,† but even he expresses only a doubt, and that by no means strongly; and those learned genealogists, Dugdale‡ and Collins,§ both unhesitatingly derive the Cavendishes "from Robert de Gernon, a famous Norman who assisted William the Conqueror in his invasion of this realm, A.D. 1066:" so that I shall take for granted that he was their forefather.

It would be out of place in a sketch like the present, to enter into any minute particulars concerning a family history so well known as that of the Cavendishes; I shall confine myself, therefore, to the names of a few of its more distinguished members, so as to connect together the Norman warrior of the 11th century, with the philosopher of the 18th.

Robert de Gernon received large grants of land after the Conquest, and his immediate descendants were long famous in Norfolk and Essex. A younger son of the family, Roger Gernon, seated at Grimstone Hall, in Suffolk, died in 1318, having had to wife the daughter and heir of John Potton, Lord of Cavendish, in Suffolk, by whom he left issue four sons, who all took the surname of Cavendish, as was customary in those days.‖

This surname is variously spelled by its older possessors,

* Lady Arabella Stuart, who was grand-daughter of Sir William Cavendish, a lineal ancestor of the philosopher, was great-grand-daughter of Margaret Tudor, daughter of Henry VII. and Elizabeth of York. She was thus the niece of Mary, Queen of Scotland, and cousin of her son James I.—Miss Strickland's *Lives of the Queens of Scotland*, vol. i., preface, p. ix.

† Vol. i., p. 302.

‡ *Baronage of England*, vol. ii., 1676, p. 420.

§ *Peerage of England*, 4th edition, vol. i., pp. 279—283.

‖ *Ibid*, p. 281: "Our surnames are chiefly derived from this origin [a territorial possession] or from personal peculiarities,—from trades and employments, or from the Christian name of the father or mother. Of these, the first is the most aristocratic, denoting a descent from an ancient baron, or at least, the lord of a manor."—(Lord Campbell's *Lives of the Chancellors*, vol. v. page 174.) The surname Cavendish comes within the last category, being derived from the manor of which Potton had the lordship.

Cavendishe, Cavendysh, Cavendysch, but is now universally written Cavendish. It was also frequently written Caundish and Candish, and even, when spelled Cavendish, was pronounced as a dissyllable. That accomplished critic and scholar, Thomas De Quincey, has pointed out to me, that up to a recent period, and perhaps even at present, Cavendish has been pronounced as if it were written Candish. In illustration of this he adduced a Sonnet of Wordsworth's, which cannot be read rhythmically, unless Cavendish be made a dissyllable. The passage is as follows :—

AT FURNESS ABBEY.

* * * * * *

"Even as I speak the rising Sun's first smile
Gleams on the grass-crowned top of yon tall Tower,
Whose cawing occupants with joy proclaim
Prescriptive title to the shattered pile
Where, Cavendish, thine seems nothing but a name."!*

The first bearer of the name of Cavendish was the Chief Justice already referred to. His father's property was small, and his prospects indifferent. He devoted himself, however, to the study of the law, and soon acquired great reputation as an advocate. "Such was his reputation," Lord Campbell tells us, "that in the year 1366, Edward III, after the peace of Bretigni, being desirous of making himself popular by good judicial appointments, raised John de Cavendish to the office of Chief Justice of the King's Bench, although he had not filled the office of Attorney or Solicitor-General, or even reached the dignity of the coif."†

The fate of this first Cavendish was a sad one. He was continued in his high office by Richard II with an increased salary ; but after acting as Judge for sixteen years, he was cruelly murdered in one of the insurrections which marked that reign. As one of those to whom the suppression of Wat Tyler's insurrection was entrusted, he had become an object of vengeance to the rebels, and their feelings of revenge towards him appear to have been greatly deepened, by the fact that his son

* Wordsworth's *Poetical Works*, royal 8vo. 1845, p. 217. I am indebted for this reference to my friend, Alan Stevenson, Esq., the engineer of the Skerryvore Lighthouse, who is profoundly acquainted with the writings of the poet.

† *Lives of the Chief Justices of England*, vol. i., p. 94.

and namesake, an Esquire of the body of Richard II, was the party who slew Wat Tyler, after he was wounded by Sir William Walworth, mayor of London. For this act, the younger Cavendish was knighted on the spot by King Richard, and granted a pension, which, however, cost his father his life. When the insurrection revived under the ferocious Jack Straw, he plundered and burned the house at Cavendish, and beheaded the Lord Chief Justice, after cruelly insulting him.*

The immediate descendants of the Chief Justice may be passed over very briefly. His son, the second Sir John, was succeeded by his eldest son William, who had an only son, Thomas, who in turn was succeeded by an only son of the same name, who was the father of Sir William Cavendish, the founder of the political greatness of the Cavendishes. Thomas, the father of Sir William, died in the fifteenth year of Henry VIII, 1524. He had two sons, George and William, both of whom deserve a place in the history of the Cavendishes: William was the second son; he began life with fewer advantages than his brother, but rose to much greater worldly distinction, and the merits which belong to George have been added by many writers to his own. This William Cavendish is the founder of the modern distinction of his family, or, at least, shares it only with his celebrated wife, of whom more will be mentioned presently. His father was a clerk in the Exchequer in the reign of Henry VIII, and appears to have trained his son to exact business habits, of which he afterwards reaped the reward. He must have been a man of talent and capacity, for he early attracted the attention of Henry, and this at a period most fortunate for him.

In 1530, he was appointed one of the Commissioners for visiting, and securing the revenues of the religious houses which the King was then busily confiscating. He continued in this and similar offices for several years, and, as might be expected, received a large share of the confiscated church property. In 1546 he was knighted, and made Treasurer to the King, a place of great responsibility and honour. He was soon afterwards made a Privy Councillor, and received from Edward VI grants

* *Lives of the Chief Justices of England*, p. 95. Collins' *Peerage*, 4th edition, vol. i., p. 284.

of lands in seven different counties. He died in 1547, leaving a large estate to his heirs.*

The descendants of Sir William Cavendish rose with almost unexampled rapidity to the highest distinctions, but before this can be understood, reference must be made to his wife, a very remarkable woman, who, as Mr. Hunter has justly remarked, was more than an equal sharer with him in establishing the fortunes of the family. Elizabeth Hardwicke occupies a considerable space in the private and public history of the peerage of our country, and was in all respects an extraordinary person. She was a younger daughter of a country gentleman in Derbyshire, who could afford her only a very trifling portion. She rapidly, however, provided for herself. At the age of fourteen she was married to Robert Barley or Barlow, of Derbyshire, a youth little older than herself. He was at the time a confirmed invalid, and died very soon after his marriage, which appears to have been merely a form, but he left all his estates settled upon his bride and her heirs. After fourteen years of maiden widowhood, she became the third wife of Sir William Cavendish, who had no male issue surviving by his former marriages. Three sons and three daughters, who were born of this marriage, survived to establish the greatness of the family. Sir William Cavendish married this lady in 1547, and died ten years after. Some four years later she became the wife of Sir William St. Loe, captain of the guard to Queen Elizabeth, and possessor of several estates in Gloucestershire. He was a widower, and had children by his former marriage, but such was his devotion to his second wife, that, in compliance with her demand, he settled all his property upon her and her heirs, and as he died without issue by her, "she lived to enjoy his whole estate, excluding his former daughters and brothers."†

The period of St. Loe's death is uncertain. Before long, however, Elizabeth Hardwicke was again a wife. Her fourth and last husband was George Talbot, sixth Earl of Shrewsbury. He had a family by a former wife, and would not settle his property away from them. But Mistress St. Loe,

* Collins' *Peerage*, vol. i., p. 289. Rev. Joseph Hunter, in Singer's *Cavendish*, vol. ii. p. xxxiv.

† Craik's *Romance of the Peerage*, vol. iii., p. 149.

would not accept him till he had arranged to give two of his children in marriage to two of hers. Her eldest son, accordingly, Henry Cavendish, was married to the Earl's daughter, the Lady Grace Talbot, and her daughter Mary to his second son and ultimate successor Gilbert. Elizabeth Hardwicke had no children by the Earl of Shrewsbury, so that all the wealth and influence she had gained by her four marriages, were brought to bear upon the advancement of her children by Sir William Cavendish. The Earl of Shrewsbury, at the period of his second marriage, was the most powerful nobleman of the realm, and his Countess shared with him in the responsible office of warden of the unfortunate Queen Mary of Scotland. This office brought the Countess into frequent communication with Queen Elizabeth, with whom she was a favourite. The Earl died twenty-three years after his marriage to Mistress St. Loe, and she survived him seventeen years, dying in 1608, on the threshold of her ninetieth year; seventy-five years after the death of her first husband. Her whole history concerns the Cavendishes, for they were both directly and indirectly gainers by all her marriages. She is said to have been beautiful, but she was fifty before she married George Talbot, and her whole career points to other gifts than that of mere beauty as the source of her prosperity. She must, however, have been a fascinating person, for all her husbands were devotedly attached to her, although in the end, her last spouse, who had the longest trial of her, exchanged his early devotion for something like positive hatred, fairly tired out of his patience by her insatiable ambition and rapacity. The Countess, nevertheless, was far more than a match for the well-intentioned, but stolid Earl Talbot. She has been hardly judged by most of her biographers. Sir Egerton Brydges accuses her of rapacity, from which she cannot easily be defended; Mr. Hunter accuses her of "reserve, perfidy, and even tyranny towards her last husband," and quotes with approbation Mr. Lodge's description of her, that she was "a woman of masculine understanding and conduct; proud, furious, selfish, and unfeeling." Her latest biographer, Professor Craik, is less severe; and the better points of her character appear to have been overlooked by her harsher judges. The affectionate terms in which she is referred to by her last three husbands, whose letters still remain in attestation of their regard for her, are sufficient proofs that, with all her shortcomings, there was much that was loveable about her. And even from the least favourable accounts of her that have come down to us, it seems plain that there was nothing mean, malignant, or cruel in her character. On the other hand, she was bold, resolute, and straightforward in her dealings, and spared no pains to advance the welfare of those to whom she was attached. Her worst faults were her insatiable ambition, and love of pomp and magnificence, which could not be gratified without the command of an amount of wealth which it required skilful management to procure. She had a passion for building, which her descendants inherited, and spent enormous sums in the erection of splendid palaces, which should perpetuate her name. One of these (Hardwicke Hall) still remains, and ranks among the noblest mansions in Derbyshire. A second, named Oldcotes, has disappeared; and the splendid structure which she erected at Chatsworth was taken down by her descendant, the first Duke of Devonshire, and replaced by the still more magnificent building which now occupies its site.*

Whatever were the Countess of Shrewsbury's faults, she was indefatigable in promoting the interests of her children; and not more indefatigable than successful. This may be judged by the fact, that whereas she was originally, the nearly portionless daughter of a country Squire, and Sir William Cavendish, though of good descent, but a younger son of a clerk in the Exchequer, the whole of their children, who survived infancy, either obtained titles, or were married to those who had them. One son was Knighted, another was made Baron and Earl, two of the daughters became Countesses, a grandson was made a Duke, and a grand-daughter, Lady Arabella Stuart, was for some years heiress presumptive to the throne of England. Another duke-

* The account of Elizabeth Hardwicke in the text, I have taken partly from Collins' *Peerage*, vol. i., article Cavendish, Duke of Devonshire; and from Dr. White Kennet's *Memoirs of the Family of Cavendish*, p. 65. I have been chiefly indebted, however, to the sketch of her life contained in the Rev. Joseph Hunter's Essay, *Who wrote Cavendish's Wolsey?* published in Singer's *Cavendish*, vol. ii., p. xlvi.; and likewise to the very full and lively account of this lady given by Professor Craik in his paper entitled "Bess of Hardwick and the Talbots," in his *Romance of the Peerage*, vol. iii., p. 145.

dom was added to the family in a later generation, and their fortunes have ever since been in the ascendant. For this unusually rapid elevation the Cavendishes were mainly indebted, at least in the first generation, to their mother; for their father, it will be remembered, did not survive his third marriage more than ten years, and he could do little for the advancement of his family. It was chiefly, indeed, through the wealth and influence which her subsequent marriages brought his widow, and especially through her connexion with the Court, which her position as Lady St. Loe and Countess of Shrewsbury gave her, as well as through the interest of her grand-daughter, Lady Arabella Stuart, James the First's cousin, that she was able to advance her family so rapidly in social rank. She owed most, however, to her own energy and boldness, for the connexion by which she was the greatest gainer, viz., that with the royal family, was brought about solely by her address, and not without hazard to herself. The marriage, indeed, which she effected between her daughter Elizabeth and the Earl of Lennox, James the First's uncle, excited the wrath of Queen Elizabeth so much, that she committed the Countess for some time to the Tower.*

The Countess of Shrewsbury wedded Henry, her eldest son, to the Lady Grace Talbot, a younger daughter of her last husband. Henry was not, however, her favourite, and in her own imperial way, she set aside the claims of primogeniture, and preferred her second son, William, who was raised to the Peerage by James the First, and is the progenitor of the Earls and Dukes of Devonshire, and of Henry Cavendish, who is the central object in this sketch.

Before, however, tracing his descent, two of his collateral ancestors must be referred to, who have a special claim to be remembered in connexion with one whose celebrity depends upon his intellectual, not upon his social or political greatness. The one of these is George, the elder brother of Sir William Cavendish, who may be regarded as the most distinguished student of literature among Henry's ancestors, and has earned a high place in the literary annals of our country. He owes this to his admirable *Life of Cardinal Wolsey*, which all critics are agreed in considering "as one of the very best specimens of English

* Craik's *Romance of the Peerage*, vol. ii., p. 353.

biography." George Cavendish, nevertheless, has narrowly missed being defrauded of this honour, which has been transferred by many writers to his more fortunate younger brother, Sir William. Later authors were led astray on this matter by Bishop Kennet, who in his ill-judged zeal to heap honours upon the progenitor of the first Duke of Devonshire, attributed to him the authorship of the *Life of Wolsey*, without, however, giving any express authority for his statement.*

* Kennet's work is entitled, *A Sermon preach'd at the funeral of the Right Noble William, Duke of Devonshire, &c., with some Memoirs of the family of Cavendish.* By White Kennet, D.D., Archdeacon of Huntingdon." In my copy of this work, a page has been interleaved by some former possessor, on which the following curious criticism is written, "It was by the interest of Bishop Burnet, that Kennet was appointed the preacher on this occasion, and the sermon gave great offence, and made some say that 'the preacher had built a bridge to Heaven for men of wit and parts, but excluded the duller part of mankind from any chance of passing it.' This charge was grounded on the 34th and 35th pages, but by the interest of the second Duke, to whom it is dedicated, he immediately obtained the deanery of Peterborough." The passage referred to as having given offence in the sermon, is one in which Dr. Kennet discusses the efficacy of a death-bed repentance, "This," says he, "rarely happens but in men of distinguish'd sence and judgment. Ordinary abilities may be altogether sunk by a long vitious course of life. The duller flame is easily extinguished. The meaner sinful wretches are commonly *given up to a reprobate mind*, and die as stupidly as they lived; while the nobler and brighter parts have an advantage of understanding the worth of their soul before they resign it. If *they* are allowed the benefit of sickness, they commonly awake out of their dream of sin, and reflect, and look upward. They acknowledge an infinite Being; they feel their own immortal part; they recollect and relish the Holy Scriptures; they call for the elders of the church; they think what to answer at a judgment seat." The italics are the author's own. The clergyman who could write thus must have forgotten the most memorable, and most certainly efficacious case of a dying repentance on record, and what was the character of him to whom it was said, "This day thou shalt be with me in Paradise."

It may seem a digression to criticise Kennet's work. The memoirs attached to the sermon, however, are frequently referred to as of special authority in reference to the history of the Cavendishes. They are unworthy of this praise. The author had access to no peculiar sources of information, and has committed more than one error; but the main fault of the work consists in its tone, which is that of fulsome and extravagant adulation of all the Cavendishes, to an extent as unworthy of the author as it was unnecessary in the case of a family whose intrinsic merits were so great. Some allowance must be made for the fashion of the time, which permitted a style of compliment which the taste of the present generation disowns; and some palliation, though no apology for the author's fault, may be found in the fact that he had an eye to preferment in what he was writing. His compliments, however, are sometimes even ludicrous, as when he praises the Countess of Shrewsbury for her dutiful behaviour to her last husband, and represents her as having had her temper and virtue exercised by the rumours at one time afloat concerning an undue intimacy between him and Queen Mary of Scotland (p. 70). This was a most unlucky

From him the error was transferred to the later editions of Collins' *Peerage*, including that by Sir Egerton Brydges, and likewise into the *Biographia Britannica*, and other works. Kennet's mistake was fully exposed in 1814, but this has not prevented its re-appearance in Lord Campbell's recent work, the *Lives of the Chief Justices of England*.* No one, however, who has read the Rev. Joseph Hunter's very interesting tract, entitled *Who wrote Cavendish's Life of Wolsey?* † can doubt for a moment that the author was George Cavendish, Sir William's elder brother.

The honours of the Cavendishes, it may be noticed, gain in a two-fold way by the transference of the authorship of the *Life of Wolsey*, from William Cavendish to George, for it not only places two distinguished men instead of one, in the list of their ancestors, but it saves Sir William Cavendish's character from a reproach which would have gone far to lessen the honour which the authorship of the work in question would have conferred upon him. The writer of that work professes to have been a zealous Roman Catholic, and laments bitterly the ruin of the religious houses, which the Reformation had occasioned. ‡ Sir William Cavendish, however, as we have seen, owed his advancement in life to the zeal with which he acted as the agent of Henry VIII in suppressing the monasteries, and to the share of the spoil which he received. It would be impossible, accordingly, had he been the author of the *Life of Wolsey*, to defend him from the charge of gross hypocrisy and double-dealing, inasmuch as he professed to be a Roman Catholic, and a very earnest and zealous one, whilst the Cardinal's usher, but never-

reference, for both the Queen and the Earl of Shrewsbury accused the Countess of being the very party who spread the scandalous report, and her husband declared to her, "there cannot be any wife more forgetful of her duty, and less careful to please her husband, than you have been."—(*Romance of the Peerage*, vol. iii., p. 221.) The last years, in truth, of the Earl of Shrewsbury's life were embittered by the quarrels between himself and his Countess, which were matters of public notoriety. A full and very curious account of the whole matter will be found in Mr. Craik's paper, "Bess of Hardwick and the Talbots," already referred to. Kennet's worst error, however, is that concerning the authorship of the life of Wolsey, and plainly arose from his inconsiderate desire to praise a direct ancestor of the Duke of Devonshire.

* Vol. i., p. 95.

† Reprinted in Singer's *Cavendish*, vol. ii.

‡ Singer's *Cavendish*, vol. ii., p. xxxvii.

theless did not hesitate, when he became the King's servant, to display the greatest activity in suppressing the religious houses whose overthrow he had affected so much to deplore. As the case now stands, however, he is quite free from this stigma on his character. He was from the first period, at least, of his public appearance, a Protestant, and could not be charged with inconsistency in abetting King Henry. His brother, on the other hand, lived and died a devoted adherent of the old faith, which condemned him to poverty and obscurity.*

Of the literary merits of the *Life of Wolsey,* I need not speak, as they are so universally acknowledged. No later bearer of the author's name has eclipsed him in literary reputation, although many of his collateral descendants have been more accomplished scholars.†

Another ancestor of the Honourable Henry Cavendish was referred to as deserving a place in this record. This is Thomas Cavendish, who, as one of those who first rounded the globe, appears peculiarly entitled, although only a collateral ancestor, to claim kin with him who first weighed it. He was a descendant of Roger, the second brother of Lord Chief Justice Cavendish, and the son, therefore, of Roger Gernon. He was, as the old writers record, "the third man, and the second Englishman which sailed round the globe." He set sail from Plymouth on July 21st, 1586, and after losing two of his ships, landed safely at the same port on September 9th, 1588. His second voyage was less successful. He could not succeed in passing the Straits of Magellan, and being driven back to the Coasts of Brazil, he was deserted by many of his associates, and died of grief and chagrin, at the unfortunate issue of his voyage.

I now return to Sir William Cavendish, with a view to trace very briefly, the descent of the special subject of my sketch from him. His elder brother Henry died without issue, and William,

* The fortune of the brothers was strikingly different. George Cavendish's son had to sell the manor from which his family took its name, and his grandson became a tradesman in London; whilst William's son became a large landed proprietor, and an earl, and his grandson was raised to a dukedom.—Hunter, in Singer's *Cavendish,* vol. ii., p. lviii.

† The best edition of the life of Wolsey is that entitled "*The Life of Cardinal Wolsey.* By George Cavendish, his gentleman-usher; with notes by Samuel Weller Singer, 1825." A part of Cavendish's narrative has been recently reprinted in the third edition of Galt's *Life of Wolsey.*

the second son, who was his mother's favourite, inherited a large estate. His early elevation to the Peerage appears to have been mainly owing to the influence of his niece, Lady Arabella Stuart, who was then in favour with James I, and obtained from him a promise that one of her uncles should be made a Baron. William was selected for this honour, and became Lord Cavendish, accordingly, at the christening of a Princess who died in infancy.* He was some years later created Earl of Devonshire.

Thus far, it appears that the Cavendishes were singularly favoured by fortune; and the first Earl seems to have been more indebted to his claims as the King's cousin, and to the zealous solicitations of his mother, than to his own merits. He was a man, however, of good capacity,† and the talent which displayed itself in his direct and collateral descendants soon showed that the Cavendishes knew well how to take at its flood the tide in their affairs, and make it lead them on to fortune.

The first Earl died in 1625, and was succeeded by his son. Meanwhile the younger brother of the former, a man of great ability, was knighted by James I, and took an active part in the public proceedings of the period. His still abler son, William, played a conspicuous part in the Civil War, and was distinguished by his zeal in the Royal cause. He bore in succession nearly every title of rank, and rose from being a Knight of the Bath, to the dignities of Earl, Marquis, and Duke of Newcastle. He is identified with the history of our country; and his second wife, Margaret, the literary Duchess of Newcastle, has secured for him additional remembrance, by the pleasing memoir which she has written of him. His only surviving son died without male issue, and the dukedom of Newcastle became extinct. It has since been repeatedly revived.

The second Earl of Devonshire, who was a man of talent,

* Craik's *Romance of the Peerage*, vol. ii., p. 368.

† Collins, in his *Peerage*, tells us that he was one of the first adventurers who settled a colony and plantation in Virginia. He had also a large grant of land in the Bermudas, and called his estate there Cavendish, a name which it still retains, or at least recently did. I presume that from this plantation is derived the name of a well-known variety of tobacco, which—so strange a thing is fame—has spread the name of Cavendish more widely than all the patriotic deeds, or scientific and literary achievements of its most illustrious bearers have done.

and possessed of many accomplishments, survived his father only three years, and was succeeded by his son, who, like all the other Earls as well as Dukes of Devonshire down to the present day, as well as the founder of the family, bore the name of William.*

The third Earl came to the title in his eleventh year, and bore it for fifty-six years. His son, the fourth Earl, and first Duke of Devonshire, is the most distinguished member of the family, unless, perhaps, we except the first Duke of Newcastle. His forefathers were zealous Royalists, and stood by the Stuarts in all their vicissitudes of fortune. The fourth Earl, however, from his early youth sympathised strongly with those who opposed the encroachments which James II sought to make upon the liberties of the English people, and his quick temper and high sense of honour brought him into direct collision with the King himself. In the end, accordingly, he took a most active and prominent part in furthering the accession of William III, for his services to whom, he was created Duke of Devonshire, besides receiving many other honours. He died in 1707, and was succeeded by his eldest son, William, who died in 1729. Lord Charles Cavendish, the third son of this second Duke, was the father of the Honourable Henry Cavendish, the subject of our memoir.†

Lord Charles Cavendish married Lady Anne Grey, fourth daughter‡ of Henry, Duke of Kent, and by her had two sons, Henry and Frederick.§

* *Romance of the Peerage*, vol. iii., p. 276.

† William, second Duke of Devonshire, died in 1729, and was succeeded by his eldest son, William, who died in 1755. His successor, the fourth Duke, died in 1764. William, the fifth duke, died in 1811, and was succeeded by the present bearer of the title, who is the sixth duke, and ninth earl of Devonshire.

‡ In the article "Cavendish," in Collins' *Peerage*, she is stated to have been the Duke of Kent's *third* daughter (4th edition, vol. i., p. 330). In the account, however, of Henry de Grey (vol. ii.. p. 521), she is called the *fourth* daughter; and the names of three elder sisters are given. I presume, therefore, that she was in reality the fourth in female descent. Her father was the thirteenth Earl, and first Duke of Kent, but died without male issue in 1740, when the dukedom became extinct. It was revived in 1799 in favour of Prince Edward, fourth son of George III, the father of Queen Victoria.—*Ibid.*, p. 519.

§ In Collins' *Peerage* (4th ed. vol. i., p. 330), Henry appears as the younger brother. But this is a mistake; Henry is styled "the eldest son" on his funeral tablet in the church of All Saints, Derby. In the books, also, of St. Peter's Col-

I close the account of this long genealogy with one remark. Could we trace the character of any family, whatever its rank, through as long a period as we can, that of the Cavendishes, we should probably find one line differ very little, at least *morally*, from another. A very few men and women would appear of great genius and great virtue; and a very few would also be found remarkable only for the magnitude of their crimes. A larger number would occur, characterised by the possession of fair talents, and displaying in their conduct an average morality; and a considerable crowd would present itself of persons who were only the creatures of circumstances, and were guided and controlled by the greater abilities and more fixed principles of their neighbours.

Families, no doubt, like races of men, present specific characteristics, which are transmitted through many generations unaltered. These, however, occur more in reference to intellectual than to moral peculiarities, in which preeminently our neighbour is our brother; and the infallible effect of varied intermarriage, without which a stock soon becomes extinct, is to lessen, or obliterate, or reverse extreme peculiarities, whether physical, intellectual, or moral. There can never, accordingly, be a very great difference between the characteristics of two families of the same nation, provided we study their history through a sufficiently long period, and set aside a few exceptive cases, which by their peculiarity and rarity afford the best proofs of the existence of the law which they trangress.

A history, like that of the Cavendishes, should thus have an interest for every one; and this is my apology for discussing it with some fulness. If there is anything exceptive in their annals, it is the high moral character which for so many centuries the family has maintained. Other high-born English families of old descent have given to their country as many noble women in every sense of the word, as many patriots, statesmen, and men of science and letters, but there are not many of them

lege, Cambridge, he is called "Filius natu maximus." An interesting sketch of his younger brother, Frederick, who was somewhat eccentric, but a man of excellent parts, and remarkable for his benevolence, will be found in the *Gentleman's Magazine*, 1812, p. 289. He was very unlike Henry, both in temper and tastes, and the brothers seldom met, but they are said to have been sincerely attached to each other.

who have not some page in their history blotted by the record of the misdeeds of their ancestors. The Cavendishes may well be forgiven, if they look back with complacency on a family history which displays so few shortcomings as theirs does; and we may all feel pride in numbering among our great philosophers the descendant of such a stock. To him I now turn.

Sir James Mackintosh has accused Ireland of being *incuriosa suorum;* but the charge may be preferred against all the divisions of the empire, so far at least as the men of science are concerned. No other European nation has so imperfect a series of biographies of her philosophers, as Britain possesses, and it is little creditable to us, that we have often to turn, as in the case of Cavendish, to the Memoirs of a foreign society, for the best record of the personal history of even our most famous students of physics. Cavendish's position is not peculiar in this respect, for we still look for a more complete life of Newton than has yet appeared,* and among philosophers who have recently departed, we are yet without biographies of Young, Wollaston, and Dalton. So careless has his own country been of the memory of Cavendish, that although he was for some fifty years a well-known and very distinguished Fellow of the Royal Society, a member for a lengthened period of the French Institute, and an object of European interest to men of science, yet scarcely anything can be learned concerning his early history. This no doubt is owing, in great part, to his own dislike of publicity, and to the reserve and love of retirement which strongly characterised him. Long before his death, however, he was so conspicuous a person in the scientific circles of London, that the incidents of his early life might readily have been ascertained. They were not, it should seem, enquired into by any biographer. Had he been a poor man of obscure birth, this might not have surprised us, but we have seen that he came of one of the oldest families in England, nor was he a far-off branch of it, for he was the grandson of a Duke by both parents, and the nephew and the cousin of one, besides counting kin on

* Since this was written, Mr. Edleston, of Trinity College, Cambridge, has announced his *Correspondence of Sir Isaac Newton, &c.*, which will doubtless prove a most valuable addition to our scientific biography. That such a work, however, should not appear till 1851, is the best justification of the statement in the text.

all sides with the aristocracy of Great Britain. He was further, immensely wealthy; so wealthy indeed, that, as M. Biot epigrammatically puts it, he was "le plus riche de tous les savans, et probablement aussi, le plus savant de tous les riches."* Nevertheless, his biographers cannot so much as agree upon the country of his birth, although his death occurred so late as 1810.

Cuvier,† Thomson,‡ and Kopp,§ tell us that he was born in England, whilst the contemporary notices of his death represent him as born in Italy. The latter is the true account. He was born on the 10th October, 1731,|| at Nice, whither his mother, Lady Anne Cavendish, had gone for the sake of her health.

His mother died when he was some two years old, and I have been able to learn nothing concerning his earliest years. The first notice I find concerning him goes back to 1742, when he became a pupil at Dr. Newcome's school at Hackney, an institution celebrated in its day for its excellent management, and largely attended by the children of the upper classes.¶

He remained at Hackney school till 1749, but no means now exist of ascertaining the precise nature of his studies, or what progress he made in them.**

* *Biographie Universelle*, 1813, tome vii., p. 456.

† *Eloges Historiques*, tome ii., p. 79.

‡ *History of Chemistry*, vol. i., p. 338.

§ *Geschichte der Chemie*, vol. i., p. 230.

|| I state this on the authority of Lord Burlington. A similar account is given in the sketch of his brother Frederick, published immediately after the latter's death. "Lady Anne Cavendish was in bad health on her marriage, and went shortly after to Nice, for the benefit of the waters there, attended by her husband. Henry was born at Nice, but his mother returning to England, Frederick drew his first breath in the country of his ancestors."—*Gent. Mag.* 1812, p. 291.

¶ Lord Campbell refers to it in his *Lives of the Chancellors*, as "a most excellent school at Hackney, kept by the Rev. Dr. Newcombe [Newcome], a sound classical scholar, and a strict disciplinarian."—Vol. v., p. 367.

** I state this on the authority of the Rev. C. J. Heathcote, of Upper Clapton, who possesses the only papers of the Hackney seminary that remain, and was good enough, at my request, to examine them for any records of Henry Cavendish. They consist, however, only of a list of plays acted by the boys of the school, in which Henry's name does not appear, and of a catalogue of the dates of admission to the school, and of departure from it, of its different members. Four other Cavendishes appear on this list, besides Henry and his brother, but no further particulars are supplied.

From school he went directly to Cambridge, where he matriculated in the first rank, on 18th December, 1749.*

He was now eighteen, and remained at Cambridge till 1753, but did not graduate. Frederick Fuller, Esq., Fellow and Tutor of St. Peter's College, Cambridge, has done me the favour to make enquiry concerning Cavendish's residence at Cambridge, and has furnished me with the following particulars, which are believed to embody all that is known concerning his occupations there. He entered at St. Peter's College, as the following extract from the books of that college shows: "Nov. 24, 1749, Honorabilis Henricus Cavendish, viri Honoratissimi Domini Caroli Cavendish Filius natu maximus, è scholâ publicâ de Hackney, annos habens octodecim, more solito examinatus et approbatus, admittitur ad mensam sociorum sub Tutoribus et Fidejussoribus M[ris] Stuart et Cox." Mr. Fuller adds, "Cavendish (as appears from other old books in the College treasury) commenced residence on the 24th of November, 1749, and resided very regularly and constantly until the 23rd of February, 1753, when he left without taking his degree. As he had then resided the full time—or nearly so, within a few days—required for the degree, and we can hardly suppose that Cavendish feared the examination, there must have been some particular reason for his neglecting to take the usual course. Perhaps he may have objected to the tests, which were then very stringent.

"Two more of the Cavendish family were studying here at the same time with the Honourable Henry Cavendish, viz., Henry's younger brother, the Honourable Frederick Cavendish, who was entered April 10, 1751, and, like his brother, left without taking any degree; and his cousin, Lord John Cavendish, the fourth son of the Duke of Devonshire, who was entered February 21, 1750, and took the degree of M.A. in the year 1753. All three of them were educated at the same school at Hackney. Among Cavendish's cotemporaries at St. Peter's College, were, the Earl of Euston, afterwards the Duke of Grafton, celebrated by Junius; Gray, the poet; and Jeremiah

* Extract from Matriculation Book, kindly furnished by the Rev. Joseph Romilly, senior Fellow of Trinity College, Cambridge, and Registrar of the University.

Markland, the Greek critic, who was senior fellow of the college at the time, and always in residence."

After leaving Cambridge, Cavendish probably went to London;* but his personal history is a blank for the next ten years, although it cannot be doubted, from his subsequent writings, that it was mainly spent in mathematical and physical studies. He joined the Royal Society in 1760, but did not contribute anything to its transactions till 1766, when he published his first paper *On Factitious Airs.* Here, accordingly, I shall suspend all reference to biographical details, and reserve the rest of his personal history until I have discussed his labours as a philosopher.

* Mr. Tomlinson has drawn my attention to the fact that Cavendish visited Paris along with his brother Frederick. This visit was probably paid when they were young men (for they had little, if any, intercourse in after-life), and before Henry became famous; otherwise some account of the journey would in all likelihood have been made public. I have no information, however, as to the date of the journey, its object, or the amount of time which it occupied.

CHAPTER II.

GENERAL SKETCH OF CAVENDISH'S SCIENTIFIC RESEARCHES AND DISCOVERIES.

FEW of our men of science have been so catholic in their tastes as Cavendish, so far at least as physics are concerned. He was an excellent mathematician, electrician, astronomer, meteorologist, and geologist, and a chemist equally learned and original. In the fullest sense of the term, indeed, he was a natural philosopher, and had he published during his lifetime all the researches which he completed, his reputation would have been much wider and more varied even than it was. He was exactly the opposite of a certain class of thinkers, whose fertility of invention, and skill or success in research, are far below their desire of distinction, and who are diligent in coining every thought, though it be but a farthing's worth, so as to put it into immediate circulation. Such men have nothing to reveal in private; the public are already in possession of all they know. Cavendish, on the other hand, dealt with his discoveries as with his great wealth, and allowed the larger part of them to lie unused in his repositories. His published papers, accordingly, give but an imperfect notion of the great extent of ground over which he travelled in the course of his investigations, and of the success with which he explored it. I shall endeavour, in the following pages, to give some idea of his unpublished as well as of his published papers, although so far as chemistry is concerned, Mr. Harcourt has left little to be done in this matter by his analyses of the Cavendish MSS.*

The obscurity which hangs over Cavendish's private history, especially in his early days, makes it impossible to determine what induced him to devote himself at particular periods to one

* *British Association Report,* 1839, p. 45.

branch of science rather than to another. He appeared first before the public as an author on chemistry, although from his early devotion to mathematical and strictly physical studies, it might have been expected that he would first have appeared as a writer on subjects connected with them. It is very probable, indeed, that some of the mathematical and physical essays which remain among his unpublished papers, are of earlier date than his first published chemical researches of 1766, but the absence of dates from the majority of his MSS. prevents any conclusion being drawn on this point. It will presently appear, also, that he could, with the greatest ease, change his subject of study, and that he was in the constant practice of carrying on together, widely dissimilar enquiries.

In the sequel of this work, I shall discuss his researches in a classified order, so as to bring all referring to one branch of science under one head; in the personal narrative they must be taken in their chronological order, at least to a considerable extent. I do not propose, however, to adhere quite strictly to this, for Cavendish's life is so barren of incident, that with the solitary exception of the Controversy concerning the discovery of the composition of Water, almost no connexion can be traced between the events of his history and the researches which he prosecuted. It will sometimes, accordingly, be preferable to depart from a strictly chronological order, which would carry us forward and backward, from one topic to another, and in so doing diminish our means of doing our author justice.

The account, moreover, of his merits and labours as a philosopher, contained in the sketch of his Life, will be more dogmatic than at first sight might seem desirable; but as full abstracts of his most important published papers are given in the sequel, the authorities for all the statements contained in the earlier part of this work will be found given at length there, and although not always minutely referred to, will be discovered without difficulty, from the many sections into which the book is divided. This chapter, in truth, is intended to inform the general reader, who may not care to study minutely the more elaborate exposition of Cavendish's merits, with which the body of the work is occupied, what he actually did achieve as a discoverer in physical science; and to prepare the way for the intelligent appreciation of the controversy in which certain of his discoveries involved him, as well as for a just estimate of his intellectual capacity. I shall chiefly refer to his published essays, as his actual reputation rests upon them; but an exception will be made in the case of the Chemical MSS., and some slight reference will occur to all his unpublished papers.

Cavendish did not give to the world his earliest researches. He probably kept many back. Two lengthened investigations, at least, the one chemical, the other physical, were completed and laid aside, in a condition ready for publication, before he commenced contributing to the *Transactions of the Royal Society of London*, in which all his papers were published.

The first of these investigations was entitled, *Experiments on Arsenic*, and remains among his papers, both in a rudimentary and completed shape. It appears to have been written out for the instruction of some friend, to whom allusion is made more than once in the course of the Essay, and the experiments referred to in it are as early, at least, as 1764, as appears from a date in the Note-Book. The Rev. W. V. Harcourt has published large and judiciously selected extracts from the MSS., which will enable the reader to understand the scope of this early investigation of Cavendish's.* It may suffice here to state that the paper in question contains an elaborate enquiry into the differences between regulus of Arsenic (Metallic Arsenic), white Arsenic (Arsenious Acid, $As\,O_3$), and Arsenical Acid (Arsenic Acid, $As\,O_5$). The properties of Arsenic Acid, and of various of its salts, the arseniates, are described with no little accuracy. The true nature of the difference between arsenic and its two acids, Cavendish did not know; but he held arsenic acid to be "more thoroughly deprived of its phlogiston" than arsenious acid; and the latter to bear a similar relation to metallic arsenic. These phrases are equivalent to the statement, that arsenic acid contains more oxygen than arsenious acid, and the latter more than metallic arsenic, which we know to be the case. The paper, is otherwise remarkable for its speculations on the nature of the "red fumes,' (nitrous acid, produced by the action of the air on

* *British Association Report*, 1839, pp. 50—58.

nitric oxide) which attended the action of nitric acid on arsenious acid, and for its discussion of the theory of the solution of metals in acids, and the reduction of the former by heat and inflammable matter.

Contemporaneously with this investigation, or a little later, Cavendish engaged in an extensive series of *Experiments on Heat.* The date February 5, 1765, occurs in the record of an observation on the 89th page of his *Note-Book of Experiments.* There can be little, if any doubt, accordingly, that we must go back many weeks into 1764, for the commencement of the researches in question. Mr. Harcourt has given extracts from the record of these,* and I shall give an abstract of them with a commentary, in the section of the work devoted to the consideration of Cavendish's observations on Heat. They were written out for a friend whose name is not given, but were not publicly referred to till some nineteen years after their completion, when certain of the results were quoted in a paper published in 1783, *on the Congelation of Quicksilver.*

They are very remarkable researches, and had they been made public in 1764 or 1765, they would have given Cavendish chronological precedence to Black in some of his discoveries, and equality of merit in others. They would have entitled him also to rank above Black's pupils and imitators: such as Irvine, Crawford, and Wilcke. This at least is certain, that Cavendish discovered for himself, and announced with admirable clearness, the fundamental laws of specific heat; and collected, probably before any one else, tables of the specific heats of various bodies. With scarcely any knowledge also of what Black had done towards the exposition of the laws of Latent Heat, and guiding himself by a totally different theory, as to its relation to solidity and liquidity, Cavendish investigated for himself the evolution of heat which attends the solidification of Liquids, and the condensation of Gases or Vapours, and the converse "generation of Cold," as he styled it, which accompanies the liquefaction of Solids and the Vaporisation of Liquids. I shall explain his views at length in the commencement of the section of the abstracts devoted to the discussion of his papers on heat.

* *British Association Report,* 1839, pp. 45—50.

Cavendish's earliest public contribution to science was, as has been mentioned, his paper on *Factitious Airs*, published in the *Transactions of the Royal Society* for 1766. It consisted of three parts: a fourth, which was not published, remains in a state of perfect completion, ready for the press, among his papers. It was evidently intended to be read to the Royal Society, for it contains a reference to the "Former Experiments read to this Society." For some reason, however, it was withheld, but has since been published by the Rev. W. V. Harcourt.*

Those four papers, for the several parts are equivalent to separate essays, are occupied with the discussion of the properties of Hydrogen, Carbonic Acid, and the gases evolved during the fermentation, putrefaction, and destructive distillation of vegetable and animal matters. They contain the first distinct exposition of the properties of hydrogen, and the first full account of those of carbonic acid, besides investigations into the combining proportion of the latter, and the properties of carbonates. They recount also the first successful attempt to determine the differences in density which characterise the gases, and suggest the probability of there being more kinds than one of inflammable air. A paper which was published by Cavendish in the *Philosophical Transactions* for 1767, may be considered as an extension of the research into the properties of carbonic acid. It is occupied with an account of the Analysis of one of the London pump waters (that, namely, of Rathbone Place), which was remarkable for the quantity of calcareous earth which it deposited when boiled. Cavendish showed that the earth was originally retained in solution by carbonic acid, which the boiling dissipated, so as to allow the earth to precipitate. The other constituents of the water were determined also, and the whole research is curious as one of the earliest tolerably successful attempts to analyse a natural water. Abstracts of these papers are given in the sequel, which those who wish to see their exact contents will consult. I refer to them here, only as showing the prominent position which Cavendish took from the first as a discoverer in chemistry. He may be counted the third in order of time among the four great English pneumatic chemists of the eighteenth century, the other three

* *British Association Report*, 1839.

being Hales, Black, and Priestley. Hales was the earliest enquirer into the properties of elastic fluids, and, without injustice to his illustrious predecessors, the immediate disciples of Bacon, and early contemporaries of Newton, who had made some progress in investigating the properties of the gases, he may be called the father of pneumatic chemistry in England. His great merit was to point out that elastic fluids may be obtained from an immense variety of organic and inorganic substances, of which they are as important constituents as the solids or liquids which may be separated from them. Hales did not recognise, unless very imperfectly, that those elastic fluids were chemically unlike, and specifically distinct, so that he spoke of them as if essentially identical with each other and with the atmosphere; and had no other name for them than simply air. His writings belong to the first third of the preceding century. The second of the pneumatic chemists, Black, appeared a little after the middle of the century, and by his celebrated essay on Magnesia Alba, demonstrated that there existed at least one gas totally distinct from the atmosphere, and able by its addition to bodies, or its removal from them, to alter immensely their physical and chemical properties. Black thus rose to a higher discovery than that reached by Hales. The latter had shown that the most solid stone might owe half or more of its weight to the presence of an imprisoned or solidified air; but he had paid little or no attention to the effect which the removal of this air had in altering the chemical properties of the substance from which it had been extracted. Black demonstrated that the fixed or solidified air did not merely increase the bulk and weight of the solid, but determined in a most striking manner its chemical properties, so that a substance which, when saturated with a peculiar air, was a bland, innocuous insoluble powder, or crystalline solid, became by the expulsion of this air soluble, caustic, and corrosive; and the difference between marble or chalk on the one hand, and quicklime on the other, was shown to be entirely dependent on the presence or absence of the gas, which Black named *fixed air*, and we name *Carbonic Acid*.

Twelve years after the publication of Black's paper, namely in 1766, Cavendish published the first of the essays we have been considering. He took up the investigation of fixed air where Black and his pupils had left it, and examined in particular its properties when free, on which Black had published scarcely anything.

Thus far Cavendish appears rather as the follower of Black than as an independent observer, although by a reference to his paper it will be seen that his investigation of the properties of free Carbonic Acid was equally original and accurate. He struck out, however, in addition, a new path for himself, and added to the solitary fixed air, a second gas equally distinct from it and from atmospheric air in properties. This was Hydrogen, of which Cavendish cannot be called the discoverer, for many of his predecessors, Boyle among others, had encountered it; but no chemist had carefully examined its properties, or at least had described them. His predecessors, indeed, knew only as much about the gas as a navigator who merely touches at a strange island, knows of its geography and various products, to whom we cannot deny the merit of being its discoverer, although we often assign much more credit to some later visitor who surveys and describes the new territory. The mere discovery of hydrogen was no great feat; for the most random experimenter, who, with or without purpose, handled the more powerful reagents, was likely to encounter a phenomenon of which the conditions are so simple as the evolution of hydrogen from the contact of iron and an acid; and the ready and explosive combustion of the gas when it meets flame could not fail to attract the attention of the most heedless observer. There can be little doubt, accordingly, that among Cavendish's predecessors backwards through several centuries, there were many who could assert equally good claims to be called the discoverers of hydrogen, of which, nevertheless, they knew exceedingly little. Cavendish did not claim to be one of them, but he could claim a merit which was much greater. Boyle, Mayow, and Brownrigg had preceded him in showing how gases may be collected, but no one had given an example of the mode of examining them. Cavendish's examination, accordingly, of the properties of carbonic acid and hydrogen, has all the interest that attaches to the first demonstration of a method of pursuing a novel investigation. It is easy to look back from our thoroughly appointed laboratories, filled with the apparatus which some ninety years have added to the chemist's instruments, since the date of the investigation we are discussing, and to criticise and depreciate the methods and results it records; and this has been done largely and unreasonably. Yet if we consider how much more genius is requisite for the devising of an apparatus or method of research which is quite new than is needed for its indefinite extension and improvement, and if we further judge the experimenter of 1766, not by his successors of 1840 or 1850, but by his contemporaries, we shall not hesitate to assign a very high rank to Cavendish, as one of the earliest investigators of the chemical properties of the gases. We find him, for example, collecting the elastic fluids on which he experimented, with various precautions to secure their purity, observing carefully from how many different sources they could be procured with identical properties, and determining with numerical precision the relative volumes yielded by different processes. The questions of their permanent elasticity, their solubility in different liquids, their combustibility or power to support combustion, their specific gravity, and likewise their combining equivalent, were all carefully enquired into. The apparatus employed, though deficient in delicacy according to modern standards, was unexceptionable in principle, and wherever that was possible, was made to yield quantitative results, so that this earliest analyst of the gases introduced the principle of rendering all descriptions of phenomena as precise as possible, and endeavoured from the first to attach a numerical value to each. We shall find, in truth, that we have done little more in later times than extend, improve, and as we complacently say, *perfect* Cavendish's processes for the analysis of gases, and that we differ from him more in our mode of interpreting certain of the phenomena he witnessed, than we do in our methods of investigating elastic fluids. Thus, he mistook altogether the source of the hydrogen which he procured so abundantly from the solution of iron, zinc, and tin, in sulphuric and muriatic acids, and referred it to the metals in which he supposed it to exist in a peculiar state of combination. Water at this time, it will be remembered, was supposed to be an element, and the composition of all the acids was unknown. No gas had been certainly traced to a liquid as its source, otherwise than as dissolved in it like carbonic acid in a mineral water; whilst Hales and Black had shown that the most fixed and solid bodies might yield from their very substance large volumes of elastic fluid. It has occurred to me as possible, that this may have been one reason which induced Cavendish to suppose that the hydrogen came out of the metal rather than out of the liquid. The consideration, however, which mainly weighed with him, and the only one to which he himself refers, was the belief common to him with the majority of his contemporaries, that the metals contained a peculiar combustible principle named phlogiston. This Cavendish supposed to abandon the metal, and, assuming the form of an elastic fluid, to show itself as the inflammable air. To his opinions on this point I shall have occasion to refer frequently in the sequel.

Hydrogen was thus the first of the combustible gases examined, and for many years, as we shall afterwards find, great confusion existed in the mind of chemists as to the number and nature of the different inflammable elastic fluids; nor did this begin to cease till the composition of water and of carbonic acid was ascertained. Cavendish, however, had clearer views on this very important point, than most of his fellow chemists. He ascertained that vegetable and animal matters, by putrefaction and destructive distillation, yielded inflammable air. He was not aware of its exact nature, but he satisfied himself by the test of specific gravity, and the volume of common air required for its combustion, that it was not identical with Hydrogen, which accordingly he distinguished as the "inflammable air from metals." He further observed that "the nature of the inflammable air was not quite the same" from animal as from vegetable substances. We shall afterwards find that he turned these observations to excellent account in the researches which led him to the discovery of the composition of water.*

Between the years 1767 and 1783, Cavendish did not appear before the public as an author on any subject directly connected

* Cavendish's observations on the peculiarity of the inflammable air from organic bodies, will be found partly referred to in the third section of the experiments on factitious air; but chiefly in Part IV, published from the MS. by Mr. Harcourt, *British Association Report*, 1839, pp. 58-62.

with chemistry, but it appears from his MSS., that he continued to prosecute chemical enquiries. Among his papers is one on which he himself has written, "communicated to Dr. Priestley," the contents of which are referred to by the latter in his account of *Experiments and Observations made in and before the year* 1772, so that Cavendish's communications to him cannot have been later than that date. The paper in question has been printed by Mr. Harcourt,* and is remarkable as containing one of the earliest distinct accounts of nitrogen. Cavendish prepared it by passing atmospheric air repeatedly through red hot charcoal, and removing the carbonic acid produced, by caustic potash. He gives the following description of it: "The specific gravity of this air was found to differ very little from that of common air; of the two, it seemed rather lighter. It extinguished flame, and rendered common air unfit for making bodies burn in the same manner as fixed air, but in a less degree, as a candle which burnt about 80″ in pure common air, and which went out immediately in common air mixed with $\frac{6}{55}$ of fixed air, burnt about 26″ in common air mixed with the same portion of this burnt air."† Cavendish gave no special name to nitrogen, which he referred to generally as *mephitic* air. It was afterwards minutely described by Lavoisier and Scheele, and was distinguished by Priestley and his contemporaries, by the name *phlogisticated air*. The quotation adduced above, shows incontestably that Cavendish discovered nitrogen for himself, and had ascertained with great precision its chief properties; but in the absence of precise dates, I hesitate to adopt Mr. Harcourt's conclusions, that the paper from which I have quoted contains "the *first* clear description of nitrogen as a distinct gas." Dr. Rutherford, of Edinburgh, the reputed discoverer of nitrogen, published his Thesis *De Aere Mephitico*, in 1772. His process for procuring the gas, for which he had the same general term as Cavendish, viz., mephitic air, resembled that of the latter chemist, except that he employed atmospheric air vitiated by respiration, not by combustion. This he passed through caustic potash, and tested by lime-water, which it did not precipitate, whilst it possessed the power of extinguishing life

* *British Association Report*, 1839, p. 64.
† *Op. Cit.* p. 65.

and flame.* The dates of publication, or announcement, of Cavendish and Rutherford's observations, are thus the same, whilst the dates of their experiments are uncertain. We cannot in these circumstances give precedence to the former, but it is certain that he was an independent discoverer of nitrogen.

For a season after making the researches referred to, Cavendish appears to have laid aside chemistry for other departments of physics. In 1771 he published the elaborate paper on the theory of the principle phenomena of electricity, which appears in the *Philosophical Transactions* for that year. In 1776 appeared the curious and interesting accounts of his *Attempts to imitate the effects of the Torpedo.* In the abstract of this paper, the exact significance of which has been a good deal mistaken, I shall point out the novel and important conclusions which it brought to light. It may suffice, therefore, here to say, that the singular power which the torpedo possesses, of benumbing those that touch it, had been referred with great ingenuity and force of argument, by Walsh and others, to its possessing the means of discharging electricity at will. In many respects, however, the action of the torpedo differed from that of any known electrical apparatus, so that some refused to believe that the benumbing sensation occasioned by the fish was an electrical phenomenon; whilst others went to the opposite extreme, and regarded animal electricity as quite different in kind from that procured from other natural or artificial sources. To decide between those views, or supplant them by a more accurate one, was no easy matter, for the torpedo could only be procured in the Mediterranean, and other warm seas, nor did the resources of electrical science, in the year 1776, when only one of its departments had been studied, suggest the means of questioning the torpedo as to its singular powers, even if it had been more accessible than it was. Cavendish, accordingly, attacked the problem in another way. He tried whether he could not successfully imitate the effects of the living fish, by a piece of apparatus constructed in imitation of it, and placed in connec-

* *Thomson's System of Chemistry,* vol. i. p. 203. Dr. Thomson adds, by way of comment to his account of Rutherford's observations, "When Hawksbee passed air through red-hot metallic tubes, he must have obtained this gas, but at that time the difference between gases was ascribed to fumes held in solution."—See *Phil. Trans. Abr.* v. p. 613.

tion with a friction electrical machine and a Leyden battery. He succeeded so well in his imitation, that all doubts as to the identity of the torpedinal benumbing power, with common electricity, were removed. The demonstration of this identity, however, was far from a simple matter, for the apparent difference between the force displayed by the torpedo, and that exhibited by the electrical machine, or Leyden jar, was very great. Cavendish, notwithstanding, not only showed that the two forces were in essence identical, but deduced from his study of their apparent differences, conclusions concerning the extent to which electricity, not of animal origin, may vary in its modes of manifestation, which are now regarded as having marked an era in the progress of the science. The most illustrious of his successors, Faraday, among others, have borne testimony to the light which was thrown upon every department of electrical enquiry, by Cavendish's demonstration, that the most opposite effects might be obtained from electricity developed in the same way, by causing its intensity to vary as compared with its quantity. Thus, though voltaic electricity was not discovered till some quarter of a century after the publication of the paper on the torpedo, Faraday found the theory which Cavendish suggested, sufficient to explain the curious and apparently contradictory voltaic phenomena which he observed so late as 1833. "The beautiful explication," observes he, "of these variations afforded by Cavendish's theory of quantity and intensity, requires no support at present, as it is not supposed to be doubted." * When it is remembered, that in 1776, friction and atmospheric electricity were the only forms of that force which had been studied, and that they were but imperfectly known, we cannot but admire the sagacity with which Cavendish found, in the very perplexities of animal electricity, the means of explaining it, and rose at once to the recognition of a law so wide in its bearing, that whilst it interpreted the difficulties of the existing science, it also furnished a key to the problems which were to vex the students of the voltaic, thermic, and magnetic electricities of a later century. In none of his essays does Cavendish appear to greater advantage than in this.

He had now been before the public, so far as one of the most

* Faraday's *Experimental Researches on Electricity*, series iii., p. 81.

reserved of men can be said to have been so, for ten years, and as all his published papers had been communicated to the Royal Society, it is not surprising that he should have been selected by that body in 1776 to describe the meteorological instruments which were made use of in their apartments. The Society had commenced in 1773 recording their observations with the thermometer, barometer, rain-guage, hygrometer, variation-compass, and dipping needle, and Cavendish was applied to, to give an account of these. His father, Lord Charles Cavendish, had devoted himself to meteorology, and had paid special attention to the improvement of the thermometer and barometer, so that probably our philosopher was early trained to the points essential to the accurate construction and employment of these instruments. What familiarity he had with the others mentioned above does not appear, but the fact of his being entrusted by the Society with the description of all their meteorological apparatus, shows how entire was their confidence in the extent of his acquirements and the accuracy of his observations. Their confidence in him is the more remarkable, that he had published no paper before 1776 referring to any of the instruments he was called upon to describe. That he had paid great attention, however, to the thermometer before this period, is certain from his unpublished papers on heat of 1764 and 1765; and as he made no concealment ot his researches, although he did not publish them, there can be no doubt that many of the members of the Society were well aware of his familiarity with meteorological instruments. The most important part of this paper is his description of the best method of accurately graduating thermometers, which will be found specially referred to in the abstract of his papers on Heat.

The succeeding year 1777, or perhaps rather 1778, marks the period when he commenced his most important chemical researches; he styled them *Experiments on Air*. They were carried on with frequent, and sometimes long interruptions till 1788, and no part of them was published till 1783. They led to the discovery of the constant quantitative composition of the atmosphere, the compound nature of water, and the composition of nitric acid. Their discussion occupies the greater part of this volume.

In 1783, also, Cavendish published his first paper on heat, embodying some of the results he obtained in 1764 in reference to the freezing or solidifying point of liquids. He returned to the subject again at intervals onwards till 1788, so that his papers on heat and his experiments on air, so far as published, extend over the same period. It will be more convenient, however, to take those on each subject continuously, than to discuss alternately the researches on heat and the experiments on air, and I shall take the papers on heat first. They are three in number, and belong to the years 1783, 1786, and 1788. All of them refer to congelation; the first to that of quicksilver, the second and third to that of the mineral acids and of alcohol. Their contents are of more interest as containing a statement of Cavendish's views regarding the laws of liquefaction and congelation, than as reporting mere phenomena. They are all, indeed, commentaries upon observations made in North America by officers of the Hudson Bay Company on the effect of great natural cold, assisted by powerful freezing mixtures in congealing mercury, nitric acid, oil of vitriol, and spirits of wine. These observations were made under Cavendish's directions, and at his cost, and abstracts of his commentaries on them will be found in the sequel. The most important of these papers was that on the freezing of quicksilver. This metal, which had long been imagined by the older chemists to owe its apparent permanent fluidity to some anomalous peculiarity, was frozen in a thermometer in 1759, O.S., by Professor Braun, of Petersburgh, who observed that its congelation was accompanied by a descent of the mercury, through many hundred degrees, and came to the conclusion that the freezing point of the metal was some 300° or 400° below Farhenheit's zero, but was unable to determine the exact point of congelation. In drawing this startling conclusion, which implied, if true, that the most enormous differences in temperature might occur between unlike regions of the globe, and that in northern latitudes the thermometer might fall through two, three, or four hundred degrees in a few hours, Braun confounded two phenomena. The one of these was the contraction which accompanies the cooling of liquid mercury; the other the further contraction which attends its solidification. The contraction due to both these causes, exaggerated by the peculiarities which attend the freezing of mercury in capillary tubes, was referred by Braun solely to the first. To his conclusion the majority of the natural philosophers of Europe assented, but there were two who, from the first, discredited it. These were Cavendish and Black, who, unaware of each other's intentions, and conducted to their results by independent researches, suggested the same way of ascertaining the true freezing point of mercury, which they felt satisfied was much higher than Braun had imagined. This method, which was put in practice by Governor Hutchins at Albany Fort, Hudson Bay, consisted in freezing the mercury, not in the tube of the thermometer, but in a separate vessel, in which the instrument was plunged, and guarding carefully against the whole of the external quicksilver becoming congealed. Cavendish, like Black, knew well from his experiments of 1764 and 1765, that when a liquid begins to congeal, it acquires a temperature which remains constant till the whole of it is frozen. He did not doubt that mercury would resemble, in this respect, the liquids upon which he had experimented, and he furnished Mr. Hutchins accordingly with an apparatus founded on this principle, which was employed with complete success. The result was, that the freezing point of mercury is not more than 39° or 40° below Fahrenheit's zero, a determination which all subsequent observation has confirmed. To Cavendish and Black is thus owing the merit of overturning the extravagant conclusions regarding the lowest natural or attainable temperature which Braun had promulgated. The merit due to either may seem small, for the observation of the exact freezing point of a particular liquid would not, at the present day, secure much honour for its observer. The determination of the congealing point of quicksilver, however, is a point of interest in Cavendish's history, not in itself, but as an evidence of the very clear and broad view which he had taken of the relation of heat to liquefaction and congelation, and his exact acquaintance with their essential phenomena at a time when, with the exception of his great contemporary Black, there was not, probably, a philosopher in Europe who understood the true nature of congelation. Mr. Hutchins's observations were not made till 1782, but the directions by which he was guided had been laid down by Cavendish in 1764 and 1765.

The other two papers on heat which discuss the congelation of the mineral acids and of spirits of wine are of less importance. They supply additional proofs of Cavendish's familiarity with the laws of heat; but as they are abstracted and commented on in the sequel, I need not refer to them more fully here. One of these papers contains a very curious example of an acquaintance on Cavendish's part with what we should now call the law of reciprocal combining proportion by weight, which is generally supposed to have been first discovered by Wenzel and Richter, and to have been fully announced by Dalton.

The experiments on air to which I now turn, supplied materials for four papers, besides leading to the observation of many phenomena which were never made public. Of these unpublished observations Mr. Harcourt has given an account, including a lithograph fac-simile of a portion of Cavendish's laboratory note-book.* I have also referred to those private records in the sections of the Water Controversy devoted to the dates of Cavendish's experiments and conclusions, so that I shall only allude to them here very generally.

In the interval which elapsed between the publication of Cavendish's first chemical papers and those we are now discussing, Priestley, the fourth of the great English pneumatic chemists, had appeared on the field; while Scheele, in Sweden, and Lavoisier, in France, besides other less distinguished observers in different parts of Europe, had effected the discovery of nearly all the gases known to us at the present day, and their study engrossed the attention of every chemist. In particular, the relation of the atmosphere to combustion demanded explanation, and the nature of the change which the air underwent when inflammables, burned within confined portions of it, deprived it of the power of further supporting combustion. At this problem all the active chemists of Europe were now working, but with very unequal success, owing to the false theory of combustion which the majority espoused, and the erroneous opinions which were current concerning the con-

* *British Association Report*, 1839, pp. 45–68.

stitution of atmospheric air. The nature of Cavendish's most important chemical discoveries cannot be understood without a reference to these points, which, however, shall be very brief.

Boyle, Hooke and Mayow in England, and Rey in France, besides other early disciples of the school of Bacon, understood the true nature of combustion in air much better than the immediate predecessors of Lavoisier. The former held as we do, that a burning body is literally fed by the air, and they apprehended with considerable clearness, that burning combustibles add something to themselves from the atmosphere. Some of these observers were also well aware that combustibles are converted by combustion into substances possessing greater weight than the original inflammable. In an evil day, however, Beccher and Stahl, two men of unquestionable genius, devised a theory of combustion which led all chemistry astray for half a century. According to their view combustion consisted in the emission from the combustible of a peculiar fiery principle, to which the name *phlogiston* was given. It was present in all inflammables, however different their appearance and properties. When they burned, it passed out of them into the air which surrounded them, and by its loss they became changed in character and quite incombustible; but if phlogiston was restored to them, they recovered their original appearance and properties, among the rest, their combustibility.

Much has been said by the historians of chemistry in praise of this theory as having served, in spite of its inaccuracy, to guide chemistry to great results, at a time when the science was not ripe for a juster theory. From this statement I must totally dissent. Its devisers assuredly were men of rare gifts, and their theory, welcomed by their fellows and immediate successors as a great boon to the science, exerted for some forty or fifty years a strange fascination over all the chemists of Europe. These forty years, however, were like those spent by the Israelites in the wilderness, after their glimpse of the Promised Land. Had Stahl and Beccher carried out the conclusions which the early disciples of Bacon had imperfectly announced, we should not have waited till the close of the eighteenth century, and the advent of Lavoisier, for the true interpretation of the nature of combustion. A Joshua would have been found some half a century sooner, and the goodly land which the chemists cultivate, would exhibit a much wider extent of fertile territory than it does at the present day.

It is a vain thing assuredly to speculate on what would have been, if what has been had not been, or had been otherwise. The progress of science, we will not doubt, is determined by great laws, which we may some day be able to trace; and as it is, we perceive clearly enough that human progression is not a continuous upward flight, but an alternation of risings and fallings, which greatly retards the degree of additional elevation attained within a given time, so that we need not wonder that chemistry should exhibit in one of its epochs a retrogression for some half a century; but at least we should refrain from calling it progress. We may believe that such relapses will become fewer and fewer, as the human race grows older, but in the meanwhile no service can be rendered to the cause of truth by affecting to deny that, especially in the early history of the sciences, we find long periods of total stagnation, and the tide even ebbing, when by our calculations it should have overflowed.

So large a portion of this work is unavoidably occupied, with discussions concerning phlogiston, that even the most general reader must have some conception of its meaning. Stahl's theory of phlogiston was not a refined speculation. It scarcely deserves to be called a scientific hypothesis. It really amounted to nothing more than the assertion, that a body was combustible because it contained something combustible; which was equivalent to the identical proposition that a body burned because it burned. This declaration instead of being a refinement of philosophy, to which only a man of science could reach, was but the reduction to terms, of a vulgar belief. It was a poetical, rather than a scientific thought; for the natural tendency of every untrained imaginative mind, as we see in children, and in the early history of all nations, is to impute every manifestation of power, to the presence in the body manifesting it, of some inner principle more or less self-sustaining, and resembling a living or vital agent.

The same spirit, which made the Greeks people the winds and the waves, the rivers and the trees, with gods; which makes the savage regard the compass needle as animated; and the child demand to see in some visible shape, the motive principle of a watch or moving toy; led the Chemists of the seventeenth and eighteenth centuries to declare that a candle burned because it contained a burning principle. I have sometimes thought that this theory was in part occasioned by the spectacle of the sun and other heavenly bodies unceasingly emitting heat and light. I have found, however, no reference to this striking phenomenon in the writings of the phlogistians; and however much the unbroken radiance of the sun might justify a popular belief in the power of combustibles simply to emit light, it could never justify the assertion of this even as a probable truth, for this would have been to explain one mystery by another. It seems to me, on the whole, that whilst poetry might have welcomed the doctrine that a blazing body throws off light and heat, as a bell utters a sound, or a flower exhales an odour, that science could only accept it as an hypothesis of no great likelihood or high value, and which at all events required at once to be tested, as to its utility as an interpreter of known phenomena, and a guide to the discovery of new ones.

The doctrine of phlogiston, however, was not dealt with thus. Instead of being treated as a doubtful hypothesis, it was employed as a perfect theory; and phenomena at variance with it were either wilfully overlooked, or compelled to adjust themselves to its Procrustean bed. A true hypothesis, or one in the main, true, is always found capable of explaining more than it professed or expected to explain. But the phlogiston hypothesis transgressed its own self-imposed conditions, and failed to explain the most simple and essential phenomena of combustion. Thus its presence in bodies was held to confer upon them combustibility, yet when transferred from a blazing combustible to air, instead of rendering the latter inflammable, and changing it into a gas which could be kindled, it changed it into one which was totally incombustible and at once extinguished flame; for phlogisticated air in its simplest form was our nitrogen. Again, phlogiston was held to be a material and therefore ponderable substance, so that its escape from a combustible should have caused the latter to diminish in weight; yet the metals and phosphorus were known to increase in weight by combustion. Thus the lameness of the phlogiston hypothesis was betrayed at its first step, and it had to be furnished with a crutch, in the shape of an assumption that it was a principle of levity, so that a body containing it weighed less than if it were absent, before it could move a step further. Many of the Phlogistians, indeed, did not adopt this assumption, which confounded so strangely absolute weight with specific gravity; but they ignored the phenomenon of increased weight, which they could not explain, and stood in the anomalous position of professors of a Quantitative Science, who should weigh and measure at every step, and yet had put aside the balance as a useless thing. This, however, was not all. That a burning body changed the quality of the air around it, whilst itself undergoing a complete change of properties, had not escaped the attention of the phlogistians. Beccher and Stahl, although they made no investigation into the nature of the change which air underwent when it supported combustion, were aware that a limited quantity of air in which a combustible had burned till it was extinguished, could not a second time support combustion, a fact indeed which was matter of universal belief from the earliest times.

Such, then, was the crude and clumsy hypothesis which was recognised as a fundamental law of all chemistry, at the period when Cavendish commenced his Experiments on Air. Their object was to ascertain what Beccher and Stahl should have ascertained before they promulgated their hypothesis, viz., what change does combustion effect upon air. The discovery of oxygen, of nitric oxide, and of other gases, and the experiments which Priestley, Scheele, and Lavoisier had been assiduously making for some years, had directed the attention of chemists to the fact, that air not only became irrespirable and unable to support combustion when exposed to the action of burning inflammables, but at the same time underwent a diminution in volume, so that a portion of it was to appearance lost.* To discover what became of the lost air was a question

* Barbarous as was the nomenclature introduced by the phlogistians, and complete as has been its abandonment, they employed one phrase for which it would be well if we possessed an equivalent. This was *phlogisticated air*, a term applied in its widest acceptation to air so vitiated as to be unable to support respiration or

which, in 1777, greatly interested the active chemists of Europe, and Cavendish's attention was specially directed to the problem, by the researches of Scheele on this point, as appears from a statement in the MSS. note-book of the former. Priestley and Lavoisier had, contemporaneously with Scheele, investigated the same subject; and all three had made some progress, especially Lavoisier, in explaining the problem. When those researches commenced, air was universally reputed to be a simple or elementary body. It was liable, according to the phlogistians, to vitiation, by the addition to it of phlogiston, so that it was referred to as being more or less phlogisticated, according to the degree of its power to support respiration and combustion. When oxygen was discovered by Priestley and Scheele, it was regarded by them as air altogether respirable, and exhibiting a maximum power of supporting combustion, because it was quite free from phlogiston. It was named accordingly *de-phlogisticated air*, and for a season the atmosphere was referred to as consisting of two parts, a "dephlogisticated part" and a "phlogisticated part," which differed from each other only in degree. By-and-by those parts were regarded as differing in kind, not merely in degree; the dephlogisticated part, or dephlogisticated air, being our oxygen, and the phlogisticated part or air, our nitrogen. Cavendish's enquiry began before this later view became general. He had proceeded but a short way in his attempt to discover what became of the air apparently lost during combustion, when he was arrested in his researches by the necessity which their successful prosecution laid him under of ascertaining the quantitative composition of atmospheric air. The problem which originally interested him presented itself in this shape. If any combustible, such as hydrogen, phosphorus, or a candle, was allowed to burn till it went out, in a portion of air confined over water, the volume of the air was observed to diminish as the combustion proceeded, and at its close the water was found to have risen through about a fifth of the space originally occupied by the air. Cavendish, with the precision which characterised all his

combustion. We have an equivalent phrase, so far as respiration is concerned, in the term *irrespirable*, but we are compelled to employ a tedious circumlocution when we wish to refer generally to air *incapable of supporting combustion.*

researches, sought to ascertain the extent to which air could thus be diminished in volume, before he decided *why* it was diminished. His note-book shows that he investigated the two questions simultaneously, but he first announced the result of his enquiry into the composition of the air. This he published in 1783, and, like all his other papers, the modest title which it bore, *An Account of a New Eudiometer*, conveyed a very imperfect idea of its contents. Referring the reader for a full analysis of these, to the abstract of the paper in the sequel, I may notice here, that it is ostensibly devoted to the explanation of an instrument for determining the proportion of oxygen in air, by observing the contraction which followed its mixture with a given volume of nitric oxide. Priestley, the first investigator of the properties of nitric oxide, had devised this process, but was too inaccurate a manipulator to make good use of it. A more careful observer, indeed, than the ingenious Priestley might have been led astray, by the employment of his nitrous gas, for it can combine with oxygen in various proportions, according to the mode in which it is mixed with air, so that the amount of contraction may be very different, although the volume of oxygen is the same. Priestley, however, and the great majority of his contemporaries were either ignorant or heedless of this fact, and conceived that the purity of air might be accurately measured, by observing the amount of contraction which attended the mixture of it with nitric oxide over water; the air being the purer the greater the extent of contraction. Experimenting in this way, they could not miss arriving at the conclusion, that the atmosphere varied excessively in purity; and we find them accordingly travelling from place to place, analysing what they called the good air and the bad air of different localities, and coming to the most extravagant conclusions as to the relative purity of specimens of it, in which all the refinements of our modern analysis would fail to detect any difference. The instruments which they employed, they characteristically termed *Eudiometers*, or measurers of the goodness of the air; the object of the analyst being to determine the freedom of the air from phlogiston, which rendered it bad in proportion to the amount of it present.* By the performance of an immense

* The word *eudiometer*, which remains in our nomenclature as the solitary

number of elaborate experiments, Cavendish succeeded in perfecting a process, by means of which he could employ nitric oxide so as to occasion a constant amount of contraction, when mixed with different portions of the same specimen of air. Having certified this, he applied his method to the determination of the two important questions: Is the atmosphere constant in composition? And if so, what is its composition? To solve these problems, he experimented for some sixty successive days in 1781, making many hundred analyses of air. He compared, also, air collected at one period of the day with that collected at another, and that of the town with that of the country. He came to the conclusion which all subsequent observations have confirmed, that no sensible difference can be detected by Eudiometrical analysis between the purity of different specimens of atmospheric air. It was universally such "that the quantity of pure air in common air is $\frac{10}{48}$,"* or as we should now word it, the per centage by volume of oxygen in air is 20.83. This number approaches very closely to that obtained in our most recent analyses, and is remarkable for its accuracy, when we consider how totally the great majority, not only of Cavendish's contemporaries, but also his successors, even among living philosophers, failed to obtain any constant results with nitric oxide eudiometers. Cavendish is entitled to be called the discoverer of the constant composition of the atmosphere, and its first accurate analyst. It may be noticed here, to prevent subsequent confusion, that the atmosphere had long occupied his attention. So far back as 1766 he had imperfectly analysed it, by observing the loudness of the report which it gave when detonated with hydrogen. This device might be called an Acoustic Eudiometer. Whilst engaged also in the enquiry which we have been discussing, he checked the results obtained with nitric oxide, by observing the diminution which air underwent when exposed to liver of sulphur dissolved in water, and when exploded with hydrogen in a shut vessel by means of the

relic of the phlogiston vocabulary, is now used in the sense of a measurer of the amount of oxygen present in air. By its introducers, however, it was intended to refer to a measurer, not of the presence of oxygen, but of the absence of phlogiston. I have referred to this point more fully in the abstract of the paper under discussion.

* MS. Laboratory Note-Book, p. 109.

electric spark. The apparatus last referred to is the one generally named at the present day Cavendish's Eudiometer, and is the instrument which, from its interest in connexion with the discovery of the composition of water, has been selected by the Cavendish Society as their emblem, and placed on the title-page of their publications. Cavendish, however, never named this instrument a Eudiometer, nor was it his device, but Volta's. The Society's emblem represents the instrument as it is constructed at the present day, not as it was used by Cavendish.* He concludes this paper with an estimate of the nature of the information which the eudiometer supplies, which he shows to be very much smaller than the majority of his contemporaries imagined. His views in this respect accord with those universally entertained at the present day, and are another monument to the caution and sagacity with which he kept himself free from the prejudices of his time, and anticipated conclusions which were not generally accepted till a recent period.

The protracted eudiometrical enquiry we have been considering, taught Cavendish the important truth, that when air was diminished in volume, or a portion of it to appearance lost, by the effect of burning combustibles or so-called phlogisticating agents upon it, the maximum amount of diminution which could be produced was equivalent in round numbers to one-fifth of the original volume of the air. He knew, also, that it was the dephlogisticated part, or pure air (oxygen), of the atmosphere, which disappeared during combustion, so that he was now fully prepared to enquire what had become of the lost oxygen. His account of this enquiry forms the first series of his Experiments on Air, which was read to the Royal Society in January 1784,

* The instrument, as Cavendish describes it, was a glass globe, with a brass stopcock, wires for passing the electric spark, and a hook, or other arrangement for hanging it to the beam of a balance. As constructed at the present day, and represented in the title-page of this and the other publications of the Cavendish Society, it is pear-shaped, and provided with a glass stopper, which can be held firmly in its place by screws, besides a glass stopcock in addition to a brass one, and a moveable stand to which it can be fixed, after it has been exhausted at the air-pump, and filled with a mixture of hydrogen and oxygen. Were Cavendish alive among us, he would not recognise the modern instrument as resembling any part of his apparatus, and it would startle him to hear it called his eudiometer, by which term he would understand the nitric oxide apparatus which he described in 1783.

exactly a year after the paper on the *New Eudiometer*. When he commenced those researches, he found an opinion prevailing, that the production of fixed air, or carbonic acid, is the invariable result of what he called the phlogistication, and we should call the deoxidation, of atmospheric air. He readily disproved the truth of this view, and also of another notion, that nitric, or sulphuric acid was produced in those circumstances; and having disposed of these erroneous opinions, he proceeded to observe with great care, what was the product of the combustion of hydrogen in air and in oxygen. Priestley, and a friend of his, Mr. Warltire, had already experimented on this subject, with a detonating globe of the same kind as that referred to previously, as called at the present day, Cavendish's Eudiometer.* Their experiments were made partly in metallic, partly in glass vessels, and when employing the latter, they observed a deposition of moisture follow each explosion, but Priestley paid no attention to this phenomenon, and Warltire referred it to the condensation of water which had been diffused in the state of vapour through the gases. It at once, however, attracted the attention of Cavendish, who from the first appears to have anticipated that in the deposited water would be found the oxygen, which disappeared during the combustion of hydrogen in air, and the explanation of the diminution in volume which attended the vitiation of air. It will be remembered that in his paper on hydrogen, of 1766, he had represented this gas as itself phlogiston. He now experimented accordingly upon it, not as an individual combustible which would yield a certain product, but as *the* phlogiston which was present in all combustibles, and the product of whose combustion would represent the universal product of combustion. He first employed hydrogen and air,

* The instrument which Volta introduced for firing explosive mixtures of gas, by means of the electric spark, and which still bears the name of Volta's Eudiometer, was a tube or cylinder open at one end. I do not know whether Volta ever employed a shut globe, but Priestley and Warltire certainly did before Cavendish, as he freely acknowledged, and they in their turn, as well as Watt, referred the device to Volta, so that it must be regretted that this apparatus has been called Cavendish's Eudiometer, especially as Monge used an exactly similar apparatus, which he also refers to Volta. It is the admirable use which Cavendish made of the detonating globe, not the devising of it, which justifies its employment as the Cavendish Society's symbol. The instrument itself might, perhaps, best be called, without reference to any one's name, the Spark Eudiometer.

varying their relative proportion, till he ascertained that ratio in which, after their explosion in a shut vessel, the air was found diminished one-fifth, whilst the residual air was free from oxygen, and possessed the properties of nitrogen. In place of the oxygen which had thus disappeared, and a volume of hydrogen twice as great which had burned along with it, there was found a certain amount of liquid. The globe, moreover, had remained shut during the experiment, so that nothing had been allowed to escape, and nothing ponderable had been lost, for the vessel was found to weigh the same after the electric spark had passed, as before the explosion. In short, there was exactly the same weight of matter in the globe after the explosion as before, but the oxygen originally present in the air, and twice its volume of the hydrogen which had been mixed with it, had disappeared as gases, and were replaced by a volume of liquid, which, of necessity, exactly equalled them in weight. Cavendish, accordingly, unhesitatingly concluded, that in the circumstances described, "almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity and are condensed into the dew which lines the glass." Having demonstrated in this way that the lost gas was accounted for, and remained in the produced liquid, he proceeded to investigate the nature of the latter. The globe explosions yielded too small a quantity of liquid for a full analysis. He burned together, accordingly, by direct combustion, a large volume of hydrogen with 2½ times that quantity of common air within a glass cylinder, and collected the liquid produced. This he found to be without taste, or smell, or action on colouring matter, and to leave no sediment on evaporation; in short, he observes, "it seemed pure water," and his full conclusion was, "that this dew is plain water, and consequently, that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water."

The proceeding quotation contains the account of the first conclusion that was drawn concerning the compound nature of water, and the possibility of producing it out of hydrogen, and the oxygen contained in air. Cavendish proceeded to try whether free oxygen, if detonated with hydrogen, would in like manner yield water. The method of procedure here was simple, for it was only necessary to fill the globe with a mixture of one volume of oxygen and two of hydrogen, and to explode it by the electric spark, to secure the entire conversion of the contents of the globe into water. Cavendish came as near this result, as a slight mistake in the adjustment of the combining volumes of hydrogen and oxygen, and the limits of error in such an experiment, at the period when it was made, on the whole permitted. In the course of these trials, however, an unexpected and perplexing phenomenon showed itself. The liquid instead of being pure water, was found in certain cases to consist in addition of an acid, which analysis proved to be the nitric, and a long and difficult investigation had to be prosecuted into the source of this acid, the composition of which it must be remembered was totally unknown in 1784. This startling phenomenon, on which the chemistry of the period could throw no light, would have stumbled the great majority of Cavendish's contemporaries, as we may assert very safely, for it led not only Priestley, but even La Place astray; and it was probably ignorance of the phenomenon on the part of Watt and Lavoisier, which saved them from being entangled in difficulties in their investigation into the nature of water. Cavendish solved the problem without one false step, and whilst he avoided the confusion in which it involved others, he built upon it an additional great discovery. After ascertaining that the appearance of nitric acid was not dependent on the source from which the oxygen was prepared, and that the acid did not show itself unless more than a combining measure of oxygen was detonated with the hydrogen, he traced its production to the presence in the globe of a little nitrogen, derived from the atmospheric air which had originally filled it, or had become mingled with the hydrogen and oxygen during their preparation or collection. He amply verified this conclusion, by showing that the artificial addition of nitrogen to hydrogen, mixed with more than one-half its volume of oxygen, increased the amount of nitric acid produced at each detonation, and on the other hand, that if the hydrogen instead of the oxygen was in excess, no nitric acid appeared, although nitrogen was present. In this way he demonstrated that the only product of the combustion of pure hydrogen and oxygen is pure water; but he was further led to a view of the composition of nitric acid, which he carried out in the second series of his experiments on air, and which secures to him the honour of being the discoverer of the composition of nitric acid, as well as of that of water.

Avoiding here any minute discussion of Cavendish's opinions on the nature of water, or of the controversy in which the publication of the paper under discussion involved him with Watt and Lavoisier, both of which questions are fully discussed in different portions of this volume, I would draw attention to the following considerations. The general conclusion to which Cavendish came concerning the nature of water, was in his own words, "that water consists of dephlogisticated air united with phlogiston;" and as dephlogisticated air was his term for oxygen, and phlogiston his term for hydrogen, this statement closely corresponds to the modern view of the nature of water introduced by Lavoisier. The two views cannot be considered identical, yet this is certain, that Cavendish was the first who consciously converted hydrogen and oxygen into water, and taught that it consisted of them.

His identification, however, of hydrogen and phlogiston, and his inheritance of the prejudices of the early phlogiston school, led him to the erroneous conclusion that every combustible contains hydrogen, and that the deoxidation of air and the oxidation of combustibles, are invariably accompanied by the production of water. In this respect he erred, but we may forgive the discoverer of so great a truth as that of the composition of water, for over-estimating its importance. To this, and to the other points glanced at in this sketch of the first series of experiments on air, I have referred fully in the abstract of the paper, and in the chapters devoted to the discussion of the Water Controversy.

The second series of experiments on air was read to the Royal Society in June 1785, about eighteen months after the reading of the first. The paper was occupied with the discussion of a point which had only been imperfectly examined in the earlier experiments, viz., the cause of the diminution in volume which attends the passage of the electric spark through air. Cavendish had imagined that this was owing to the combustion of inflammable matter in the apparatus. He now demonstrated, that though this might be an occasional and slight cause of the diminution of air exposed to electric sparks, it certainly was not the chief cause of its diminution, which, as it appeared, was mainly owing to the combination of the nitrogen and oxygen of the air, to produce nitric acid. In his first series of experiments, he had shown that if nitrogen mixed in small quantity with hydrogen, was exploded by the electric spark, along with excess of oxygen, nitric acid was produced. He now showed that the hydrogen might be omitted, and that if a mixture of pure nitrogen and pure oxygen be exposed to electric discharges, it will yield nitric acid; and farther, that if those gases be mixed in a proper proportion, over caustic potash, they may be entirely condensed, or converted by the electric spark into nitric acid, which combines with the alkali to form nitre. On the other hand, he pointed out that pure nitrogen, or pure oxygen, if taken singly, was not sensibly affected by the spark. The negative result with the single gases, as contrasted with the positive result, when both were taken, demonstrated, as Cavendish showed, that the acid resulted from a combination of the two to produce it. He did not, however, although he was aware of the possibility of such an explanation, affirm that nitric acid was a direct compound of nitrogen and oxygen, as we do at the present day. True to the doctrine which he had announced in his earlier paper, he contended that nitrogen was a compound of hydrogen and nitric acid, and that the effect of the passage of the electric spark was to occasion the combination of this hydrogen with the oxygen, whilst the nitric acid separated. He was thus not the direct asserter of the modern doctrine of the composition of nitric acid, which he deliberately set aside, although aware of the terms in which Lavoisier would have announced it; and in this he perhaps showed a little wilfulness, as well as neglect of that quantitative method of procedure which he in general practised more than any of his brethren, and which if it had led him on this occasion to employ the balance, would have compelled him to change his theory. His views, notwithstanding, concerning the combination of the two gases to produce the acid, were most explicit and unhesitating, and saved him from the error of Priestley and La Place, who inferred from the earlier observations on the productions of nitric acid, that it, as well as water, was a compound of hydrogen (or at least inflammable air) and oxygen. Cavendish, accordingly, has always been considered the discoverer of the composition of nitric acid, although in strictness of speech this is not the exact merit which an historian at the present day can assign him, seeing that he manifestly regarded nitric acid as a simple, or at least undecompounded body, whilst nitrogen, according to him, was a compound; his view being exactly that of a consistent phlogistian, who did not employ the balance, and in whose eyes nitrogen was a body constituted like a metal, of a calx (the nitric acid) and phlogiston. This opinion appears at the present day extremely different from that we entertain, but in 1785, when the most enlightened chemists were only in a transition-state from the phlogiston to the antiphlogiston doctrines, the difference went for much less than it does now-a-days: nevertheless, it should be freely acknowledged that Lavoisier put the true interpretation upon Cavendish's views. Yet the merit which can be assigned to the former is in reality small, for he discredited the reality of Cavendish's observations, and could not succeed in verifying his results, which would scarcely have been the case had he tried the necessary experiments with the same strong conviction of their reality which prompted his observations on the production of water. Cavendish was induced, by the failure of Lavoisier and others, to return to the subject; and in 1788 he published the last part of his *Experiments on Air*, in the shape of a record of the successful repetition, by a Committee of the Royal Society, of his observations on the conversion of a mixture of nitrogen and oxygen into nitric acid by the electric spark. The foreign philosophers, in truth, had in one case at least been quite successful, but did not perceive that they were so.

In the latter part of the original record of his trials on this subject, Cavendish prosecutes the enquiry into the nature of nitrogen, which he had in part pursued in 1772. He draws attention to the fact, that it was uncertain whether the phlogisticated (non-oxygenous) part of the atmosphere consisted entirely of a single gas; and he proceeded to test this by trying whether a given volume of the phlogisticated part was entirely convertible into nitric acid by explosion with oxygen. He found that it was, and thus supplied a demonstration of the homogeneous nature of nitrogen, such as none of his contemporaries could have given.

The paper of 1788 was the last upon chemistry which Cavendish published. The opinions of Lavoisier, which were then rapidly becoming general, diverted chemistry into channels foreign to those into which Cavendish had guided it; and although he regarded the triumphs of the great French chemist with characteristic composure, and even interest, he appears to have been repelled by the new aspect which chemistry was assuming, from engaging in its further prosecution. The catholicity of his scientific tastes and the serenity of his nature made a change of studies, or rather the abandonment of one among the many he always prosecuted, an easy matter.

His remaining published papers refer to meteorology and astronomy. In 1790 he published an essay on the height of a remarkable aurora seen in 1784. In 1792 an elaborate paper appeared, on *The Civil Year of the Hindoos,* of which it may suffice to give here the title. In 1797 a letter from him to Mr. De Mendoza Y Rios was published, containing *A method for Reducing Lunar Distances.* In the succeeding year appeared the paper which, next to those on the discovery of the composition of water and of nitric acid, and that on the torpedo, has made him most famous. This is the celebrated enquiry into the density of the earth, which was communicated to the Royal Society in 1798. The apparatus he employed was contrived by his friend the Rev. John Michell, who died before he had an opportunity of experimenting with it. It came into Cavendish's hands, who adopted the principle it embodied, but had the chief part constructed afresh, so as to ensure greater accuracy in the results it was expected to yield. Without attempting here to describe minutely the nature of the experiments made with this apparatus, it may suffice to say that they consisted in observing, with many precautions, the movements of a long lever delicately suspended by the centre, so as to hang horizontally, and furnished at either extremity with small leaden balls. When two much larger and heavier balls of the same metal were brought near the smaller ones, the latter were attracted towards them with a certain force, the measurement of which supplied one essential datum for the determination of the mean density of the earth. It was determined by Cavendish to be 5·4; in other words, our globe, according to him, is nearly five times and a half heavier than the same bulk of water would be. The experiments referred to are so difficult of performance, and the density of the globe is a point so important in reference to a multitude of astronomical and geological problems, that the "Cavendish experiment" (as the researches taken as a whole are generally called) has always been regarded with the greatest interest by men of science. In 1837, Professor Reich, of Freiberg, published the account of a careful repetition of the Cavendish experiment. He made the density of the earth, as Cavendish had done, 5·4. In a later repetition, however, made by the late Francis Baily, the astronomer, with extraordinary precautions to secure accuracy, a different result was obtained. After four years of protracted, though interrupted trial, during which several thousand observations were made, the final result was, that the density of the earth was higher than Cavendish had represented it, namely 5·6, or more than five and a half times that of water. No greater compliment to Cavendish's accuracy can be desired than that afforded by the fact that so accomplished a natural philosopher as the late Mr. Baily, provided with all the improvements which forty fertile years had added to mechanical contrivances, and aided by the counsel of many distinguished natural philosophers, with a committee of the Astronomical Society taking part in the proceedings, and Government defraying the cost of the experiments, did not, after nearly three years of actual experimenting, find a greater difference than that stated above, as distinguishing his result from Cavendish's.

The last paper which the latter published, was on an improvement in the manner of dividing astronomical instruments. It appeared in 1809, the year before his death.

Such then, is a brief sketch of all Cavendish's published essays as well as of his unpublished papers referring to Chemistry.

I have made an exception in the case of the chemical essays to the general rule which the conditions of this volume necessitate, viz. that attention should be limited to published papers only.

Cavendish's unpublished essays, however, upon chemical subjects (including heat), with the exception of that on arsenic, to which I have referred very briefly, have so direct a bearing upon his claims as a discoverer, the discussion of which occupies the largest part of this volume, that I have thought it necessary to notice them, especially as I profess chiefly to deal with his merits as a chemist, and write at the request of a society instituted for the promotion of chemistry and the sciences nearly allied to it. It would be doing Cavendish great injustice, if no reference were made to his other unpublished papers, for although his actual reputation is based upon what he made known to the world in his lifetime, and is all that the historian of chemistry is concerned with, it is otherwise with his biographer, who is not only entitled, but is required to form his estimate of his capacity from all the materials accessible to him. Unpublished papers, of the genuineness and authenticity of which there is no question, may at any time be produced as posthumous works, and alter the accepted estimate of an author's merits. The limits of this volume, nevertheless, do not permit the publication of abstracts, however brief, of all Cavendish's MSS.; nor am I competent to the task, if there were room for its being undertaken. I shall be satisfied with drawing attention to these papers, and with concurring in the hope expressed by Mr. Harcourt, that at least the more important among them will yet be published in full.

The references to them which follow, are founded upon a personal inspection of the original MSS., which, with the exception of those referring to electricity (at present in the hands of that most competent critic of their merits, Sir William Snow Harris), were placed at my disposal through the good offices of the Rev. W. V. Harcourt, and the courtesy of Lord Burlington, to whom they belong. Thus much may be said of them here. The portion most interesting in reference to the personal narrative, is contained in a parcel entitled "Journeys." It contains the account of a series of tours made through England during the summer and autumn of 1785, 1786, 1787, and 1793; besides the record (in Cavendish's handwriting) of a journey undertaken by Dr. Blagden through Belgium in 1789, and an untitled essay apparently occupied with a summary of the geological observations made in those journeys, and at all events, containing a general sketch of the upper formations prevalent throughout England. The records of those tours are in the shape of journals, drawn out at leisure, and for the most part, in Cavendish's handwriting. Some of them have been transcribed by a clerk, but they have been revised by Cavendish, who has corrected the blunders committed by the transcriber in writing unfamiliar scientific terms, and has supplied the gaps which occur at intervals, where the copyist apparently has been unable to decipher certain words in the text from which he transcribed. Cavendish appears to have been accompanied in these journeys by Dr. Blagden, who is frequently referred to. The Rev. J. Michell is also referred to, but I am not certain that he was a party in any of the excursions. They were so extensive as to include the larger part of England, but especially the Southern, Midland, and Western counties. Cavendish was as far as Whitby on the north-east coast, and Truro on the south-west, and one journey was devoted in greater part to a tour through the southern counties of Wales. He does not appear, however, to have visited the south-eastern counties of the island.

The object of these journeys was entirely scientific, and was mainly the investigation of the geology of the districts passed through, but much attention was also devoted to the mining operations and metallurgical processes which could be witnessed in the same places. Of the value of the geological observations I do not pretend to form an estimate, but they were evidently made with the same caution and precision which marked Cavendish's experimental enquiries. Heights were determined by the barometer; the temperature of springs carefully ascertained; the thickness, inclination, relative succession, and physical appearance of the rocks minutely noticed, and specimens of the characteristic minerals collected for analysis. The journals recording those observations, including the general sketch of the arrangement of the superficial strata of the island, deserve the attention of the historians of English geology. They embody the results of three very able observers, Cavendish, Michell, and Blagden, who, so far back as 1785, commenced a patient and extensive exploration of the geology of the country, the record of which can scarcely fail to contain much that is interesting to students of science at the present day. These journals also contain, as already stated, minute accounts of various manufacturing processes related to chemistry. In Wales, Cornwall, Derbyshire, Warwickshire, Staffordshire, Yorkshire, and elsewhere, Cavendish witnessed the mining, reduction, and working of tin, copper, lead, iron, steel, alum-schist, and the like, which he describes at great length, and with his usual admirable clearness. All interesting pieces of machinery also, which he encountered on his way, such as Watt's improvements on the steam-engine, which were explained to him at Birmingham by the great engineer himself, are referred to, and occasionally figured; and the recipes for producing peculiar effects, which he obtained from those engaged in the chemical operations he witnessed, are carefully recorded. No allusions occur to merely personal incidents or adventures; to the scenery, except as a geological character; or to individuals, however famous, unless as authorities on some fact thought worthy of being mentioned. The journal is limited to recording, in the fewest possible words, the strictly scientific observations made during a series of tours, which were prosecuted on Sundays as well as week-days, with one undeviating purpose, which nothing was allowed to disturb.

The remaining MSS. consist of papers on subjects included under mathematics, mechanics, optics, physical geography, meteorology, and astronomy, besides miscellaneous observations,

CHAPTER III.

CONTROVERSY BETWEEN CAVENDISH, WATT, AND LAVOISIER, CONCERNING THE DISCOVERY OF THE COMPOSITION OF WATER.

In the preceding chapter I have endeavoured to give some account of what Cavendish effected, alike by his published and unpublished labours, towards the extension of physical science. A detail of his labours seemed the best preparation for a just estimate of his merits as a scientific enquirer and his integrity as a man. In truth, unless to those who are already familiar with what he has done to extend our knowledge of physics, an exposition of his deserts would be little else than an appeal to their prejudices or ignorance. To those, however, who have interested themselves in the recent literature of chemistry, and who have made its progress in our own country a matter of special study, the position which Cavendish at present occupies in the eyes of no inconsiderable section of the learned public, will prepare them for the discussion which follows. Those who for the first time concern themselves with his labours, will be surprised to learn that the modest, retiring, and almost inordinately cautious man, whose early history and scientific researches have been detailed, has been accused within the last few years, both of incapacity and dishonesty, and by no obscure assailants, for among them rank more than one of our distinguished men of letters and science, whose connexion with the vexed question I have to consider, gives it an interest apart from that which it intrinsically possesses.

The ancient Egyptians counted no man's reputation certain, till death had set its seal upon him and his survivors had solemnly pronounced judgment upon his life and character. Their judgment, however, was a swift one, and was passed before the grave closed over its object.

It could have been wished that the posthumous ordeal to which Cavendish has been subjected, had followed his decease as speedily as it would have done had he prosecuted chemistry some thousand years ago, in the land where tradition asserts it to have had its origin. In that case he would have been tried by those who could speak from personal knowledge as to the charges preferred against him, and he would have been quickly acquitted. As it was, he went down to the grave with the gathered honours of more than threescore years and ten upon his head, and all who could have witnessed to his integrity had followed him to the tomb, before he was summarily denounced before a great public body as having unfairly claimed the honour of making a famous discovery, and of having done a brother philosopher a grievous wrong. These accusations have reference to the claim which he so modestly asserted, to be entitled the discoverer of the composition of water, and were first made extensively public some ten years ago.

The experiments on the composition of water were made in the summer of 1781. Owing, however, to the additional observations which were necessary in order to ascertain the nature of the acid which showed itself, and the long series of experiments which Cavendish, with the extraordinary patience and caution which characterised him, thought it necessary to perform before publishing his entire results, as well as in consequence of other researches, which were prosecuted simultaneously; his paper entitled *Experiments on Air*, was not read to the Royal Society till January 1784. The delay which thus happened has caused his claim to be entitled the discoverer of the composition of water to be contested; the rivals put forth being no other than the celebrated James Watt and the great French chemist, Lavoisier.

When Cuvier, in 1812, as Secretary of the French Academy, read an éloge on Cavendish, who was elected in his old age a member of the Institute, he said, in reference to the subject of his notice, that "his demeanour, and the modest tone of his writings, procured him the uncommon distinction of never having his repose disturbed either by jealousy or by criticism."*

It was reserved for Cuvier's distinguished successor, Arago, the present perpetual Secretary of the French Academy, to become Cavendish's accuser. When it fell to Arago's lot to

* *Eloges Hist.*, tom. ii., page 104.

write the éloge of Watt, which was published in 1839, he came to this country to collect materials for the purpose; and in the course of his researches among Watt's published and private papers, he arrived at the conclusion that Watt, and not Cavendish, was the discoverer of the composition of water. He published this, accordingly, to the French Academy, accompanied by what amounted to a direct charge against Cavendish of deceit and plagiarism, inasmuch as he was said to have learned the composition of water, not by experiments of his own, but by obtaining sight of a letter from Watt to Priestley. Throughout the whole éloge, moreover, Cavendish is made to appear as a mean, jealous, vain, and dishonest person, who by a cunning trick appropriated to himself the discovery of another, to whom he did not make even a show of restitution till he was detected in the fraud.

The scientific men of Great Britain were startled at the charge brought against Cavendish. Of all her illustrious philosophers, he was, without exception, the very last in reference to whom it was possible to believe that the accusation could be true. A man to whom applause had ever been hateful, and who had systematically avoided and declined the honours which his countrymen would willingly have conferred upon him, was not likely suddenly, and on a single occasion, to grow covetous of distinction and to seek to gain it by fraud. Moreover, it soon appeared that Arago had studied the papers of Watt (as was natural in his *Eulogist*) much more fully than those of Cavendish, and that his views were in consequence, to a great extent, one-sided. No time was lost in calling in question his accuracy. The French Academy had heard the one side argued, the British Association was to hear the other.

In August 1839, soon after Watt's *Eloge* was published, the British Association for the Advancement of Science met at Birmingham, and the President for the year, the Rev. W. Vernon Harcourt, in one of the most eloquent of the many able addresses which have been delivered at its opening meetings, took the opportunity of vindicating Cavendish, and of pointing out the mistakes which he regarded Arago as having committed. At a meeting of the French Academy subsequent to this, Arago affirmed that Harcourt's account was insufficient to establish Cavendish's claim as superior to that of Watt, and brought forward the distinguished French chemist Dumas as concurring in his opinion. When the report of the British Association for 1839 was published, Harcourt replied to these observations in a postscript to his address, which contains a most able analysis of the documents bearing upon the subject, and a thorough discussion of the whole question. He was at the trouble even to publish a lithograph fac-simile of Cavendish's *Laboratory Note-Book*, that no room might be left for complaint of incompleteness in his statement. Since 1839 various additional writers have taken part in the discussion, to whom I do not make any reference here, as their writings are described and criticised in the earlier sections of the portion of the body of the work entitled *The Water Controversy*. This controversy, in its original shape, was not carried on in public, so far as the English rivals, Cavendish and Watt, were concerned, but the publication of the *Watt Correspondence* has admitted us behind the scenes, or rather has converted the complaints which Watt made in private letters to his friends, into public impeachments of Cavendish's capacity and fair-dealing. Lavoisier was from the first accused by the English chemists, by Cavendish publicly, by Watt privately, of having acted unfairly towards them. Watt, however, was much more concerned to defend himself against his English than his French rival, whilst Cavendish took no formal notice of Watt's implied claim to priority in the disputed discovery, and limited himself to repudiating the demands of Lavoisier. No reply was made by any one of the three illustrious rivals, to the implied or asserted superior claims of his opponents, a matter greatly to be regretted, for hopeless obscurity now darkens some of the most important questions on which the controversy turns, and, as might be expected, these are the very points on which the partisans of the several claimants pronounce most confidently.

As an incident in Cavendish's personal history, I have at present only occasion to refer to the *origin* of the Controversy; but as his character has been much more seriously assailed in the publications that have appeared in consequence of its revival, and as this has enabled us to appreciate the merits of the original dispute much better than we should otherwise have done, both aspects of the question will be considered, and to some extent together.

Omitting all reference to the precursory experiments and speculations which shadowed forth the discovery of the compound nature of water long before that was effected, I take up the question in the spring of 1781. Some time before the 18th April (for the exact date is unknown) of that year, Dr. Priestley, availing himself of Volta's ingenious electric eudiometer, made what he calls a "random experiment," with a view "to entertain a few philosophical friends." It consisted in exploding a mixture of common air or oxygen with hydrogen, in a shut glass vessel, by sending an electric spark through it. When the spark had passed, and the explosion was over, the sides of the glass vessel were found to be bedewed with moisture, but to this phenomenon Priestley paid no attention. One of the philosophical friends who witnessed this experiment, was Mr. Warltire, a lecturer on Natural Philosophy in Birmingham, whose name, otherwise unknown in the history of science, emerges for a moment from obscurity in connexion with Priestley's random explosion, and immediately fades again into oblivion. With great ingenuity he proposed to test by a trial similar to Priestley's, whether "heat is heavy or not." To avoid the risk of injury from the explosion, he employed a copper flask, which he filled with a mixture of air and hydrogen,* and then weighed the vessel and its contents. When an electric spark was passed through it, and the mixture exploded, great heat was evolved, and after the flask had cooled, it was weighed again to ascertain if it had become lighter by the loss of the heat which had been given off. In several trials of which Priestley and Withering were witnesses, the flask appeared on the second weighing to have lost weight; from which Warltire seems to have concluded that heat is a ponderable body.†

* Priestley and Warltire both call the gas they used, "inflammable air;" it was probably hydrogen, and certainly at least contained it, for it yielded water on combustion. The meaning of the term "inflammable air," as employed by the chemists of last century, is one of the most important questions in dispute in the Water Controversy, and will be found fully discussed in one of the sections devoted to that subject.

† These researches are detailed in the Appendix to Priestley's second volume of *Experiments and Observations on Air*, 1781. The appendix is wanting in some copies of this volume. Those which contain it have the pages marked by an asterisk. The first is * 395.

Whilst these experiments were in course of trial by Priestley and Warltire, Cavendish was engaged in the experiments referred to in the preceding chapter, as undertaken with a view to ascertain what change air underwent, when bodies were made to burn in confined portions of it till they were extinguished. Among other combustibles whose effect upon air he was trying in this way to discover, was hydrogen, and when he heard of Warltire's experiment, he repeated it.

He employed, however, like Priestley, a strong glass vessel, instead of a copper flask, performing the experiment otherwise, *generally* as Warltire had made it. The result as to loss of weight he could not verify. He found occasionally a slight difference between the first and second weighings, but commonly none at all, and he rejected, in consequence, the conclusion at which Warltire seems to have arrived as to the ponderability of heat. The deposit of dew, however, on the sides of the vessel, which Priestley disregarded, and the cause of which Warltire totally misapprehended, he looked upon as a phenomenon "likely to throw great light" upon the subject he was pursuing, "and well worth examining more closely." The details of the experiments he made in the course of this enquiry are given elsewhere. It will be sufficient, therefore, to mention here, that hydrogen and air were exploded in various proportions, till that one was discovered (namely, 2 measures of hydrogen and 5 measures of air) which secures the entire withdrawal of the oxygen of the air and its conversion into water. Pure hydrogen and oxygen were then taken in the proportion warranted by the results obtained with air, *i.e.*, one measure, or nearly so, of the latter gas, to two measures of the former. When this mixture was fired, no gas remained in the globe after the explosion, but instead of the hydrogen and oxygen which had lost their gaseous form, a certain weight of pure water was found. And as the vessel and its contents had undergone no change in weight, or parted with anything ponderable during the explosion, whilst a certain volume of gas had been replaced by a certain volume of water, the conclusion was unavoidable, that the ponderable matter of the gas was in the liquid, and therefore that it consisted, weight for weight, of the hydrogen and oxygen, which had lost the elastic form in producing it.

Such, accordingly, was the inference which Cavendish drew, not certainly with all the precision with which we apprehend that truth at the present day, but with as much clearness as any predecessor of Lavoisier has done.

Cavendish, however, did not immediately publish this or any other conclusion, as warranted by his experiments. He was at no time in haste to publish; he was prosecuting other researches in which he was interested, and he contemplated an extensive enquiry, which he afterwards carried out, into the nature of combustion, the causes of the vitiation of the air, the properties of the atmospheric gases, and certain other topics, before the completion of which he was not anxious to make his views public. There was a special reason, moreover, for delay. The liquid which resulted from the detonations was very carefully analysed, and proved in all of the experiments with hydrogen and air, and in some of those with hydrogen and oxygen, to be pure water; but in certain of the latter it contained a sensible quantity of nitric acid. Till the source of this was ascertained, it would have been premature to conclude that hydrogen and oxygen could be burned into pure water. It would have been well for Cavendish, however, if he had published at once his results such as they were, or failing that, had preserved entire silence regarding them, till his enquiry was completed. In either case there would have been no Water Controversy. As it was, he made them known himself to Priestley, and by his friend Blagden to Lavoisier, and the effect was, that through the first Watt came, in a way to be presently mentioned, to enter the lists as his rival in England, and through Blagden, Lavoisier was led to the observations on which he founded his claim to be called the discoverer of the composition of water.

The researches of Cavendish which have been referred to, were made in the summer of 1781, soon after the publication of Warltire's Experiments on the ponderability of heat,* and were communicated to Priestley before 26th March, 1783,† and to Lavoisier before 24th June of the same year.‡ They were not read to the Royal Society till January 15th, 1784; and thus

* *Philosophical Transactions*, 1784, p. 134.

† *Watt Correspondence*, p. 17.

‡ *Mémoires de l'Académie des Sciences pour* 1781 (printed in 1784), p. 472.

it happened, that Watt and Lavoisier, although their researches, whether original or not, were later than Cavendish's in point of time, nevertheless, in consequence of having announced their views with a certain publicity in 1783, appear with a *primâ facie* character of priority to him, as claimants of the disputed discovery. Much of the complication of the Water Controversy has resulted from the fact, implied in the preceding statement, that whilst Cavendish can establish beyond question, priority of observation, Priestley, Watt, and Lavoisier come before him as having given more or less overt publication in a *written* form to views concerning the nature of water similar to his.* It will conduce to perspicuity to separate the French from the English claims, and in order of time the latter fall to be considered first.

In his published paper (Experiments on Air) of 1784, Cavendish states that the experiments on the production of water from its elements, which he made in 1781, were mentioned by him to Priestley, "who in consequence of it made some experiments of the same kind as he relates in a paper printed in the preceding volume of the [Philosophical] Transactions." The volume referred to is that for 1783, in which Priestley with his wonted candour has acknowledged his obligation to Cavendish, before circumstances compelled the latter publicly to claim it.† The significance of these experiments, which Priestley did not repeat without committing a serious blunder, of which, however, he was not aware, was imperfectly appreciated by Priestley, but he gave an account of them (without apparently referring to Cavendish as their originator) to his friend Watt, who at once perceived their value, and wrote to Priestley demonstrating what conclusion his experiments warranted. This letter, which was a commentary on all the researches of Priestley, referred to in his paper of 1783,‡ Watt designed to be publicly read to the Royal Society along with the paper on which it commented, and had this been done, the Water Con-

* Lavoisier's communication to the French Academy in June 1783, was perhaps an oral one, but as it is registered in the records of the Institute, I refer to it as having been written.

† *Philosophical Transactions*, 1783, p. 426.

‡ *Ibid.*, 1783, p. 398.

troversy might never have arisen. At all events it would have assumed a different aspect, and Lavoisier could not have preferred any claim to be the discoverer of the composition of water by synthesis. Fortunately, or unfortunately, however, for all parties, Priestley discovered, after receiving Watt's letter, that a series of researches which had seemed to both to demonstrate the transmutability of water into atmospheric air, was totally fallacious, and as this had interested Watt quite as much as the transmutability of inflammable air and oxygen into water, he requested that the letter might not be read, and his request was complied with.* The letter, however, was shown to various members of the Royal Society, and remained under the nominal custody of the president, Sir Joseph Banks, for about a year, when Watt, roused by the news that Cavendish had laid his views before the Royal Society (January 15th, 1784), claimed to have his letter read, and implicitly asserted the priority of his conclusions, which were thus dated from April 26th, 1783. This is all that we learn from the documents published in the lifetime of Watt and of Cavendish, and the question between them, as it appears in these, is entirely one of relative priority, which might be disposed of without much difficulty. But from the Watt Correspondence, which was published in 1846†, we discover that Watt was induced to believe that Cavendish had borrowed from his letter, the views which he published as his own; and in private Cavendish was deliberately accused of shameful plagiarism. It is this charge which has so embittered the Water Controversy, especially since its revival, and the circumstances which led to its being preferred are now to be considered.

James Watt was not a man disposed by nature or circumstances rashly to accuse a brother philosopher of unfair dealing. "In his temper and dispositions," as Lord Jeffrey, who knew him well, tells us, "he was not only kind and affectionate, but generous and considerate of the feelings of all around him; . . . and such," he adds, in another part of his eloquent eulogium, "was the influence of his mild character and perfect fairness and liberality, even upon the pretenders to these accomplish-

* *Philosophical Transactions*, 1784, p. 330.

† *Correspondence of the late James Watt on his discovery of the Theory of the Composition of Water, &c.* Edited by J. P. Muirhead, Esq., F.R.S.E.

ments, that he lived to disarm even envy itself, and died, we verily believe, without a single enemy." *

The philosopher who has been accused of borrowing from Watt without acknowledgment, although a much more reserved and less demonstrative person than his rival, bore as high a character among the majority of his contemporaries and successors. Without prejudging the question, accordingly, which is now to be considered, and seeking only to warn the reader of the twofold perplexity of the problem before us, I may remind him of the high opinion of Cavendish, expressed by Cuvier in the passage already quoted from the *Eloge.*† Sir Humphry Davy also, who was aware of the feelings entertained by Watt towards Cavendish, and cherished the most friendly regard for the former, offered this unsolicited tribute to the character of his rival, "unambitious, unassuming, it was with difficulty that he was persuaded to bring forward his important discoveries. He disliked notoriety, and he was, as it were, fearful of the voice of fame."‡

At the very threshold of the Water Controversy we thus encounter a perplexing dilemma. Two unusually modest and unambitious men, universally respected for their integrity, famous for their discoveries and inventions, and possessed of rare intellectual gifts and accomplishments, are suddenly found standing in a hostile position towards each other, and although declining to publish their own unquestioned achievements, are seen contending for a single discovery, which the one believes the other to have learned at second-hand from revelations made to a common friend, and which that other accuses his rival of having gathered from a letter that he was allowed to peruse. A misunderstanding such as this would never have occurred had Watt and Cavendish been intimate in 1783. As yet, however, the friendly intercourse which afterwards subsisted between them had not commenced. The one was resident in London, the other in Birmingham, and each was informed of the other's doings by

* The eulogium will be found appended to the English translations of Arago's Eloge of Watt, the exact titles of which are given in the section of the *Water Controversy* entitled "Bibliography." It is also contained in the *Encyclopædia Britanica*, article *Watt*, and in the collected Essays of Lord Jeffrey.

† *Eloges Hist.*, tom. ii., p. 104.

‡ *Collected Works*, vol. vii., p. 128.

third parties, upon whom mainly, though unequally, rests the blame of having occasioned the Water Controversy; nor does it materially lessen our regret to find that those who made mischief between the great philosophers, did so with the best intentions. The parties in question were Dr. Priestley, J. A. De Luc, and Dr. afterwards Sir Charles Blagden, all men eminent in science and of unblemished character. Through the first, a knowledge of Cavendish's experiments passed to Watt, and a knowledge of Watt's conclusions to Cavendish; by the second, Watt was informed that Cavendish had deliberately pilfered his theory; and the third, who was Cavendish's assistant, reported the latter's conclusions, as well as those of Watt, to Lavoisier, whom he accused in the name of both the English philosophers, of having appropriated their ideas. Blagden, also, made certain alterations in the MS. of Cavendish's first *Experiments on Air*, and whilst superintending, in his capacity of Secretary of the Royal Society, the printing of that paper, and of Watt's rival essay, suffered certain typographical errors to occur, which involved himself and his principal in accusations of unfairness. The critics of the Water Controversy who have advocated the claims of Watt, have been unsparing in their denunciations of Blagden, and in their praises of De Luc, whilst Priestley has been alternately praised and blamed, his evidence being eagerly claimed by their respective advocates, when it went to favour Watt or Cavendish, and as summarily refused when it militated against the views they sought to defend. These rival claims solely affect Priestley's intellectual reputation, which has suffered, and must suffer, by his share in the discovery of the composition of water. His moral character is unsullied, perhaps even exalted, by the part which he took in this curious controversy. No one of Cavendish's advocates, so far as I am aware, has come to the defence of Blagden, although it is unquestionable, that if he be convicted of the charges brought against him, Cavendish's honour must suffer also; nor has any admirer of Cavendish disputed the justice of the eulogium which the friends of Watt have one and all passed upon De Luc. I hope, however, to be able to show, that, with the exception of a little excusable carelessness in correcting printers' proofs, Blagden was guiltless of any wrong towards Watt or unfairness towards Lavoisier, and that to

De Luc belongs the unenviable distinction of having been the deliberate *mischief-maker* who provoked the Water Controversy. In what way Priestley, De Luc, and Blagden, came to be involved in the dispute between Cavendish, Watt, and Lavoisier, will appear from the following account.

The experiments which Cavendish made on the formation of water, in 1781, he communicated, as we have already seen, to Priestley, not later than the spring of 1783, and to Lavoisier in the summer of the same year. Priestley announced the result of his repetition of them to the Royal Society, and Lavoisier the result of his similar trials to the French Academy in June 1783, whilst Watt's letter had been in Priestley's hands, but withheld at its author's request from public reading, since April 26th of that year. All parties went on with their researches, and towards the end of the succeeding November, Watt took courage to have his views published, and proceeded to throw them in an amended and fuller form, into the shape of a letter to his friend De Luc, which bore date 26th November, 1783, and was intended for public reading at a meeting of the Royal Society. That letter, however, was scarcely despatched, before news came from Paris, that Lavoisier had stolen a march upon his cautious and too tardy English rivals, and we find Watt writing to Kirwan, on the 1st December, announcing that "M. 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."*

This suspicion was confirmed by Kirwan, who informed Watt, on the authority of Blagden, that he had explained to Lavoisier the views of both the English observers concerning the composition of water.† Watt felt very indignant on learning this, and all the more, that De Luc sought to defend Lavoisier from the charge of plagiarism, only, however, with the effect of increasing Watt's indignation at him.‡ Meanwhile, the letter from Watt to De Luc was not presented to the Royal Society, and Cavendish, who had completed one great section of his protracted *Experiments on Air*, com-

* *Watt Corr.*, p. 37. † *Op. Cit.*, p. 39. ‡ *Op. Cit.*, pp. 40–42.

municated his results to that body on January 15th, 1784. In this paper he formally and publicly announced, besides much else, the views which he had in the preceding year made known to Priestley and Lavoisier. De Luc, who was in Paris at the time, heard some account of them towards the end of February after his return to England, and requested to be allowed to read Cavendish's manuscript, which was at once granted, and, on the 1st March, he wrote to Watt, giving him a sketch of its contents. The spirit in which he did this, cannot be sufficiently regretted, for it led him to prepossess Watt with the darkest suspicions against Cavendish, and this at a time when the indignation of the former at the proceedings of Lavoisier, had rendered him peculiarly sensitive to his claims as a discoverer, and jealous of any interference with them. Had De Luc shown but a small portion of the charity towards Cavendish, in estimating the originality and independence of his views, which he so vainly and extravagantly extended to Lavoisier, no misunderstanding would have occurred between Watt and Cavendish. De Luc, however, plays so important a part in the Water Controversy, that it is necessary the reader should have some acquaintance with his character. It will bear the closest inspection.

Jean André De Luc was born at Geneva in 1727, where, up to his forty-sixth year, he divided his time between commercial tasks, scientific studies, and a prominent share in the religious and political disputes which agitated his native republic in the latter years of the past century. He was in high esteem among his fellow-citizens, but a reverse of fortune, and an increasing wish for scientific occupation, induced him some time after 1770 to abandon Geneva and repair to England, where he became reader to Queen Charlotte, George III's consort. This office, however, did not necessitate constant residence at the British Court, so that he was able to gratify his interest in social as well as scientific progress, by lengthened journeys on the Continent. In 1798 he was appointed Professor of Geology at Gœttingen, and he spent several years thereafter in Germany. After the battle of Jena he returned to England and resumed his duties as reader to the Queen. He resided chiefly at Windsor, where he was highly esteemed by all the members of the royal circle. He died on November 7th, 1817, in his 91st year.

De Luc was an honourable, earnest, and accomplished person; not a man of genius, but certainly one of great talent. He published works on various departments of science, and especially several treatises on geology, which excited much interest at the period of their publication, but are now forgotten. As a meteorologist, however, he will long be remembered. We are indebted to him for the first accurate hygrometer, and for important improvements on the thermometer and barometer. On the two latter instruments, indeed, he is still an acknowledged and esteemed authority; and from the records of his life that remain, it would appear that he was an upright, intelligent observer, a most indefatigable worker, and a sincerely religious man. The space at my disposal does not allow me to refer to De Luc at greater length, but I wish to guard against seeming to depreciate either his intellectual capacity or his moral worth. On the other hand I would pointedly refer to his possession of these, as investing his interference in the Water Controversy with an authority which it would not have possessed, had he been a less accomplished and worthy person.*

The 1st of March, 1784, marks the commencement of the Water Controversy, so far as the English rivals are concerned. On that day De Luc wrote to Watt, informing him that Cavendish expounded and proved his system word for word, without saying anything regarding him.† On the fourth of the month ‡ De Luc writes again, zealously defending Lavoisier and La Place from the charge of plagiarism, but urging it against Cavendish, who is declared to have used the very words (*vos propres termes*) which Watt employed in his letter to Priestley, of the contents of which neither he nor his assistant could be ignorant. De Luc is charitable enough to suppose it possible that Cavendish was an unconscious pilferer, and acknowledges that the readiness with which he and Blagden complied with his request to see the MS. of the *Experiments on Air*, although aware of his intimacy with Watt,

* For the particulars of De Luc's Life I am indebted to the *Biographie Universelle* and the *Penny Cyclopædia*, the last of which contains an excellent sketch of the philosopher, with a list of his writings.

† "On expose et prouve votre système, mot pour mot, et on ne dit rien *de vous*."—*Watt Corr.*, p. 43.

‡ *Ibid.*, pp. 44–47.

as well as the unblemished characters of those he suspected, were at variance with the notion that Cavendish was a deliberate plagiarist. That he was one however, whether aware of it or not, was a point which from the first De Luc considered as beyond dispute, and explained on the more charitable hypothesis of Cavendish's proceedings, by the suggestion that he had unconsciously derived the idea of the compound nature of water from Watt's letter to Priestley, which had excited little attention when read by Cavendish, but had nevertheless planted a germ in his mind, which expanded after many months into the theory which he ultimately published. At the same time De Luc, as if desirous to provoke Watt to the highest pitch of indignation, counselled him, if anxious to increase his wealth, to avoid exciting the ill-will of others; in other words, not to enter the lists against Cavendish as the claimant of a discovery, if he wished to sell his steam-engines. A more certain method of inducing a highly honourable, courageous, and unsordid man like Watt, to defend his claim to the last, could not be conceived. The most wilful mischief-maker, in truth, could hardly have devised a better means of provoking jealousy towards Cavendish than to give Watt such advice as De Luc did. When Watt accordingly replied to De Luc he resented this advice in strong terms, and referred scornfully to "the illustrious house of Cavendish," as one he could despise so far as his pecuniary fortunes were concerned.* He declines in the same letter to make an illiberal attack on Cavendish; nevertheless, he counts it *barely* possible that his rival had heard nothing of his theory, and in apparent forgetfulness of his concession of at least a *bare* possibility the other way, he speaks of "the plagiarism of Mr. C."; so effectually had De Luc infected him with his own ungenerous and unjust suspicions.

That the prejudgment of Cavendish's fair-dealing, of which Watt and De Luc were guilty, was unjust, however excusable on the part of one goaded into indignation as the former was, does not admit of a moment's denial. As yet, neither of Cavendish's summary condemners was entitled to become so much as an accuser. The only proofs which De Luc professed to give of plagiarism, were, that Cavendish used the same words in announcing his views that Watt did; and that he must have read Watt's letter to Priestley. Granting, however, for the sake of

* *Watt Corr.*, p. 48.

argument, both those points, it did not follow that Cavendish borrowed from Watt; for two chemists of the phlogiston school, who arrived, independently, at the same conclusion, could not but use similar terms in stating it, since the nomenclature of that school was an extremely restricted one, admitting of very little variation in the terms which its disciples employed. Watt in truth wrote to De Luc "On the slight glance I have been able to give your extract of the paper, I think his [Cavendish's] theory very different from mine;"* so that the supposed suspicious identity of language, on which De Luc dwelt, does not seem to have been recognised by Watt. And even if Cavendish's words had been absolutely identical with those of his rival, which they are not, as will afterwards appear, the identity could only have justified the apprehension that they *might* have been borrowed, not certainly the summary conclusion, that they *must* have been. So also, before the perusal of Watt's letter to Priestley of April, 1783, was set down as the certain source of Cavendish's conclusions, published in 1784, it should have been ascertained whether he had held any such views before he read the letter, a thing most probable when it is remembered that he had been investigating the subject since 1781. Yet neither Watt nor De Luc made any enquiries of this kind, but at once decided that it was in the highest degree unlikely that Cavendish had acquired his views concerning the composition of water in any other way than by pillage; and the slender charity which acknowledged it as barely possible that it might be otherwise speedily gave way.

De Luc thus did Cavendish and also Watt a great wrong, by hastily deciding the case against the former, and filling his rival's mind with suspicions against him. He was not to blame for zealously espousing Watt's cause. He had been made a party to it, to some extent, by the letter which Watt addressed to him for public reading in November, and he was under obligations to his friend at Birmingham, for assistance towards procuring materials for a projected work on heat and elastic fluids, circumstances which would materially increase his desire to serve him.† But I do blame him unhesitatingly and severely, for impeaching Cavendish as he did, when a little reflection must have shown

* *Watt Corr.*, p. 48. † *Ibid.*, p. 5.

him that he was in many respects disqualified from being umpire between the English rivals.

De Luc was not resident in London, but as reader to the Queen he followed the motions of the Court, and spent much of the year at Windsor,* so that he was entirely out of the way of hearing what researches Cavendish might be prosecuting. We have it on his own authority also, that he rarely attended the meetings of the Royal Society, so that he placed another bar in the way of knowing what Watt's rival had been doing.† His acquaintance with English, moreover, though creditable for a foreigner, was limited. Miss Burney (afterwards Madame D'Arblay), who was attached to the Court at the same time as De Luc, refers to him in her diary, under December 1st, 1785, in the following terms: "Upon Mrs. Delany's coming to Windsor, the Queen had *Cecilia* read to her again, and by M. De Luc, who can hardly speak four words of English!"‡ Some allowance must no doubt be made for the wounded vanity of the authoress of *Cecilia*, who would set down De Luc's broken English as a personal wrong; but after we strip the statement of the exaggeration that plainly pervades it, we cannot credit De Luc with a great command of our language a year before he provoked Miss Burney by the style in which he read her *Cecilia* to the Queen. He had thus another obstacle in the way of learning what researches were engaging the attention of the English philosophers. Moreover, he was not a chemist. When Watt sent him the letter expounding his views on water, De Luc excused himself from an immediate reply, "Le lauguage chimique ne m'étant pas bien familier,"§ a fact which he totally forgot when not long after he summarily pronounced that Cavendish's words were suspiciously identical with those of Watt, of which one so ignorant of chemical nomenclature was by his own showing a very imperfect judge.

Lastly, De Luc went to Paris in December 1783, and passed there the month of January and a portion of February,‖ so that he was absent from England when Cavendish's paper, *Expe-*

* *Watt Corr.*, p. 249. Note to Lord Brougham's Historical Note, by Mr. James Watt (jun.).

† *Watt Corr.*, p. 43.

‡ *Diary and Letters of Madame D'Arblay*, vol. ii, p. 361.

§ *Watt Corr.* p. 3.

‖ *Ibid.*, p. lviii.

riments on Air, was read, and thus had not the advantage of hearing the comments upon it which other philosophers made.

When all these disqualifications are considered, it will appear that there was scarcely one among Cavendish's scientific contemporaries less entitled to judge him than De Luc, and that he most culpably hastened to condemn him, before he was justified in so much as accusing him. The haste, in truth, was extreme. On the very day on which De Luc received Cavendish's MS., he wrote to Watt impeaching his rival; and commenced an analysis of his paper, which he completed and despatched in four days,* without, so far as appears, making enquiries at any of the members of the Royal Society, or others, as to the proceedings of Cavendish. De Luc has justly earned the title of *the* mischief-maker, *par excellence*, in the Water Controversy. Nor can Watt be acquitted of the charge of having judged Cavendish hastily and uncharitably. He would not publicly accuse him, but he did not spare him in private, and he acted in public on the principle that his rival had wronged him. This appears from the *Watt Correspondence*, which does not exalt our estimate of the generosity of feeling of the great engineer, although much allowance must be made for the sinister influence of De Luc's well-intentioned but unhappy interference. The immediate effect of this was to induce Watt, as an injured man, to seek redress. Having occasion, accordingly, to visit London, he had an interview with Sir Joseph Banks, the President of the Royal Society, and it was arranged that both the letter to Priestley, of 26th April, 1783, and that to De Luc of 26th November, 1783, should be successively read. The former, accordingly, was read on the 22nd, and the latter on the 29th April, 1784.†

From Sir Joseph Banks and the other officials of the Royal Society, Watt received every courtesy, but his jealousy of Cavendish was not to be appeased, the more so that De Luc fomented it. What transpired at the interview with the President does not appear, but it ended in his addressing a note to Watt, asking him to have his *Letters on Air* read to the Royal Society.‡ Watt, who regarded the president's request as made

* *Watt Corr.*, pp. 43–47. † *Ibid.*, p. lxii. ‡ *Ibid.*, p. 49.

in a "very civil manner," desired De Luc to call on him, and settle about the reading, which accordingly he did; not, however, without seeking to disabuse Watt's mind of the notion that Sir Joseph Banks had any peculiar desire to have the letters read, since he had informed De Luc that they should certainly be read if Watt wished it, but not otherwise. *

It may reasonably be suspected that De Luc was not the most suitable person to arrange matters with Sir Joseph. At all events it was a most ungracious act to complain as De Luc did, as if it were alike the duty of the president of a society to go about beseeching aggrieved authors to trouble its peace by claims of priority, and to authorize them to say that they did so at his request.

Watt had too much good sense to expect such entreaties from Sir Joseph Banks, or to refuse to request the reading of a letter which had remained unread only in compliance with its writer's explicit desire, and in the end he declared that "Sir Joseph Banks has behaved with great civility and kindness in the affair of the letters." † The tone, however, of his immediate reply to De Luc, is that of an injured man, distrustful of the whole Royal Society, and regarding his rival with undiminished suspicion. "In relation to Sir Joseph Banks," he writes his friend, "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." ‡ That Watt held some one's honour to be compromised by the treatment of his letter, appears probable from the preceding reference; and a little further down he betrays more strongly the feelings of jealousy which he obstinately entertained towards his rival. "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." § And in the close of the letter, his general distrust of the Royal Society breaks out again;—"I shall certainly send the letter to yourself

* *Watt Corr.*, p. 50.
† *Ibid.*, p. 60.
‡ *Watt Corr.*, p. 51.
§ *Ibid.*, p. 51.

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." *
It is painful to read these passages. In April 1783, Watt sent a letter to the Royal Society, which its fellows were most willing to hear read, but its author changed his mind and withdrew it. As soon as he indicated a wish, in the succeeding spring, to have this letter publicly read, he was met more than half-way by Sir Joseph Banks, the friend of Cavendish, and encouraged to present it to the Royal Society. Though Cavendish, moreover, had been guilty of all he was accused of, the Society had not implicated itself in his guilt, real or imagined, by receiving his paper, which it had not the shadow of a plea for refusing; and it left it open to Watt, as it did to every one else, to reclaim against the demands of any of its members. More it could not possibly have done, for even if it had been satisfied that Cavendish had wronged Watt, and robbed him of the credit of his discovery, he had put it out of the society's power to assist him in vindicating his rights, by declining, in 1783, to have his letter published. It was very unreasonable, therefore, of Watt, to suspect the Royal Society as he did, but his prejudiced feeling towards it did not abate. On the same day on which he wrote to De Luc (12th April, 1784), he wrote to Sir Joseph Banks, courteously thanking him for his kindness; nevertheless, with a pettishness and an exaggerated modesty, unworthy of so truly great and modest a man as Watt certainly was, he requests that the Royal Society "will also excuse the defects of my style, which must naturally be concluded to savour more of the mechanic than of the philosopher."†

Sir Joseph Banks did his best (April 15th) to appease Watt's irritation by thanking him for communicating his letters to the Society, and spared him the necessity of requesting their publication, by informing him that he wished them to appear in the next volume of the *Philosophical Transactions*, although, in strict rule, a committee of the Council, and not the President,

* *Watt Corr.*, p. 51. † *Ibid.*, p. 53.

were the judges of this. Two days later (April 17th,) Watt, writes to De Luc, mentioning Sir Joseph's good offices, and among other things, says : "Do as you think proper; I am sure you have my reputation in the matter more at heart than I have myself."* A fact too true, and not to be forgotten, as showing how important an agent De Luc was, according to Watt's own testimony, in determining the temper in which he regarded Cavendish.

Of the same date is a letter from Watt to Sir Joseph Banks, in which he refers to his letter to De Luc of November 26th, 1783, and points out certain alterations made in it, "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, M. De Luc can produce the original in my own hand writing which can be compared with this present copy."† From this allusion it appears that Watt was anxious to establish his claims as dating from November 1783, and that he suspected certain parties of having an interest in denying this. That one of these was Lavoisier, is certain, from an allusion in the letter, and we cannot doubt that another was Cavendish.

Four other letters passed between London and Birmingham, referring merely to alterations in style and other little matters, from 23rd April to 5th May inclusive, in which I do not find anything calling for notice.‡ On the 11th, Sir Joseph Banks writes Watt, informing him that both his letters "appeared to meet with great approbation from large meetings of Fellows;"§ and on the 12th De Luc writes, delighted with the steps Watt had taken to authenticate his letters and dates, and adds, "le Chevalier Banks s'y est prêté volontiers."|| Watt acknowledges in reply (May 14th), his obligations both to Sir Joseph and to De Luc;¶ and surely he might now be content. He had perilled his claims to priority over Cavendish (considered for the present as dating only from January 1784), by the voluntary and unsolicited withdrawal of his letter to Priestley of April 1783, and the delay which he permitted in making public his letter to De Luc of November 1783. These letters, hitherto private, were

* *Watt Corr.*, p. 55. † *Ibid.*, p. 56. ‡ *Ibid.*, pp. 57-59.
§ *Ibid.*, p. 59. || *Ibid.*, p. 60. ¶ *Ibid.*, p. 60.

now accepted by the Society as public documents, and whatever they entitled their writer to claim, he could now claim from the period at which they were written. The satisfaction, however, with which Watt learned this, was not very abiding, nor did it in any degree lessen his indignation at his rivals. On the 15th May, he writes to his friend Mr. Fry, of Bristol, "I have had the honour, like other great men, to have had my ideas pirated. Soon after I wrote my first paper on the subject, Dr. Blagden explained my theory to M. Lavoisier at Paris, and soon after that, M. Lavoisier invented it himself, and read a paper on the subject to the Royal Academy of Sciences. Since that, Mr. Cavendish has read a paper to the Royal Society on the same idea, without making the least mention of me. The one is a French financier; and the other a member of the illustrious house of Cavendish, worth above 100,000*l.*, and does not spend 1000*l.* per year. Rich men may do mean actions. May you and I always persevere in our integrity, and despise such doings."* I make this quotation with great pain. It is mournful to think that such a man as Watt should have written thus. The repetition of the sneer at the "illustrious house of Cavendish," and the singular endeavour to connect the wealth of his rivals with the wrong they had done one who was in the way himself to become a wealthy man, show a bitterness of feeling, and an unreasonableness, greatly at variance with the prevailing temper of a naturally generous man. The blame, however, belongs more to those who published a *private* letter containing such unhappy passages, than to him, who, in the confidence of friendship, gave utterance to his unsparing indignation.

From the preceding account it will appear that Watt and De Luc made no enquiry into the proceedings of Cavendish, but proceeded from the first on the assumption that he had committed theft, and that the final conclusion to which they came, after taking the steps which they deemed requisite to establish Watt's claims, rested on exactly the same suspicions as their earlier inference. It will further be observed, that no charge was brought against Cavendish, or complaint of any kind made by Watt in either of the letters which had been read to the Royal Society. He writes to Blagden (May 27, 1784), "My only

* *Watt Corr.*, p. 61.

reason for wishing my letter to Dr. Priestley to be read before the Royal Society was, to shew them what my ideas on the subject were, at the time it was written."* Nor does anything at variance with this appear in the later letter to De Luc. It is of importance that this should be noticed, for many critics of the Water Controversy write as if Watt had publicly accused Cavendish of plagiarism, implying of necessity the priority of Watt, and accordingly they draw unfavourable conclusions from the absence of a reply. But Watt made no charge of plagiarism, and did not even claim priority. He simply asserted that his views concerning water went back to 1783, and he left it to his rivals to show that theirs were of earlier date. Had a conference occurred between Watt and Cavendish, the former could not have failed to learn how long the latter had preceded him in experimenting on the production of water from its elements, and would not have refused to believe that his rival had entertained views concerning its true nature, before him. He afterwards, indeed, acknowledged that Cavendish had preceded him to some extent,† and he would have done it at the time, and more fully, had he been better aware of his rival's proceedings. Much, however, as it must be regretted that Watt made no enquiry into Cavendish's researches, in justice to the former, it should be acknowledged, that it is a delicate matter to contest priority, especially where no formal charge has been preferred; and that so far as his published papers represent him, Watt appears to advantage as standing only on the defensive. The Correspondence, however, which the friends of Watt have made a public document, shows him claiming priority, accusing Cavendish of plagiarism, and of unfairly making no reference to him; and when these charges are considered, we cannot acquit Watt of blame in never seeking a conference, directly, or through a common friend, with Cavendish, before he reproached him. A conference might have been declined, but at least it should have been invited; and some positive evidence of unfair behaviour towards him should have been in Watt's hands, before he circulated, even in private, reports to the discredit of Cavendish.

We are now to see what steps Cavendish took to right himself and to defend his good name. They were, as might be

* *Watt Corr.*, p. 63.

† *Phil. Trans.* 1784, p. 332.

expected from his character, very few. No overt charge had been preferred against him, nor is there the least reason to imagine that the contents of Watt's letter to his friends would be divulged to him. The reclamation, however, of priority by Watt, which followed so closely on De Luc's request to be allowed to read Cavendish's *Experiments on Air* (MS.), and in which De Luc took so active a part, made sufficiently manifest what Watt's feelings were. The inaccurate account, moreover, of Cavendish's researches, even in the amended version of the letter to De Luc, showed that its writer questioned, if he did not deny, that any one had theorised before himself on the composition of water. It is not unlikely, also, that Sir Joseph Banks called Cavendish's attention to the interviews he had had with Watt and De Luc, and apprised him of the claims set up for the former. At all events the public reading of Watt's letters, with their inaccurate reference to the researches of 1781, showed plainly enough what demands their writer made; and Cavendish took advantage of the opportunity which elapsed between the public reading (January 1784) and the printing (July 1784) of his paper, to make three additions to it, which introduced new elements of strife into the controversy, and brought newnames conspicuously forward as concerned in it. Two of these additions occur in the body of the paper, in the handwriting of Blagden, who was Cavendish's assistant; the other is a postscript written by Cavendish himself, in whose writing the text of the paper is also.* In the first of these insertions, or, as the friends of Watt love to call them, "interpolations," Cavendish states that "All the foregoing experiments on the explosion of inflammable air, with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. Priestley, who, in consequence of it, made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the *Transactions* [1783.]"† The remainder of this insertion reports a similar but more extended account of Cavendish's researches,

* The MS. remains in the archives of the Royal Society. A full account of it will be found in the sequel.

† *Phil. Trans.* 1784, p. 134.

as given by Blagden to Lavoisier in 1783, and will be referred to again.

The paper by Priestley in the *Philosophical Transactions* of 1783, to which Cavendish refers, is the one on which Watt's first letter was a commentary. It is entitled *Experiments relating to Phlogiston, and the seeming Conversion of Water into Air.** The second part (p. 414) is entirely devoted to the latter subject, and recounts an extensive series of experiments on the transmutability of water into atmospheric air, in the possibility and reality of which, Priestley as well as Watt had, for a season, entire faith. The former, however, even at the period when he was most satisfied with his experiments, which he afterwards found to be quite delusive, found many persons sceptical as to their significance in demonstrating the alleged transmutation. With a view, accordingly, to strengthen his doctrine, that water might become air, by showing that air might conversely become water, he "gave particular attention to an experiment of Mr. Cavendish's, concerning the reconversion of air into water, decomposing it in conjunction with inflammable air" (p. 426). He then describes the experiment he made, in which he employed oxygen and inflammable air (from charcoal), and states, at the close of his description, "the result was such as to afford a strong presumption that the air was re-converted into water, and therefore, that the origin of it had been water" (p. 427). In the passages quoted, Priestley, it will be observed, uses the word *air*, in a vague sense, in conformity with the fashion, or rather ignorance, of the time, for as yet the specific characters of the gases were imperfectly known; and the prevailing idea was, that they were only modifications of atmospheric air. Moreover, Priestley was mainly anxious to employ Cavendish's experiments to show that gas might become liquid, and, therefore, that liquid might become gas; whilst he fell back on his own researches to prove that the particular liquid water, might become the particular gas, atmospheric air. This, however, and certain other peculiarities of Priestley's statement, do not concern us at present. It is of importance, as being the record of the very experiments on which Watt founded his views concerning the nature

* *Phil. Trans.* 1783, p. 398.

of water, so that we may learn from it what Watt stated in neither of his letters (and seems unaccountably enough, not to have known), namely, that he was indebted to an avowed repetition of Cavendish's experiments, for the grounds of those conclusions which he so summarily decided his rival could only have borrowed from him.*

How much Cavendish told Priestley, does not certainly appear from any existing document; and the abettors of Watt's claims, accordingly, represent Priestley as having been informed solely of experiments of Cavendish's, without having been enlightened as to what his conclusions were. This view, however, is quite untenable, for whatever difficulties may attend the determination of the exact nature and amount of the information Cavendish gave Priestley, the passage just quoted is sufficient to prove that conclusions as well as experiments were communicated by the former. For the full proof of this, the reader is referred to the detailed analysis of the question given in the sequel. It will be sufficient here to say, that Cavendish did not make random trials; neither did he, as some have asserted, interest himself only, or chiefly, in ascertaining whether Warltire was right in thinking that by the detonation of hydrogen with air or oxygen, it could be shown that heat was ponderable. What interested him, as he tells us himself, was the deposition of liquid, which succeeded each explosion, and which promised to throw light on the important problem, what becomes of the air which disappears or loses its elasticity during combustion. He prosecuted the enquiry till he ascertained how much of atmospheric air, namely, one-fifth part of it by measure, and what portion of it, namely, its oxygen, disappeared during its maintenance of combustion; and till he discovered that if hydrogen were the combustible burned in it, or exploded with it, a measure of that gas equal to about twice the bulk of the oxygen, lost its elasticity simultaneously; whilst in place of both gases their conjoint weight of liquid was found. He verified this result

* The friends of Watt assiduously, but, as I believe, quite vainly, deny that Watt's conclusions had reference to the *particular* experiments recorded by Priestley in the passage quoted in the text; but, with the exception of Arago, they acknowledge that Watt founded his theory upon an avowed repetition of Cavendish's experiments. The subject will be found fully discussed in the sequel.

by substituting for atmospheric air, oxygen, which he mixed with twice its volume of hydrogen; and showed that (with the exception of a trace of impurity) a globeful of this mixture could be completely deprived of elasticity; and that the weight of gas which disappeared was replaced by an exactly equal weight of liquid. Finally, he ascertained that this liquid was pure water. Thus much, Cavendish affirms, he told Priestley. Thus much, in effect, Priestley acknowledges that Cavendish told him, and Priestley's acknowledgment was made before Cavendish's reference to it, and details exactly such experiments as have been recorded, only the repetition was exceedingly imperfect and inaccurate compared with the original trials. The more impartial, at least, of Watt's advocates, substantially acknowledge all that has been stated above; and the question so keenly contested between them and Cavendish's friends, is, did Cavendish's revelation to Priestley include only such details as those recorded, or did it also embrace conclusions? I think there are few impartial readers who will feel much difficulty in acknowledging that conclusions were communicated. In truth, Watt's friends have one and all carefully avoided giving their definition of an "experiment." They would, apparently, have us believe, that it is only the handling of certain pieces of apparatus, and the unreflecting observation of certain phenomena, without any hypothesis as to their cause, or any conclusion as to their significance.

That the experiments of many men are no better than this may at once be acknowledged. But the less extreme advocates of Watt confess that Cavendish had *a* purpose in his trials, and that he was watchfully observant of the appearance of moisture as a product of combustion; and yet they will have us believe that he—whose death Sir Humphry Davy mourned as the greatest loss English science had sustained since the decease of Newton, and whose skill as an interpreter of nature the scientific world universally acknowledged—was, on this solitary occasion, no better than a child amusing itself with an electrical machine and some glass vessels. Even this demand upon our faith might with some modification be honoured, had we no further information than that Cavendish went through certain operations with certain pieces of apparatus. But it is not denied by his opponents that he learned so much from these alleged aimless and uninterpreted trials, that although one of the most reserved and uncommunicative of men, he broke through the silence he so much loved, and reported his experiments to Priestley. On the latter, moreover, his narration made such an impression, that he proceeded to repeat the experiments reported to him, and to make them known to the world. The notion that any man should minutely report to another observations which taught the reporter nothing, is surely absurd, and the character of Cavendish makes the absurdity more glaring. But it is deepened, when we consider to whom he divulged his researches. His confidant was Priestley, who, least of all men, except his colleague Warltire, could be expected to take an interest in Cavendish's trials, unless they brought to light something important. They were an avowed repetition of the Birmingham experiments, which Priestley made to entertain his friends, and Warltire tried to determine the ponderability of heat. On the latter point, Cavendish could only say that he found no proof that heat was heavy in the results of his explosions. He could, however, confirm the statement that each detonation was followed by the appearance of a liquid, and could tell Priestley that this was the most important phenomenon his random trial had brought to light, and that it did not result as Warltire imagined, from the mere deposition of water previously diffused as vapour through the gases; for if these were taken in a certain proportion, they could be *entirely* condensed, and converted into water. This was a perfectly new conception to Priestley. Neither he nor Warltire had made the most distant approximation to it, nor could they have done so, for it was only by a series of *quantitative* observations that such a fact could be discovered, and theirs were solely *qualitative*.

Here, then, was a truth which Cavendish had learned from his accurate, quantitative repetition of Priestley and Warltire's inaccurate qualitative trials, and which no one in the world but himself knew till he made it known to Priestley. In what precise terms the latter was informed of the truth we do not know, but it is unquestionable that he was made acquainted with conclusions as well as facts. The *fact*, Cavendish ascertained (to take, for brevity's sake, the simpler case of hydrogen and oxygen, omitting that of hydrogen and air), was this :—When two measures of hydrogen and one of oxygen are exploded together in a shut globe, they lose their elasticity, or cease to exist as gases, and in their place is found an equal weight of water. This fact might not have been regarded by Cavendish as justifying any conclusion, or it might have been seen to warrant various conclusions, among which he hesitated to select, and therefore published none. But he did reveal a conclusion to Priestley, and it was this: that hydrogen and oxygen were *turned or converted into water;* which was equivalent to saying that water consists of hydrogen and oxygen. That he told Priestley thus much, appears from the terms in which Priestley refers to Cavendish in his paper of 1783. The language is very remarkable. Priestley does not make the slightest allusion to Cavendish's experiments as having been a repetition of his and Warltire's. They had been so modified by their repeater, and had brought to light so unexpected a truth, that Priestley preferred no claim to them, but spoke of them considered as a whole, as "an experiment of Mr. Cavendish's concerning the re-conversion of air into water." He called it *re*-conversion, because he believed that water was convertible into air (gas or gases), and regarded Cavendish's experiments as complementary to his own. So convinced was he of their importance, that he repeated them with gases carefully prepared, so as to exclude from them moisture; compared the weight of the gases burned with that of the water produced, and found it, as nearly as he "could judge, equal," and, in consequence, he came to the conclusion that there was "a strong presumption that the air [inflammable air from charcoal, and oxygen] was re-converted into water, and therefore that the origin of it had been water." His repetition was very inaccurate, and he committed the grievous blunder of substituting for Cavendish's hydrogen the mixture of combustible gases obtained by heating charcoal, which he conceived to be an anhydrous gas. These matters are referred to elsewhere, but they do not concern us here; neither does Priestley's want of entire confidence in the significance of the experiments as establishing the conversion of the gases into water. The wonder is, considering how inaccurate his experiments were, that he got so far as to entertain "a strong presumption" concerning the lesson they taught.

What is pre-eminently important is, that long before Watt had written his first letter, or had been supplied by Priestley with the account of his repetition of Cavendish's experiments, which was the basis of the conclusions concerning the composition of water announced by Watt, Cavendish had taught Priestley the truth, which Watt, after learning it from the pupil, declared the master had borrowed from him. Much anxious endeavour has been made by the advocates of Watt's claims, to deny this, to the extent, at least, of asserting that Cavendish told Priestley only facts, and that it does not appear when he communicated them. As for the latter point, which may be noticed first, it is quite unnecessary to enter into any minute enquiry into dates. It is not denied that it was between the summer of 1781 and the spring of 1783, that the communication was made by Cavendish to Priestley, nor can it be denied that Priestley's experiments were later than this communication, and Watt's conclusions later than Priestley's experiments. And as for the communication of Cavendish having had reference only to "facts," if, among these, the friends of Watt include the conversion of hydrogen and oxygen into water, there need be no dispute between them and the defenders of Cavendish; for the conversion in question was *a conclusion* from the phenomenal data supplied by the globe-detonations, as every logician must acknowledge; so that his assailants must cease to affirm that Cavendish taught no conclusion. Priestley does not say in so many words, that he was informed that inflammable air and oxygen could be burned into water, but he implies this most plainly. He tries Cavendish's experiment not as an impartial repeater of it: he anticipates that it will establish the convertibility of gases into water; he hopes that it will, and so enable him to strengthen his own converse doctrine, and he thinks, after repeating it, that if it does not infallibly demonstrate, it, at least, strongly warrants the belief, that a conversion of inflammable air and oxygen into water can be effected. Had he faithfully imitated his teacher's method of experimenting, he would have been entirely satisfied of this, and I believe he only hesitated, because his erroneous method of procedure rendered it impossible that he should have made sures of hydrogen and one of oxygen are exploded together in a shut globe, they lose their elasticity, or cease to exist as gases, and in their place is found an equal weight of water. This fact might not have been regarded by Cavendish as justifying any conclusion, or it might have been seen to warrant various conclusions, among which he hesitated to select, and therefore published none. But he did reveal a conclusion to Priestley, and it was this: that hydrogen and oxygen were *turned or converted into water;* which was equivalent to saying that water consists of hydrogen and oxygen. That he told Priestley thus much, appears from the terms in which Priestley refers to Cavendish in his paper of 1783. The language is very remarkable. Priestley does not make the slightest allusion to Cavendish's experiments as having been a repetition of his and Warltire's. They had been so modified by their repeater, and had brought to light so unexpected a truth, that Priestley preferred no claim to them, but spoke of them considered as a whole, as "an experiment of Mr. Cavendish's concerning the re-conversion of air into water." He called it *re*-conversion, because he believed that water was convertible into air (gas or gases), and regarded Cavendish's experiments as complementary to his own. So convinced was he of their importance, that he repeated them with gases carefully prepared, so as to exclude from them moisture; compared the weight of the gases burned with that of the water produced, and found it, as nearly as he "could judge, equal," and, in consequence, he came to the conclusion that there was "a strong presumption that the air [inflammable air from charcoal, and oxygen] was re-converted into water, and therefore that the origin of it had been water." His repetition was very inaccurate, and he committed the grievous blunder of substituting for Cavendish's hydrogen the mixture of combustible gases obtained by heating charcoal, which he conceived to be an anhydrous gas. These matters are referred to elsewhere, but they do not concern us here; neither does Priestley's want of entire confidence in the significance of the experiments as establishing the conversion of the gases into water. The wonder is, considering how inaccurate his experiments were, that he got so far as to entertain "a strong presumption" concerning the lesson they taught.

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His position was a remarkable one. Of all those who from first to last, have taken part in the Water Controversy, Priestley alone was the friend of both the English rivals, and in circumstances to learn what each had discovered for himself, and had borrowed from his brother philosopher. This was plainly the belief of both Cavendish and Watt, each of whom implicitly appealed to Priestley to attest his originality and priority, Watt in his letter of 1783, Cavendish in the addition made to his paper in 1784. But the umpire thus selected, never decided, so far as any existing document shows, in favour of either claimant, although he long survived the period of the original controversy, and again and again commented on Cavendish and Watt's theories of the nature of water, with the authors of which he continued from first to last to be on the most friendly terms. Yet Priestley, whatever were his faults, was certainly a very frank and candid person, by no means reluctant or afraid to utter his opinions on any subject or on any occasion. Here too, he was invited by both parties to arbitrate between them; and it appears at first sight not a little perplexing, that he did not come forward, either to assign the whole merit to one or other of the claimants, or to apportion it between them, according to his conviction of their relative deserts. From his decision there could have been no appeal, and had he assumed the office of mediator between Cavendish and Watt, there would have been no Water Controversy. The advocates of Watt's claims have eagerly caught at anything that seemed to show that Priestley favoured their client's cause, but Priestley, as even they tacitly acknowledge, utters on the subject, but an "uncertain sound." The following passage was written by him in 1785, the year after the commencement of the Water Controversy. It contains the only deliberate comparison of Cavendish and Watt's merits, which, so far as I am aware, he ever published. Watt's advocates quote the latter part of it, as serving their cause; Cavendish's friends have not appealed to it. I give it entire, that the reader may judge of its import for himself:

"In the experiments of which I shall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. Cavendish, Mr. Watt, and M. Lavoisier. Mr. Cavendish was of opinion that when *air* is faith alike in Cavendish's experiments and in his own repetition of them, and came, in 1785, to affirm that it was impossible to burn hydrogen and oxygen into an equal weight of water, and that the product of their combustion was nitric acid, as well as water. He regarded the language of Cavendish and of Watt, accordingly, as at variance with the facts of the experiment they professed to expound, and he naturally thought it needless to enquire which first went astray. Nevertheless, it is not a little difficult to understand how it happened that Watt was so profoundly ignorant, as he appears to have been, of Cavendish's experiments which preceded and were the occasion of Priestley's. The latter told all the world publicly and explicitly, that he followed Cavendish, but he did not apparently make this known to Watt, for although the latter was minutely acquainted with the contents (though not, perhaps, with the very words) of Priestley's Essay of 1783, and wrote his first letter as a Commentary upon that Essay, he does not once allude to the experiments upon which he based his theory of the composition of water, as primarily Cavendish's, but speaks of them as if they had been original researches of Priestley's. An imperfect and inaccurate reference to Cavendish's priority was eventually made, but not till 1784, immediately before the letters to Priestley and De Luc were publicly read to the Royal Society.* That Watt made an ungenerous concealment of his knowledge of Cavendish's researches, I do not think, but it is important to notice that he can be acquitted of this, only on the supposition that he had either been left in ignorance of Priestley's obligation to Cavendish, or was not alive to the importance of the information on this point, which Priestley gave him. To this question I shall return, so far as it affects the feelings which Cavendish may be supposed to have entertained towards Watt. Meanwhile, I only notice, that as the reference made by Watt to his English rival's labours is much less accurate and ample than it should have been, by one who, apart from his private sources of information, had access, in 1784, to the published declaration of Priestley in his paper of 1783, which it is difficult to believe Watt would not read in the *Philosophical Transactions*; it seems certain that he undervalued the import-

* *Phil. Trans.* 1784, p. 332.

ance of Priestley's statements concerning Cavendish's previous researches, for he wrongs them both in his reference to the latter. He does Priestley injustice by attributing to Cavendish the first observation, that moisture is produced during the combustion of oxygen and inflammable air, an observation which Cavendish never claimed to have been the first to make, but expressly assigned to Warltire and Priestley, to whose joint experiments he referred as the source of his own knowledge of the fact.* At the same time, Watt wronged Cavendish by not attributing to him the earliest observation of the truth, that the weight of water produced during the combustion of inflammable air (hydrogen) and oxygen, equals the weight of the gases which lose their elasticity during its production, although Priestley gave the whole credit of the discovery to Cavendish. We have no means now of determining in what proportions the blame of this misstatement is to be divided between the two philosophers of Birmingham, but that it did not originate in any wilful concealment on the part of either, I entirely believe. The most ungenerous of critics must find it impossible to impute to Priestley any jealousy of Cavendish, and only those who are as unjust towards Watt, as some of his partizans are towards his rival, will accuse the great engineer of intentional injustice. Priestley plainly must have been singularly unfortunate and sparing in his reports to Watt, of the connexion between his researches and those of Warltire and of Cavendish, or Watt would not have failed to do justice to his informant, if to no one else. Watt, however, must have been very indifferent to the authorship of the facts on which he based his theory, or he would have sought out information for himself. In Priestley's volume *On Air*, published so far back as 1781, he would have found the records of experiments made by two of his own townsmen (Priestley and Warltire) on the combustion of inflammable air and oxygen, even if his friend and neighbour had not privately communicated them to him, which he is so likely to have done; yet in 1784 he gave an account of matters totally at variance with the statements in that volume. Nor can he have conferred with Priestley on this subject, frequently as they conversed together on the convertibility of water into gas, or he

* *Phil. Trans.* 1784, p. 126.

would have been referred to the account of 1781, and set right as to the nature of the observations which Warltire, Priestley, and Cavendish had severally made on the appearance of water when inflammable air is burned.

Priestley thus occupies a very singular position in the Water Controversy. He was the confidant alike of Cavendish and of Watt, as to their theories of the nature of Water; he was implicitly appealed to by both, to decide between them as claimants of the disputed discovery; and he was apparently in possession of information which should have enabled him to dispose of the Appeal. In no document published, however, has he done so, nor is there any prospect of unpublished papers throwing much additional light on his views. The late Mr. James Watt (junior) states that "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 made in America."*

That this supposition was a just one can scarcely be doubted, but an important series of letters from Priestley to others, during the period when he was engaged in his researches into the nature of water, was not exposed to the destruction which befel the papers in his own possession in 1791, and this series has since returned to the possession of his descendants. Through the kindness of Miss Finch, of Birmingham, a grand-daughter of Dr. Priestley, I have been favoured with the loan of thirty-eight letters, written by him to his celebrated friend Josiah Wedgwood, and his son Thomas. The greater number of the letters are to the elder Wedgwood, especially the earlier ones, and they extend through 1781, 1782, 1783, 1784, 1787, 1788, 1789, 1790, 1791, and 1792, *i. e.*, exactly over the period of most interest in reference to the Water Controversy.† The nearest surviving relative also of James Keir, Esq. (whose accomplishments and skill as a chemist were highly honoured by Priestley), has granted me the perusal of twelve letters addressed to him by the latter.

* Letter to Mr. Muirhead. *Watt Corr.*, p. xiv.

† These letters were sent to Miss Finch by F. Wedgwood, Esq., of Barlaston, Stone, Staffordshire, and, as I learn from him, constitute the entire extant correspondence between Priestley his grandfather Josiah, and his uncle Thomas Wedgwood.

Their dates (so far as they are dated) are 1782, 1787, and 1788. Many more passed between the fellow-chemists, but the greater number have perished.* From the collateral relatives of Priestley, likewise, resident in Leeds, I have obtained three letters written by him, of dates 1786, 1791, and 1792. These were obtained for me by Mr. William O. Priestley, a student of great promise, who attended the University of Edinburgh last winter (1849-50), and in whose father's possession they now are. They were entirely on personal matters, and did not throw any light on the Water Controversy.

The letters to the Wedgwoods, and those to Keir, but especially the former, are full of references to Priestley's views on the composition of water; and in them, if anywhere, might we expect to find some expression of opinion concerning the good faith of Cavendish and Lavoisier; but although their names, and those of Watt and Blagden, are referred to in connexion with the theory of the composition of water, not the slightest allusion occurs to any jealousy between the rivals.† Some extracts from them, including all the direct references to Cavendish, Watt, and Lavoisier, will best show what light they throw on the Water Controversy. The correspondence may be said to be almost entirely chemical. Mr. Wedgwood, with the liberality which characterized him, supplied Priestley with as many clay and porcelain retorts, tubes, and other pieces of apparatus, as he chose to ask for, and made of the shapes and materials which he prescribed. The burden, accordingly, of most of Priestley's letters is such as the first sentence of the first epistle may illustrate: "26th May, 1781. To Josiah Wedgwood, Esq. Dear Sir,—I must take the liberty to give you this trouble

* For the knowledge that such letters existed, and the introductions which enabled me to procure them, I am indebted to my friend Dr. Percy, of Birmingham, who, besides his own active co-operations, secured me the good services of Dr. James Russell, of the same town, who spared no trouble in furthering my wishes.

† A warm and lasting friendship subsisted between Keir and Priestley, and the loss of any part of their correspondence is greatly to be lamented, especially that referring to water. My informant in reference to the Keir Correspondence, states that "unfortunately, part of these letters, perhapst he most interesting in the collection, for they told of his experiments previous to the discovery of the decomposition of water, have, I fear, been consumed at the fire at Abberley Hall, having searched for them in vain. These letters showed how very nearly Dr. Priestley touched upon that important discovery."

about the *earthen retorts* you were so good as to promise me, and of which I am in great want."* In other letters their receipt is acknowledged, their performances are recorded, the experiments intended to be tried with them are announced, and the results which eventually were obtained. Wedgwood took much interest in his friend's researches, and besides his gifts of earthenware, yearly put at Priestley's disposal a sum of money for his personal expenses. The latter, accordingly, was naturally anxious to satisfy his benefactor that he was making a good use of the apparatus supplied to him, and to justify himself in accepting his friend's pecuniary grants, to which direct reference is frequently made in the Correspondence. He was thus led into a minuteness of detail in his letters to Wedgwood, which secured the frankest expression of his opinions; so that we may with considerable justice test his opinions concerning the Water Controversy, by the legal maxim, "De non apparentibus et de non existentibus eadem ratio." Where he says nothing, he probably had nothing to say.*

The chief topics enlarged on in the earlier letters are the same as occupy the first section of the *Watt Correspondence*, and refer to those researches into the convertibility of water into air, which interested Watt and Priestley so much, and led to the former's letter, and the latter's paper, of 1783.

The letters of 1781 do not contain anything specially referring to the question before us. They are chiefly occupied with the results procured by heating in retorts "various earthy substances, especially to ascertain the pabulum of subterraneous fires" (May 26th). On 6th March, 1782, Priestley reports an experiment which grew out of those detailed before, in which he reduced oxide

* Unless where otherwise mentioned, the letters are to Josiah Wedgwood, and the passages in Italics are those which are underlined by Priestley. Thomas Wedgwood was not above twelve or thirteen years old in 1784.

† Priestley has been charged with something like meanness or greed in accepting pecuniary assistance from his friends, but, as I believe, most unjustly. The correspondence referred to above gives a very favourable impression of both parties. The writing was apparently mainly on one side, Wedgwood's replies generally taking the shape of a box of retorts, or other "ware." But we see plainly, on the one hand, a very generous wealthy man, delighting to assist, in the most unostentatious and unpatronizing way, a friend less blessed with this world's riches; and, on the other, a modest, poor philosopher, unceasing in his grateful acknowledgments of the kindness shown him, yet never servile in his professions of obligation, nor unconscious of the fact that in his own eyes and in those of his friend, his untiring, fruitful labours cancelled the debt.

of lead to the metallic state, by heating it by the sun's rays concentrated by a lens in an atmosphere of inflammable air, which he thinks is thus proved to be *phlogiston*. It was the prosecution of this idea which led him to theorise on the nature of water, and to interest himself in the speculations of Cavendish and Watt. On 16th September (1782) he announces his supposed discovery, that charcoal is entirely convertible by heat into inflammable air, a mistake which afterwards led himself and Watt seriously astray, and which figures conspicuously in the Water Controversy. On 8th December he announces that he can convert water into permanent air, by distilling it with lime at a strong heat; and on 8th January, 1783, that the lime is unnecessary, and that the mere heating of water in one of his friend's earthen retorts, was sufficient to convert it into air. Great demands are now made on the stock of retorts at Etruria ; and Wedgwood, probably not a little surprised to find that his vessels possessed so wonderful a property as that of transmuting water into atmospheric air, sends two sagacious queries along with a fresh supply (23rd January, 1783). Priestley had explained to him that though the retorts were air-tight, a portion of the water placed within them came through their walls, and vaporised from their outer surfaces. Wedgwood in reply asks, "If water passes through the retort outwards, may not air pass inwards?" * This query Priestley disposes of in the negative, very ingeniously, but he afterwards found, as Watt did also, that Wedgwood had detected the true state of matters, and that the supposed transmutation of water into air was only an exchange of steam, and the atmospheric gases through the pores of the retorts, which became permeable at a high temperature. For the present, however, Priestley proceeded with increased alacrity in his transmutations, and discovered, as he imagined, that hydrogen and oxygen could be burned into carbonic acid (7th March 1783) ; a mistake which introduced confusion into his own notions, and those of Watt, concerning water.†

* This has been copied by Josiah Wedgwood's secretary, Mr. Chisholm, in Priestley's letter (Jan. 23rd, 1783), as an interlineation after it had been received. Francis Wedgwood, Esq., of Barlaston, Stone, Staffordshire, by whom Priestley's letters to his grandfather (Josiah) were sent to Miss Finch, from whom I had them in loan, kindly enabled me to verify this point.

† *Watt Corr.*, p. 17.

On the 23rd of the same month, Wedgwood is informed of an experiment tried by his friend, which is the most important of all that he made, so far as the Water Controversy is concerned. It is thus announced: "I have lately made such experiments relating to the conversion of water into air, as must, I think, satisfy even Mr. Kirwan. By the electric explosion I decompose dephlogisticated and inflammable air, and I find the weight of the latter in the *water* I get from it."* The wording of this statement is ambiguous, and might seem to imply that only the weight of the inflammable air burned was found in the water produced by its combustion. There can be no doubt, however, that Priestley signified that the weight of both the gases concerned in the combustion, was equalled by that of the water they yielded. This at least is the proposition which he undertakes to prove in his paper of 1783,† and there also he informs us, that his experiment was a repetition of one previously tried by Cavendish. To Wedgwood, however, he was as silent on this point, as he seems to have been to Watt, and apparently for the same reason, that the result interested him much less than his own fancied discovery, that water could be transmuted by porous retorts, into atmospheric air.

An important interval here elapses, during which he addresses no letters to his friend, and which we know was occupied by himself and Watt, in drawing up for the Royal Society the conclusions they had drawn from the experiments in question. On the 6th of May, however, Priestley writes from London to Etruria, explaining that he has discovered the delusive nature of his supposed transmutations of water into air, and verifying Wedgwood's sagacious conjecture as to the true nature of the phenomena witnessed with the porous retorts. This discovery led Priestley to alter the concluding part of his paper and its title, and induced Watt to withhold his letter from publication.‡

Priestley's paper, meanwhile, was read to the Royal Society, and he proceeded with his researches into the evolution of gases

* The same experiment is referred to in the *Watt Correspondence*, p. 17, under date March 26, 1783.

† *Phil. Trans.* 1783, p. 427.

‡ *Watt Corr.*, p. 25-30.

from substances heated in Mr. Wedgwood's excellent retorts. Among other things, he heated nitre, and obtained from two ounces of it, 812 ounce measures of oxygen, and he tells Wedgwood, that his own idea is, that the nitrous acid of the nitre is charged into the air, but that Mr. Watt "still thinks that it is water that furnishes the air" (24th July, 1783.)*

His next letter to Wedgwood is of date 16th January, 1784,† and contains some very remarkable passages. The italics in the quotations mark the words underlined by Priestley. "The great problem with us *aerial philosophers* (not navigators), of late, has been to find what becomes of dephlogisticated and inflammable air when they are made to unite, as by explosion, &c.; some saying that they make *water*, others *fixed air*, *&c.* The following experiments show that, in different circumstances, they make *both*, and also, that dephlogisticated air incorporates with *iron* in a great proportion."

This letter was written the day after Cavendish's paper on Water was communicated to the Royal Society. It is not probable, however, that a knowledge of its contents can have reached Birmingham before the letter was written. It cannot be doubted, nevertheless, that Priestley referred to Cavendish and Lavoisier's views. From the *Watt Correspondence* it appears that in November 1783,‡ Priestley was aware of Lavoisier's opinions, and he had known Cavendish's since at least the preced-

* Watt published this opinion in 1784. "Nitre," says he, "besides its water of crystallization, contains a quantity of water *as one of its elementary parts*, which water adheres to the other parts of the nitre, with a force sufficient to enable it to sustain a red heat. When the nitre is melted, or made red hot, the acid acts upon the water, and dephlogisticates it; and the fire supplies the *humor* with the due quantity of heat to constitute it air, under which form it immediately issues." *Phil. Trans.* 1784, p. 336. I need scarcely say that Watt was mistaken in imagining that nitre contains water, and that the oxygen which the salt yields when heated is derived from that liquid. His views on this subject, which he illustrates at length in his paper, will be referred to frequently again.

† This letter is endorsed by Josiah Wedgwood, *June* 1784. Priestley's own date, however, exactly corresponds to those of his letters of 1783, in three of which *January* is written as in the letter of 1784. Francis Wedgwood, Esq., of Barlaston, tells me that he suspects that his grandfather did not always docket his letters when he received them, and has given cogent reasons for this opinion. Priestley's indistinct writing of *Jany.* might easily be mistaken for *June*, at a hurried glance. I have asked the opinion of three unbiassed parties, including a great grandson of Josiah Wedgwood's, and they all read the word *January*.

‡ *Watt Corr.*, p. 35.

ing March. They were the only parties who contended that inflammable air (hydrogen) and oxygen *make* water, and *only* water, when they combine. Kirwan was the great advocate of the production of fixed air, or carbonic acid, by the combustion of the gases referred to; and Priestley and Watt held, on the authority of the experiments of the former, that *both* water and carbonic acid might be produced by the union of inflammable air and oxygen.* Priestley, however, mentions no names, and does not hint that Watt was aggrieved, although he knew that he complained of Lavoisier's behaviour to him. His silence in this respect is in keeping with the whole tenor of his proceedings, and shows how utterly heedless he was of the personal differences which attended the discovery of the composition of water. He was one of the most zealous of polemics on all subjects, and never more serene and complacent than when, like Ishmael, his hand was against every man, and every man's hand against him. Yet living, as, like the fabled Salamander, he delighted to do, in an atmosphere of fire, he refused to take a side in a controversy which might have been expected to rouse all his combativeness; and on perhaps the solitary occasion in his life, when he was solicited to become a combatant, he quietly answered the contending rivals by ignoring the fact that they were at issue, and acting as if there were either no dispute, or at least nothing worth disputing about.

The indifference which Priestley thus displayed, in assisting to verify the views of his friends, he showed as conspicuously in reference to the truth and consistency of his own views. In the letter from which I have been quoting, a statement occurs of the greatest importance in reference to more than one vexed question in the Water Controversy. It is as follows:—"Another experiment shows a remarkable difference between inflammable air from *metals* and that from *charcoal*. Having mixed a quantity of each of them with half as much dephlogisticated air, I exploded that from iron, and found neither *water* nor *fixed* air; but exploding the mixture that contained the inflammable air from *charcoal*, $3\frac{1}{2}$ ounce measures of the mixture yielded an

* See in illustration of this the papers of Cavendish, Watt, and Kirwan, in *Phil. Trans.* 1784. Priestley's letter speaks for itself.

evident quantity of *water* and ⅘ths of an ounce measure of pure fixed air."

This singular passage, it will be observed, contains an implicit contradiction of what Priestley had published to the world in 1783. In announcing in his paper of that date, his repetition of Cavendish's experiments on the conversion of gases into water, he had affirmed that he could burn the inflammable air from charcoal (a mixture of hydrogen with different compounds of carbon) along with oxygen, into their conjoined weight of water. He now states, in contradiction of that incredible statement, that carbonic acid as well as water results from the combustion in question; but as if to atone for correcting one error, he commits another quite as great, and makes the extraordinary assertion, that inflammable air from iron, *i. e.* hydrogen, may be exploded with oxygen, and yet no water be produced.

It is further evident that he regarded the combining measure of the charcoal gas as identical with that of hydrogen, and double that of oxygen, a fact, as it will afterwards appear, of some importance, in reference to the exact doctrine taught by Watt in opposition to Cavendish, concerning water. Before, therefore, either Cavendish or Watt had given his views to the world, Priestley had lost faith in the conclusions of both. He held it possible that hydrogen might be burned and yet water not be produced, and he believed that carbonic acid was as common a product of the combustion of inflammable air and oxygen as water.

On the 23rd of January, 1784, he returns to the question of the source of the oxygen yielded by melted nitre, and announces that he has changed his opinion concerning the presence of nitrous acid in oxygen, as he had found that the acid was partly volatilized, partly dissolved by the water during the collection of the gas from nitre. "This," he continues, "greatly favours Mr. Watt's hypothesis, that air [oxygen] is dephlogisticated water. In this business I am little more than the *bellows-blower*."* This is the most important reference to the opinions of Watt concerning water, and to Priestley's share in producing them, which I have found in the Wedgwood Corres-

* That "air" here signifies oxygen, is evident from the nature of the experiment, and from Watt's exposition of his views, referred to in the preceding note.

pondence. With characteristic frankness, the latter disavows all but a slender share in leading to their formation, and emphatically underlines the title he gives himself. In truth, as he now dissented from them, he could not with any propriety claim a share in them. The hypothesis which he attributes to Watt has not reference merely to the evolution of oxygen from nitre, but to the general doctrine announced in his letter to Priestley, of April 1783, which as yet, at its author's request, remained unread at a public meeting of the Royal Society. That doctrine was, that water consisted of phlogiston, or inflammable air, and dephlogisticated air. By depriving water of the former, oxygen was obtained, and by such a loss of the phlogiston of the water hypothetically present in it, nitre was supposed to yield oxygen. I do not comment upon this passage because it is less full and explicit than one which Priestley published in 1785,* which has already been quoted, and will be referred to again.

A blank occurs here in the correspondence, for several months, at the critical period when De Luc and Watt were accusing Cavendish of plagiarism, and when, possibly, had letters remained, we should have learned Priestley's opinion concerning the claims of the rivals. The letter next in chronological order bears date 8th November, 1784, and was therefore written after Cavendish and Watt's views on the compound nature of water had been given to the world in the *Philosophical Transactions* for that year. Nor can Priestley have been without some private information concerning the feelings entertained at Birmingham towards Cavendish. His letter, however, betrays no consciousness of these, and is chiefly occupied with an account of the repetition of Lavoisier's experiments on the decomposition of water, but his name is not mentioned. After referring to the passage of water and spirits of wine, in the state of vapour, through a red-hot copper tube, he continues: "Iron, I find, gained one-third in weight in this process, and gives one-half more inflammable air than it does when dissolved in acids, the reason of which I believe to be, that much of the phlogiston is always retained in the solution of metals in acids. On comparing the experiments, I *now* think that the inflammable air is furnished by the *iron*, and that there is no decomposition of the

* *Phil. Trans.* 1785, reprinted in *Experiments on Air*, 1786, p. 71.

water. Mr. Watt thinks so too." According to this statement, Priestley and Watt imagined that when steam is passed over red-hot iron, it displaces the phlogiston hypothetically present in it, and unites with the calx of the metal. This appears more distinctly from what follows :—"Iron that is thus increased in weight, and has yielded so much air [hydrogen] (which, by the way, has not the least *offensive smell*, which has been so much complained of in filling balloons), is reduced to its former state by heating in charcoal. In this process, instead of yielding *water*, as we all imagined it would, it yielded a prodigious quantity of *inflammable air*, but of a peculiar kind, for it is about as heavy as common air, owing, as I found, to its containing a great quantity of *fixed air* combined with it, so as not to be separated by lime-water, but only by decomposition with pure air by the electric spark." The expectation of Priestley and his friends evidently was, that the phlogiston of the charcoal would displace the water from the calx of iron, and so reproduce the latter in the metallic state; and the whole passage shows, as other statements do also, how incredulous Watt was, concerning Lavoisier's great discovery that water may be analysed into hydrogen and oxygen.*

The remaining letters of Priestley to Wedgwood are of much later date than those already referred to. They contain, however, several important allusions to the nature of water, which may best be introduced here as enabling us fully to understand the point of view from which Priestley looked at the rival claims of Cavendish, Watt, and Lavoisier.

In a letter not dated by its writer, but endorsed 1787,† Priestley renews the correspondence with Etruria, which had been interrupted for a considerable interval, for the very cogent reason naïvely acknowledged at the commencement: "Having been engaged in courses of experiments that did not require the use of earthen retorts, &c., I have not troubled you of a long

* The heavy inflammable air referred to by Priestley, was evidently (in greater part at least) carbonic oxide, obtained by heating the oxide of iron (produced by the passage of steam over the red-hot metal) with charcoal; and the fixed air he supposed to be contained in it, was *produced* by the union of the carbonic oxide with the oxygen with which he exploded it.

† The endorsing is in the handwriting of Josiah Wedgwood, as his grandson, Francis Wedgwood, Esq., has enabled me to verify.

time. But now, having many things in view which I cannot do without your assistance, I am obliged to have recourse to it." The letter, however, does not allude to water, and is the only one of that year. On 8th January, 1788, he explicitly refers to the different views entertained concerning the composition of water, and seems at length to accept the office of umpire between the disputants, though not exactly in the way they desired :—

"As the experiments in which I am now engaged, promise to be of some consequence with respect to what has of late been the subject of philosophical discussion, I give you the earliest account of the probable issue of them.

"They completely refute the hypothesis of dephlogisticated and inflammable air composing *only water*. The decomposition of them always produces *acid*, and Dr. Withering finds it to be as yet in all cases the nitrous. They give reason to think that the great quantity of *water* that has been found in this case is nothing more than was either diffused through the airs, or was necessary to their aerial form. I almost conclude that water is the basis of all kinds of air. One of my experiments (on *terra ponderosa*) proves that it [water] is a considerable part of *fixed air*, not less than one-third of its weight; though it has been thought to consist of nothing but dephlogisticated and inflammable air.

"My experiments seem to render doubtful the conclusion that Mr. Cavendish draws from his, as I get nitrous acid from dephlogisticated air, without any that is phlogisticated. This is the case whether the dephlogisticated air be got from manganese, red precipitate, or red lead."

This important passage contains the first reference to an opinion from which Priestley never afterwards receded. Water he believed to be present in all gases, and hydrogen and oxygen he held might produce by their combustion, nitrous (nitric) acid. Cavendish had encountered the same *apparent* phenomenon, but had shown that it resulted from the presence of a little nitrogen in the gases burned together. Priestley, however, thought he had secured perfect purity of the hydrogen and oxygen, and discarded this explanation. He thus dissented alike from Cavendish, Watt, and Lavoisier, and totally abandoned his original doctrine, that inflammable and dephlogisticated air were entirely convertible into water.

He returns to the subject in his next letter (endorsed by Mr. Chisholm, Mr. Wedgwood's secretary, March 18th, 1788), in which, referring to his observations, reported in the preceding epistle, he says, "These experiments, I cannot help thinking, prove the *decomposition of water* to be a fallacy, and establish the doctrine of *phlogiston*."

On August 18th, (1788), he writes, "I see clearly the cause of the fallacy in M. Lavoisier's experiments and my own, in which we found *pure water*, when I now always find some *acid*." He then refers anew to experiments, which prove, as he thinks, that inflammable air from iron (hydrogen), in certain circumstances, unites with oxygen to produce carbonic acid; so that he had now come to the conclusion, that water, carbonic acid, and nitrous (nitric) acid, might all be produced by the union of hydrogen and oxygen. "The objection," he continues, "that Mr. Cavendish and Dr. Blagden made to my experiments, was, that the acid I procured was from *phlogisticated air* [nitrogen], but this I have abundantly obviated, for in all the processes the more there is of this air (or of any other kind that cannot be decomposed by it), the less acid I find." *

Assured as he thus thought himself, of his accuracy, which, however, he never asserts dogmatically, Priestley proceeded to set his contemporaries right, on a point on which he alone was all in the wrong, and in his next letter to his friend (Oct. 9,

* Had Priestley studied Cavendish's paper of 1784 he would not have made this mistake. Cavendish showed that the conditions for the production of nitric acid were, excess of oxygen, a moderate proportion of hydrogen, and a very small one of nitrogen. If the last were greatly increased, no nitric acid appeared. This negative result was procured when atmospheric air (containing 4-5ths of its volume of nitrogen) was detonated with hydrogen. Cavendish and Blagden appear to have pointed out this to Priestley when he communicated his second paper on phlogiston to the Royal Society (*Phil. Trans.* 1788), for in a letter to Wedgwood, not dated, but evidently written before that quoted in the text, he says, "the only objection that was made to my conclusions was, that the acid I got was from the phlogisticated air, which I could not exclude." Both letters are important, as showing that Cavendish and Priestley discussed a second time the question, what is the product of the combustion of hydrogen and oxygen? which had occupied them in 1781 and 1783; and that the latter, in 1788, in spite of all that had been written on the subject, in England and France, fell into the mistake against which Cavendish had guarded himself and others in 1784.

1788), he announces that he has "drawn up *a third paper* to send to the Royal Society," to show what he has done, "in a business so much agitated as the doctrine of *phlogiston*."* No further reference, however, to this question, which in Priestley's estimation was inseparable from that of the true nature of water, occurs earlier than 1790.† In an undated letter (endorsed by Josiah Wedgwood's secretary, Mr. Chisholm, October 1790) of that year, he writes, "My chemical pursuits have been directed to the great question now depending on the *decomposition of water, &c.* But still, whether I decompose the two kinds of air by an explosion in a copper tube, or by a slow burning, or in the manner of the French, I never fail to produce *acid*, though they now say they find none at all, and even have made ounces of water perfectly pure."

In the next letter (February 16, 1791), he enforces the same erroneous doctrines still more emphatically. "It was ob-

* Among some letters which Mr. Francis Wedgwood, of Barlaston, has allowed me to peruse, is one in the handwriting of Mr. Chisholm, Josiah Wedgwood's secretary, addressed to Priestley, but unsigned. It is evidently, however, from Wedgwood, and in answer to Priestley's letter referred to in the text. This appears from the writer's acknowledging "your good letter of the 9th" (9th October, 1788), and declining his friend's offer to send him a separate copy of his paper. The following extracts from it are of interest in reference to the Water Controversy :—

"I must, therefore, once for all, beg your acceptance of my best thanks for the early communications, from time to time, of your truly valuable discoveries, which now become more and more interesting; and I most sincerely wish you health with every convenience for the prosecution of them.

"I cannot forbear expressing my particular satisfaction to find that my old favourite, phlogiston, is likely to be restored to its former rank in the chemical world. * * * * * * Mr. Watt's conjecture of nitrous acid being *contained* in inflammable air, as the vitriolic is in sulphur, pleases me much, though I confess there is one circumstance which appears rather unfavourable to it; for I understand it to be by *combustion* that the acid is detached from the phlogiston, and one would expect the *nitrous* acid to be rather decomposed than developed by that process."

The allusion here, evidently, is to the appearance of nitric acid as a product of the combustion of *apparently* pure inflammable air (hydrogen ?) and oxygen, which all chemists now agree with Cavendish in referring to the presence of nitrogen as an impurity in the gases, which when burned together yield the acid, but which Watt, like Priestley and La Place, referred to an erroneous source.

This letter is the only one from Wedgwood to Priestley of which I have any knowledge. The MS. from which I have quoted was probably a copy of the original.

† The letter quoted in the text is the last belonging to 1788. There is only one belonging to 1789; it is addressed to Francis Wedgwood (the son of Josiah), and refers to private matters.

jected, though on insufficient grounds, to my former experiments, that the *acid* I produced came from the *phlogisticated air* that was necessarily mixed with the dephlogisticated that I made use of. But I now with great certainty make air so pure, that I am confident it contains no mixture [of] phlogisticated air whatever, and yet the explosion of this air, with a due proportion of inflammable air, produces more acid than when the air I used was less pure. I also use no air pump, filling my copper vessel with water, and displacing it by the mixture of air to be exploded.

"Admitting, therefore, what I am not disposed to dispute, that the slow combustion of the two kinds of air by the French philosophers, produces nothing but the purest *water*, it must be admitted that a different mode of combining the same elements in my process makes *nitrous acid*. . . . . The French experiment makes nothing against the doctrine of *phlogiston*, as it only proves that it enters into the composition of water."*

On February 26th, (1791), he reiterates this declaration more emphatically, "I an at pleasure make either *nitrous acid* or *pure water* from the same materials, viz., dephlogisticated and inflammable air. If there be a surplus of the dephlogisticated air, the result is always acid, if of the inflammable air, it is mere water. Extraordinary as this is, it is uniform, so that both M. Lavoisier and myself have been right. The doctrine of *phlogiston*, however, stands firm, and it only appears it is one element in the composition of water.

"I shall send a paper on this subject to the Royal Society in the beginning of the next week. *It will decide this long contest.*"

The italics in the last sentence are mine, and the words are very significant. The chosen umpire of the English rivals, it will be seen, put them out of court, made himself a party to the dispute, compromised matters with the French rival, and de-

* Priestley's disuse of the air-pump, in the hope, evidently, that he would avoid contaminating with nitrogen the hydrogen and oxygen which he exploded together, was of no avail in securing purity of the gases. The water with which he filled his copper vessel was certain to contain air (and therefore nitrogen) dissolved in it, which would be displaced by the hydrogen and oxygen, and become mixed with them. The conditions for the production of nitric acid as laid down by Cavendish were therefore secured, provided only *excess* of oxygen was present, and that there was, appears from the next letter (February 26th, 1791) referred to in the text.

clared the contest at an end. His judgment, we may be certain, satisfied no one but himself. It was ignored by his contemporaries, and has been reversed by his successors.

The remaining letters to Wedgwood contain no further reference to water. The "invasion of the Goths and Vandals," as Priestley styles the Birmingham riots of July 14th, 1791, is referred to in his next epistle, which is dated from London (July 26th, 1791), and a fresh supply of retorts is requested to replace those which had been destroyed by the rioters. Two other letters of 1791, one to Josiah, the other to Thomas Wedgwood, refer to the same subject. The last two of the series addressed to Thomas Wedgwood (Feb. 25, and March 17, 1792), allude to the same topic, and to some private matters.

Priestley's letters to Keir are of less importance. He explains to him in two letters of date 1782, his reduction of metallic oxides by hydrogen, when heated by the sun's rays concentrated by a lens, and his conclusion that phlogiston is "the same thing with inflammable air in a combined state." The next letter is of date, Dec. 15, 1787, and refers to the acid produced by the combustion of apparently pure hydrogen and oxygen. From it we learn the fact, not mentioned elsewhere, that Priestley at first supposed the acid to be sulphuric. "I have procured a solution of copper in some acid, which to all appearance is the same, and I think the vitriolic." In the trials referred to, the explosions were made in a copper vessel, a portion of which was dissolved by the acid produced. Had Priestley, however, called to mind, Cavendish's *Experiments on Air* of 1784 and 1785, he would have tested at once for *nitric* acid, which he afterwards found the acid to be. Other allusions to the appearance of an acid occur of the same nature as those in the letters to Wedgwood, but less numerous, and eight of the letters to Keir are not dated, except that some of them have the day of the week in which they were written, marked on them. The following is the only additional passage which it seems necessary to quote. It is from an undated letter: "That water is essential to every kind of air, I am now strongly inclined to believe, having found it to be so in respect to *inflammable* and *fixed air*, and so great a quantity being found on the decomposition of dephlogisticated air. It is probably that which gives them their *aerial form*, and may be called their *common basis*."

I have quoted fully from the *Wedgwood Correspondence*, because one important influence in occasioning the protraction of the Water Controversy, has been neglect or misapprehension of Priestley's views, especially on the part of the advocates of Watt. The letters extend over a period of ten years, during which no chemical enquiry interested Priestley more than the nature of water, and the downfall of the doctrine of phlogiston, which the alleged compound character of water threatened. He knew the views of Watt, Cavendish, and Lavoisier; he was jealous of none of them, and he never wronged them, except unintentionally, when he misunderstood them. Watt, moreover, was his friend, Cavendish only his acquaintance, and Lavoisier in some respects his rival. Wedgwood also, was a friend of Watt's, as several references in the correspondence, to pieces of apparatus sent from Etruria to him, show.* There were few, therefore, to whom Priestley was more likely to have expressed his opinion concerning Cavendish's good faith than to Wedgwood. The absence, accordingly, of the slightest reference to Watt's English rival as having wronged him, shows how little importance Priestley attached to the accusation. It is singular, however, that no reference should occur to the rivalry between Cavendish, Watt, and Lavoisier, in the writings of so zealous a friend, and so frank and outspoken a person as Wedgwood's correspondent; yet his silence is perhaps not difficult to account for. It was impossible, changing his views so rapidly and entirely as he did, that he should feel much interest in defending opinions which he disbelieved. A glance at the *Wedgwood Correspondence* will illustrate this. Before it commenced in 1781, Priestley had exploded hydrogen and oxygen, as a random experiment, and attached no importance to the appearance of water which resulted from the explosion. He reported, however, his friend Warltire to believe that the water was simply deposited from the oxygen, in which it had preexisted

* This will probably not be disputed by any one. I may state, however, that Mr. Francis Wedgwood informs me that his grandfather Josiah was, "according to his belief, on very friendly terms with James Watt," and that his uncle Thomas and his father were on the same footing with Mr. James Watt, junior.

ready formed. In 1782, he had demonstrated, as he thought, that water was transmutable into atmospheric air; and that hydrogen and oxygen can be burned into carbonic acid. In 1783, he abandons the former view, and announces on the authority of an inaccurate repetition of Cavendish's experiments, that inflammable air and oxygen can be burned entirely into water. In 1784, he asserts that these gases may be burned into carbonic acid and water, but that it is possible to explode hydrogen and oxygen together, without obtaining either; whilst the inflammable gas from heated charcoal yields both. In 1787, he affirms, that inflammable air and oxygen *always* produce nitrous acid by their combustion; and that water is essential to the existence of every gas. In 1788, he urges, in strong terms, what he had formerly stated less decidedly, that the decomposition of water is an entire fallacy; and finally, he declares in 1791, that if oxygen be burned with excess of hydrogen, it yields nothing but pure water; but if burned with a deficiency of that gas it affords nitrous acid as well as water. He thus ended in 1791, with the doctrine he had learned from Waltire in 1781, that water is simply deposited from the gases which yield it when they burn. He returned to the belief which, perhaps, he had never abandoned) that water was an element, and that it was present in every gas, and that nitrous acid was as certain a product of the combustion of hydrogen as water; nor does it seem beyond question that he ceased to believe that carbonic acid may be produced out of the elements of water.

It thus appears, that when Watt's jealousy was most strongly roused against Cavendish and Lavoisier in the end of 1783 and the beginning of 1784, Priestley had totally lost faith in the experiments of Cavendish, and in his own repetition of them (on which Watt had founded his theory of the nature of water), as well as in Lavoisier's similar researches. Nor did Priestley return to a modified faith in them till 1790, when, moreover, he affirmed that inflammable air and oxygen could produce nitric acid, which Cavendish, and Lavoisier totally disbelieved, and from the first had denied.

The tenor of the *Wedgwood Correspondence* is thus entirely in keeping with the published statements of Priestley; and it seems to me in no degree probable that any material evidence towards the adjustment of the claims of Watt and Cavendish, has been lost to us in the missing papers of Priestley. He showed no reluctance to return to the subject in his published papers, where, had he held a decided view as to the reality and greatness of the asserted discovery, he would certainly have told us with his accustomed frankness, what share he thought Cavendish and Watt had in making it. But if he did not do this in his familiar letters, it is not surprising that he should write in his published papers, as if there were no rivalry concerning the discovery; and any one who reads the passage I have previously quoted from his *Experiments and Observations relating to Air and Water* (1785*), without any other knowledge than it supplies, would infer, as he would also from the Wedgwood letters of the same date, that the most cordial harmony reigned between Cavendish, Watt, and Lavoisier, and that they were exactly at one in their conclusions. Yet Priestley certainly knew that Lavoisier was accused by the English chemists of unfair appropriation of their views;† nor is it probable that he can have been ignorant of the feelings entertained towards Cavendish by Watt. The probability is all the other way. We may, therefore, safely accept Priestley's published statement ‡ as all he cared to utter respecting the disputed claims, and it amounts only to an ascription to the rivals, of identity of belief as to the cardinal facts, with a difference of expression as to their significance. Cavendish, Watt, and Lavoisier are thus treated as independent asserters of the same truth. The language in which Watt expresses it is preferred, and the others are said to concur in it, but priority is implicitly ascribed to Cavendish, and the later date of Watt's conclusions is marked by their being stated to have been drawn "from some experiments of which I (Priestley) gave an account to the Society," which are unquestionably the repetition of Cavendish's experiments, contained in the paper of 1783. Priestley thus declined to take a side in the Water Controversy, and nevertheless retained the esteem of the rivals. Nor would it be fair for us to blame him, because we may imagine that had he boldly said,

* Ante, p. 85.

† *Watt Corr.*, p. 35, and *Phil. Trans.* 1784, p. 134.

‡ *Experiments and Observations on Air*, 1786, p. 71, or *Phil. Trans.* 1785, p. 279.

Cavendish knew thus much in 1781, Watt thus much in 1783, we should have been enabled to do entire justice to both. It should not be forgotten, in judging Priestley, that he did not claim his own share in the discovery of the Composition of Water, nor take any steps to correct the inaccurate account which Watt gave,* or the defective one sanctioned by Cavendish,† of his priority as the observer of the very important initial fact, that drops of a liquid inferred to be water bedew the sides of a vessel in which inflammable air and oxygen are burned. He who was so utterly indifferent to his own share in a discovery, could not be expected to concern himself much about the amount of merit which should be parcelled out among the three great claimants of it, who had forgotten him, and were abundantly able, without any help, to defend the right, which each asserted, to have the lion's share. It was no resentment of their indifference to him which kept Priestley silent, but the far more powerful motive, that he thought the lions were fighting for a shadow. It was a delusion, according to him, at the period when the rivalry was keenest, that water was a compound, and a mistake that it equalled in weight the burned gases which produced it. Hydrogen and oxygen always yielded nitric acid, sometimes carbonic acid, when burned together. Priestley thus, for his part, desired to cry *peccavi*, and no doubt thought that the great rivals would soon be among the penitents also. He, at all events, set them the example, and avoided fomenting what he must have considered a most needless and idle dispute.

Priestley, then, must be acquitted of everything like evasion, cowardice, or partiality, in his dealings towards the original disputants in the Water Controversy. His bias must have been in favour of Watt, but so far was he from giving way to this, that the latter evidently distrusted him, as the entire transference of his defence to the hands of De Luc, and the total absence, in the Water Controversy, of any appeals to Priestley,

* *Phil. Trans.* 1784, p. 332.

† *Phil. Trans.* 1784, p. 126. Cavendish's reference if taken along with Priestley's own account of his experiments and of Warltire's, which is adduced as the authority for the reference, cannot mislead; but if read alone, it would certainly convey the impression that Warltire, not Priestley, first observed the deposition of moisture to follow the detonation of inflammable air (hydrogen, or one of its compounds) with oxygen.

except as a witness to the authenticity of the letter of 1783, seem to show. An enquiry into the nature and source of this distrust will materially assist us in deciding how far, and in what way, Priestley was to blame for the origin of the Water Controversy. Watt's distrust, I believe, had reference solely to the accuracy of Priestley's experiments and conclusions, not to his friendly feelings, which he knew remained unabated. After learning that he had been led astray by his friend, as to the transmutability of water into air, Watt withdrew his *entire* paper, and as Priestley, before its publication, discovered his error as to the charcoal-gas yielding on combustion only water, and declared that hydrogen yielded none, it was vain to quote his early experiments as justifying Watt's conclusion. When Watt published his views accordingly, which he did not do till Cavendish and Lavoisier had put it beyond question, that water, weight for weight, is the only product of the combustion of hydrogen and oxygen, he took care to fortify Priestley's original statements, which the latter was now in private retracting, by adducing the Parisian repetition of Cavendish's experiments as "clearly proving" the "essential point," that "the deflagration or union of dephlogisticated and inflammable air, by means of ignition, produces a quantity of water, equal in weight to the airs; and that the water thus produced appeared, by every test, to be pure water."*

Watt was plainly justified in preferring the advocacy of De Luc to that of Priestley in 1784, and the last must to some slight extent be reprehended for being the innocent cause of some part of the jealousy of Watt towards Cavendish, and for having abridged our means of doing justice to either. Had he told Watt all that Cavendish told him, and told it as Cavendish's, the great engineer would never have listened to De Luc's insinuations against his rival's honour. Had he accurately repeated Cavendish's experiments, moreover, or accurately observed what his own researches brought to light, the question between the English claimants of the disputed discovery would have been greatly simplified, and Lavoisier would have been altogether excluded from a claim to the initial discovery.† It cannot but be regretted, therefore, that the medium

* *Phil. Trans.* 1784, p. 333.

† *Watt. Corr.*, p. 34.

of communication between Cavendish and Watt should have been Priestley, whose great and peculiar gifts as a scientific observer, were unsuited for a delicate quantitative investigation, such as an enquiry into the product of the union of hydrogen and oxygen preeminently was. He cannot be acquitted, also, of hasty observation, which led him into strange inaccuracies, nor of inattention to the views of others, which led him unintentionally to misrepresent them; and the contradictory statements on the same subject which he printed side by side, without comment or attempt at reconcilement, imply a peculiarity of intellectual organisation singularly disqualifying him for the office of umpire to which he was, without seeking it, preferred.*

I cannot say thus much in depreciation of Priestley, whom I honour for capacity, courage, honesty, earnestness, ingenuity, energy, and almost unequalled industry, without seeking to guard against doing him injustice. It has been one of the most unfortunate results of the Water Controversy that it has led to accusations against many besides the principals involved in it, and I count it no digression to save Priestley's character from being misapprehended in connexion with it. It should be remembered, then, that Priestley had seen so much of the evil of obstinate adherence to opinions, which time had rendered decrepit, not venerable, and had been so richly rewarded in his capacity of natural philosopher, by his adventurous explorations of new territories in science, that he unavoidably and unconsciously over-estimated the value of what was novel, and held himself free to change his opinions, to an extent not easily sympathised with by minds of a different order. Some men love to *rest* in truth, or at least in settled opinions, and are uneasy till they find repose. They alter their beliefs with great reluctance, and dread the charge of inconsistency, even in reference to trifling matters. Priestley, on the other hand, was a 'Follower after Truth,' who delighted in the chase, and was all his life long pursuing, not resting in it. On all subjects which interested

* Thus, in the volume of *Experiments and Observations on Air* for 1786, he reprints the statement of 1783, that the inflammable air from charcoal and oxygen can be burned into their united weight of pure water, although he had announced to Wedgwood in 1784, that he had discovered that this was a mistake (as it certainly was), and that carbonic acid as well as water was a product of the combustion. His different views, indeed, on water, are irreconcileable.

him, he held by certain cardinal doctrines, but he left the outlines of his systems to be filled up as he gained experience, and to an extent very few men have done, disavowed any attempt to reconcile his changing views with each other, or to deprecate the charge of inconsistency. It is impossible not to admire his moral courage and candour in frankly acknowledging this, and his faith in Wisdom justifying herself without his entering on a defence of his alterations in belief. Nevertheless, I think it must be acknowledged by all who have studied his writings, that in his scientific researches, at least, he carried this feeling too far, and that often, when he had reached a truth, in which he might and should have rested, his dread of anything like a too hasty stereotyping of a supposed discovery, induced him to welcome whatever seemed to justify him in renewing the *pursuit* of truth, and thus led him often completely astray. Priestley, indeed, missed many a discovery, the clue to which was in his hands, and in his alone, by not knowing where and when to stop. This peculiarity, however, is not more manifest in his researches into the true nature of water, than in his other scientific investigations, and no special blame belongs to him for his errors in connexion with the former.

Further, from the most praiseworthy motives, Priestley never concealed a discovery till he had completely realized its certainty, or hoarded a truth, content like Cavendish to have learned it for himself. As one who knew him well* has justly said, he was characterised by "extreme ingenuousness of character and honesty of purpose. Whenever he discovered a new fact in science, he instantly proclaimed it to the world, in order that other minds might be employed upon it besides his own." Priestley greatly furthered the progress of chemistry by this speedy publication of his discoveries, but it unavoidably led him to make known unfinished researches, and involved him in contradictions which a less liberal and more cautious mode of procedure would have prevented. We must set off the immense service he rendered science by the free and rapid publication of his ingenious observations, against the harm he occasionally did by premature announcement of supposed discoveries.

* The late T. L. Hawkes, of Birmingham. Dr. Russell has furnished me with Mr. Hawkes' judgment of Priestley's character.

I must also plead in defence of Priestley, a very curious mental peculiarity of his, to which he has drawn attention in his autobiography, as it will assist us in explaining his behaviour in the Water Controversy. Priestley's words are as follows:—"As I have not failed to attend to the phenomena of my own mind, as well as those of other parts of nature, I have not been insensible to some great defects, as well as some advantages attending its constitution; having from an early period been subject to a most humbling failure of recollection, so that I have sometimes lost all ideas of both persons and things that I had been conversant with. I have so completely forgotten what I have myself published, that in reading my own writings, what I find in them often appears perfectly new to me, and I have more than once made experiments, the results of which had been published by me."*

To this psychological characteristic of Priestley I refer the more confidently that my attention was drawn to it by his grand-daughters, as a well-known peculiarity of their ancestor, without any reference to its bearing upon the Water Controversy. Dr. Russell, of Birmingham, also informs me that Priestley's intimate friend and associate, Mr. Thomas Lakin Hawkes, bore testimony to his "lack of memory, which he ascribed to his great mental activity." This appears a true theory of what was a source of strength, as well as of weakness to Priestley. To be able utterly to forget what had shortly before entirely occupied his thoughts was in one respect the best possible preparation for a new research. And those who, like myself, have read with astonishment, the mere list of Priestley's varied publications on nearly every branch of human enquiry, will find in his "most humbling failure of recollection," one key to the success with which he prosecuted so many and so diverse enquiries.†

It was also, however, a source of weakness, and probably furnishes the explanation of some of the transactions in the Water Controversy, which appear at first sight so inexplicable.

* *Life of Priestley*, Centenary edition, p. 74, where a remarkable example of total failure of recollection is given.

† Priestley's case was not singular. I have heard it remarked by a competent judge, that many of our most successful barristers owed their success to their power of completely dismissing from their thoughts a case in which they had been engaged as soon as it had been decided.

I have no special point to prove by referring to it; but it seems to me, that the possession of a memory so treacherous as Priestley's was, may perhaps explain how it happened that Watt was unaware, as he seems in 1783 to have been, that the experiments on which he based his theory of the composition of water, were repetitions of those of Cavendish, and, therefore, why he omitted all reference to the latter's priority; and further, it may throw some light on the existence in Priestley's paper of 1783, of the incredible description given there of the results of the combustion of charcoal gas and oxygen, and in general, may help to account for the many opposite theories of the nature of water, which Priestley published and republished without hinting at their contradictory character, much less seeking to remove it. One who could totally forget his own experiments, must have been at least equally liable to forget those of others, and when he substituted charcoal gas for hydrogen, in repeating Cavendish's experiments, it may have been, not as a supposed improvement, but in entire forgetfulness that hydrogen was the gas he was desired to employ. At all events, Priestley's singular obliviousness must be taken into consideration by all who are aware of the difficulty of reconciling his dealings towards Cavendish and Watt, with the only tenable hypothesis of his character, viz., that he was a very honourable, ingenuous, and truthful person, the possessor of a most active, versatile intellect, which education and unceasing exercise had trained to a rare degree of acuteness.

Having thus disposed of the share which Priestley took in the Water Controversy, after he was appealed to by Cavendish in the first of the three passages which he added to his paper of 1784, before it was printed, I proceed to the consideration of the second, so-called interpolation which has led to much discussion, both in reference to its contents and to its writer. Like the first interpolation, it is in the handwriting of Sir Charles Blagden, but the important part which he took in the controversy is best reserved for later discussion, and as the added passage went forth to the world as an authorised expression of Cavendish's sentiments, and formed an integral part of his published paper, it is of much more importance to determine what its significance is, than to settle why it was penned by Cavendish's amanuensis, Blagden, and not by himself. It forms one among the personal incidents of the Water Controversy, inasmuch as it contains the only direct reference which Cavendish made to Watt throughout the discussion. It is as follows:—"As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention, in a few words, the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. Watt says is true.*" Cavendish then proceeds to explain (as he had already announced to the world in 1783) that he is not a believer in elementary heat; and that even if he were, he should not think it necessary to insist upon its evolution accompanying the union of hydrogen and oxygen, as of more importance than its development during other chemical combinations. The passage is elsewhere commented on, so far as the doctrine it teaches is concerned. I consider it here only in its character of a personal reference by the one rival to the other.

The advocates of Watt read this passage by the light of the suspicions which they inherit from De Luc, and some (Sir David Brewster and Mr. Muirhead, amongst others) have gone the length of affirming that it contains an implicit acknowledgment of Watt's priority, and a confession of the wrong his rival had done him in not mentioning his name in the original draft of the *Experiments on Air*, communicated to the Royal Society on January 15th, 1784. Yet certainly his words do not convey this meaning, although those who contend for it rest their argument solely on the words. These make no allusion to obligation; they make no acknowledgment of priority; they do not even assert identity of opinion, but are chiefly occupied with the assertion and defence of a difference in the doctrine which Cavendish taught. Watt, a pupil of the school of Black, was a firm believer in the materiality of heat, and attached great importance to its evolution when water was produced from its elements. Cavendish was of the school of Newton, and regarded heat as immaterial, neither did he think that there

* *Phil. Trans.* 1784, p. 140. The passage is enclosed between square brackets in Mr. Muirhead's reprint. *Watt. Corr.*, p. 135.

was anything specially remarkable in its evolution when hydrogen and oxygen united, in which opinion the chemists of the present day are universally at one with him. But when he found Watt insisting on its special importance in reference to the production of water, he added a passage to his paper to show that he had deliberately, not through ignorance, avoided enlarging on this.*

In this commentary Cavendish treats Watt's doctrine as being generally the same as his own, a fact on which the advocates of the latter build much, but we shall presently find, that though the statement by the rivals of their doctrines was substantially identical, there was an important difference indicated by Cavendish in his first interpolation, in their use of the term *phlogiston* or inflammable air, to denote the combustible element of water. This does not, however, concern us at present. What is important is the information which the passage quoted gives us as to the *animus* of Cavendish towards Watt. It is creditable to him in the highest degree. The advocates of Watt, when they reproach Cavendish for so tardy a reference, as they count it, to their client's views, contrive most singularly to forget two things. *Firstly*, that Watt had made it impossible for any one (except Priestley, to whom it was addressed) to quote from his letter of 1783, as containing his views on the composition of water, by withdrawing it from public notice, so that it was rendered a *private* letter. If any reason were given at the time for this withdrawal, it must have been the same as that announced more recently, namely, its author's loss of faith in certain of the conclusions contained in it, whilst Cavendish was entirely ignorant what the conclusions were which Watt had abandoned, so that for anything he knew, or could know, they were the very opinions which, in 1784, were reclaimed as never having been distrusted. *Secondly*.—The advocates of Watt have still more strangely forgotten, that in neither of his letters did he make the slightest acknowledgment of his obligation through Priestley to Cavendish, for the experimental data on which his conclusions were founded. He set the example (perhaps unconsciously)

* In the preceding chapter, and in the abstracts of his papers on heat, it is shown that Cavendish held this doctrine before the Water Controversy arose. See especially his paper on the freezing of mercury in *Phil. Trans.* 1783.

of injustice to his rival (if injustice there were on either side). Of deliberate injustice I have already acquitted Watt, and I am willing to believe that he had forgotten, or was ignorant of Cavendish's experiments having been made in 1781. I shall further concede, for argument's sake, that Priestley, whose share in the matter it is so difficult to determine, was alone responsible for Watt's ignorance of Cavendish's researches; and further, that the latter were, as nearly as possible, mere observations of phenomena, from which no conclusions were drawn. Nevertheless, Cavendish had first observed that equality of weight between the gases burned and the water produced, without the certainty of which Watt's conclusions were, as he acknowledged, baseless. The former, therefore, on the lowest estimate of his deserts, might well feel astonished that his name was utterly passed over in Watt's letter of 1783, and that the truth which he had taught Priestley was referred to, as if discovered by his pupil, whilst that pupil freely acknowledged that he had learned it from Cavendish. This omission by Watt might, with great show of justice, have been set down by Cavendish as a wilful and unfair disregard of his claims. De Luc, we have seen, without the slightest enquiry, gave vent to gratuitous suspicions against his friend's rival. Had Cavendish or Blagden copied his evil example, he might have pleaded not surmises, but the fact confessed by Watt, that he was indebted to Priestley for his experimental data, and could have confidently asked if it were very credible that Priestley's intimate friend and fellow-townsman could be ignorant of the truth, so willingly published by the former, that he had only imitated Cavendish. A *primâ facie* view of matters would certainly render it probable that Watt had deliberately concealed what Priestley told him.

When Watt, therefore, in April 1783, addressed to the Royal Society a document expounding his views on the nature of water, and totally omitting Cavendish's name, he showed a disregard of the claims of the latter *to at least the experiments* which might have been pleaded in justification of a corresponding omission of all reference to him. At the present day, with our perfect assurance of the unimpeachable integrity of Watt, and our knowledge of the contradictory and perplexing dealings of Priestley, we may find nothing strange in the silence of the former. But Cavendish might well wonder, that when every one else knew that Priestley had only copied his experiments, Watt alone, who took such an interest in the copy, should refer to it as an original; and had Cavendish or his friends been found looking suspiciously at Watt, we must have conceded that they had grounds for their suspicion. In saying this I purposely take the lowest possible ground for Cavendish, and assert that even had he borrowed his conclusion from Watt without acknowledgment, he could plead that the latter had borrowed his facts without acknowledgment from him, so that Cavendish had only followed a bad example, and been guilty of reprisals.

But such low ground I gladly leave. Cavendish needs no such defence. We have hitherto looked at his dealings towards his rival, through the distorting medium of De Luc's suspicions, which the advocates of Watt would have us believe, supply a faultless magnifying-glass for discerning his virtues and Cavendish's transgressions. The position, however, which Cavendish himself assumes in the interpolation under notice, is that of one who had done Watt no wrong, and owed him nothing, and from this point of view I now look at his dealings. And plainly, if it were the case, that Cavendish had interpreted his experiments and had come to the conclusion which he ultimately connected with them, before he reported them to Priestley, he owed Watt *less* than nothing. Because Watt hastened, in 1783, to publish his interpretation of phenomena which were known to both, whilst Cavendish preferred to prosecute his researches before he made any part of them public, there certainly lay no claim on Cavendish to refer to Watt, when he formally made known to the world what he had discovered for himself. No one will assert that two independent discoverers of the same truth are required by justice (although they may be by courtesy and good feeling) to expatiate on each other's merits. And in the particular case of rivalry before us, Cavendish knew that he had long preceded Watt (however ignorant Watt might be of the fact), and could appeal to Priestley as a witness to the truth, that he had demonstrated to him that hydrogen and oxygen could be converted or compounded into water, before the possibility of such an occurrence had become known to Watt. The latter only asserted this truth in other words and later in time. Will the most exacting critic say that Cavendish was even in courtesy, required to point out, that another had later than himself drawn a conclusion identical with his, from similar experiments? But this is not all. It was Cavendish's *very* experiments on which Watt was really, however unconsciously, founding his theory; for all that *seemed* true in Priestley's avowed repetition, was untrue as regarded it, and true only as connected with the accurate original trials.

However ignorant Watt might be of this, Cavendish was well aware of it: he might have asked at the hands of those who were so sensitively jealous of Watt's rights, whether he had none, and with what show of justice they reprehended him for not acknowledging that Watt, who omitted all reference to him, should have it punctiliously conceded that he had drawn from experiments performed by his rival (which had reached him half-interpreted, at least, through Priestley), conclusions which Cavendish, their original performer, had from the first taught as inevitable consequences of the truths he had discovered. If the advocates of Watt should feel tempted to dispute the relevancy of this argument, their own client would shut their mouths. Watt had taken his case out of court, and forbidden any one to offer an opinion upon it. His letter of 1783 he had suddenly withdrawn (he did not at the time say why), and he *certainly* would have reproached any one who, after its withdrawal, had imputed to him the belief that water was transmutable into atmospheric air; nor was there any clue by which it could have been discovered by those not on terms of intimacy with him, that he still held faith in the convertibility of inflammable air and oxygen into water. The letter of 1783, indeed, which Watt's advocates speak of somewhat magniloquently, as having been deposited "in the archives of the Royal Society," was at one time in the hands of Priestley, at another in those of Sir Joseph Banks, and had actually to be reclaimed from those of De Luc, in 1784, before it could be read publicly; nor would Sir Joseph read it, as we have seen, till he was a second time formally requested to do so by Watt. It is most idle in these circumstances to affirm that Cavendish or any one else was bound by a document, which was half-public, half-private, which its writer partly retracted, partly avowed, without informing any but his intimate friends, to what extent, and for what reason, he disbelieved some portions of it, and hesitated to publish others; which when ultimately reclaimed was neither in the hands of Dr. Priestley, to whom it was addressed, nor in those of the president, or secretaries, or council of the Royal Society, who were its official guardians if it were consigned to the keeping of the society, but in those of De Luc, who had no apparent right to it, and from whom the president had to demand it.

It thus appears that Watt had put it out of the power of Cavendish, however much he had wished to do so, to refer to the opinions expressed in the private letter of 1783, and that Cavendish is, therefore, altogether blameless for taking no notice of Watt in the first draft of his paper of 1784. Further, it appears that Watt's conclusion was in statement substantially identical with Cavendish's; was drawn from his experiments; and was formed later in time than his inference that hydrogen and oxygen are convertible, weight for weight, into water. So that Watt only re-announced at a later date (and not accurately) the views which Cavendish had already taught Priestley, and this without the slightest reference to the former. Cavendish, accordingly, so far from being blameworthy, for not praising Watt, might with justice have taxed the latter with wronging him, and totally omitting his name, although (as all but the extreme partizans of Watt acknowledge) he was at least entitled to the whole merit of the *experiments*, whoever might claim the conclusions; and had Priestley's unsolicited testimony to prove that the latter were substantially his also.

Thus far, then, Cavendish's deportment to Watt was irreproachable; nor was it till the latter published his views that they could be criticised. He did this, as we have seen, in April 1784, and Cavendish commented on them in the first two additions made to his paper. The spirit in which he did so was altogether commendable. There can be little question that one object of the first interpolation was to counteract the impression which Watt's letters unavoidably conveyed, that Priestley's experiments were original, and to explain the doctrine which Cavendish had taught him and Lavoisier.

This first interpolation, as well as the second, contains the only expression of opinion which Cavendish ever published, and so far as is known, ever uttered, concerning Watt's claims and opinions, as contrasted with his own. He might have reproached his rival for doing him injustice, or have passed over his name in silence without wronging him. He did neither; but accepting Watt's claim to be considered as an independent teacher of a theory of the composition of water, he pointedly drew attention, not only to the priority of his own views, but to the connexion of Priestley and Watt's conclusions with experiments made, not with hydrogen, as his were, but with charcoal gas. He made no reference to Watt's first letter of 1783, but commented solely on his paper of 1784, which consisted of two letters and several additions and alterations of different dates in 1783 and 1784, and he did so solely to express dissent from certain of its views. Cavendish's total avoidance of any complaint of wrong done him, his temperate simplicity in urging his own claims, and his utter indifference to personalities, were entirely in keeping with his character. Watt appears in unfavourable contrast with Cavendish in his treatment of his rival. So long as he omitted all notice of him, he might be acquitted of intentional wrong, but when he made a deliberate reference to Cavendish's researches, he should have been careful to give an accurate account of them. Yet I should be slow to blame so generous a man as Watt, and would willingly make large allowance for the effects of the sinister influence which the mischief-making De Luc was exerting upon his candid spirit, when, smarting under the sense of a supposed wrong done him by his rival, he referred to Cavendish's researches. Watt's own advocates, however, have selected his scanty and inaccurate reference to his rival, as something which erred on the side of praise, and have left me no choice but to point out how unwise and unjustifiable this laudation is. The reference by Watt to Cavendish, is contained in a note to his paper of 1784.* The latest date attached to the paper is November 26th, 1783, but the note was not written then, and probably not till March 1784. Mr. Muirhead informs us "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 M. De Luc, of 26th Novem-

* *Phil. Trans.* 1784, p. 332.

ber, 1783; but is added at the bottom in pencil, in his own hand."* It seems probable, accordingly, that it was written by Watt when he visited London in the end of March or the beginning of April 1784, and had communicated with De Luc, in whose possession the letters, afterwards published, were. At all events, it seems certain that the pencilled note formed part of Watt's paper when it was read to the Royal Society, for he had no access to his MS. after it was read and had been consigned to the secretary of the Royal Society.†

The point is of some importance, as affecting the question whether Cavendish was aware of the existence of this note, when he wrote the two interpolations already considered. If it formed part of the paper when read, as it seems unquestionably to have done, he could not be ignorant of its contents; and it left him no choice but to add such passages before his paper was printed, as he did in the two first interpolations. It was thus an interpolation of Watt's, which led to the interpolations of Cavendish. The note is as follows, "I believe that Mr. Cavendish was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired;"‡ and Mr. Muirhead thus comments upon it, "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*."§ Watt was certainly guilty of great inadvertence in writing thus. We may not doubt that his ignorance was unfeigned, but we can unhesitatingly affirm that it was wilful; and that the error in his notice of his rival's doings did not lie in its overpraise. In truth, Watt's ignorance of the proceedings, not only of Cavendish, whom he counted his enemy, but of Priestley, whom he knew to be his friend, adds another to the many perplexities of the Water Controversy. Cavendish never pretended to have been the first to observe the "dewy deposit," but imputed that observation to Warltire and Priestley; and referred to the latter's *Experiments and Observations on Air* (1781), as containing the earliest account which was known to him, of the appearance of moisture succeeding the combustion of

* *Watt Corr.*, p. xxxiv.

† *Watt. Corr.*, p. 59-61.

‡ *Phil. Trans.* 1784, p. 332.

§ *Watt Corr.*, p. xxxiv.

inflammable air and oxygen. It is almost inexplicable that Watt should have been worse informed on this point than Cavendish. The latter quoted from a volume printed in 1781 at Birmingham, where Watt resided, written by his friend Priestley, and containing an account of experiments by his fellow-townsman Warltire; and yet it should seem that Watt was ignorant of its contents in 1784, and was utterly unaware through private channels, although in constant communication with Priestley, that he had preceded Cavendish in observing the phenomenon in question. This is certainly strange, but our wonder is increased when we consider that Watt had other means of information open to him, than the volume of experiments and observations of 1781. Cavendish had referred his readers, in his paper of 1784, to that work, and had quoted from it the observations of Priestley and Warltire; and De Luc, as we have already seen, made an analysis of the MS. of Cavendish's paper for Watt, and sent it to him. Yet this analysis, if it were an accurate one, must have included some reference to Warltire and Priestley, and should have saved Watt from robbing them of an honour which was repudiated by Cavendish, to whom he unjustly transferred it. And further, the printing of Watt's paper was not commenced till August 1784,* up to which period its author was at liberty to suggest any alteration which he thought proper. It seems most unlikely, however, that before this period, he should not have read Priestley's paper *On Phlogiston, and the seeming Conversion of Water into Air*, published in the *Philosophical Transactions* for 1783. This was the paper on which Watt's first letter was a commentary, and which it originally accompanied. It contained many references to himself, and with the greater part of its contents he was well acquainted. How then, did he continue to be ignorant of the fact, that it contained an account of Cavendish's experiments, in which it was implicitly asserted,—not that he had first observed the "dewy deposit," an observation which Priestley had already claimed for himself and Warltire,—but the much more important, and indeed, cardinal truth, that in certain proportions, a given weight of hydrogen and oxygen may be converted into the same weight of water?

* *Watt. Corr.*, p. 68.

There is something much more inexplicable in Watt's ignorance on the points referred to, than in anything attaching, on any hypothesis, to the dealings of Cavendish or Blagden. Nor can the blame be transferred from Watt to any one else. He stands self-condemned. The note appended to his paper shows, that he had become aware that, to some extent, Cavendish had preceded him, and the contents of the note profess to exhibit to what extent he had been anticipated by his rival. Its great inaccuracy, and above all, its injustice to Priestley, I accept as evidences of its having been the honest expression of its writer's notions at the time when it was written, of Cavendish's share in the disputed discovery. But that Watt took no pains to ascertain the truth, and chose to hazard a surmise, rather than investigate the matter in dispute, is most manifest; for Cavendish's MS. or De Luc's Abstract, or Priestley's Papers, or Priestley himself, or Blagden, or Sir Joseph Banks, or Maty and Planta, the Secretaries of the Royal Society, or Kirwan, besides others, could have prevented him from committing the great mistake of which he was guilty. I dwell upon this, not in order to show how idle is Mr. Muirhead's declaration that Watt assigns Cavendish too much merit, but because it demonstrates beyond all question, that Watt was incapacitated from doing Cavendish justice by his wilful ignorance of the latter's proceedings, and thus yielded at once to the insinuations of De Luc, who was equally ill-informed concerning them.

I need not further urge that Watt's interpolation in reference to his rival, places him in a less favourable light than Cavendish's interpolations in reference to him. I could, however, say more. Cavendish might with great justice have reproached Watt for reaping where he had not sown, and gathering an unripe crop which was not his, whilst its true proprietor waited only for its full ripening to celebrate his harvest. He might have asked whether it was either just, or generous, that another, made acquainted with his earlier researches, should step in between him and their completion, and Watt could have defended himself against the charge only by pleading his total and extraordinary ignorance of all that his rival had done, and all that his friend Priestley had written concerning his doings. That Cavendish preferred no such charge is to his credit, for had he entertained one tithe of the suspicions towards Watt, that

Watt entertained towards him, he could have justified his calumniations of his rival by tenfold more numerous and more plausible grounds for his jealousy, than De Luc or Watt did or could show for their suspicions of him, and he might most justly have resented in terms of the strongest indignation, the account of his researches which Watt published to the world. It is Cavendish, not Watt, that must be commended for a liberal and generous interpretation of the dealings of his rival.

These illustrious men appear no more in our pages in contention with each other, and the claims of the great French rival of both will be considered latest of all. Before, however, he can be referred to, the dealings of the third aider and abettor in the Water Controversy, Dr. Blagden, must be considered. He is alleged to have assisted Cavendish in wronging Watt, and he was umpire between them and Lavoisier, against whom he gave his judgment, so that the position he occupies is an important one on any view of the Water Controversy, and is rendered additionally so by the unsparing denunciations which have been heaped upon him by the friends of Watt since the revival of the Controversy.

Dr. Blagden was Cavendish's assistant in his scientific investigations, and acted also as his amanuensis. He penned, as we have already seen, two of the interpolations in Cavendish's paper.* In May 1784, he was appointed one of the Secretaries of the Royal Society, and in this capacity superintended the printing of Cavendish and Watt's papers, in which certain errors of date were permitted to occur, for which he was more or less responsible. It was through him, as Lavoisier acknowledged, that he became aware of Cavendish's experiments on the combustion of hydrogen and oxygen, and he wrote a letter to Crell, in 1786, accusing Lavoisier of plagiarism, and referring to Watt's theory of the composition of water, as similar to Cavendish's.† The special charges preferred against Blagden shall be minutely considered in the detailed analysis of the Water Controversy. It will suffice, therefore, here to state, that the advocates of Watt have in different degrees accused Blagden of

* The third, which was a postscript in Cavendish's handwriting referring to Lavoisier, will be noticed in the sequel.

† *Chemische Annalen*, 1786, pp. 58—61: Translated by Mr. Muirhead, *Watt Corr.*, p. 71.

officious intermeddling between Cavendish and Watt; of having done something unfair in writing the interpolations in the manuscript of the former; of having been in a condition to affirm whether Cavendish or Watt first formed a theory of the composition of water, and of having kept silence, to the disadvantage of the latter; of having, with most culpable carelessness (if his fault was not greater), suffered errors to occur in the papers of Cavendish and Watt, which gave the former a fictitious priority; and of having done Lavoisier some injustice, in the report which he gave of his experiments, made in imitation of Cavendish's, in 1783.

These are formidable charges, and they are urged, though by no means in equal degree, by Arago, Sir David Brewster, Lord Brougham, Lord Jeffrey, Mr. James Watt, junior, and Mr. Muirhead; and in such a shape that they more or less inculpate Cavendish. The asserted, or suggested motive, to all these false dealings on the part of Blagden, was the salary which Cavendish paid him, and the legacy which he bequeathed to him. Although, accordingly, it is not incumbent on me, as the biographer of Cavendish, to enquire into the character of Blagden, it is requisite to ascertain whether the good name of the former is tarnished by his connexion with the latter; and to demonstrate, if possible, either that Cavendish was not responsible for the transgressions of Blagden; or still better, that the latter was not a transgressor, and involved neither himself nor his principal in responsibility or blame. The latter is the proposition which I hope to prove, and I will commence the proof by observing, that the advocates of Watt have said either too little or too much in reference to Blagden. If he was guilty of even a part of the offences laid to his charge, he should have been held up to the universal scorn of mankind, as a treacherous and venal deceiver, who for base lucre was guilty of crimes which, had they been amenable to a court of justice, would have been styled falsehood, perjury, and forgery. And in that case Cavendish should have been openly denounced also, as having connived at crimes committed to serve him, and as standing in the position of the wealthy receiver of stolen goods, who bribed the poor thief to steal for him. Blagden might have pleaded with Shakspeare's apothecary in *Romeo and Juliet*, "My poverty, but not my will, consents;" but what could the millionaire philosopher plead in his defence? He was the greater transgressor of the two, and added cowardice to his other crimes. These charges the advocates of Watt have not in so many words preferred against Cavendish and Blagden, and yet if their accusations were justifiable, they should have taken this shape.

If, on the other hand, Watt's defenders had no other evidence to adduce, condemnatory of Blagden, than that which they have adduced, they have said greatly too much in depreciation of his good name, and their fault is the greater, that they generally avoid precise charges, and leave hanging over the heads of Blagden and Cavendish a dark ill-defined cloud of suspicion, which from the standing-point of their detractors, looks black as midnight, whilst it eludes the grasp of those who seek to show that it is but a mist created by prejudice, and that the only cloud over Cavendish's head is a halo of glory.* It is time that these charges should be made definite, and that the questions should be asked and answered categorically: 1. Was Blagden guilty of the offences laid to his charge? 2. Was Cavendish conscious of Blagden's guilt? If the first question be answered in the negative, the second will not require consideration. As essential to this, I offer the following sketch of Blagden's life and character, which I have taken some pains to render as accurate as possible. No detailed notice of him, so far as I am aware, has appeared in any English work, probably in consequence of his death having occurred at an advanced age in Paris. The esteem, however, in which he was held by the French savans, led to a short sketch of him being published in the *Moniteur* for September 22nd, 1820, page 1296. This was written by M. Jomard,† and through the kindness of M. F. Delessert, of Paris,

* In the concluding sections of the Water Controversy, the opinions of the individual advocates of Watt are referred to separately; and in the section entitled Bibliography, the reader will find the means of consulting their writings, from which I do not quote here any particular pages, because Blagden figures so prominently in the works in question, that I could only write *passim* against the reference to his name; and, in justice to the writers, their whole account of him should be consulted. I may notice, however, that Lord Jeffrey adopts but a small part of the charges preferred against Blagden, and that Lord Brougham does not judge him very severely. Both of these writers, however, in common with the other advocates of Watt, make pointed reference to Cavendish's pecuniary grants to Blagden, and significantly connect these with his sins of omission or commission.

† Jomard was one of the principal editors of *La Description d'Egypte*, published by the French Government after the return of Bonaparte's Egyptian expedition.

I have obtained a copy of it, as well as of the inscription on Blagden's tomb in Père la Chaise, which is of some importance in reference to dates. By R. H. Blagden Hale, Esq., also, of Cottles, Melksham, Wiltshire, the nephew and executor of Sir Charles Blagden, I have been favoured with much interesting information. The life of Blagden was destitute of any very remarkable incidents, and his character is of more importance to my present purpose, than his personal career. I have applied, accordingly, to all those who were acquainted with him, to whom I had access, for information as to his reputation in the eyes of his contemporaries. By Robert Brown, Esq., of the British Museum, and Dr. Thomas Thomson, of Glasgow, who were friends of Blagden, I have been most kindly favoured with their judgment regarding him ; and with Mr. W. A. Cadell, who knew him, I have had many conversations concerning his character. Their opinions are given in the sequel.

Charles Blagden was born at Wooton under Edge, in Gloucestershire, in April 1748. The day of his birth I have not learned, but Mr. Blagden Hale informs me that he was baptised on the 19th of the month. He had not the advantage in youth of attending a public school or either of the universities. He acquired, however, a considerable acquaintance with languages, for which he must have been mainly indebted to his own exertions, for he was early educated for the profession of medicine. He practised this, for some time privately, but about 1776, he received a medical appointment in the army, and went to America to do duty with the troops.* He returned to this country, as Mr. Blagden Hale informs me, "about the latter end of 1779, or

He had access to the best sources of information. M. F. Delessert writes to me, "Sir Charles Blagden était très lié avec ma famille, où il était reçu presque journellement pendant ses séjours à Paris, et en particulier avec feu mon frère, M. Benjamin Delessert. C'est mon frère qui avait recueilli une grande partie des renseignemens qui ont aidé à faire la notice rédigée par M. Jomard, membre de l'Institut (encore vivant [1849]), qui avait aussi été lié avec Sir C. Blagden." A memoir of B. Delessert, who was a remarkable man in many ways, appeared in the *Journal des Débats*, 1850.

* Jomard refers to Blagden as having been "médecin en chef dans les armées," and Mr. R. H. Blagden Hale styles him "Physician to the Forces." If I mistake not these titles are applied at the present day, only to the senior medical officers of the army, who occupy the highest posts. So far as I can discover, Blagden's original position was what we should now indicate by saying that he was an army surgeon.

beginning of 1780," and held an appointment at or near Plymouth, which he resigned after filling it a short time, and with it his connexion with the army. Soon after this he became associated with Cavendish, who settled an annuity upon him. When this connexion began does not exactly appear. Mr. R. Brown, however, informs me that Blagden was in Plymouth up to 1782, and that a letter from him in his possession, of that date, shows that he had not then become Cavendish's assistant. This is a point of some importance, for Cavendish's experiments on the production of water from its elements were made in 1781, and cannot have been witnessed by Blagden. In June 1783, however, he visited Paris and gave an account of them to Lavoisier,* on the authority of Cavendish; and from a reference in his letter to Crell, published in 1789, it appears that he was acquainted with Cavendish's views in the spring of 1783, so that he must have become his assistant either in 1782, or early in 1783. He continued with Cavendish for several years, but they ultimately parted, at what period I do not know, but probably not before 1789. Among the papers entrusted to me by Lord Burlington, I find three letters of Blagden's to Cavendish, dated Dover, 1787, and couched in very friendly terms, implying no cessation of good feeling up to that period. These letters refer to the conferences of the English and French commissioners in connexion with the trigonometrical surveys of the two kingdoms. Blagden accompanied General Roy, and Cassini and Legendre are alluded to as the most prominent parties on the French side. From the tone of these letters I gather that Cavendish and Blagden did not part before 1787. In one of them, for example (September 23rd, 1787), Blagden writes, "Be so good as to open and read, or get read, any letters that you think may contain news." The letters here referred to must have been in German or some other language unknown to Cavendish, and it should seem that they were addressed to Blagden at Cavendish's residence, so that they were certain to c me under his notice. Among the Cavendish MSS. also, is a parcel of papers entitled "Journeys," referred to in the preceding chapter, one

* *Phil. Trans.* 1784, p. 135; Crell's *Chemische Annalen,* 1786, pp. 58-61; *Mémoires de l'Académie des Sciences pour* 1781, p. 472; *Watt. Corr.*, pp. 39, 71, 129, and 176.

portion of which is a journal in Cavendish's handwriting, entitled "Dr. Bl. journey, 1789," and occupied solely with an account of the geology of the districts passed through, in a six weeks' tour through Flanders and a part of Germany. Blagden appears to have furnished a report of the geology of the territory in question, accompanied by specimens of the characteristic minerals, of which a list is given by Cavendish. I gather from this, that the journey referred to, was undertaken, in part at least, at Cavendish's desire, and that the formal parting which is understood to have happened between him and Blagden, did not occur till at least 1789.*

When or why Cavendish and Blagden parted, I have not discovered, but the cause of their difference, whatever it was, did not prevent the former from leaving his assistant a large legacy at his death. They were both reserved and undemonstrative persons, and neither has enlightened us as to the circumstances which brought them together or separated them. The only answer I have been able to obtain to queries on the latter point has been, that the union or co-partnership was found "not to suit."

On May 5th, 1784, Blagden was elected one of the Secretaries of the Royal Society of London, in room of Mr. Maty, who had resigned.† The date of his election is important, as affecting the question when he became responsible for errors committed during the printing of the *Philosophical Transactions*. Its occasion also was peculiar. The Royal Society, in 1784, was divided into two parties, the one assailing, the other defending, the mode in which Sir Joseph Banks had fulfilled the duties of President. When Maty, who was one of the President's assailants, resigned, the election of his successor became a trial of strength between the opposing parties. "Dr,

* Blagden accompanied Cavendish in some if not in all of the geological journeys in England referred to in the last chapter, and appears more than once to have acted as his geological commissioner on the continent. Under date 1788, I find among the Cavendish MSS. a letter from his friend Michell, in which Blagden's geological observations are referred to. In reply, Cavendish writes, "Dr. Bl. has sent me the miner [alogical] account of his journey as far as Paris," of which he proceeds to give an abstract, concluding with a more special reference to his assistant's recovery from an illness in the course of his journey, than he was wont to bestow on the personal affairs of his acquaintances.

† Weld's *History of the Royal Society*, vol. ii. p. 561.

Horsley and his party brought forward Dr. Hutton as a candidate, in opposition to Dr. Blagden, who was supported by the President and Council. Before the election, Sir Joseph Banks circulated the subjoined card amongst the Fellows:—'In consequence of Mr. Maty's resignation of the Secretaryship, at the last meeting of the Royal Society, the President takes this method of acquainting you that, at his desire, *Dr. Blagden* has declared himself a candidate for that office. From Dr. Blagden's known abilities and habits of diligence, the President does not doubt but he will, if elected, fulfil the duties of the station with advantage to the Society. *Soho Square, March* 29, 1784.'"*

On the election-day Blagden had a hundred more votes than his rival, and his appointment by so large a majority, showed so unequivocally the strength of the President's party, that the dissensions which had troubled the Society almost entirely ceased. Blagden continued in office till November 30, 1797.† With Sir Joseph Banks he was on most intimate and friendly terms, having access to his house at all times, and spending much of his time there whilst in London. He had thus many opportunities of meeting the distinguished men of science from foreign countries, who received so general and cordial a welcome from Sir Joseph; and, till the commencement of the first French Revolution, he spent a considerable portion of each year on the Continent, visiting different parts of Germany, Italy, Switzerland, but especially France. On one of those occasions he resided for some time at the Bavarian Court, and acquired the friendship of Benjamin Thompson, better known as Count Rumford.

At the Peace of 1814 he resumed his visits to the Continent with the title of Knight, which had been given to him in 1792, in recognition of his services to science. "Every year," says M. Jomard, "he came to pass more than six months at Paris or Arcueil. None of his countrymen have done more justice to the labours and discoveries of the French, or have contributed more than he to the happy relations which have subsisted for six years (1814—1820) between the savans of the two countries."‡

His last visit to France was paid in 1819, in the autumn of

* Weld's *History of the Royal Society*, vol. ii. p. 165.

† *Op. Cit.*, p. 561.

‡ *Moniteur*, Sept. 22, 1820.

which he travelled to Paris, and took up his residence at Arcueil, in the neighbourhood, in the house of his friend Berthollet, where he died suddenly of apoplexy, on March 26th, 1820.*

The salient points of Sir Charles Blagden's character have been sufficiently indicated to us by those who knew him. He was not a man of genius, his writings display no originality, nor has he any place among discoverers in science. On the other hand, he appears to have preferred to occupy himself with the labours of even those he might have rivalled, and was further remarkable for the multitude of eminent men with whom he was intimate. The large circle of British men of science who attended Sir Joseph Banks' soirées, must have been more or less known to him, including all his contemporaries, the Fellows of the Royal Society. He had some acquaintance with Dr. Samuel Johnson and Sir Joshua Reynolds, and assisted Smeaton in writing his work on the Eddystone Lighthouse.† La Place, Cuvier, Berthollet, and Benjamin Delessert, he could call friends, and he had some acquaintance with Lavoisier, Denon, and Humboldt, as well as with other contemporary philosophers, whom he saw at the meetings of the Institute, or encountered in the Parisian scientific circles. ‡

According to Jomard, indeed, he preferred France to all other countries, and the fact of his having been on terms of so great intimacy with the French savans, is one of importance, for one of the accusations brought against Blagden is, that he wronged Lavoisier, and may have been misinformed or forgetful as to his researches. It seems well, therefore, to notice here, how frequently he visited Paris, and how welcome he was there, both before and after the occurrence of the controversy which has cast a shadow over his good name.

The friend or acquaintance of so many distinguished men

* Jomard speaks of Blagden as having exhibited none of the infirmities of age at 80, but as he was born in 1748 and died in 1820, he can only have been 72 years old at his death. The date of his death I take from the inscription on his tomb in Père la Chaise, of which M. F. Delessert has sent me a copy: "Ce monument est situé au Père la Chaise, au cimetière de l'est, 19me division, sur le bord du chemin."

† Mr. W. A. Cadell is my authority for the reference to Reynolds and Smeaton.

‡ This appears from the information supplied to me by Mr. Blagden Hale, Mr. Robert Brown, and M. F. Delessert. Jomard refers generally to the intimacy between Sir Charles Blagden and the French savans in the passage previously quoted, and I may add that Sir Charles left legacies to La Place, Berthollet, and the step-daughter of Cuvier.

must either have possessed some fascination of manner, or other personal grace, which made him acceptable to the majority of those he encountered, or his acquirements in science must have been such as to secure their esteem; if he did not (as one would be tempted to imagine from the very large circle of those known to him) possess alike the intrinsic merit and the external grace. So at least, it should seem, thought no less severe a critic than Dr. Samuel Johnson, whose judgment in 1780, Boswell thus reports, "Talking of Dr. Blagden's copiousness and precision of communication, Dr. Johnson said, 'Blagden, sir, is a delightful fellow.'"* Such, however, was not the general opinion. Mr. Robert Brown, who drew my attention to this passage, dissented from it as a description of Blagden's manner, which he thought formal; and Mr. Cadell's judgment is, that it was "stiff and cold." Even Jomard says of the object of his eulogium, "Sous des dehors calmes et quelquefois impassibles, il cachait un cœur bienveillant et généreux;" and in Mathias's *Pursuits of Literature*, a well known satirical poem of the last century, he figures as "prim Blagden."† There can thus have been no charm in Blagden's manner; and his reputation must have been in spite of, not in consequence of it. It rested mainly on his acquaintance with recorded facts in nearly all the physical sciences, and his diligence in keeping pace with the progress of discovery. Boswell refers to his "copiousness and precision," as being notorious. Sir Joseph Banks confidently recommended him to the Fellows of the Royal Society, as a man of known abilities. Mr. R. Brown spoke of him as "au courant du jour" in all branches of knowledge. Jomard uses the same phrase, on which he enlarges.‡ His papers in the *Philosophical Transactions* demonstrate this. His essay on the congelation of quicksilver in the volume for 1783, may be taken as an example of his extensive acquirements, and his skill in arranging and expounding facts.

He was a great economist of time, and very methodical and

* Croker's *Boswell's Johnson*, vol. iv. p. 362.

† Seventh edition (1798), p. 72. Mathias spares no one. The reference to Blagden is incidental, and was manifestly made only to justify a foot note, in which this caustic and often coarse writer attacks the Royal Society.

‡ "Personne n'était plus au courant que lui des voyages nouveaux, des recherches dans les sciences, des découvertes industrielles, des productions de toute espèce."—*Moniteur*, Sept. 22, 1820.

assiduous in all his pursuits. His habits were unvarying; so much so, that Jomard says he always took the same road from his residence (at Brompton, I believe) in London, to Sir Joseph Banks' house and to the Royal Society's apartments, although he had his choice of several ways, and the one he selected was the longest.

Thus far, there is no difficulty in ascertaining what Blagden's character was. He appears before us as a somewhat formal and ungenial person, more an object of respect than of love to those who knew him. Such has been the disposition of probably the greater number of natural philosophers; at all events, it has characterised some of the most illustrious among them, and I do not stop to apologize for Blagden, or to seek to lessen in any way the impression which the faithful sketch I have tried to give may produce.

In two respects his character has been commentedon to his disadvantage, and these only call for special consideration. He is accused of having been greedy of money; and it is at least insinuated, that this tempted him to wrong Watt and Lavoisier, and to bestow unjust praise on his benefactor Cavendish. The advocates of Watt, indeed, all enter a more or less decided protest against the impartiality of Blagden's testimony, on the ground of his having had a pecuniary interest in praising Cavendish. The following passage from the life of Cavendish by Lord Brougham, may serve as an illustration of the mode in which Blagden is referred to:—"Having formed a high opinion of Dr. (afterwards Sir Charles) Blagden's capacity for science, he [Cavendish] settled a considerable annuity on him, upon condition that he should give up his profession and devote himself to philosophy; with the former portion of which condition the Dr. complied, devoting himself to the hopeless pursuit of a larger income in the person of Lavoisier's widow, who preferred marrying Count Rumford. He [Cavendish] left Sir Charles a legacy of £15,000; which was generally understood to have fallen much short of his ample expectations."*

* *Lives of Men of Science* (first series), pp. 445, 446. Blagden had reason to congratulate himself on the refusal of his suit to Lavoisier's widow, if it is more than a piece of harmless gossip, which Lord Brougham reports. Count Rumford soon separated from Madame Lavoisier, and the marriage is understood to have been a very unhappy one. Blagden was very kind to Rumford's daughter, Sarah.

That Blagden amassed wealth, and did not throw it away, is certain. In this respect he resembled Cavendish, Watt, Lavoisier, Hooke, Newton, Wollaston, and Black, not to mention others. His habits were frugal and unostentatious, and he is understood to have speculated to profit in the French funds, and thus to have greatly increased his wealth. According to the judgment of his relations, however, "avarice, or a desire to grasp or acquire money by any but fair means, he was entirely free from," and the verdict of less partial witnesses is in keeping with this decision. Jomard, who had no inducement to commend Blagden's liberality, unless he was assured of it, and who might have left unnoticed the question of his wealth, and the use he made of it, enters on the subject freely, and notices it thus: "Sa modération extrême lui permettait d'augmenter de plus en plus ses revenus. Mais autant il était dans l'aisance, autant il était généreux, même magnifique dans ses libéralités. Pour acquitter un service qu'un écu aurait payé, il donnait deux pièces d'or; si l'on hésitait, il en ajoutait deux autres, craignant qu'on ne fût pas satisfait. A Arcueil, il ouvrait généreusement sa bourse aux pauvres; il coopérait à des actes de bienfaisance, à des établissemens d'utilité publique. On pourrait citer de lui encore plus de preuves de bonté de l'âme que de traits de singularité ou de bizarrerie."*

Such a statement as the one just quoted is surely sufficient to outweigh the traditions or suspicions which represent Blagden as having been an avaricious man. His will also points in the opposite direction. By it he provided liberally for his relatives, but he took care also to remember his scientific friends, whom many more wealthy philosophers have altogether forgotten when making the final disposition of their riches. Berthollet, the daughter of Madame Cuvier, and the daughter of Count Rumford, received each 1,000*l.*; Dr. Thomas Thomson, of Glasgow, 500*l.*; and La Place, 100*l.*, "to purchase a ring."†

On this point it seems needless to enlarge, nor will I stop

* *Moniteur*, 22nd September, 1820.

† The probate of the will was under 50,000*l.* Other parties besides those mentioned in the text received legacies. Berthollet and Rumford's daughter are understood to have received benefactions from Blagden during his life-time.

to enquire whether Blagden was dissatisfied with the sum left him by Cavendish, or thought to enrich himself by a wealthy marriage. It is no such rare thing to find a legatee discontented with the bequest left him by a very wealthy testator, or to see even philosophers anxious to wed rich wives, that the certainty of Blagden having done what is vaguely charged against him, would imply that he had acted dishonourably. Against any unfavourable interpretation which might be put upon these transactions, even if they were unquestionable, we have the positive testimony of those who knew Blagden, that he was a strictly conscientious man. Such, we may feel certain, was Sir Joseph Banks' estimate of his character, from the confidence he reposed in him, and such also, from Jomard's account, was the opinion entertained towards him by Berthollet and the other French savans who knew him best. His relatives freely bear witness to his integrity.

Mr. Robert Brown informs me, that although reserved in his manner, Blagden was an upright, honourable man. Dr. Thomas Thomson, of Glasgow, writes to me thus:—"Sir Charles Blagden, you are aware, was assistant and operator to Mr. Cavendish, in his experimental investigations. I knew him well, and considered him as a man of perfect honour and integrity. I have the same opinion of Mr. Watt." . . . "Both Watt and Cavendish acted fairly and honourably." . . . . "The attack upon the integrity of Blagden . . . . I think unfair. Blagden had no motive that I can conceive for acting the part assigned him."* I have already quoted the letter from De Luc to Watt, in which he acknowledges that Blagden's character was such as to render it most unlikely that he had acted unfairly between Watt and Cavendish. Against such testimony as that adduced, suspicions can avail nothing. In truth, the impeachment of Blagden belongs almost entirely to the revival of the Water Controversy, for during his lifetime

* Dr. Thomson has also favoured me with his estimate of the share which the English rivals had in making the disputed discovery. In conformity, however, with the principle I have adopted throughout this volume of discussing the Water Controversy as a question of evidence, not of authority, I have not quoted Dr. Thomson's opinion on the relative merit of Cavendish and Watt, great as his title to offer a judgment is. I have given, however, in full, his opinion concerning the integrity of the disputants.

he corresponded with Watt, De Luc, La Place, Berthollet, and others, who took an active share in the earlier transactions of the dispute, without his good name being ever called in question.

Thus much settled, I have now to consider the specific charges that are brought against Blagden. The *first* accusation is, that he was guilty of some crime, or fault, in writing the interpolations in Cavendish's paper. The advocates of Watt do not pointedly accuse Blagden of having acted unfairly in penning those passages, but they speak of this transaction as if it implied officious intermeddling on his part, and had been done to injure Watt. There is a further charge against Blagden, that in his official capacity as Secretary of the Royal Society, he permitted the interpolations to appear as if they had been portions of the original MS. The last charge is quite just, so far as the fact is concerned, but not as implying any unfair dealing on the part of Blagden.

It was the practice of the Royal Society in 1784, and long before and after, to permit the contributors to its Transactions, to alter their communications between the period of their being read and printed. Into the proof of this I have entered at length in the section of the Water Controversy, entitled *Interpolations.* It will suffice, therefore, here to point out, in illustration of this, the one sufficient and important fact which the advocates of Watt have not perceived, or at all events have not acknowledged, viz., that Watt's paper contains as many undated and unmarked interpolations as Cavendish's, and that Blagden, who had the superintending of the printing of Watt's paper in 1784, no more insisted on the interpolations in it being enclosed in brackets, or otherwise indicated, than he did on those in Cavendish's paper. There was nothing unfair in the interpolations made by Cavendish, nor did Watt suffer by their being printed continuously in the text of his rival's paper, as if originally a part of it. The importance of the added passages depended entirely on their truth, and was not in any respect affected by the part of the paper in which they appeared. They were either true or false, and if true, they might have been added as postscripts, or printed as notes, with dates attached to them, had such been the practice of the Royal Society, without the claims of Watt and Cavendish being in the slightest degree altered by the procedure. Mr. Muirhead, in the partial application of an invidious rule, has marked by brackets the interpolations in Cavendish's paper, but not those in Watt's (both of which are reprinted in the sequel to the *Watt Correspondence*), and an impression is thus conveyed, that Cavendish or Blagden, or both, had done something wrong in making or suffering interpolations. If Mr. Muirhead had supplied the brackets, so as to mark the added passages in Watt's paper, it would be seen to contain as many interpolations and anachronisms as his rival's paper does. The title, for example, and two at least of the notes, were not added till the spring or summer of 1784, yet they are printed as part of the original text of a paper dated November 1783. Let it be observed, however, that Watt had not been guilty any more than Cavendish of wrong-doing in thus altering or supplementing his paper. The interpolations in both cases were innocent and laudable, and were made with the entire sanction of the officials of the Royal Society. If this be the case, however, it must be a matter of exceedingly trifling moment, in whose handwriting two truthful declarations were engrossed. Only those who judge Blagden by the light cast by the unfounded suspicions entertained by De Luc and Watt against him and Cavendish, can regard the question of handwriting as one of the slightest importance. Since, however, it has been urged, it may be noticed that it is an argument in favour of both Cavendish and Blagden, that the interpolations were written by the latter, for they are very short and might have been rapidly written by Cavendish himself, had he intended to pass them off as parts of the original paper, whereas they appear in the handwriting of his secretary, and on separate sheets of a smaller size than those containing the text of the essay, and of a different quality of paper.* In the second interpolation, likewise, Cavendish reveals by the wording, that the passage had been added after Watt's paper had been read, and was not, therefore, a part of his original text. That Blagden urged Cavendish to make those interpolations, there is no proof. But even if there were, it would imply nothing discreditable to the former. When Watt's zealous friend, De Luc, was doing so

* Weld's *History of the Royal Society*, vol. ii. p. 173.

much for him, it was nothing less than a duty that Cavendish's friend Blagden, should guard his reputation. So long, indeed, as De Luc and Blagden were careful to publish only truths, it could not be matter of accusation against either that he did publish them in the defence of his friend.

The second charge against Blagden is a more serious one, and affects the errors in date in the papers of Cavendish and of Watt, printed in 1784. The first of these was read on January 15th of that year, as appears by its title in the *Philosophical Transactions*.* But in certain copies of the paper which were printed separately, the year was marked 1783 instead of 1784. How, or when this alteration of figure was made, and who was responsible for it, is not quite certain. The blame, however, if blame there be, lies as much with Cavendish as with Blagden, for it was not part of the latter's official duty to superintend the printing of *private* copies of the papers which appeared in the *Philosophical Transactions*. From a reference, however, in *Crell's Annalen* (1785, part iv. p. 324), to Cavendish's research as contained in "*Experiments on Air*, London, by J. Nichols, 1784, 4 Mai. p. 37," which had been sent to the editor by Sir Joseph Banks, it appears in the highest degree probable, if not certain, that the private copies were paged separately from 1 to 37, whilst in the *Philosophical Transactions* for 1784 the numbers run from 119 to 153.† It seems, further, likely that during the alteration of the paging the error was detected and not allowed to appear in the *Transactions*; or if they were printed first, the error may have been committed during the substitution of the one set of figures for the other. In whatever way, and by whomsoever committed, few impartial readers will doubt that the error was accidental, for there was absolutely nothing to be gained by it, inasmuch as, *firstly*, the separate copies of a paper reprinted from the *Transactions* of a Society, have no authority, unless in so far as they exactly agree with the text

* "Cavendish's paper is printed as having been read on January 15, 1784. It was commenced at the meeting of that date, but the reading occupied the time of two evenings."—Weld's *History of the Royal Society*, vol. ii. p. 173.

† I have tried in vain to procure access to one of the detached copies of the *Experiments on Air*. It is not a little singular that Crell should refer to his copy as marked 1784, not 1783, as if there were no error of date on it. Should any of my readers encounter a copy, he would do a service by printing a description of the title-page and dates, along with the numbers on the pages.

of the Transactions; and *secondly*, the substitution of 1783 for 1784 only carried back the entire question of the claims of Watt, Cavendish, and Lavoisier, by a year, without, in the slightest degree, altering their *relative* position as claimants of the disputed discovery. The whole proceedings were antedated by a year, but everything else remained as it was.

But (which is most decisive of all), Cavendish, as soon as he became aware of the error, wrote to Mongez, the editor of the *Journal de Physique à Paris*,* requesting him to correct it, and in so doing not only acknowledged, and made reparation for the accidental error which had occurred, but by apologizing in his own name, and not in that of Blagden, for its existence, took upon himself the responsibility of having allowed it to appear.

For the error in Watt's paper, Blagden certainly was responsible, and the advocates of Watt have been unsparing in their denunciations of the secretary, whom they do not exactly accuse of having wilfully committed the blunder, but of something as nearly approaching a wilful error as can very well be. Watt's paper, as it was ultimately printed, assumed the shape of a letter to De Luc, dated November 26th, 1783, but through an error of the press, 1784 was printed instead of 1783, so that Watt had the disadvantage of having his letter post-dated by a whole year. That the error was accidental, the paper bore upon its face, for the letter was stated to have been "*Read April* 29, 1784," which it manifestly could not have been, if not so much as written till the succeeding November; yet, as it appeared in the *Philosophical Transactions* for 1784, it was impossible that the mistake could apply to the time of reading, which might otherwise have been supposed to have been 1785, and the conclusion could not have been avoided by one who observed the incongruity of the two dates, that the error applied to the time when the letter was written, not to that when it was read. At all events, the irreconcileable character of the numbers could not fail to strike any one who consulted the paper with a view to determine questions connected with the chronology of the disputed discovery. And if such a perplexed reader did what was imperative on him, before condemning the editor of the *Transactions*, but which the advocates of Watt seem never to have done, viz.,

* British Association Report, 1839, p. 65.

consulted the errata, which, in those days of careless editing, had always a prominent place on the concluding leaf of each volume of the *Transactions,* he would have found that the much maligned Blagden had pointed out that November 1784 should have been 1783. I will not demur to Blagden's carelessness in permitting so unfortunate a blunder to occur, being not only lamented, but even reprehended. The advocates of Watt, however, represent him as having taken the first opportunity which his secretaryship afforded, of interfering with the printing of the *Transactions* to permit, or to commit an error most unfavourable to Watt, whereas the recency of his appointment to the office of editor of the *Philosophical Transactions* should surely be regarded as entitling him to a lenient and charitable judgment, when he is found to have suffered an error of the press to occur whilst discharging duties with which he was not yet familiar. And as this has not been acknowledged as in candour it should have been, I must point out a little more fully how excusable Blagden's carelessness is. *Firstly,* then, there are fewer errors in the volume of *Philosophical Transactions* for 1784 than in many that preceded it, so that Blagden cannot be accused of having neglected his editorial duties according to the standard of editorship of his day. *Secondly,* he offered to send Watt the proofs of his paper to Birmingham if he wished it, which one proposing to wrong Watt would certainly not have done. *Thirdly,* the letters of Blagden to Watt contained in the *Watt Correspondence,* show a solicitude to do the great engineer justice, and to publish his paper in the way which he preferred, altogether at variance with the notion that from a venal or envious motive, Blagden designed to do him wrong. *Fourthly,* Blagden informed Watt that he might obtain separate copies of his paper when he pleased, so that he exposed himself to the risk of being discovered to have committed a fraud before the volume of the *Transactions* was published, not to refer to the certainty of the detection after it had appeared. *Fifthly,* the MS. from which Blagden had to print Watt's paper, consisted primarily of two documents, of one of which, parts only were to be selected for publication by the secretary, and besides, of a title, and of certain notes and alterations which were sent by Watt in letters to Sir Joseph Banks and to Blagden. It is not very surprising, therefore, that in the construction of a single methodical paper out of several letters, an error should have occurred in copying the date of one of them. Watt himself confounded the date of his letter to Priestley with that of his letter to De Luc;* and Mr. Muirhead, who puts the worst interpretation on Blagden's permission of a typographical error, contrived to commit one as suspicious (*i. e.* as innocent) as Blagden's, in copying from the plain print of a part of Watt's paper.† I have no fear of the judgment which will be passed upon Blagden by those who set aside the suspicions of De Luc and Watt, and consult the errata of the *Philosophical Transactions* for 1784.

When thus it has been shown that, according to the testimony of his contemporaries and his survivors who knew him best, Blagden was an upright, honourable man; that in the matter of the interpolations he acted only as clerk, and was a party to no wrong; that the first error in date was a harmless one, confined to a few unauthoritative copies of Cavendish's paper, and acknowledged and corrected by Cavendish as soon as it was discovered; and that the second typographical error was a self-evident one, made in very excusable circumstances, and corrected simultaneously with its publication to the world: I think that I am fully entitled to repeat, that the advocates of Watt have said greatly too much in disparagement of Blagden's good name, and that they ought in justice to retract all that they have said, or to impeach him boldly, and produce proofs of his villany which as yet have not been shown.

It remains, so far as this subject is concerned, that the cruel and gratuitous suspicion that Cavendish, if he did not bribe Blagden to lie and cheat for him, at least put his dependent in such a position that he saw he could only earn his bread by committing fraud in behalf of his master, should for a moment be noticed I would not willingly put in the mouths of the advocates of Watt a darker suspicion than they actually entertain, but in spite of all their cautious wording, and reservations, and qualifications, they prefer a charge amounting in its most naked

* *Watt Corr.*, p. 70.

† *Watt. Corr.* Reprint of Watt's paper, p. 78, where "meeting" occurs instead of "reading." (Note—7th line from bottom.)

shape to this, that (1) Blagden was dishonest for the sake of Cavendish; and (2) that Cavendish paid Blagden money. That the money was the *cause* of the dishonesty is further implied where it is not affirmed; and the only point which is not absolutely pressed, is, that Cavendish *desired* that dishonesty should be practised, or *intended* to reward it. If this view of the fair-dealing of Blagden be true, it should be shown by the advocates of Watt, that Cavendish made Blagden's salary dependent on his services; whereas it was secured to him in the form of an annuity,* as was but just, when Blagden abandoned his own profession to associate himself with Cavendish. He had thus nothing to gain by becoming a party to any fraud which might serve his principal. It is for the advocates of Watt, moreover, to make out a consistent theory of Blagden's dealings on their own hypothesis of his character, so as to reconcile it with the fact implied in the accusation, that the party accused, as all acknowledge, was a very shrewd person, and, according to their view also, a very selfish one, and nevertheless only succeeded, if the charges are true, in involving himself and his principal in needless and unprofitable suspicion by sanctioning two errors in date which no clever rogue would have committed. I shall presently return to the consideration of the proceedings of Cavendish and Blagden, as looked at from an impartial point of view; for as yet, I have considered them solely by the light of those suspicions of De Luc and Watt, which, according to the friends of the latter, supply the only just criterion by which to test the proceedings of the English rivals. Before doing this, however, it is necessary to consider the part which Blagden took in reference to Lavoisier's claims, as his defence of Cavendish's rights against them led to a reference to the demands of Watt as late as 1786, in which the advocates of the latter find fresh grounds of complaint against Blagden. And as this cannot be done without special reference to Lavoisier's demands, I shall dispose of them in the course of the discussion.

In June 1783, Blagden during one of his visits to Paris, gave *some* account of the researches of Cavendish and Watt into

* The annuity amounted to 500*l*. (*History of the Royal Society*, vol. ii. p. 173.)

the production of water from its elements. At a later period, as all acknowledge, Lavoisier, for the first time in his experience, observed that the combustion of a given weight of hydrogen and oxygen yielded a nearly equal weight of water, and announced the result to the French Academy, in June 1783, whilst Watt's letter was still in abeyance, and before Cavendish had made his observations known, either directly or through the medium of Priestley's announcement of the repetition of them. When Lavoisier ultimately published his account of these experiments, he stated that they were first performed on 24th June, 1783, in the presence of Blagden and others, and that the result was reported to the Academy on the following day (June 25th). In the first of these references he acknowledges that Cavendish had previously burned hydrogen in close vessels, and that he had obtained "a very sensible quantity of water," but Lavoisier professed to have been the first to observe that the weight of water produced was equal to the weight of gases burned, and that water was not a simple body, but consisted of hydrogen and oxygen.*

The accuracy of Lavoisier's statement was called in question, so soon as it became known in England, and as Blagden was acknowledged to have been Lavoisier's informant, it came to be a question affecting the veracity of the one or the other, including on the side of Blagden, Cavendish, whom he represented. Blagden, without hesitation, impeached Lavoisier's veracity, and implicitly accused him of plagiarism, and this in three different ways. As early as December 1783, when his recollection of what he had told Lavoisier must have been unimpaired, he authorised Kirwan to tell Watt that he had explained the English theories "minutely" to Lavoisier in the preceding summer.† In 1784, Cavendish stated in the concluding part of the first interpolation in his paper, that a friend of his (whom no one doubts was Dr. Blagden) gave an account of his experiments and conclusions to Lavoisier in the summer of 1783, and that he was with difficulty persuaded to believe that the gases burned together could be converted into their joint weight

* *Mémoires de l'Académie des Sciences*, 1781 (printed in 1784), pp. 472—475; *Watt. Corr.*, pp. 176—180. A detailed analysis of Lavoisier's experiments is given in a section of the Water Controversy devoted to their consideration.

† *Watt. Corr.*, p. 39.

of water, and only adopted this conclusion when he had obtained the same result in his repetition.* It may also be noticed here, that Cavendish's so-called third interpolation, or rather postscript, which is in his own handwriting, was a temperate and altogether unpolemical criticism of Lavoisier's anti-phlogiston doctrines, which will be found fully noticed in the abstract of the *Experiments on Air*. This postscript has not given occasion to any discussion, and therefore is not further referred to here.

Finally, in 1786, Blagden wrote a long letter to Crell, who had published several inaccurate accounts of the circumstances attending the discovery of the composition of water, which he attributed to Lavoisier, whose views Cavendish was stated only to have confirmed, whilst Watt's name was not mentioned. In this letter, Blagden, in his own name, accused Lavoisier of having concealed the facts that his experiments were made in consequence of what Blagden had told him, that the weight of water produced equalled the weight of the gases burned, and that Cavendish and Watt had founded upon those researches a conclusion identical with Lavoisier's, except that he did not employ the word *phlogiston* as a name for the combustible element of water. Without entering here into any investigation into the merits of Lavoisier, to which I have sought to do justice in a special section of the Water Controversy, it may be noticed that he never replied to the accusations which Cavendish, Watt, and Blagden preferred against him, and that none of his contemporaries or successors, even among his own countrymen, have been able to defend him from the charge of plagiarism.* Nor did Blagden's denial of Lavoisier's fair-dealing lead to any cessation of intercourse between the former and the French philosophers, who would certainly have resented the charges brought against Lavoisier, had they been untrue. The part of Blagden's letter, however, which mostly concerns us is, that in

* *Phil. Trans.* 1784, p. 134.

† Lavoisier's account of his experiments is at variance with the contemporaneous account of them given by his colleague La Place, written a few days after they were made, in a letter to De Luc. (*Watt Corr.*, p. 41.) La Place there asserts that up to the 28th June, 1783, that is three days after Lavoisier's announcement, they did not know whether the quantity of water produced represented the quantity of gas consumed. Berthollet also, in a letter to Blagden, of which I have published the greater portion in the sequel, imputes the discovery of the composition of water to Cavendish.

which he states that Watt's conclusions became known to him "about the same time" (the spring of 1783) as those which Cavendish had drawn from his own experiments.

The advocates of Watt assert that Blagden must have known whether Cavendish or Watt first formed his theory, and that he would have stated that Cavendish was the first, if he could in honesty have done so. Without entering into an elaborate defence of Blagden, which will be found elsewhere in the volume, I may notice here, that according to the interpretation which Watt's advocates themselves put upon an ambiguous letter of Kirwan's, Blagden first learned Watt's theory from Cavendish.* Whether this be the true interpretation or not, it is certain that Blagden could not have been ignorant of Cavendish's researches after the spring of 1783, for the first public account of them, that, namely, in Priestley's paper, accompanied Watt's letter to the Royal Society. On the other hand, Blagden did not become Cavendish's assistant till 1782, and could not, therefore, be cognizant of his experiments of 1781, till long after they had been made, and Priestley is the only person to whom Cavendish professes to have explained them before the spring of 1783. It is of much more importance, however, to notice that the object of Blagden's letter to Crell, of 1786, was not to discuss the claims to priority between Watt and Cavendish, but those of Cavendish against Lavoisier. The last-named philosopher had in effect called in question the veracity both of Blagden and of Cavendish, and Blagden only was in a condition to disprove Lavoisier's statement, and to remind him of what he had told him. There was a peculiar necessity, also, for addressing a letter to Crell, because, through imperfect information, he had, in two different numbers of his journal, imputed to Lavoisier what even he did not claim.† There was no occasion, however, for Blagden to discuss Watt's claims in Crell's journal, for they had not been noticed therein. Nor could Blagden have spoken on his own authority in reference to them, as he, and only he, could do in regard to the statements of Lavoisier. An English journal was the fitting place in which to canvass

* *Watt. Corr.*, p. 39.

† See in illustration of this the quotations from Crell's Annalen, which are published in section 11 of the Water Controversy contained in the sequel.

the claims of the English rivals, and Cavendish, Priestley, and Watt, were the only parties competent to discuss it.

I feel no difficulty, accordingly, in agreeing with the advocates of Watt in their belief that Blagden's silence in reference to the relative priority of Cavendish and Watt, was deliberate. But I entirely differ from them as to the motive which led to his silence. It is as unimportant as it is impossible, to determine whether Blagden knew by what period of time the one of the English rivals had preceded the other. We are apt to forget that the *Watt Correspondence*, which we now see in print, put forth by Watt's descendants as a public claim, in his name, of priority over Cavendish, was, during the lifetime of the rivals, a bundle of private letters. Ostensibly there had been no dispute between Cavendish and Watt, even in 1784, and each had, in his own name, published all that he deemed requisite to justify his claims. In 1786 there was nothing to revive the dispute; on the other hand, Cavendish and Watt had shaken hands at one of Sir Joseph Banks' soirées, and we shall find in the next chapter, that in the year when Blagden wrote to Crell, Cavendish, in one of his Geological tours, found his way to Birmingham, and took unusual interest in studying the great engineer's beautiful inventions. We may feel certain, therefore, that however willing Cavendish might be to defend his veracity against Lavoisier's implied denial of it, that with his characteristic indifference to fame, he would, if consulted in the matter, forbid Blagden to enter upon the question between himself and Watt. Nor could Blagden have anything to gain by renewing the strife between the English rivals, or by affirming, in the name of Cavendish, in a German journal, what Cavendish could so much better say for himself at a meeting of the Royal Society, where Watt himself, with their respective friends, could meet face to face. I come to the conclusion, therefore, that no reproach attaches to Blagden for avoiding the discussion of the question of relative priority as between Cavendish and Watt, and I will add that he deserves praise for his bold impeachment of so influential a person as Lavoisier, and for hazarding the loss of the friendship of the French philosophers, by his open vindication of the priority of Cavendish.

In bringing to a close this long discussion of the parts taken by the rivals in the Water Controversy, and their friends, I would remark that, with the exception of Lavoisier, they ought all to be acquitted from the charge of having acted, or sought to act with deliberate injustice, in defending their own or their friends' claims. Priestley, in particular, had it in his power to have appropriated to himself a large part of the merit which belongs to Cavendish or to Watt, or to both; nor would it have been easy for those whose ideas he borrowed to have identified and recovered their property. Although, accordingly, no man, deserves to be praised for being simply honest, yet it is but just to Priestley to set off against whatever is obscure or inexplicable in the part which he took in the Water Controversy, the certainty that his good name is if possible heightened by his behaviour in it. On Cavendish's head no blame rests. No explanation was asked by Watt at his hands; no just acknowledgment of even his experiments was made by his rival; yet he did justice to Watt, so soon as the public reading of his paper allowed him to comment upon his views, and he did not resent the imperfect account of his own. Blagden, also, stands exculpated from any heavier charge than that of excusable carelessness in correcting a printer's proof on one occasion. Nor did the part he took in vindicating Cavendish's claims, in any way exceed what every honourable man would consider himself justified in doing, to defend the good name and the reputation of a calumniated friend, as well as to assert his own veracity. De Luc stands acquitted of any design to wrong Cavendish, or of having acted otherwise than according to his conscientious convictions of what was the truth. But he was guilty of most blameable haste in coming to a conclusion concerning the conduct of Cavendish, and of not less blameable neglect in not obtaining the information without which he was not entitled to pronounce a judgment. Watt is open to the reproach of having listened too readily to the suspicions of De Luc, and of not having taken the means which were accessible to him for ascertaining what Cavendish had done before him, although he published a decision on Cavendish's merits. This charge has not hitherto been preferred against Watt. He has had the good fortune to have had a much greater number of able and earnest advocates than Cavendish has enjoyed. These zealous defenders have, with great success, conveyed the impression that the first wrong was committed by Cavendish, and that Watt only acted on the defensive; whilst Blagden officiously assisted, if he did not perhaps precede Cavendish, in wronging Watt, to whom De Luc rendered only such services as friendship imperatively demanded. Against this view of matters I protest, as a complete inversion of the true position of the parties referred to.

The beginner of the Controversy was De Luc, whom Watt joined, and they compelled Cavendish and Blagden to assume the defensive, in vindication both of their intellectual and moral reputation. Cavendish did Watt no wrong in omitting all reference to his name in the first draft of his paper, for the private letter of the latter had been withdrawn by himself, and was not subject to public comment by any one. When De Luc accordingly found no reference to Watt in the MS. of Cavendish's paper, which was freely and courteously sent to him by its author, he should in the first place have repaid the courtesy by communicating with Cavendish, and ascertaining what view he entertained of Watt's opinions. For anything De Luc knew, or was entitled to assume, he might have found that when it was explained, that Watt had never lost faith in that part of his letter which referred to the production of water from inflammable air and oxygen, and intended to publish that to the world, as an opinion he had held since 1783, Cávendish was quite willing to acknowledge the independence of Watt's conclusions, and their similarity to his own. At the same time De Luc might have learned that Cavendish had first made the experiments which Priestley repeated in 1783, and that in repeating them charcoal gas had been substituted for hydrogen; and Cavendish might further have informed him that he was the first, as Priestley could testify, who turned or converted a given weight of hydrogen and oxygen into the same weight of water. Had De Luc known this, he would certainly have acted otherwise than he did, and so, assuredly, would Watt also. Had De Luc and Watt, indeed, only known the one fact, that Cavendish had, in 1781, discovered the equality of weight between the gases burned and the liquid produced, and that this liquid was pure water, they must have judged him in a very different spirit, and confessed that one-half, at least, of the merit of discovering the composition of water belonged to him.

De Luc's neglect of all this prevented the possibility of an amicable understanding between Cavendish and Watt, so that even to this day, we are in ignorance as to what their true feelings were towards each other in 1784. Further, De Luc's two grounds of suspicion, viz., the similarity of language between the statements of Cavendish and of Watt, and the certainty of Cavendish having read Watt's letter of 1783, would have lost one half of their weight, even to a prejudiced mind, had its possessor been aware that the supposed plagiarist had long preceded him whom he was alleged to have pillaged, and had announced a theory in words which were entirely his own. In total and wilful ignorance of all that Cavendish had done, De Luc did not even proceed to verify his suspicions, but acted upon them as if they had been demonstrated truths. Equally negligent was Watt, who has written his self-condemnation in the pencil note attached in 1784 to the MS. of his paper, which even the most zealous of his advocates confesses to be strangely inaccurate.

A heavy retribution awaited De Luc for his hasty and ill-judged interference. He was accused himself in later life of shameful plagiarism from Black, and had some difficulty in satisfying even Watt that the charge was unjust.* He was certainly innocent, but he may have looked back with some remorse and with some sympathy, when he tasted in his own person the fruits of hasty and inaccurate investigations, like those by which he had calumniated Cavendish. His punishment certainly was not greater than his offence, for the evil effects of his mischief-making have increased instead of diminished by the process of time; and this leads me to the consideration of the revival of the Water Controversy by Arago, on which a few words must be said.

This Controversy will, I believe, be looked back to in later times as exhibiting one of the most remarkable examples of a myth unconsciously fashioned out of a legendary tradition, and that not by poets, but by men of science. The advocates of Watt, are not, I suspect, aware to what extent they have been assuming as evidence points which never ranked higher than suspicions. The revival of the Water Controversy was merely

* *Edin. Rev.* 1803, p. 21, and 1805, Appendix, pp. 502—515.

the revival of the ignorant surmises of De Luc; and it happened thus:—Watt retained copies of the letters which he wrote in reference to his theory of the composition of water, and preserved those relating to this and kindred topics, received from his friends. These letters form the *Watt Correspondence*, which has been so ably edited by Mr. Muirhead. After Watt's death, these letters passed into the hands of his son, the late Mr. James Watt, junior, who naturally set a high value upon them, and as naturally adopted explicitly the opinions of his father, which he found expressed in them. With commendable filial piety also, he showed those letters to various men of science, and sought to induce them to adopt and defend the views which they contained. In this he was not very successful till Arago visited this country in 1834, to collect materials for his *Eloge of Watt*. He was already satisfied of Watt's right of priority, and the perusal of these letters which were shown to him "put," says Mr. James Watt, "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."* The *Eloge* was published in 1838, and the *Correspondence* on which so much of it was based was published, as we have already seen, in 1846, so that all have now the means of comparing Arago's conclusions with the materials which led him to form them. I do the distinguished Secretary of the French Academy no injustice, when I say that he has committed the same fault as De Luc did, whose unjust surmises he has revived but not justified. This is a grave charge to bring against so great a philosopher as Arago, and I feel peculiar reluctance in urging it at a time when all lovers of science lament that a grievous infirmity has made him a sufferer, and abridged his means of advancing the branches of knowledge which he has done so much to extend. But these feelings I must set aside, content to be blamed if I can only show the justice of my charges. I must then say that Arago has not investigated the claims of Watt against Cavendish, with the care which should have been bestowed by one who was about to

* *Watt Corr.*, pp. x, xi.

add the weight of so deservedly illustrious a name to the support of Watt's demands, and who, before the assembled philosophers of France, proceeded to call in question the integrity, as well as the science, of Cavendish. I find in the *Eloge* of Watt many errors, which for ten years have contributed to spread over the civilized world a false view of the characters of Cavendish, Priestley, Blagden, and in truth, of the whole Royal Society. I mention some of these, although the task is an invidious one: but it cannot be avoided by one who would do justice to Cavendish. I make in the first place, this general charge against Arago, that he did not study the papers of either Cavendish or Priestley, although without a knowledge of them, it is impossible to judge impartially between the rivals in the Water Controversy. In consequence of this, he gives the following singularly inaccurate account of matters : "Priestley records in detail, and as his own, experiments which prove, that the water produced by the combustion of a mixture of hydrogen and oxygen, has a weight exactly equal to that of the two gases which are burned. Cavendish, some time after, claims this result for himself, and insinuates that he had communicated it verbally to the Chemist of Birmingham."*

Had Arago consulted Priestley's paper, he would have found that it did not describe a single experiment on the production of water from *hydrogen* and oxygen; and that the only experiments that it does record, were made with the inflammable air from charcoal (a mixture of various gases) and dephlogisticated air. And if he had consulted Cavendish's paper, he would have found that he did not insinuate, but broadly declared, that Priestley had repeated, but with a difference in the quality of the combustible gas, his experiments on the production of water; and that he referred to Priestley as having already publicly acknowledged both those points.

The conclusions of his own countryman, Lavoisier, Arago also mistakes, for he affirms that in 1784 the word hydrogen was applied to the combustible constituent of water, although it was plainly impossible that such a word should come into use before the composition of water was discovered; and it is certain that Lavoisier does not use it in his papers on water of

* *Watt Corr.*, p. 230. Mr. Muirhead's translation of Arago's Eloge.

that year, but speaks of hydrogen as "*air inflammable aqueux*," or "*principe inflammable aqueux*."

The eulogist of Watt has also throughout his work, taken for granted that the word *phlogiston*, which was applied by Watt as well as by Cavendish to the inflammable constituent of water, necessarily signified hydrogen, and in so doing, has been guilty of a most manifest *petitio principii*, as other advocates of Watt, and especially Lord Jeffrey, the ablest of them all, acknowledge. I hesitate to say more, but these are not the only errors. Macquer is represented as having in 1776, proved by *analysis*, that pure water accompanied the combustion of hydrogen, whereas this chemist made no analysis, but only surmised that the liquid was water. Cavendish is thus deprived of the merit which is unquestionably his. Warltire is further stated to have first "imagined that an electric spark could not pass through certain gaseous mixtures, without causing some change in them;" and yet Arago has himself shown in his *Eloge of Volta*, that he was the first to employ the electric spark to detonate explosive gases; and Warltire professes only to have imitated Priestley, who, in his turn, avowedly borrowed from Volta! Again, Cavendish is represented as having observed no more in 1783, than that water may be obtained by exploding a mixture of oxygen with hydrogen; whereas Priestley testifies to Cavendish having then discovered that the weight of water equalled that of the gases burned. Watt's letter to Priestley of 1783 is referred to by Arago as having been "preserved in the Archives of the Royal Society of London," whereas it had to be demanded from De Luc by Sir Joseph Banks, when it was publicly read in 1784. Further, great importance is attached to Watt's letter having remained "in the Archives of the Society;" and Cavendish is accused of having ultimately referred to this letter as one with which he had only become acquainted when it was read before the Royal Society; whereas Cavendish says nothing whatever as to when he first became acquainted with the letter in question, but simply concerns himself with certain of its contents as having been formally made public. Arago also omits all allusion to the cause of Watt's letter of 1783 not having been read till 1784, declaring that circumstances, which he suppresses, "because they do not affect the present enquiry, delayed this reading for a year." Yet, surely, it was a point of the greatest importance to the enquiry, that Arago's readers should know that it was Watt himself who withheld the letter from being read; and this because he had lost faith in certain of the conclusions announced in it. Cavendish, moreover, whose case is so summarily disposed of, is styled "a pretender" to the disputed discovery. "Numerous errors" of the press are referred to as having occurred in the printing of the *Philosophical Transactions* for 1784, not one of which "was favourable to Watt!" This reference conveys a very exaggerated impression of the wrong dates, for there was only one error in the *Transactions,* and that was corrected in the erratum. Arago's apology, moreover, for Cavendish's alleged treatment of Watt, is even more painful to read than his accusation. "On the subject of discoveries," he says, "the strictest justice is all that can be expected from a rival or a competitor, however high his reputation may already be." This is a mournful announcement, if it expresses what the secretary of the largest scientific body in the world believes to be the temper entertained by discoverers in science towards each other; and a woful lesson to be taught by such an authority to youthful students, who are excused from being generous, provided they are barely just to their rivals.

In 1840, after Mr. Harcourt had pointed out certain of those errors, Arago sought to vindicate himself before the French Academy; and in reply to the fact adduced in contradiction of certain of his assertions, that Priestley had retracted his original statement, that the inflammable air from charcoal, when burned with oxygen, yielded nothing but pure water, demanded whether he was called upon to study memoirs of 1786 and 1788, in reference to a discovery of which the latest date was 1784. Yet Arago had no objection to quote Blagden's letter to Crell, of 1786, in support of the claims of Watt, and could not, therefore, on the plea of date, refuse to consult Priestley's papers of that and later years, which, as we have already seen, have a most important bearing upon all the questions in dispute.

Arago sought also to justify his substitution of the word hydrogen, for phlogiston, in expounding Watt's views, by showing that he had made a similar substitution in expounding Cavendish's; as if this were not a *petitio principii* of the strangest kind, since it was matter of certainty that Cavendish's phlogiston was hydrogen, whereas it was matter of doubt and dispute what Watt's phlogiston was, and it should have been proved, not assumed to be hydrogen. The only proof, however, of this point, which Arago gave, was to assert that in 1784, *phlogiston*, inflammable air, and hydrogen, were all names applied to the combustible element of water. Yet the last of these terms had not yet come into use, and the second was applied to every combustible gas known at the time; the specific name for hydrogen being inflammable air *of* or *from the metals;* and phlogiston, a word which signified one thing in the mouth of Cavendish, with whom it primarily stood for hydrogen, and another in that of Watt, with whom it primarily stood for inflammable air from charcoal, or perhaps for combustible gas.

The mistakes I have pointed out have remained uncorrected by their author since 1838, and Mr. Muirhead reprints in 1846 Arago's judgment regarding the success of his own demonstration of Watt's priority, which is as follows: "the settlement of a question of priority, when it turns, as in the above instance, on *the most careful* examination of printed memoirs, and the most minute comparison of dates, assumes the character of *a very demonstration.*" *

Yet Arago's successors, not excepting Mr. Muirhead, who also asserts (the italics are his own) that the advocates of Cavendish "can point out no inaccuracies in *our statements of fact—our dates—our references—or*, we believe we might safely add, *our conclusions*,"† have explicitly or implicitly denied the accuracy of many of Arago's statements. Few things, indeed, are more strange in this strange controversy, than that some ten years after Arago had published his alleged complete demonstration, and subsequently to the publication of the *Watt Correspondence*, in which Mr. Muirhead offers a demonstration still more complete of Watt's priority, it should be conceded by Lord Jeffrey, in his review of the *Correspondence*, that it had not yet been demonstrated that Watt had *a claim at all.* In that article, his Lordship, for the first time, made out a logically consistent case for Watt, by endeavouring to show that he signified by phlogiston, hydrogen. ‡

* *Watt Corr.*, p. 233. † *Watt Corr.*, p. cxiii. ‡ *Edin. Rev.*, Jan. 1848.

This curious result of matters has chiefly been occasioned by nearly all the defenders of Watt looking at the question between him and Cavendish, through the distorting medium of De Luc's unjust suspicions. For this, Arago is chiefly to blame, as having revived these in his *Eloge* of Watt. How seldom foreigners, however accomplished, can interfere to advantage in contests between the rivals of another nation, whose works are written in a language imperfectly known to the strangers, is strikingly illustrated by the ill-success which has attended the interference in the Water Controversy of two philosophers so unusually gifted and accomplished as De Luc and Arago; and I will add, that the fact of neither having specially devoted himself to chemistry, has thrown another difficulty in the way of their doing justice to the question of which they constituted themselves judges. It has been affirmed, indeed, that the Water Controversy is a question more of evidence than of chemistry, but to say this is to assert a distinction without a difference, for it is a question chiefly of chemical evidence, and those who have laid down this canon for their own guidance and justification, have at once disobeyed it, and produced all the chemical evidence they could procure to defend their views. More than half the question, in truth, turns upon the *facts* in dispute, not upon the conclusions which they, if ascertained, would warrant, and the greater number of the facts are chemical.

That men should take different sides in a vexed question like the Water Controversy, cannot be matter of surprise. There is room for different views being held by equally honest and impartial observers. Mankind will never be at one in an adjudgment of merit between Cavendish, Watt, and Lavoisier, any more than they will be unanimous on other subjects which concern them a great deal more; nor shall I imitate those who offer to furnish a demonstration, which in the end proves to be a very one-sided decision. Where so much is doubtful, we must be content to doubt, and I am greatly more anxious to exonerate Cavendish from the charge of unfair dealing, than to insist upon my estimate of his share in the disputed discovery as the just one. I might be indifferent to Arago's imperfect acquaintance with the necessary documents, if it affected only Cavendish's intellectual reputation, but I cannot pass it uncondemned when it occasioned his honour to be called in question. Arago should have studied the papers of Cavendish and Priestley much more carefully than he did, before he insinuated the shadow of a doubt concerning the integrity of the former The eulogist of Watt did not occupy the most favourable position for doing justice to the great engineer's rival, and should have been especially cautious how he judged him. The effect of his incaution has been most mournful.

Who cannot but lament that for ten years some of the ablest men of Britain have spent (nay mispent) their time, in constructing out of De Luc's idle suspicions a dark myth which has obscured the good names of Cavendish, Priestley, Blagden, and in truth of all the Fellows of the Royal Society in 1784, from the President downwards ; besides extending its baleful influence to the nameless printers of the *Transactions*, who are involved in suspicion because some unlucky man among them, mistook a 3 for a 4 ? Watt, also, has been no gainer by De Luc's ill-judged interference. Had he left matters alone, there would probably have been no interpolations in Cavendish's paper, and his priority could not in some respects have been defended in the way it now can. De Luc's successors also, have (unconsciously as I believe) deprived themselves in many cases, of the means of doing justice to the question which they discuss, by their inheritance of his suspicions. There is much reasoning in a circle, in the advocacy of Watt's claims. De Luc's suspicions, for example, are accepted as showing why an error of the press was permitted ; and the error of the press is referred to as demonstrating the justice of his suspicions. No one asks what Blagden's character was, but contents himself with referring to it, as if it were what De Luc *suspected* it was, or rather perchance might be. Whatever is doubtful becomes clear by a reference to De Luc's uncharitable hypothesis, the rule being to accept it as a true theory, and to put the worst construction on every thing obscure in which Cavendish and Blagden were concerned. It is not surprising that the more extreme defenders of Watt have done this so long, that they have totally forgotten that neither De Luc nor Watt ever went beyond suspecting, and that they both betrayed entire ignorance of the proceedings of Cavendish. But it is a little strange that they should not have taken more pains to do what De Luc and Watt left undone, namely, prove that their suspicions were well founded. The reader who desires to be impartial must keep this steadily in view. De Luc and Watt are no authorities regarding the motives or the conduct of Cavendish or of Blagden. We have all the evidence on which they built their suspicions before us, and much more of which they knew nothing, and we have no veil before our eyes to hide or colour the truth, and no personal interest to serve by taking a side. I cherish the confident expectation that all who act impartially, will come to the conclusion that Cavendish and Blagden deported themselves as honourable men, and I hope also to persuade many that they had not occasion to pilfer or connive at pilfering, inasmuch as the intellectual property alleged to have been borrowed or stolen, was the fruit of Cavendish's capital, and had been from the first in his possession. The discoverer of the composition of water in 1781 had no occasion to borrow from its partial asserter in 1783.

I close this chapter by observing, that the conclusion regarding intellectual merit to which I have come, is, that Watt did not signify by phlogiston, hydrogen, and did not assert in the equivalent terms of his own day, that water consists of hydrogen and oxygen; and further, that the conclusion to which he came, such as it was, was arrived at later in time than Cavendish's just conclusion, and was drawn from a repetition of his experiments. For Cavendish I claim that he was the first who observed and inferred that water consists of hydrogen and oxygen; and to Lavoisier I assign the merit of having simplified and perfected Cavendish's conclusion, and of having been the first to prove the composition of water by analysis. I acknowledge Watt to have been an independent and original theorist on the composition of water, and to have largely contributed to the dissemination of the true theory of its nature. I must refer the reader, however, to the concluding sections, and the summary at the close of the Water Controversy for the fuller statement and justification of the views which I have taken of the merits and demerits of the three great rivals.

CHAPTER IV.

CONCLUDING EVENTS OF CAVENDISH'S LIFE.—ESTIMATE OF HIS MORAL AND INTELLECTUAL CHARACTER.

The year 1783, which has figured so largely in the preceding chapter as the period when Cavendish announced one of his great discoveries, was also an important epoch in his personal history. In the spring of that year, his father died,* and Henry was free to pursue his own tastes uncontrolled by any one. The incidents, however, of his later life are involved in little less obscurity than those of his earlier days, but we have much fuller details concerning his character in manhood than in youth. How exactly he spent the thirty years which elapsed between the period of his leaving Cambridge in 1753, and the death of his father in 1783, is not at all certain. For reasons which I have not been able to ascertain accurately, Lord Charles Cavendish restricted his son to a small yearly pecuniary allowance, and much importance has been attached to this fact by certain of Cavendish's biographers, as affording some explanation of the peculiarities of his character. The period when he acquired possession of an ample fortune, is thus a point of more importance, than at first sight might appear, in the history of a man, who for a large portion of his life possessed immensely more wealth than he knew how to spend. I am not at all certain, however, from what quarter, or at what period, Cavendish inherited the riches which ultimately passed into his hands; but from some facts to be presently mentioned, it would seem that he had become a wealthy man before his father's death. In the absence of any authoritative statement on this point, I shall quote the declarations already made public regarding it,

* Collins' *Peerage*, addend. et corrig., vol. i. p. 565.

which are, no doubt, substantially true, although in several respects they are incompatible with each other. Dr. Thomas Thomson, who was acquainted with Cavendish, says in his interesting sketch of him :—" During his father's lifetime he was kept in rather narrow circumstances, being allowed an annuity of 500*l*. only, while his apartments were a set of stables, fitted up for his accommodation. It was during this period that he acquired those habits of economy and those singular oddities of character which he exhibited ever after in so striking a manner. At his father's death he was left a very considerable fortune; and an aunt, who died at a later period, bequeathed him a very handsome addition to it, but in consequence of the habits of economy which he had acquired, it was not in his power to spend the greater part of his annual income."*

Cuvier gives a fuller but somewhat different account of Cavendish's early poverty, and the source of his wealth.† " Cadet d'une branche cadette, it était assez pauvre dans sa jeunesse, et ses parens le traitaient, dit-on, en homme qui avait l'air de ne devenir jamais riche. Le hasard, ou son mérite réel, en décida autrement.

" Un de ses oncles, qui avait fait la guerre aux Indes, et qui en rapportait une très grande fortune, conçut pour lui un attachement particulier, et la lui laissa tout entière.‡

* *History of Chemistry*, vol. i. p. 336.

† I attach considerable importance to Cuvier's statements, for although he gives no authority for them, it is probable that he derived his information from Blagden, who must have been on terms of special intimacy with the French philosopher, to whose step-daughter he left a legacy. It must be acknowledged, however, that as Blagden did not become Cavendish's assistant till at least 1782, and probably not till after the death of Lord Charles Cavendish in 1783, he could only speak on the authority of others as to the events of Cavendish's early history. He had peculiar opportunities notwithstanding, for learning these. I refer to this, because the Earl of Burlington, a descendant of Cavendish's chief heir, cannot furnish any information regarding the exact source of the philosopher's wealth. I am constrained, accordingly, to quote Cuvier, unauthenticated as his statements are, because he is in some respects the most authoritative biographer of Cavendish. His position as secretary to the French Academy, of which the subject of his *Eloge* was a member, gave him a right which others did not possess, to demand information ; and his friend Blagden could, directly or indirectly, afford him the most valuable assistance in collecting the materials for his *Eloge*. Cuvier also wrote in 1812, two years after Cavendish's death, and therefore at a time when more, probably, could be learned concerning him than at any later period.

* *Eloges Historiques*, tome ii. pp. 102, 103.

Biot, who wrote a year later than Cuvier, assigns the year 1773 as the year when Cavendish became "le plus riche de tous les savans." "Un de ses oncles," says he, "qui avait été Général outremer, étant revenu de ses courses en 1773, avait trouvé mauvais que la famille eût négligé son neveu, et, pour l'en dédommager, l'avait fait, en mourant, héritier de toute sa fortune, qui se montait à plus de 300,000 liv. de rente."*

I have not been able to discover who the General "outremer" was; or whether it was an uncle, as Cuvier and Biot assert; or an aunt, as Thomson states, who left Cavendish his fortune. The question, assuredly, is one of no moment. It would be of some importance, however, to ascertain the exact period when Cavendish ceased to be dependent on the limited income granted him by his father. His independence must have been attained not later than 1783, for in this year (if not in 1782) he settled an annuity of 500*l.* on Blagden. On this view he was fifty-one years old, or, if we accept Biot's date, forty-one, before he was the possessor of great riches. It is certain, at all events, that for the first half, if not for a greater part of his life, his pecuniary resources were extremely limited. A gentleman, who had frequent interviews with him when he visited the British Museum, understood "that he was kept by his father on an allowance of only 120*l.* a-year until he was forty years of age."* A senior member of the Royal Society Club, also, who frequently met Cavendish at its dinners, learned from Dr. Dryander, "that for some years Cavendish attended the club regularly, and that he had only the five shillings in his pocket to pay for the dinner,—not a penny more. His father, it appears, allowed him to attend, and gave him the exact five shillings to pay for the dinner."‡

Whatever obscurity or incongruity attaches to the statements I have quoted, those who made them are manifestly at one as to the two cardinal facts, that Cavendish was, for the first forty years of his life, a poor man, and for the last thirty-nine an unusually wealthy one. No one seems able to assign a reason

† *Biographie Universelle*, tome vii. p. 456.

* Communicated by Charles Tomlinson, Esq.

‡ Ibid. "The expense of the dinner was limited to five shillings, and black pudding was always a standing dish."

for his father's niggardliness towards him. There is good reason, however, for believing that the cause of Lord Charles Cavendish's parsimony to his son has been misapprehended. Dissatisfaction with Henry for not entering on public or professional life, is the alleged ground of his father's illiberality towards him. Cuvier, Biot, and Lord Brougham assert as much, but give no authority for their statements, and I learn from Robert Brown, Esq., that Lord Charles Cavendish was not a rich man, and that he allowed his son as much as he could afford.* Mr. Brown also states that Henry's father, himself a scientific man, appreciated his son's genius, and never treated him harshly or unkindly. Our great botanist had much better and ampler opportunities of estimating the character of Cavendish and of his father than the French biographers of the former, or Lord Brougham had. Mr. Brown's opinion on this point in truth is authoritative, and it is entirely in keeping with the probabilities of the case. It would assuredly have been very singular if Lord Charles Cavendish had not taken peculiar interest in a son whose scientific tastes were so congenial to his own, and who must have displayed from an early period many marked disqualifications for success in a political or professional career.

Between 1783 and 1810, it is scarcely possible to select one date as more entitled than another to be considered as marking an important period in Cavendish's personal history. The dates of his researches mark the great events of his life, but those have been already given. His series of journeys might at first sight seem to afford an exception to this statement, but as the records of these were almost entirely limited to a bare

* Biot states, " Il n'eut pendant sa jeunesse que le sort réservé en Angleterre aux branches cadettes, c'est-à-dire, une fortune très médiocre. Cavendish dédaigna les emplois aux quels sa naissance pouvait le porter, et ses parents, prénant sa modération pour l'apathie, s'éloignèrent de lui."—*Biogr. Univ.*, tome 7, p. 455. Biot's sketch, however, is not very accurate. He represents Cavendish as having been born in 1733, instead of 1731, and as the second son of the Duke of Devonshire, whereas he was the grandson of the second duke by his third son. In his estimate of Cavendish's wealth he is also in error, as Lord Brougham has shown (" Life of Cavendish," in *Lives of Men of Letters*, p. 430). His Lordship, however, states that Cavendish's " family, aware of the talents which he early showed, were anxious that he should take the part in public life which men of his rank are wont to do, and were much displeased with his steady refusal to quit the studies which he loved" (p. 430).

summary of scientific phenomena, we learn exceedingly little from them concerning their writer's personal history. The only fact, indeed, which I have found in the diaries of those journeys, which may be said to have an interest in reference to Cavendish's personality, is a visit which, in 1785, he paid to his great rival, Watt, at Birmingham. The meeting was a friendly one, and much intercourse must have occurred between the philosophers, especially in reference to the researches and inventions of Watt.*

No allusion is made to any difference having occurred between the parties who met in August 1785, for the first time after their rivalry in 1784. Their early reconciliation, or rather their refusal to act as if there ever had been a difference between them, is a pleasing fact in the history of two men so estimable as Cavendish and Watt are, and of whom it is painful to think ill. Students of human nature tell us that we hate those to whom we have done wrong, but there are no signs of hatred in Cavendish's visit to Watt. That so very shy and reserved a person as the former should have sought out his rival and taken the greatest interest in his inventions, is incompatible with the

* The following quotations from the MS. journal of Cavendish's journey in 1785, will illustrate this:—

"At Birmingham we were informed by Dr. Withering and Mr. Watt, that the part of the road through which we descended off the Lichny on the north side, when fresh cut, appeared evidently to be a granulated quartz" (p. 32).

"The fashionable excellence of gilt buttons is, that they may look red, much like copper. For this purpose the gilt button, scarce polished up, is dipped into a solution of some salts, amounting, as Mr. Watt said, to a kind of aqua regia" (p. 34).

"Mr. Watt's new method of giving a circular motion by the steam-engine is by making a small wheel fastened at the bottom of the bar suspended to the beam of the steam-engine, pass round a larger wheel, without revolving at all on its own axis" (p. 35).

"Mr. Watt mentioned, that having found that some steam is condensed in the cylinder of the steam-engine, though surrounded with steam, he made an experiment to discover what happened" (p. 36).

"Mr. Watt thinks to have ascertained by experiment, that the less heat water is converted into steam with, the more latent heat it requires to assume the elastic form" (p. 37).

"Mr. Watt considers the heat of steam at 212 both sensible and latent, as near 1160" (p. 38).

"The engine was of Watt's construction" (visit to Mr. Rathbone's works, Coalbrooke Dale.) (p. 45).

"The steam-engine at Bradley was upon Watt's construction" (p. 58).

"Mr. Watt has contrived a furnace to burn the smoke, which he means to apply to the steam-engine. The draught of air is conducted backward" (p. 62).

hypothesis, that on a solitary occasion he grudged him the merit of a discovery, or robbed him of it. Watt, also, must have welcomed the visit, and taken pains to explain to Cavendish all that was likely to interest him, for the references to the engineer's doings are very minute, and Cavendish exhibits an interest in his claims as an inventor, such as he rarely took in questions of relative priority or originality. Thus, referring to one of Watt's devices for regulating the motion of the steam engine, he says, "It was invented, or at least the patent for it obtained, by a Mr. Picard, who has sold it to the present proprietor; but Mr. Watt claims the original idea.* It is not uninteresting also to notice that Blagden accompanied Cavendish in this journey, and when I further add, that Watt's son informs us that his father, "after becoming, in 1785, a Fellow of the Royal Society, formed the personal acquaintance of Mr. Cavendish, and lived upon good terms with him,"† I think I may with confidence affirm, that any feeling of resentment which had been entertained by Cavendish and Watt towards each other, whilst strangers in 1784, was exchanged for mutual respect as soon as they met in 1785.

At this period Cavendish's reputation was widespread, in spite of his solicitous endeavours to prevent himself becoming famous. It may be well, therefore, to refer here to his position in London between the years 1783 and 1785, when his most remarkable chemical researches were either made or published. His town residence was close to the British Museum, at the corner of Montague-place and Gower-street.‡ Few visitors were admitted, but some found their way across the threshold and have reported that books and apparatus formed its chief furniture. For the former, however, Cavendish set apart a separate mansion in Dean-street, Soho. Here he had collected a large and carefully chosen library of works on science, which he threw open to all engaged in research, and to this house he went for his own books as one would go to a circulating library, signing a formal receipt for such of the volumes as he took with him.§

* *Journey in* 1785, p. 33. † *Watt Corr.*, p. iv.

‡ Information supplied by Charles Tomlinson, Esq.

§ Ibid. Cuvier, *Eloges Histor.*, tome i. p. 104.; Biot, *Biogr. Univ.* t. 7, p. 456.

His favourite residence was a beautiful suburban villa at Clapham, which, as well as a street or row of houses in the neighbourhood, now bears his name.* "The whole of the house at Clapham was occupied as workshops and laboratory."† "It was stuck about with thermometers, rain-gauges, &c. A registering thermometer of Cavendish's own construction, served as a sort of landmark to his house. It is now in Professor Brande's possession.‡" A small portion only of the villa was set apart for personal comfort. The upper rooms constituted an astronomical observatory. What is now the drawing-room was the laboratory. In an adjoining room a forge was placed. The lawn was invaded by a wooden stage, from which access could be had to a large tree, to the top of which Cavendish, in the course of his astronomical, meteorological, electrical, or other researches occasionally ascended.§

The hospitalities of such a house are not likely to have been overflowing. Cavendish lived comfortably, but made no display. His few guests were treated, on all occasions, to the same fare, and it was not very sumptuous. A Fellow of the Royal Society reports, "that if any one dined with Cavendish he invariably gave them a leg of mutton, and nothing else."‖ Another Fellow states that Cavendish "seldom had company at his house, but on one occasion three or four scientific men were to dine with him, and when his housekeeper came to ask what was to be got for dinner, he said, 'a leg of mutton!' 'Sir, that will not be enough for five.' 'Well, then, get two,' was the reply."

With this glance at Cavendish's style of housekeeping and general social deportment, I pass on to the only two remaining dates in reference to his personal history, which seem to require

* Cavendish House, Clapham Common, is a low white building, opposite the fifth mile-stone from Cornhill, now occupied by Mr. Herbert, whose lady has furnished Mr. Tomlinson with some interesting anecdotes of the philosopher, given elsewhere. He was an object of interest and perplexity to the residents in Clapham, among the more ignorant of whom he passed for a wizard.

† Information supplied by Dr. Davy, who received it from Mr. Newman, the instrument-maker.

‡ Information given to Charles Tomlinson, Esq., by a Fellow of the Royal Society.

§ Information furnished by Mr. Tomlinson, who visited Cavendish House and learned from its present occupant, and from Dr. Sylvester, of Clapham, the facts which are embodied in the text.

‖ Information supplied by Mr. Tomlinson.

notice. The first is the 25th March, 1803, on which he was elected one of the eight foreign associates of the French Institute.* The second, and excepting the date of his birth, the most important epoch in his history, is 24th February, 1810, on which he died in his seventy-ninth year. The striking circumstances of his death are mentioned in the sequel. He was buried in All Hallows, or All Saints Church, Derby, where Elizabeth Hardwicke built for herself a splendid tomb, round which the ashes of many generations of her descendants rest in peace beside her own.†

A more eventless life, according to the ordinary judgment of mankind, than that of Cavendish, could scarcely be conceived. His character, however, was a very remarkable and interesting one, and I shall try to explain what its prominent peculiarities were. The most striking of these, at a first glance, was, a singular love for solitariness, and a reluctance to mix with his fellows, which I may perhaps best denote by saying, that Cavendish was one of the most ungregarious of beings. The following quotations from the writings of some of his more eminent contemporaries, who were well qualified to form an estimate of his disposition, will illustrate this, as well as most of his other characteristics.

Professor Playfair, of Edinburgh, visited London in 1782, and was frequently present at the meetings of the Royal Society Club, one of the very few places of comparatively public resort which Cavendish attended. The impression made upon Playfair is thus recorded :—

"Mr. Cavendish is a member also of this meeting. He is of an awkward appearance, and has certainly not much the look of a man of rank. He speaks likewise with great difficulty and hesitation, and very seldom. But the gleams of genius break

* Biot, in *Biographie Universelle*, tome vii. p. 456.

† Elizabeth Hardwicke was a great proprietrix in Derbyshire, and founded an almshouse in the town of Derby. Her descendants, accordingly, were people of note in the town and county, and were honoured after death by the citizens of the former in a somewhat unusual way. A kind of public funeral was granted to all the Cavendishes, and therefore, I presume, to Henry; for on the death, two years after him, of his brother Frederick, he was buried "in the family vault, in All Saints, Derby, the corpse being met, as when a Cavendish is buried has been customary, at the entrance of the town by the mayor and thirty burgesses in mourning."—(*Gentleman's Magazine*, 1812, p. 291). Whether this practice still continues I do not know.

often through this unpromising exterior. He never speaks at all but that it is exceedingly to the purpose, and either brings some excellent information, or draws some important conclusion. His knowledge is very extensive and very accurate; most of the members of the Royal Society seem to look up to him as to one possessed of talents confessedly superior; and, indeed, they have reason to do so, for Mr. Cavendish, so far as I could see, is the only one among them who joins together the knowledge of mathematics, chemistry, and experimental philosophy."*

I have already quoted a reference to the same effect from Dr. Thomas Thomson. He further states of Cavendish:—"He was shy and bashful to a degree bordering on disease; he could not bear to have any person introduced to him, or to be pointed out in any way as a remarkable man. One Sunday evening he was standing at Sir Joseph Banks', in a crowded room, conversing with Mr. Hatchett, when Dr. Ingenhousz, who had a good deal of pomposity of manner, came up with an Austrian gentleman in his hand, and introduced him formally to Mr. Cavendish. He mentioned the titles and qualifications of his friend at great length, and said that he had been peculiarly anxious to be introduced to a philosopher so profound and so universally known and celebrated as Mr. Cavendish. As soon as Dr. Ingenhousz had finished, the Austrian gentleman began, and assured Mr. Cavendish that his principal reason for coming to London was to see and converse with one of the greatest ornaments of the age, and one of the most illustrious philosophers that ever existed. To all these high-flown speeches Mr. Cavendish answered not a word, but stood with his eyes cast down, quite abashed and confounded. At last, spying an opening in the crowd, he darted through it with all the speed of which he was master, nor did he stop till he reached his carriage, which drove him directly home."†

Sir Humphry Davy, in addition to the eloquent eulogium passed on Cavendish, soon after his death, left this less studied but more graphic sketch of the philosopher amongst his papers:

* *Works of John Playfair*, vol. i. appendix, lxxxiv.

† *History of Chemistry*, vol. i. p. 337. From Henry Lawson, Esq., of Lansdown Place, Bath, who was one of the few admitted to the intimacy of Cavendish, I have received a similar account of his treatment of a French philosopher, which perhaps, however, is but another version of the incident related by Dr. Thomson.

—" Cavendish was a great man, with extraordinary singularities. His voice was squeaking, his manner nervous, he was afraid of strangers, and seemed, when embarrassed, even to articulate with difficulty. He wore the costume of our grandfathers; was enormously rich, but made no use of his wealth. He gave me once some bits of platinum, for my experiments, and came to see my results on the decomposition of the alkalis, and seemed to take an interest in them; but he encouraged no intimacy with any one. . . . . He lived latterly the life of a solitary, came to the club dinner, and to the Royal Society, but received nobody at his own house. He was acute, sagacious, and profound, and, I think, the most accomplished British philosopher of his time."*.

Lord Brougham gives the following account of his estimate of Cavendish's character:—" He was of a most reserved disposition and peculiarly shy habits. This led to some singularity of manner, which was further increased by a hesitation or difficulty of speech, and a thin shrill voice. He entered diffidently into any conversation, and seemed to dislike being spoken to. He would often leave the place where he was addressed, and leave it abruptly, with a kind of cry or ejaculation, as if scared and disturbed. . . . . He hardly ever went into society. The only exceptions I am aware of are an occasional christening at Devonshire or Burlington House, the meetings of the Royal Society, and Sir Joseph Banks' weekly conversaziones. At both the latter places I have met him, and recollect the shrill cry he uttered as he shuffled quickly from room to room, seeming to be annoyed if looked at, but sometimes approaching to hear what was passing among others. His face was intelligent and mild, though, from the nervous irritation which he seemed to feel, the expression could hardly be called calm."†

Mr. W. H. Pepys gives the following interesting description of his interviews with Cavendish:—"The first time I saw him was at Sir Joseph Banks' house in Soho-square; it was a general meeting of men devoted to science. I was relating to Sir Joseph some experiments that I had been making with the voltaic battery, when I observed an old gentleman in a complete (faded violet) suit of clothes, and what was then termed a

* *Davy's Collected Works*, vol. vii. p. 139.

† *Lives of Men of Letters, &c.*, pp. 444, 446.

knocker-tailed periwig, very attentive to what I was describing. When I caught his eye he retired in great haste, but I soon found he was again listening near me. Upon enquiry I heard it was Mr. Cavendish, but at the same time was cautioned by Sir Joseph to avoid speaking to him as he would be offended. if he speaks to you, continue the conversation; he is full of information, particularly as to chemistry.

"I met him soon after at the Royal Society Club, and sitting very near him he commenced some enquiry upon what I had said at Sir Joseph's, which plainly showed he had remembered me. His speech was hesitating and excited, but he was very quick of comprehension."*

Dr. Davy, who met Cavendish one or two years before his death, gives a similar account of his appearance and manners. "I well remember him, as he was in the habit occasionally, between 1808 and 1809, of coming to the laboratory of the Royal Institution, drawn there, no doubt, by the researches then in progress. His dress was that of the gentleman of the preceding half century. The frilled shirt-wrist, the high coat collar, the cocked hat, in brief, almost the court dress of the then and the present time. His appearance was, apart from his dress, nowise distinguished: of fair complexion, small, and not marked features, a feeble and somewhat hesitating voice. He was then aged, turned, I believe, of seventy; but though his body seemed infirm, his conversation and queries denoted quickness and acuteness, and undiminished vigour of mind, and *that* I think, was the impression on my brother's mind, who always held him, as his writings testify, in the greatest respect."†

The following account of Cavendish is from one of our most accomplished chemists, who communicated it orally to Mr. Tomlinson, from whom I received it. "When I was a very young man—a new Fellow of the Royal Society—I always looked upon it as a great honour to be noticed by Cavendish, and so did the other young members of the society. We used to dine at the Crown and Anchor, and Cavendish often dined with us. He came slouching in, one hand behind his back, and taking off

* Letter from William Hasledine Pepys, Esq., to Lord Burlington, from whom, with Mr. Pepys' sanction, I received it.

† Letter, April 9th, 1850.

his hat (which by the bye he always hung up on one particular peg), he sat down without taking notice of anybody. If you attempted to draw him into conversation he always fought shy. Dr. Wollaston's directions I found to succeed best. He said, 'the way to talk to Cavendish is never to look at him, but to talk as it were into vacancy, and then it is not unlikely but you may set him going!'"

J. G. Children, Esq., was often in the company of Cavendish, and thus refers to his interviews with him; "I am now the Father of the Royal Society Club. I remember Cavendish well, and have often dined at the Crown and Anchor with him. When I first became a member of the club, I recollect seeing Cavendish on one occasion talking very earnestly to Marsden, Davy, and Hatchett. I went up and joined the group, my eye caught that of Cavendish, and he instantly became silent: he did not say a word. The fact is he saw in me a strange face, and of a strange face he had a perfect horror. I don't think I had been introduced to him, but I was so afterwards, and then he behaved to me very courteously. He was an old man when I joined the club, and was regarded by all as a great authority." *

The most remarkable illustration, however, of Cavendish's excessive shyness is, perhaps, that contained in the following account of his reluctance to make his appearance at the soirées of Sir Joseph Banks, which he frequently attended. It was communicated by a senior member of the Royal Society to Mr. Tomlinson, from whom I obtained it: "I have myself seen him stand a long time on the landing, evidently wanting courage to open the door and face the people assembled, nor would he open the door until he heard some one coming up the stairs, and then he was forced to go in."

He was thus to appearance a misanthrope, and still more a misogynist. He was reported among his contemporaries, indeed, to have a positive dislike of women. Lord Burlington informs me, on the authority of Mr. Allnutt, an old inhabitant of Clapham, "that Cavendish would never see a female servant, and if an unfortunate maid ever showed herself she was immediately dismissed." Lord Brougham tells us that Cavendish "ordered his dinner daily by a note, which he left at a certain

* Reported by Charles Tomlinson, Esq.

hour on the hall table, where the housekeeper was to take it, for he held no communication with his female domestics from his morbid shyness."*

I might multiply illustrations of Cavendish's extreme repugnance to encounter females, but two additional instances may suffice. At one time he was in the habit of walking in the neighbourhood of Clapham with methodical accuracy at a particular hour of the day. Two ladies, who watched his movements, and had observed the punctuality with which he reached the same spot at the same hour every day, took a gentleman with them on one occasion to catch a sight of the famous philosopher. "He was in the act of getting over a stile when he saw to his horror that he was being watched." He never appeared in that road again, and his walks in future were taken in the evening.†

The following incident occurred at a meeting of the Royal Society Club, in the early part of this century, and was reported by one of the most accomplished Fellows of the Society to Mr. Tomlinson. "One evening we observed a very pretty girl looking out from an upper window on the opposite side of the street, watching the philosophers at dinner. She attracted notice, and one by one we got up and mustered round the window to admire the fair one. Cavendish, who thought we were looking at the moon, bustled up to us in his odd way, and when he saw the real object of our study, turned away with intense disgust, and grunted out Pshaw!"

In the preceding statements I have quoted largely and verbatim from the materials at my disposal, that the reader may have the means of forming an estimate for himself of the difficult character of Cavendish. The portrait prefixed to this

* *Lives of Men of Letters, &c.*, p. 446. Cavendish, says another authority, "one day met a maid servant on the stairs with a broom and a pail, and was so annoyed that he immediately ordered a back staircase to be built."—(Information communicated to Mr. Tomlinson.)

† "His favourite, and, indeed, his only walk at Clapham, was down Nightingale Lane, nearly opposite his house, from Clapham Common to Wandsworth Common, and so round the road back to his own house. This walk he always took in the dusk of the evening, and he always walked in the middle of the road, never on the side path. He was never known to speak to any one, or to touch his hat to any one who took off his. In short, his desire seemed to be alone and to be left alone."—(Information furnished by Dr. Sylvester to Mr. Tomlinson.)

volume supplies an additional means of realising the appearance of the philosopher, as he was seen by those whose testimony I have adduced.*

A striking unanimity pervades the references made to Cavendish by those who saw him, and so far as the mere externalities of his character are concerned, we cannot readily misapprehend it. We picture him to ourselves an excessively shy, silent, awkward, and embarrassed person, barely enduring the looks of men, and fleeing from the gaze of women. His reluctance to encounter his fellows has been ascribed by some to the chilling influence of the straitened circumstances under which his early life was spent; but, as I conceive, with no propriety. His poverty was only relative, and his tastes were not expensive. Were I disposed to impute to outward circumstances the development of an individuality so well marked as his, I should lay most stress

* The Cavendish Society is under the greatest obligation to Charles Tomlinson, Esq., for the discovery of the portrait of Cavendish, and for the arrangements by which he secured its being engraved at a merely nominal cost. The original is a water-colour sketch contained in the print-room of the British Museum, of the existence of which few were aware before Mr. Tomlinson drew attention to it. Dr. Paris had obtained a copy, but so little was it known that a portrait was extant, that Lord Brougham in his *Life of Cavendish* (page 446) says, "It is not likely that he ever should have been induced to sit for his picture; the result, therefore, of any such experiment is wanting." His Lordship's statement is true in one sense, though not in that in which he intended it, for Cavendish certainly neither sat nor stood for his likeness, and probably was not aware of its existence. Its history is singular, as appears from a passage, extracted from a volume which Mr. Tomlinson found in the Library of the British Museum, entitled "*Sketches of the Royal Society and Royal Society Club, by Sir John Barrow, Bart., F.R.S. Lond.* 1849."

Barrow, at the request of Alexander, 'the excellent draughtsman to the China Embassy," who wished to take a full-length portrait of Mr. Cavendish, applied to Sir Joseph Banks, to know if the philosopher would consent to have his portrait taken. Sir Joseph, in reply, assured Barrow that he would certainly receive a blunt refusal, as had been the case with himself, on making the same request. He brought about, however, a meeting between Cavendish and Barrow, who thus reports the result:—

"I could not, however, find a favourable occasion for proposing the portrait, and at last Alexander, who was bent upon having it, said, if I would invite him to a club dinner, he could easily succeed, by taking his seat near the end of the table, from whence he could sketch the peculiar greatcoat of a greyish green colour, and the remarkable three-cornered hat, invariably worn by Cavendish; and obtain, unobserved, such an outline of the face as, when inserted between the hat and coat, would make, he was quite sure, a full length portrait that no one could mistake. It was so contrived, and every one who saw it recognised it at once. I think Alexander told me he should leave it at the British Museum; but whether it be there I know not."

upon the early loss of his mother, whose "affectionate kindness" his brother Frederick "frequently lamented that he had never known."* Kindly feminine care in early life, and especially that of a mother or a sister, might have done much towards infusing human sympathies into Cavendish's heart, and rendering his nature more affectionate and genial. He lost his mother, however, when he was two years old, and from his eleventh till his twenty-second year was at school or college, so that there is great reason to believe, that at the most critical period of his life, he was not exposed to the salutary influences of a happy home, which might have tempered the peculiarities of his character. Whilst I say this, however, I by no means wish to exaggerate the effect which his education among strangers may be supposed to have produced upon him; at best, his early orphanhood and his comparative poverty can but have fostered singularities over which external circumstances had, after all, little control. Hundreds of youths have been poor, and motherless, as Cavendish was, and have, nevertheless, grown up warmhearted, generous, and even enthusiastic men. Frederick Cavendish was exposed to the same influences as his brother Henry, but became, notwithstanding, an exceedingly cheerful, genial, and benevolent, though somewhat eccentric man. The peculiarities, indeed, of a character like Henry Cavendish's must be referred much more to original conformation, than to anything else; and whatever may have been the restraints which his father imposed upon him, it seems certain that one so widely connected with the aristocratic and wealthy families of his country, as he was, might have procured pecuniary and other assistance, towards the prosecution of any lawful or honourable enterprise on which he wished to enter, from some of his relatives. His brother, as well as himself, followed no profession, and both became ultimately wealthy men. Henry, in all likelihood, might have escaped from paternal restraint, long before death released him from its bondage, had he wished to have been free. All other causes, accordingly, seem to me of slight importance, as sources of Cavendish's peculiarities, compared with the influence which the strongly marked original elements of his nature exerted upon him.

What these were, will appear as I proceed with the analysis

* *Gentleman's Magazine*, 1812, p. 289.

of his character. Whether from original or acquired indifference, he exhibited from the first period when we have the means of forming a judgment concerning him, a *passive* selfishness in all his dealings. With his relatives he had very little intercourse. The biographer of Frederick Cavendish says of him, that "For his brother Henry he ever had a truly fraternal affection, which *seems* to have been fully repaid, though they met but seldom."* Had Henry, however, exhibited his regard for his brother largely or openly, the terms in which it is referred to would have been very different.† His heir, Lord George Cavendish, visited him but once a-year, and remained only half-an-hour at each visit. Towards those not of his own blood, he was, if possible, still more indifferent. The only one whom he admitted to daily intercourse with him, and that but for a few years, was Blagden, and he finally became estranged from him. Sir Joseph Banks, perhaps, knew him more intimately than most men did, yet after all they were only acquaintances. Every one entitled to give an opinion on the subject, to whom I have applied, gives the same judgment on this point, which I may state once for all, in the words of one of my informants, "Cavendish was the coldest and most indifferent of mortals." This selfishness was entirely passive, as I have already implied. Its strange betrayer might, in his later years, have obtained for himself distinctions of all kinds, but even scientific eminence, the only kind of fame for which he cared, if indeed he cared even for that, he made no struggle to attain, and he prevented his brother philosophers from placing him on the height to which they would willingly have raised him, by keeping back many of his most remarkable discoveries. He had thus the same law for himself as for his brethren, and, after a fashion of his own, kept the golden rule,

* *Gentleman's Magazine*, 1812, p. 291.

† The following is the only account I have received of intercourse between the brothers. It was communicated to me by Mr. Tomlinson, who received it from a Fellow of the Royal Society :—

"On one occasion Cavendish travelled in France with his brother Frederick. On landing at Calais they stopped at an hotel, and in retiring for the night passed a room, the door of which was left open, and they saw in passing a dead body laid out for burial. Nothing was said at the time, but the next day the following conversation took place between the brothers on their road to Paris :—

"'Fred. C., *loq.*—Did you see the corpse?

"'Henry C., *res.*—I did.'"

and did unto others as he would have others do unto him. A demand, accordingly, upon his sympathy, seems to have surprised him. Sir Humphry Davy was indebted to him for "some bits of platinum," but tacitly appealed in vain for assistance in prosecuting his electrical researches. "The last time I saw Cavendish," says Mr. W. H. Pepys (in the letter to Lord Burlington, already referred to), "was at the Royal Institution, in the apartments of Sir H. Davy (then Mr. Davy). It was just before the subscription was entered into for the extended voltaic battery, and upon Davy expressing regret that he feared he should not obtain sufficient for the object, he [Cavendish] joined most truly in deploring the want of liberality in the patrons of science to carry it into effect. He did not seem to think he was called upon to take any active step to forward the desired object." Yet had any one asked Cavendish to sign a cheque in Sir Humphry's name for 500*l.*, he would probably have done it at once. I infer as much at least from what Mr. Pepys further tells us.—"At one time Mr. Cavendish had a large library in London, which was in a bad state of arrangement. It was proposed to him to allow a gentleman, who was not very well off, to reside in the house, as being a clever man he would in return arrange the books, and render the library more useful for consultation, which Mr. Cavendish freely allowed. After this gentleman had resided there a considerable time, and had succeeded in classing the books, he left to go to the country. Mr. Cavendish, dining one day at the Royal Society Club, some person present mentioned this gentleman's name, upon which Mr. Cavendish said, "Ah! poor fellow: how does he do? How does he get on?" "I fear very indifferently," said this person. "I am sorry for it," said Mr. C. "We had hopes you would have done something for him, sir." "Me, me, me, what could I do?" "A little annuity for his life; he is not in the best of health." "Well, well, well, a check for ten thousand pounds, would that do?" "Oh sir, more than sufficient, more than sufficient."

Similar acts of liberality are understood to have been performed by Cavendish on other occasions. According to Cuvier, "il a soutenu et avancé plusieurs jeunes gens qui annonçaient des talens."* Who those parties were does not appear. The judgment, however, of a Fellow of the Royal Society, who had good opportunities for coming to a conclusion on this point, is, "that Cavendish did some good in a very ungracious manner;"† and it could scarcely be expected, that one who took almost no care of his own property, should concern himself much about the prosperity of others. His famous answer to his bankers, who were alarmed at the immense sum of money which had accumulated in their hands, is the best proof of his indifference to pecuniary affairs. Dr. T. Thomson gave the first account of this singular transaction.‡ I give another version, from the graphic pen of Mr. W. H. Pepys:—

"The bankers where he kept his accounts, in looking over their affairs, found he had a considerable sum in their hands, some say nearly eighty thousand pounds, and one of them said, that he did not think it right that it should lay so without investment. He was therefore commissioned to wait upon Mr. Cavendish, who at that time resided at Clapham. Upon his arrival at the house he desired to speak to Mr. Cavendish.

"The servant said, 'What is your business with him?'

"He did not choose to tell the servant.

"The servant then said, 'You must wait till my master rings his bell, and then I will let him know.'

"In about a quarter of an hour the bell rang, and the banker had the curiosity to listen to the conversation which took place.

"'Sir, there is a person below, who wants to speak to you.'

"'Who is he? Who is he? What does he want with me?'

"'He says he is your banker, and must speak to you.'

"Mr. Cavendish, in great agitation, desires he may be sent up, and, before he entered the room, cries, 'What do you come here for? What do you want with me?'

"'Sir, I thought it proper to wait upon you, as we have a very large balance in hand of yours, and wish for your orders respecting it.'

"'If it is any trouble to you, I will take it out of your hands. Do not come here to plague me.'

* *Eloges Hist.*, tome i. p. 104.

† Information received from Mr. Tomlinson.

‡ *History of Chemistry*, vol. i. p. 336.

"'Not the least trouble to us, Sir, not the least; but we thought you might like some of it to be invested.'

"'Well! well! What do you want to do?'

"'Perhaps you would like to have forty thousand pounds invested.'

"'Do so! Do so, and don't come here and trouble me, or I will remove it.'"*

In spite of this ungracious demeanour, and his undeviating indifference to the affairs of his fellow-men, Cavendish awakened an interest, almost reaching to affection, in some of those who knew him. He had no vices. In the sight of man he was blameless. The "good haters," whom Dr. Johnson loved, would have been puzzled to justify themselves in hating Cavendish. No one could know him and not respect him. Many, probably, longed to love him, but felt that he acted on them like an electrified conductor on the bodies in its neighbourhood, which it has no sooner attracted than it violently repels. Some few, apparently, were able to make the transition from admiring respect to loving regard. The following letter, which had found its way into the electrical MSS. of Cavendish, brings him more within the circle of human sympathies than any other document which I have encountered. It is dated 16th March, 1792. The writer was an officer in the navy, and ultimately Hydrographer to the Admiralty:—

"Dear Sir,—I was very sorry yesterday to hear that you were prevented coming amongst us by an attack of the gravel; it brought to my recollection that old Balchier mentioned at the Club one day, that nothing was more efficacious in that complaint than *lintseed tea.* I hope, however, the complaint is going off, as it was said you were better. That you may soon come amongst us is the sincere wish of all your friends, and of none more truly than of, dear Sir,

"Your most affectionate,

(Signed) "A. Dalrymple."

* Letter to Lord Burlington, communicated to me: "Mr. Cavendish [at the period of his death] was the largest holder of bank-stock in England, and died worth 1,157,000*l.* in different public funds, the value of which was estimated at 700,000*l.*" He had besides a freehold property about 8,000*l.* a-year, and canal and other personal property; 50,000*l.* also were in the hands of his bankers.—(*Gentleman's Magazine,* 1810, p. 292.)

The Rev. J. Michell, also, who devised the apparatus with which, in a modified form, Cavendish determined the density of the earth, seems to have been a warm friend of the philosopher's. Among the Cavendish MSS. I found a long letter from Michell to him, dated 14th August, 1788. It refers chiefly to Geology, and to a specimen of *black lead* which accompanied the letter. Its conclusion runs thus: "with best respects to yourself, and due compliments to all friends, when you see them, particularly those of the Crown and Anchor, and Cat and Bagpipes Clubs, I am, &c."

There is in this passage an appeal to Cavendish's social feelings and kindly human sympathies, which seems to show that he possessed some warmth and geniality of character. It was with no little interest, accordingly, that I fell upon a paper among the Cavendish MSS., evidently containing the draft of a reply to Michell; but I was disappointed. "I am much obliged to you" says the writer, "and to Mr. B. for the plumbago, and to you for your letter." The properties of the plumbago, and the geological questions raised by Mr. Michell, are then largely considered, but no acknowledgment is made of his greetings, nor does any reference occur to the Crown and Anchor or Cat and Bagpipes Clubs.* Michell, perhaps, knew better than to expect an answer on these points.

There are thus some faint glimpses of Cavendish occasionally appearing among his fellow *men* in a capacity which at

* Hoping that some light might be thrown on Cavendish's character, by a knowledge of the nature of the last of the Clubs referred to, I made enquiry concerning it, and Mr. Tomlinson took a great deal of trouble in endeavouring to discover the place of its assembling, and its object. For some time we were entirely at fault; but at length that valuable journal, *Notes and Queries*, solved the problem, so far, probably, as it can now be solved. The Cat and Bagpipes, it appears, was once well known: "A public-house of considerable notoriety, with this sign, existed long at the corner of Downing Street, next to King Street. It was also used as a chop-house, and frequented by many of those connected with the public offices in the neighbourhood."—(*Notes and Queries*, Nov. 9. 1850, p. 397.) The nature of the Cat and Bagpipes Club, of which Cavendish and Michell were probably members, remains undetermined. One is tempted to imagine, that in the society of some trustworthy, select few, Cavendish may have indulged in a temperate conviviality and have unbent, for some half hour or so, from the rigid indifference which generally characterised him.

The Crown and Anchor was the tavern at which the Royal Society Club held its meetings.

least redeems him from the charge of misanthropy. There was at least one lady, also, who was ready to defend him from the charge of being a woman-hater, for she had to thank him for saving her from the attacks of an infuriated cow. Unfortunately she is dead, or through her testimony we might have learned that Cavendish did not hate women, but was only awkwardly shy and afraid of them.*

After all, however, it must be acknowledged, that out of the monk's cell, and the prisoner's dungeon, there have been very few men who have lived for nearly four score years, and held so little communication with their fellows, or made so few friendships as Cavendish.

To the other objects of common regard which excite and gratify the fancy, the imagination, the emotions, and the higher affections, he was equally indifferent. The Beautiful, the Sublime, and the Spiritual seem to have lain altogether beyond his horizon. The culture of the external senses, which the prosecution of researches in the physical sciences, secures to all who are successful in their study, did nothing in Cavendish's case, to quicken the perception of beauty, whether of form or sound or colour. Many of our natural philosophers have had a strong and cultivated aesthetical sense; and have taken great delight in one or other or all of the fine arts. For none of these does Cavendish seem to have cared. Unlike Black, he was indifferent to elegance of form in his apparatus, which, provided it were accurately constructed, might be clumsy in shape and of rude materials. He insisted, however, on its perfect accuracy.†

* The lady in question was Mrs. Keer, formerly resident in Clapham. She appears to have taken much interest in Cavendish's proceedings, probably from a feeling of gratitude, and was fond of referring to them. His interposition to save her from the mad cow, excited a great sensation in Clapham, where so much was not expected from him, for he was considered a confirmed woman-hater. Mr. Tomlinson learned these particulars from Mrs. Herbert, the present occupant of Cavendish House, Clapham Common.

† Dr. Davy, who inherited a large part of Cavendish's apparatus from Sir Humphry, writes to me regarding it:—"Cavendish seemed to have in view in construction, efficiency merely, without attention to appearance. Hard woods were never used, excepting when required. Fir wood (common deal) was that commonly employed. The same disregard of mere appearances was shown in his laboratory. A lady of rank (I believe it was the Duchess of Gordon), on paying Cavendish a visit at Clapham, saw, I have heard it related, a long row of utensils never intended to

The grandeur of natural scenery—the changing aspects of the skies—the striking differences between the inhabitants of different parts of a region—the historical associations inseparable from certain localities, and much else, on which Saussure, Humboldt, Dalton,* Darwin, Forbes, and other scientific pilgrims, expatiate so largely, find no place in Cavendish's Journal of his Travels. Again and again, we read with expectation; "At —— I observed,"—and look forward to something remarkable being described, but the sentence ends—"the barometer." Cavendish crosses the country like a railway surveyor; turning neither to the right nor to the left, or deviating from his route only to make such announcements as, that "At Stroud we were informed that the old canal had not yet paid any dividend to the proprietors, and that scarcely anything but coals were brought up it, at the rate of 3*s.* a ton."† One solitary passage, however, I have found, which infuses vitality and a human interest into these formal diaries; and shows that their writer had deep down in his nature, the common sympathies of humanity. He is describing the banks of the Severn, and says, "The Terras-walk commands a remarkable scene, from the singular appearance of these rocks all around, but especially on the opposite side of the river Severn, the eastern, and from the fine view of the river underneath. The remains of the old Castle, battered by Oliver Cromwell, exhibit a remarkable instance of a leaning Tower, which produces a fine effect."‡

meet the eye, and on expressing surprise at their number and arrangement, was hurried by them without explanation." They were employed in the crystallisation of saline solutions, by spontaneous evaporation.

The following anecdote, which Mr. Newman, the instrument-maker, of Regent Street—communicated to Dr. Davy and to Mr. Tomlinson—strikingly illustrates the statement in the text:—

"My father for many years worked for the trade, and I remember a wind-guage which he made for Nairn and Blunt, who had received the order from Cavendish. It consisted of a train of wheels worked by a vaned fly, and it registered its results in the same manner as a common gas-meter does. When this anemometer was finished, my father had to attend at Nairn and Blunt's, where he met Cavendish, who insisted upon his taking the whole apparatus to pieces, and then, by means of a file and a magnifying glass, he tested the pinions to see that they were properly hardened and polished, and of the right shape, according to his written directions."

* Meteorological Essays; Appendix to second edition.

† Journey of 1785, p. 8. ‡ Ibid. p. 56.

The character of Cavendish would be incomplete if I left unnoticed his *apparent* irreligiousness. It may have been only apparent. One so very reserved on ordinary affairs, is likely to have been especially uncommunicative on a subject which he might consider lay only between his Maker and himself. I should be the last to pronounce judgment on his religious belief, for he gave no utterance on the subject, and others can only surmise concerning it. It is part, however, of the legitimate function of a biographer, to chronicle the extent to which the moral and spiritual affections of our common nature showed themselves in the subject of his sketch; and even if I wished to avoid the consideration of Cavendish's religious character (which I do not), I should feel compelled to notice the observations already published on this point. Biot states that Cavendish "était d'une morale austère, religieux à la manière de Newton et de Locke."* This reference (for which Biot, no doubt, had some authority) is ambiguous, but I presume that it refers to the doctrinal faith of Cavendish; for no one would impute to him any manifestation of the religous earnestness and fervour of Newton and Locke, who gave spontaneous, public utterance to their religious convictions, and counted it a duty and a pleasure to bear witness to what they believed to be the truth. The significance of Biot's allusion is also rendered doubtful, by the uncertainty that still prevails as to the exact doctrine which Newton held concerning the Trinity.† It cannot be doubted, however, that the biographer intends to signify that there was some peculiarity in Cavendish's religious belief; and that his views were, to a certain extent, what are termed by theologians *unorthodox*,‡ and probably Arian or Unitarian. Mr. Fuller, of St. Peter's College, Cambridge, writes to me on this subject, "I find there is a sort of hereditary belief here, that Cavendish was not only a favourer of unitarian notions,

* *Biographie Universelle*, tome vii. p. 456.

† Into this vexed question I do not enter. The reader will find opposite views urged with great ability by Sir David Brewster in his life of Newton (*Family Library*, *No*. 24), and by Professor A. De Morgan in his very interesting sketch of the philosopher (*Knight's Cabinet Portrait Gallery*, vol. xi. p. 109).

‡ I use the word in its technical sense, as having a definite meaning attached to it by all religious parties in this country.

but decidedly a Unitarian. I am not able to discover any foundation for the belief except tradition."

Cavendish also, as already mentioned, is supposed to have left Cambridge without a degree, from reluctance to submit to the stringent religious tests applied in his day to candidates for degrees.*

This is all that I have been able to learn concerning his doctrinal belief. Whatever it was, it did not lead to any open confession of faith. Cavendish did not ally himself with any religious body. He is understood to have "never attended a place of worship."† The only service, indeed, in any degree religious in which, so far as I can discover, he ever took part, was the christening of some of his young relatives, and I am not certain that he did more than attend the christening dinner.‡

A Fellow of the Royal Society, who had good means of judging, states that, "As to Cavendish's religion, he was nothing

* On this point Mr. Fuller has favoured me with the following information:—"So far as I can ascertain, there has *never* been any religious test at Cambridge administered at matriculation, or at any period of a student's university career previous to his taking a degree. In this respect we differ from Oxford, and we have accordingly students of all denominations, religious and irreligious (and I even remember a Mussulman), who study here, but leave us without taking any degree. The first religious test is administered on taking the first degree—Bachelor in Arts, Law, or Medicine, and it is a declaration to the effect that the candidate is *bonâ fide* a member of the Church of England as by law established. A candidate for a higher degree—Master of Arts, or Doctor in either of the faculties, must submit to a more stringent test, viz., he must sign the 36th Canon, the Articles, and the Liturgy of the Church of England.

"Practically, it is found that the former test does not exclude many whose opinions are somewhat at variance with either the doctrine or practice of the Church, but not widely so; while the second test excludes many who do not outwardly dissent, but yet have conscientious scruples on certain points.

"When Cavendish was at Cambridge, the second and more stringent test was administered to candidates for the Bachelor's degree as well as the Master's. The change to the present system was introduced by grace of the Senate, dated June 23, 1772, and another grace dated March 26, 1779, principally, I believe, through the instrumentality of Archdeacon Paley."

† Information furnished to Lord Burlington by Mr. Allnutt, of Clapham, who only, however, professed to state his belief on the matter in question.

‡ Lord Burlington writes me: "I have heard my grandmother say that he [Cavendish] once came to a christening, and that it being at that time the custom to make a present to the nurse, he put his hand in his pocket, and presented her with a handful of guineas without counting them."

at all. The only subjects in which he appeared to take any interest, were scientific. An unaccustomed glow seemed to come over him when some new point in mathematics was spoken of; but if the conversation relapsed into general topics, or even the exciting politics of the day, he turned aside, and all the cold indifference of his nature returned." *

This opinion seems confirmed by the references in Cavendish's Journals. The most important, perhaps, of all his experiments on the composition of water, was made on a Sunday; and, in his journeys, all days of the week were alike, so far as geological or meteorological observations were concerned.

From what has been stated, it will appear that it would be vain to assert that we know with any certainty what doctrine Cavendish held concerning Spiritual things; but we may with some confidence affirm, that the World to come did not engross his thoughts; that he gave no outward demonstration of interest in religion, and did join his fellow men in worshipping God. What worship he offered in private we do not know, but the striking circumstances of his death prove that to the last he excluded others from a knowledge of his belief or non-belief in a future state, and in a God to whom he should be required to answer for the deeds done in the body. He died, and gave no sign, rejecting human sympathy, and leaving us no means of determining whether he anticipated annihilation, or looked forward to an endless life. I have reserved, accordingly, the notice of his death till now, of which several accounts have been given. Dr. T. Thomson writes,—

"When he found himself dying, he gave directions to his servant to leave him alone, and not to return till a certain time which he specified, and by which period he expected to be no longer alive. His servant, however, who was aware of the state of his master, and was anxious about him, opened the door of the room before the time specified, and approached the bed to take a look at the dying man. Mr. Cavendish, who was still sensible, was offended at the intrusion, and ordered him out of

* Information furnished to Charles Tomlinson, Esq.

the room with a voice of displeasure, commanding him not by any means to return till the time specified. When he did come back at that time, he found his master dead."*

Dr. Davy gives a slightly different account. His authority, as he informs me, was a person of the name of Harrison, who was at one time in the employment of Mr. Ramsden, the instrument-maker, and afterwards for a period in that of Cavendish:—

"He died, I have been assured, in the most tranquil manner. A person employed by him about his apparatus told me, that the last thing Mr. Cavendish called for was a glass of water, and then he desired to be alone; his attendant being uneasy respecting his state, retired to a distant part of the room. Mr. Cavendish drank some of the water, turned on his side, and shortly expired, without uttering a word or even a sound, much in the manner of his illustrious contemporary, Dr. Black, who died as if he had fallen asleep, with an unspilled basin of milk on his knees, sitting in his chair."†

A still fuller and somewhat dissimilar description of the closing scene of Cavendish's life, has been sent me by H. Lawson, Esq., of Landsdown Place, Bath:‡

"He went home one evening (I believe from the Royal Society) and passed silently as usual to his study. His man-servant observed blood upon his linen, but *dared* not ask the cause. He remained ill for two or three days, and on the last day of his life, he rang his bell somewhat earlier than usual, and when his valet appeared, called him to the bedside, and said,—

"'Mind what I say—I am going to die. When I am dead, *but not till then*, go to Lord George Cavendish, and tell him of the event. Go!'

"The servant obeyed.

"In about half an hour Cavendish rang his bell again, and calling his servant to his bedside, desired him to *repeat* what he

* *History of Chemistry*, vol. i. p. 339.

† *Collected Works of Sir Humphry Davy, edited by Dr. Davy*, vol. vii. p. 139.

‡ For a reference to this acquaintance of Cavendish's, I was indebted to Dr. Davy, who obtained it from Mr. Newman, the instrument-maker, of Regent Street, London.

had been told, '*When I am dead, &c.*'—'Right. Give me the lavender water. Go.'

"The servant obeyed, and in about half an hour, having received no further summons, he went to his master's room, and found him a corpse."

Dr. Elliotson, whose family, as he mentions, resided at Clapham, so that he had good means of ascertaining the truth, confirms the general accuracy of these accounts of Cavendish's death.* Nevertheless a description of the closing scene of his life, considerably at variance with those quoted above, has been given to the world by Sir John Barrow, on the authority of Sir Everard Home. Sir Everard's veracity has been called in question, but there seems little reason to doubt that he was a faithful witness in this case; and it is certain at least, that he gave a similar account to a Fellow of the Royal Society, who reported it to Mr. Tomlinson. The substance of Sir Everard's statement was, that Cavendish sent his servant out of the house, "ordering him not to come near him till night, as he had *something particular to engage his thoughts, and did not wish to be disturbed by any one!*" The servant, who believed his master to be dying, summoned Sir Everard Home, who hastened to Clapham. "He found Cavendish in bed, very much exhausted, and apparently in a dying state. Mr. C. seemed rather surprised to see him there; and said that Sir E. could be of no use to him, for that he was in a dying state; and blamed his servant for bringing him (Sir Everard) down from town, for that at eighty years of age he thought that any prolongation of life would only prolong its miseries. Sir E. insisted on remaining with him during the night. The patient remained tranquil, and shortly after daybreak departed this life."†

After all, however, the various accounts of Cavendish's death

* *Physiology*, fifth edition, p. 1044.

† *Sketches of the Royal Society and Royal Society Club, by Sir John Barrow.* Sir E. Home, after Cavendish's death, examined his repositories, in the presence of his servant, with the following result—according to Barrow:—"In one of the chests of drawers they found many old-fashioned articles of old jewellery, parts of embroidered dresses, &c., and, among other valuable articles, an old lady's stomacher so beset with diamonds that when it came to be examined and valued I think Sir E. mentioned its worth as something like 20,000*l.*"

do not differ, so far as essentials are concerned; and I would willingly believe that the "something particular," which he told his servant was to engage the undisturbed attention of his last, and solemn, silent hours, was his [preparation for the unseen world into which he knew he was about to pass.

Such, then, was Cavendish in Life and in Death, as he appeared to those who knew him best. The account I have given of him has necessarily assumed the character of a Mosaic, made up of fragments furnished by different hands. I have thus supplied each reader with the means of drawing a likeness for himself, and it only remains that I offer very briefly my own estimate of the character of the Philosopher. Morally it was a blank, and can be described only by a series of negations. He did not love; he did not hate; he did not hope; he did not fear; he did not worship as others do. He separated himself from his fellow men, and apparently from God. There was nothing earnest, enthusiastic, heroic, or chivalrous in his nature, and as little was there anything mean, grovelling, or ignoble. He was almost passionless. All that needed for its apprehension more than the pure intellect, or required the exercise of fancy, imagination, affection, or faith, was distasteful to Cavendish. An intellectual head thinking, a pair of wonderfully acute eyes observing, and a pair of very skilful hands experimenting or recording, are all that I realise in reading his memorials. His brain seems to have been but a calculating engine; his eyes inlets of vision, not fountains of tears; his hands instruments of manipulation which never trembled with emotion, or were clasped together in adoration thanksgiving, or despair; his heart only an anatomical organ, necessary for of the circulation of the blood. Yet, if such a being, who reversed the maxim "nihil humani me alienum puto," cannot be loved, as little can he be abhorred or despised. He was, in spite of the atrophy or non development of many of the faculties which are found in those in whom the "elements are kindly mixed," as truly a genius as the *mere* poets, painters, and musicians, with small intellects and hearts and large imaginations, to whom the world is so willing to bend the knee. He is more to be wondered at than blamed. Cavendish did not stand aloof from other men in a proud or supercilious spirit, refusing to count them his fellows. He felt himself separated from them by a great gulf, which neither they nor he could bridge over, and across which it was vain to stretch hands or exchange greetings. A sense of isolation from his brethren, made him shrink from their society and avoid their presence, but he did so as one conscious of an infirmity, not boasting of an excellence. He was like a deaf mute sitting apart from a circle, whose looks and gestures show that they are uttering and listening to music and eloquence, in producing or welcoming which he can be no sharer. Wisely, therefore, he dwelt apart, and bidding the world farewell, took the self-imposed vows of a Scientific Anchorite, and, like the Monks of old, shut himself up within his cell. It was a kingdom sufficient for him, and from its narrow window he saw as much of the Universe as he cared to see. It had a throne also, and from it he dispensed royal gifts to his brethren. He was one of the unthanked benefactors of his race, who was patiently teaching and serving mankind, whilst they were shrinking from his coldness, or mocking his peculiarities.* He could not sing for them a sweet song, or create a "thing of beauty" which should be "a joy for ever," or touch their hearts, or fire their spirits, or deepen their reverence or their fervour. He was not a Poet, a Priest, or a Prophet, but only a cold, clear Intelligence, raying down pure white light, which brightened everything on which it fell, but warmed nothing—a Star of at least the second, if not of the first magnitude, in the Intellectual Firmament.

His Theory of the Universe seems to have been, that it consisted *solely* of a multitude of objects which could be weighed, numbered, and measured; and the vocation to which he considered himself called was, to weigh, number, and measure as many of those objects as his allotted three-score years and ten would permit. This conviction biassed all his doings, alike his great scientific enterprises, and the petty details of his daily life. *Πάντα μέτρῳ, καὶ αριθμῷ, καὶ σταθμῷ,*

* Cuvier recounts this pleasing anecdote of Cavendish's austere liberality:—"Un jour le gardien de ses instrumens vint lui dire avec humeur qu'un jeune homme avait cassé une machine très-précieuse; '*Il faut*,' repondit-il, '*que les jeunes gens cassent des machines pour apprendre à s'en servir; faites-en faire une autre*"—(Eloges Historiques, t. i., p. 104.)

was his motto; and in the Microcosm of his own nature he tried to reflect and repeat the subjection to inflexible rule, and the necessitated harmony, which are the appointed conditions of the Macrocosm of God's Universe. The little peculiarities of his domestic affairs, which might otherwise appear trivialities, on which only the spirit of idle gossip could dwell with relish, have for me a much deeper interest as tokens of a strongly developed will, which gave a singular consistency and unity to all the proceedings of its possessor. Cavendish did all things in the same spirit. He was a hero (to the extent of his heroism) even to his valet-de-chambre. Throughout his long life, he never transgressed the laws under which he seems to have instinctively acted. Whenever we catch sight of him we find him with his measuring-rod and balance, his graduated jar, thermometer, barometer, and table of logarithms; if not in his grasp, at least near at hand. Many of his scientific researches were avowedly *quantitative*. He weighed the Earth; he analysed the Air; he discovered the compound nature of Water; he noted with numerical precision the obscure actions of the ancient element Fire. Each, like some visitor to a strange land, was compelled to submit to a scrutiny, in which not only its general features were noticed, but everything pertaining to it, to which a quantitative value could be attached, was set down in figures, before it went forth to the scientific world, with its passport signed and sealed. The half-mythical calendar of the Hindoos was submitted to the same ordeal, and made to yield consistent numerical results. The electricity of the Torpedo; the freezing of mercury; the appearance of an Aurora Borealis; the hardness of a London pump-water; the properties of carbonic acid and of hydrogen, and much else, were equally subjected to a canon which knew of no limitations, and required that every phenomenon and physical force should be held to be governed by law, and admit of expression in mathematical or arithmetical symbols. It seems, indeed, to have been impossible for Cavendish to investigate any question otherwise than quantitatively. If he is making hydrogen, he tells us how much zinc, or iron, or tin he took; and what quantity of gas its solution in sulphuric or muriatic acid yielded, although he had no apparent purpose to serve in measuring the volumes of elastic fluid produced. If he plunges a candle into a mixture of nitrogen and air, or carbonic acid and air, he counts carefully the number of seconds during which it burns, and with unwearied patience varies the proportion of the gases. If he is preparing oxygen, he records in his note-book the weight of mercury he took, the quantity of nitric acid in which he dissolved it, and the amount of gas which the resultant oxide of mercury yielded, although he need have attended to nothing except that he had pure oxygen. It would, apparently, have been painful to him to have experimented otherwise. Nor was this all: he insisted on the trivial routine of outward life, following a law as inflexible and imperative as that which rules the motions of the stars. He wore the same dress from year to year, taking no heed of the change in fashions. He calculated the advent of his tailor to make a new suit of clothes, as he would have done that of a comet, and consulted the almanac to discover when the artist should appear.* He hung up his hat invariably on the same peg, when he went to the meetings of the Royal Society Club. His walking-stick was always placed in one of his boots, and always in the same one.† He dispensed charity by a singular numerical rule, not according to the deserts of those for whom assistance was craved, into whose wants he made no inquiry. He settled beforehand the value of a commodity which he wished to purchase, and referred to it as if its worth in money admitted of as precise an arithmetical determination, as the com-

* "Ses habillements ne changeaient jamais de forme, de couleur, ni de matière; constamment vêtu de drap gris, on savait d'avance, par l'almanach, quand il fallait lui faire un habit neuf, de quelle étoffe et de quelle couleur il fallait le faire; ou si, par hasard, on oubliait l'époque de cette mutation, il n'avait besoin, pour la rappeler, que de proférer ce seul mot, *le tailleur*."—(*Biographie Universelle*, tome vii. p. 456.)

A similar account as to Cavendish's possessing no wardrobe, and owning but one suit of clothes at a time, was given to Mr. Tomlinson by Mrs. Herbert, of Cavendish House, Clapham Common.

† "His boots were brought down and put against the dining-room door always in one spot, and in one particular position, with the point of his stick standing in one particular boot."—(Information given to Mr. Tomlinson by Mrs. Herbert.)

Cuvier relates a similar fact: "Quand il montait à cheval, il devait trouver ses bottes toujours au même endroit, et le fouet dans l'une des deux, et toujours dans la même!" Cuvier, however, probably mistook the whip for the walking stick, for it does not appear that Cavendish was an equestrian.

bining proportion of a chemical element or the orbit of a planet.* When he rode out in his carriage, he measured the number of miles which he travelled by a *way-wiser* attached to the wheels.† He would not take books out of his own library, without giving a receipt for them, nor indeed willingly do anything otherwise than in the most simple, uniform, and methodical manner possible.

Such was he in life, a wonderful piece of intellectual clock-work; and as he lived by rule, he died by it, predicting his death as if it had been the eclipse of some great luminary (which in truth it was), and counting the very moment when the shadow of the unseen world should enshroud him in its darkness.

Whatever, accordingly, we may think of the ideal which Cavendish set before him, we must acknowledge that he acted up to it with undeviating consistency; and that he realised it to a far greater extent than most men realise the more lofty ideals which they set before them. The pursuit of truth was with him a necessity, not a passion. In all his researches he displayed the greatest caution, not from hesitation or timidity, but from his recognition of the difficulties which attend the investigation of nature; from

* "When any one called upon him with a subscription list for some charitable or benevolent object, it was his custom to look down the list for the *largest* subscription. He would then pull out his cheque-book, and write a cheque for the amount of the largest sum subscribed by any one individual, neither more nor less. This practice becoming known, some persons, thinking, perhaps, that a small sin is justifiable if it lead to a great good, would enter a large nominal amount in their subscription list, and thus cheat Cavendish into a larger subscription than he would otherwise have given."—(Information supplied to Mr. Tomlinson by Dr. Sylvester, of Clapham.)

The following curious fragment of the draft of a letter I found among the Cavendish MSS. on one side of a sheet of paper entitled "Musical Intervals," and occupied with figures. It is printed verbatim, but the italics are mine—

"Sir,

"You would have heard from me sooner if it was not that I had [blank]

"I forgot to ask you yesterday when you would have me return the plans you sent me. I would have told you yesterday how much I would give for the estate, had it not been that it is so much less than what you said you had refused that I thought it to no purpose. If, however, you have a mind, I will let you know what I think it worth, and at the same time, *as I hate hagling*, will tell you the utmost I will give for it, but in that case you may depend upon it that I shall not offer any more."

† The way-wiser (an antique wooden instrument) is now in the museum of King's College, London, to which it was presented by Mr. Newman, of Regent Street.

his delight in reducing everything to numerical rule, and his hatred of error as a transgression of law. *Cavendo tutus* was the motto of his family, and seems ever to have been before him.

He died as he had lived, taking no pains to perpetuate the memory of a fame which could not be kept from proclaiming itself, even during his lifetime. His enormous wealth he left to his relatives, who would not have grudged, had he bequeathed some small portion of his great possessions to the furtherance of the sciences, to which his life was devoted. But from his kinsmen he had received his wealth, and to them, increased a hundredfold, he returned it.* His scientific successors, who would have been grateful in their early struggles for some pecuniary help towards successfully prosecuting studies, which do not always secure to their prosecutor even daily bread, have remembered that he, who forget them in his last testament, forgot also himself; and spent none of his wealth in prolonging his memory upon earth. He has enriched us all, by his lessons and his example, by his methods of research and his great discoveries; and we have paid him only an honour which he deserved, when we named ourselves after him, and founded a Cavendish Society.

* Cavendish left a considerable legacy to the Earl of Besborough, who was not, I believe, a connexion of his, in consequence, as is stated, of the pleasure he derived from the Earl's conversation at the Royal Society Club dinner. His Lordship was not a man of science. The immense bulk of Cavendish's wealth, however, went to his brother, and to Lord George Cavendish and his family.

CAVENDISH AS A CHEMIST.

The published Chemical Researches of Cavendish refer chiefly to the gases, and are contained in seven papers contributed to the *Phil. Trans.*, and published at intervals from 1766 to 1788. I shall take up those papers in their chronological order, as they form a series, naturally following each other. Their consideration forms the best introduction to the discussion of the controversy regarding the discovery of the composition of water, which cannot, in truth, be understood without an acquaintance with them. After its consideration, the three remaining chemical papers, which treat of Congelation, will be discussed.

Cavendish's pneumatic researches are remarkable for the number of discoveries they unfolded. They contain, besides less important announcements, the first full exposition of the properties of hydrogen and carbonic acid; the demonstration of the constancy in composition of atmospheric air, and its first tolerably accurate quantitative analysis; the record of the famous experiments which led to the detection of the non-elementary nature of water, and by an extension, and slight modification, to the discovery of the composition of nitric acid.

When Cavendish began those fruitful labours, pneumatic chemistry had barely come into existence. More than one chemist in different countries, and at different periods, had noticed and described the production of permanent elastic fluids as an accompaniment of chemical reactions. Paracelsus had some slight acquaintance with hydrogen.* Van Helmont, the introducer of the word *gas*, had distinguished more or less explicitly carbonic acid, and certain of the combustible gaseous compounds of carbon, and sulphur, with hydrogen.† Boyle

* Hoefer, *Hist. de la Ch.*, t. ii., p. 16.

† *Op. cit.*, t. ii., pp. 142—144.

had encountered carbonic acid and hydrogen,* and Mayow† was familiar with the latter.

Those chemists, however, had at best but a faint conception of the individual gases, as specifically distinct substances, and were too little acquainted with their unlike properties, to be successful in convincing themselves or others, that each gas had constant characters, by which its identity might always be recognised. A belief that air might be generated, *de novo*, sometimes as it appeared, identical with atmospheric air, sometimes different, was common probably to all the chemists of the latter part of the eighteenth century. But beyond this they had not got. The extension of pneumatic chemistry could result only from a study of the *differences* which the several artificial airs presented; but chemists paid little attention to those differences, or explained them away, when they were too striking to escape notice, and dwelt only, or chiefly, on the points of similarity, or identity, between the gases they described, and atmospheric air.

To such a length was this exclusive consideration of the *common* properties of atmospheric air and the gases carried, that Stephen Hales, in his celebrated Statical Essays,‡ pronounced them substantially identical. From the details he gives, he must have prepared in the course of his researches, oxygen, hydrogen, nitrogen, chlorine, carbonic oxide, carbonic acid, sulphurous acid, and coal gas; besides other gases. Nor did he fail to observe the diversities in odour, colour, solubility in water, combustibility, respirability and the like, which occurred among those elastic fluids. Nevertheless, he looked upon them as identical with atmospheric air, because they agreed with it in elasticity, and as it also seemed, from his inaccurate determinations, in specific gravity. Their striking differences in sensible characters, he regarded as resulting from the casual impregnation of the one true air, with foreign matters, not as essential and distinctive properties of specifically dissimilar elastic fluids. After the fashion of his day, accordingly, he spoke vaguely of

* *New Physico-Mechanical Experiments*, 1659; and *New Experiments touching the relation between Flame and Air*, 1671.

† Hoefer, *Hist. de la Chim.*, t. ii., p. 268.

‡ *Vegetable Staticks*, collected in a vol. in 1727; and *Haemastaticks*, 1732. The chemical results are given in appendices.

the air being "tainted" or "infected" with certain hypothetical "fumes," "vapours," or "acid and sulphurous spirits."*

The Rev. W. V. Harcourt has dwelt at length on the comparatively clear apprehension which Boyle and his immediate successors had of the fact, that there existed other permanently elastic fluids than air.† But, in truth, their experiments went no further than to show that a permanent gas was frequently developed during chemical changes, and if they held that in any case this gas was different from atmospheric air, it was but an opinion. They entered into no proof of its justice, and, for anything they published to the contrary, the gases they examined might have been common air, altered in certain of its properties by the intermixture with it, of other substances.

The approbation with which Hales' Essays were welcomed over Europe, and the tacit assent which was accorded to his general conclusion that there is but one true air, show how slight and unabiding was the impression which Boyle's faint discrimination of "factitious airs," had made upon his successors. More than twenty years elapsed after the publication of the Haemastaticks, before any one disputed the justice of Hales' views; nor was it then done directly.

In 1754, however, the appearance of Black's celebrated inaugural dissertation, demonstrated the existence of at least one air, possessed of constant chemical properties, unlike those of the atmosphere.‡ It proved this incidentally; for the chief

* A fuller account of Hales' chemical labours will be found in the *British Quarterly Review* for August, 1845, pp. 229—233.

† *London and Edinburgh Phil. Mag.*, Feb. 1846, p. 123.

‡ Considerable confusion exists as to the date of Black's earliest publication on fixed air, and the uncertainty which prevails on this subject has been increased by the contradictory numbers which Prof. Robison gives in his edition of the Chemist's Lectures. The latter is made to say that the year in which his first account of fixed air was published, was 1757 (*Lectures*, vol. ii. p. 87). This, however, is certainly a mistake, resulting from an oversight, either on the Author or Editor's part; for a printed copy of Black's *Inaugural Dissertation* is preserved in the Library of the University of Edinburgh, and I find on its title-page the date 1754. The treatise is styled *Dissertatio Medica Inauguralis de humore acido a cibis orto et Magnesia Alba.* It consists of two sections; the first strictly medical, and apparently intended to justify the presentation of the essay as a *Dissertatio Medica;* the second chemical, and containing the views of the Author on fixed air. When the dissertation was published in English, the first section was omitted, and the second was entitled "Experiments upon magnesia alba, quick-lime, and other alkaline substances," and appeared along with an "Essay on Evaporation," by Dr. Cullen. The

object of the essay was not to offer a formal denial of the prevailing views, regarding a one universal or elementary air, but to assign a reason for the difference in properties between the caustic and mild, alkalis and alkaline earths.

In his dissertation Black says very little about fixed air as an elastic fluid, and does not profess to have ascertained many of its properties when free. He states distinctly his conviction that it is different from common air, and gives some reasons for his opinion; but he excuses himself from assigning to it a specific name, and from entering on an exposition of the characters of the gas, "which will probably be the subject of my further inquiry."*

Nevertheless, Black made a great advance beyond all his predecessors. Hales, confirming and immensely extending the older views, had shown by the amplest evidence, that air or gas was an abundant constituent of most substances. Black now showed that in one case at least it was not less abundant than important as an element of bodies. He entered into no minute discussion of the secondary doctrine which his essay embodied, and did not, in truth, except in the briefest terms, directly enforce it, but the conclusion which his researches almost unavoidably compelled, was, that a gas which by its absence or presence, made all the difference between quick lime and chalk, between a mild and a caustic alkali, must be something quite peculiar, and very unlike Common Air.†

copy from which I quote bears date 1782, and is called the fourth edition, but it is a simple reprint of the earlier issues. Black's experiments were first printed in English in the second volume of "Essays and Observations, Physical and Literary, read before a Society in Edinburgh," p. 172. This volume appeared in 1770, but Black's paper is dated June 5, 1755, the period, probably, when it was read to the Society.

I note these dates, because some discussion has recently occurred as to the exact period of Black's discovery. Robison's date of 1757 cannot be considered as authenticated by Black, as it was not published till after his death; 1755, on the other hand, was the date given during his lifetime, and is more trustworthy. To the world at large Black's opinions were not fully known till they were printed in the Edinburgh Essays in 1770; but he had publicly announced them to the University of Edinburgh in 1754, and taught them from the Chair of Chemistry in the University of Glasgow from 1756 downwards.

* *Experiments upon Magnesia*, &c., p. 72.

† M. Jaquin, who defended Black's general views against his foreign assailants, nevertheless held that fixed air was identical with common air. Hoefer, *Hist. de la Chim.*, t. ii., p. 364.

Here Black, so far as he has published his views in his Experiments on Magnesia, left the subject, content to show that carbonic acid had very marked properties when *fixed;* and Cavendish, who was to some extent anticipated by Macbride, showed, twelve years later, that it had equally marked properties when free. Many of their predecessors deserve most honourable mention in connexion with pneumatic chemistry, but Black was the founder of the chemistry of the gases. The word gas had no certain plural till his time, and Cavendish was his acknowledged pupil.

His first communication on the gases is entitled "Three Papers containing Experiments on Factitious Air," and was published in 1766.* It begins with a definition of factitious air, as "any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art." This is followed by a reference to Dr. Black, whom he states his intention of following, in applying the name Fixed Air to the gas contained in the earthy and alkaline carbonates. He discusses inflammable air, however, before carbonic acid. Of the former he gives no definition, but he employs the name (inflammable air) as one already in use, and familiar to his readers. Van Helmont had pointed out that certain of the intestinal gases burn with a peculiar flame.† Boyle‡ and Lemery§ recorded their observation of the combustibility of hydrogen; a phenomenon which it is probable many others also noticed. Hales had prepared many varieties of combustible gas or inflammable air; among others, coal gas. The fire-damp of mines had likewise begun to attract the attention of scientific men;|| and the title *Inflammable Air*, appears to have come gradually into use, by general consent, to distinguish all the known gases which were combustible in air. Some such title was plainly necessary after the recognition of fixed air as a distinct gas. It was a general term applied to all combustible gases, but admitted of limitation by connecting it with the source of the gas. Thus Cavendish

* *Phil. Trans.*, 1766, p. 141.

† Hoefer, *Hist. de la Chim.*, t. ii., p. 144.

‡ *New Experiments touching the relation between Flame and Air*, 1671.

§ Hoefer, *Hist. de la Chim.*, t. ii., p. 297.

|| *Phil. Trans.*, 1765, p. 219.

refers in his paper to inflammable air *from* the metals, and to inflammable air *from* putrefying animal matters.

As a necessary prelude to an account of his experiments and their results, he gives a description of his pneumatic apparatus. In its general arrangements it was identical with that of Hales, and inferior to Priestley's. Cavendish's pneumatic trough had not the latter's simple but important addition of the Shelf, so that, like Hales, he hung his gas-jars, or bottles, by strings, with their mouths downwards, below the surface of the water.* In other respects his arrangements were sufficient, but more effective than elegant. He made almost no advance on his immediate predecessor in the invention of apparatus for collecting and preparing gases, and left important improvements to be suggested and introduced by his successors. Cavendish, in truth, was not remarkable for an inventive spirit, but eminently conspicuous for setting before him a standard of accuracy in working, such as few of his fellow-chemists at that period cared to acknowledge. His strong mathematical bias, induced him to seek for quantitative results in all his researches, and he modified apparatus to make this attainable, where the instruments in use were not of service. But if the apparatus ready to his hand was sufficient for his purpose, he took it as he found it, without spending time on its improvement. His great caution and love of simplicity, made him averse to novel or complicated arrangements, and he suggested very few. No two persons, in truth, were more unlike in this respect than he and Priestley, who was inexhaustible in contrivances, and unhesitating in trying them. The latter, I think, is entitled to the first place among devisers and introducers of chemical pneumatic apparatus; and next to him comes Hales, who preceded him in time. Between them, they leave little merit to be ascribed to Cavendish as a mechanical inventor, but he made better use of his scanty apparatus as an analyst of the gases than either of them did.

* Brownrigg, perhaps, gave the first idea of the Pneumatic Shelf in 1765. His shelf, however, was above the level of the cistern or trough, on which it was fixed as a perforated lid or cover, and the jars were prevented by wedges from sinking too deeply through the holes in the board, which were wider than the jars. It was a *rack*, therefore, rather than a shelf, and less convenient than Hales' and Cavendish's method of suspension. (*Phil. Trans.*, 1765, p. 235.)

He divides his paper on factitious air into three parts. The first treats of Hydrogen, the second of Carbonic Acid, and the third of the Gases evolved during Fermentation and Putrefaction. Some discussion has recently occurred as to whether or not Cavendish should be regarded as the discoverer of hydrogen. It seems needless, however, to raise the question. He does not himself claim the discovery, which we have seen had been made in the previous century by Boyle and others, but refers to the gas as one already familiar to those he addresses. In one place he prefaces his account of experiments on the explosibility of a mixture of air and hydrogen, by the statement, "it has been observed by others." Who those were, may be learned from Dr. T. Thomson's statement, in reference to hydrogen, that "its combustibility was known about the beginning of the eighteenth century, and was often exhibited as a curiosity."* He adduces two authorities in support of this statement, one, a work (*Cramer's Elementa Docimasia*) published in 1739.

The chief facts which Cavendish observed were the following. Zinc, iron, and tin were the only metals which he found to generate inflammable air when dissolved in acids, and that only by solution in diluted sulphuric or muriatic acid. Zinc dissolved in both acids with greater rapidity than iron or tin, but yielded the same amount of gas, whichever acid was employed. Iron yielded the same quantity of inflammable air, in specimens of dilute sulphuric acid of different strengths. Tin dissolved best in warm muriatic acid. An ounce of zinc produced about 356 ounce measures of gas; the same weight of iron 412, and of tin 202 ounce measures.

All those metals dissolved readily in nitrous (nitric) acid, and generated air (nitric oxide), which was not inflammable. They also dissolved with effervescence in hot oil of vitriol, and discharged "plenty of vapours which smell strongly of the volatile sulphurous acid, and which are not at all inflammable."

From those observations Cavendish concluded, that when the metals in question are dissolved in dilute sulphuric or muriatic acid, "their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air;" but when they are dissolved in nitrous acid or strong oil of vitriol, the

* *System of Chemistry*, 6th ed., vol. i., p. 217.

phlogiston of the metals unites to the acid used for their solution, and flies off with it in fumes, and the phlogiston loses its inflammability. The sulphurous acid which is evolved when oil of vitriol is employed, is thus represented as being phlogisticated sulphuric acid (as Stahl, indeed, named it);* a compound of the phlogiston of the metals with the oil of vitriol of the acid. What change the nitrous acid underwent, Cavendish was not certain; but with that ready reference to phlogiston as the key to all difficulties, which so strikingly characterises even the ablest chemists of last century, he observes that the change which the acid had undergone, "can hardly be attributed to anything else than its union with the phlogiston." The inflammable air, on the other hand, he thought not likely to consist of any combination of phlogiston and acid, because its quality was the same whether sulphuric or muriatic acid was used in preparing it; and "also, because there is an inflammable air, seemingly much of the same kind as this, produced from animal substances in putrefaction;" and "there can be no reason to suppose that this kind of inflammable air owes its production to any acid."

From the preceding quotations it will appear that Cavendish believed the hydrogen which was evolved, to proceed, not from the diluted acid, but from the metal as it underwent solution, and in truth to be the very phlogiston of the metal in the gaseous form. This is the first announcement of the identity of phlogiston with inflammable air, which ultimately became one of the cardinal doctrines of the disciples of the later Phlogiston school. Cavendish afterwards changed his opinion, and held that inflammable air was in all probability a combination of phlogiston and water,† as will fully appear when his views concerning the nature of water are under discussion. This alteration of view has led to his earlier opinion being overlooked in the course of the Water Controversy, and much unnecessary criticism has been expended on his later, and as it is assumed, his only conclusion concerning phlogiston.‡ It seems well, therefore, for the sake of its subsequent application, to notice that not only did Cavendish originally hold that inflammable

* Kopp, *Geschichte der Chemie*, i. theil, p. 232.

† *Phil. Trans.*, 1784, p. 140.

‡ Watt Corresp., p. ciii., *Edinb. Rev.*, January, 1848, pp. 103, 104.

air was phlogiston, but he was the first, at least in England, who broached this doctrine. It was afterwards taken up by Priestley and Kirwan, and its truth apparently demonstrated by special experiments, so that Cavendish referred to it as their doctrine, not his own.* With him it was simply an hypothesis. The reasons which induced him to change his view, I shall afterwards consider, as they are important in reference to his speculations on the composition of water. It is important, however, to notice, that Priestley and Watt changed their opinions concerning the nature of inflammable air in the same way.† According to the final belief of all three, it was what we should now call a hydrate of phlogiston.

The properties of hydrogen which Cavendish observed were the following. It did not lose its elasticity by keeping, and was not sensibly absorbed by water, or by fixed or volatile alkalis. Others had remarked its explosibility with common air, and he proceeded to try the effect of varying the proportions of air and hydrogen. A mixture of one part of inflammable air and nine of common air would not burn at the mouth of a bottle, but allowed a flame to spread through it. A mixture, on the other hand, of 8 parts of inflammable air and 2 of common air, burned, but did not explode. When about twice or four times as much hydrogen was taken, a loud explosion was heard. From these experiments Cavendish drew the general conclusion, that inflammable air, like other inflammable substances, "cannot burn without the assistance of common air," and that it must be mixed with more than its own volume of the latter, to produce complete combustion. He seems, however, to have over-estimated the volume of common air required, for he mixed 2 volumes of hydrogen with more than 7 of air, whereas 5 of the latter would have sufficed.

It is not a little curious that Cavendish should make no reference in the record of his experiments on the inflammability of hydrogen, to the appearance of moisture as an accompaniment or product of the combustion of the gas. He certainly overlooked the phenomenon at this time, for at a later period he referred its first observation to Warltire, who experimented in 1781.‡

* *Phil. Trans.*, 1784, p. 137. † *Op. cit.*, p. 330. ‡ *Ibid.*, p. 126.

An additional series of trials was made, with a view to ascertain whether "the air produced from different metals by different acids," was equally inflammable. Five different sorts were used: 1. A recently prepared specimen from zinc and sulphuric acid. 2. A similar specimen which had been kept for a fortnight. 3. Gas from zinc and hydrochloric acid. 4. The same from iron and sulphuric acid. And 5. From tin and hydrochloric acid. No difference could be observed in their relative inflammability.

The specific gravity of the four last-mentioned varieties was then tried, and the process followed is interesting as the first *successful* attempt to compare the density of a gas with that of common air. A bladder was used as the containing vessel. It was filled with hydrogen, emptied to get rid of traces of atmospheric air, and filled again with the former gas. It was then weighed, and thereafter the hydrogen was replaced by air, and the bladder weighed a second time. A bladderful of hydrogen was thus found to weigh about 41 grains less than the same volume of common air. If, therefore, the density of air be assumed to be 800 times less than that of water (which Cavendish thought must be near the truth), then hydrogen will be 7 times lighter than common air; but if the latter be 850 times lighter than water, as Hauksbee estimated it to be, then hydrogen will be nearly 11 times lighter than air. Either of these numbers, or the intermediate one which would have been obtained had air been taken as 815 times lighter than water, are, I need not say, much too high for pure hydrogen. Cavendish, however, was so far at least aware of this, and points out the uncertainty that attended the settlement of the density of common air, and the difficulty of preventing air from mixing with the hydrogen, and the diffusion through the latter of water-vapour. He determined the amount of water by forcing a known quantity of hydrogen through a glass tube containing pearl-ashes, which he weighed before and after the passage of the gas. In this way he found inflammable air to contain nearly $\frac{1}{9}$th of its weight of moisture. To check the results obtained with the bladder, he made another series of determinations of the specific gravity of hydrogen, upon a different principle. His apparatus exactly resembled one of those employed at the present day for ascertaining the amount of carbonic acid in a limestone. It consisted of a glass bottle nearly filled with dilute sulphuric acid, to the neck of which a drying tube was luted containing pearl-ashes in coarse powder, to arrest the water-vapour which accompanied the hydrogen. This apparatus was carefully weighed, and also a portion of zinc. The latter was thereafter introduced into the bottle, the tube luted, and the whole left till the metal had entirely dissolved. The apparatus was then weighed a second time, and the *weight* of the hydrogen which had been discharged thereby ascertained. The volume of gas which this weight represented was learned by a reference to former experiments, in which the number of grain measures of gas which a given weight of zinc would evolve, had been ascertained. In one of the trials in this way, of the density of hydrogen, 254 grains of zinc had been dissolved. From the previous experiments, that weight of metal must have set free 90,427 grain measures of hydrogen, and the second weighing showed that the weight of this was $10\frac{3}{4}$ grains. The density of hydrogen as determined in this way was $10\frac{1}{2}$ times less than that of air.

Similar trials were made with hydrogen from zinc and hydrochloric acid, and from tin and hydrochloric acid. "By a medium of the experiments, inflammable air comes out 8,760 times lighter than water, or 11 times lighter than common air." Hydrogen, however, in reality is 14·4 times lighter than air.

Cavendish appears to have been the first who employed that important little pneumatic instrument, the drying tube, for depriving gases of moisture. His second method of determining the density of hydrogen was both original and ingenious; and his first was unexceptionable in principle, and brought out a result, such as no previous experimenter had obtained. Cavendish, indeed, has been referred to as "the first person who attempted to determine the specific gravity of airs, by comparing their weight with that of the same bulk of common air."* Mr. W. V. Harcourt, however, has shown that Hauksbee as well as Greenwood preceded Cavendish in this attempt.† So also did

* Thomson's *Hist. of Chemistry*, vol. i., p. 343; and Lord Brougham's *Lives of Men of Letters and Science of the time of George III.*, p. 431.

† *Lond. and Edinb. Phil. Mag.*, Feb. 1846, pp. 120, 121.

Hales, who compared the weight of 540 cubic inches of 'air of tartar' with that of the same volume of common air. He used a pear-shaped glass vessel, open below, the mouth of which he closed with a piece of bladder during the weighing.*

None of those observers, however, detected any difference between the density of the gases they examined and that of common air; partly, because, as in Hauksbee's case, the difference was small, but chiefly, because the vessels were too large, and the balances they employed not sufficiently delicate. Hales, too, had evidently prejudged the question, and did not expect his airs to differ in specific gravity. On the other hand, he thought a common density next to a common elasticity, the best proof that the various gases he prepared were "true air, and not a mere flatulent vapour."†

Cavendish, then, was not the first who investigated the specific gravity of the gases, but he was the first who ascertained that they have different densities.

The paper on inflammable air concludes with an account of trials, as to whether or not it could be obtained by the action of copper on hydrochloric acid. Cavendish found that inflammable air could not be procured in this way, but that a gas was produced which "immediately loses its elasticity, as soon as it comes in contact with the water." This elastic fluid, which was gaseous hydrochloric acid, he did not examine. In 1772, Priestley repeated this experiment, and speedily discovered that neither copper nor any other metal was essential to the evolution of the condensible gas, which was yielded abundantly by spirit of salt when it was heated, and could be collected over mercury. He called the elastic fluid Marine Acid Air.‡

The title of the second part of Cavendish's paper runs thus: *Experiments on Fixed Air, or that species of Factitious Air which is produced from Alkaline Substances, by Solution in Acids, or by Calcination.*

The properties of carbonic acid, apart from its relation to the mildness and causticity of alkalis, had been investigated to some extent, as we have seen already, by Dr. Black and Macbride before Cavendish studied them. As some difference of

* *Vegetable Staticks*, 2nd ed., p. 190. † *Op. et loc. cit.*
‡ *Experiments on Air*, 1775, vol. i., p. 143.

opinion has been expressed recently, as to the extent to which Dr. Black had anticipated Cavendish in reference to carbonic acid, it is desirable to notice that the former published no detailed account of the characters of fixed air.* From his lectures it appears that in 1754 he had discovered many of the properties of carbonic acid, and these, it cannot be doubted, he exhibited to his students at the University of Glasgow, from 1756 downwards. It is certain, however, that, unless in his lectures, he published nothing on the subject, except his Inaugural Dissertation of 1754; and a reference to that work will decide whether Cavendish took up new ground in 1766, or only repeated what Black had already made known, not merely to his class-pupils, but also, through the press, to students of science at large. The only properties of free carbonic acid, which are referred to in the "Experiments upon Magnesia Alba, &c.," (which it will be remembered is the Chemical Section of the Inaugural Dissertation in an English dress,) are its solubility in water, and its production of a precipitate with lime-water.†

Black thought that it performed the function of an acid, and "that as the calcareous earths and alkalis attract acids strongly, and can be saturated with them, so they also attract fixed air, and are in their ordinary state saturated with it;"‡ but he makes no reference to its taste, or to its action on colouring matter, as proofs of its possessing acid characters.

The only other point, except its functional acid character, upon which he insists, is, that fixed air is quite distinct from common air; but he reserves an investigation into the points of difference for a future research, which he only partially completed, and never made public through the press. The following passage will show how much was left undone by Black: "Quicklime, therefore, does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, which is dispersed through the atmosphere, either in the

* Lord Brougham's *Lives of Men of Letters and Science of George III.'s Reign,* p. 330; and Harcourt's *Letter, Lond. and Edinb. Phil. Mag.,* Feb. 1846, p. 118.

† *Experiments on Magnesia Alba,* p. 56.

‡ *Op. cit.,* p. 50.

shape of an exceedingly subtle powder, or more probably in that of an elastic fluid. To this I have given the name of *fixed air*, and perhaps very improperly; but I thought it better to use a word already familiar in philosophy, than to invent a new name, before we be fully acquainted with the nature and properties of this substance, which will probably be the subject of my further inquiry."*

Cavendish prepared carbonic acid by dissolving marble in muriatic acid. He found that the gas was soluble in water, and was rapidly absorbed by the caustic alkalis, but might be preserved over mercury for upwards of a year, without any loss of elasticity or change of property. To determine the extent to which carbonic acid is soluble in water, he made use of a mercurial pneumatic trough, a piece of apparatus which Priestley has been supposed to have been the first to employ. Into a graduated jar filled with mercury, he passed up measured volumes of gas and water at intervals, and ascertained in this way "that water, when the thermometer is at 55°, will absorb rather more than an equal bulk" of the fixed air. In the course of these experiments, however, he found that water did not always absorb the same amount of gas; and conceiving the latter to be pure, he drew from this observation the conclusion, that the "fixed air contained in marble consists of substances of different natures, part of it being more soluble in water than the rest." On this opinion of Cavendish's, Black passed the sagacious criticism: "I suspect, however, that this was a deception, proceeding from the common air which water contains, and which arises with the fixed air during the extrication of this last from the alkaline substances."† With this criticism, another celebrated chemist concurred. "Dalton has since given," says Dr. Thomson, "a satisfactory explanation of this seeming anomaly, by showing that the absorbability of fixed air in water is proportional to its purity, and that when mixed with a great quantity of common air, or any other gas not soluble in water, it ceases to be sensibly absorbed."‡ It has been overlooked, however, that Cavendish was aware of the

* *Experiments on Magnesia Alba*, p. 72.

† *Lectures* by Robison, vol. ii., p. 91.

‡ Thomson's *Hist. of Chem.*, vol. i., p. 342.

fact pointed out by Dalton. In 1784, the former wrote thus: "Though fixed air is absorbed in considerable quantity by water, as I showed in *Phil. Trans.*, vol. lvi., yet it is not easy to deprive common air of all the fixed air contained in it, by means of water. On shaking a mixture of ten parts of common air to one of fixed air, with more than an equal bulk of distilled water, not more than one-half of the fixed air was absorbed."*

In continuation of the inquiry, Cavendish ascertained that cold water dissolves more carbonic acid than hot; and "that water heated to the boiling-point is so far from absorbing air, that it parts with what it had already absorbed." Spirit of wine, the specific gravity of which is not given, was found at the temperature of 46° to absorb "near $2\frac{1}{4}$ times its bulk of the more soluble part of this air." Olive-oil very slowly absorbed more than an equal bulk of fixed air, the thermometer being between 40° and 50°.

The specific gravity of carbonic acid was determined with a bladder in the same way as that of hydrogen had been. Cavendish inferred its density to be 1·57, air being 1·0. This is a very fair determination, if allowance be made for the imperfection of the apparatus, and the presence of both water and muriatic acid in the gas, which passed directly from the gas-bottle into the bladder. In consequence of these impurities and the imperfections of the method adopted, the specific gravity of carbonic acid appeared to be greater than it really is. According to the more careful trials of recent observers, it is 1·529, not 1·570. A series of experiments was made on the influence of carbonic acid in arresting combustion, which led to the observation of the curious fact, that the presence of a comparatively small proportion of fixed air in common air, is sufficient to deprive the latter of the power of supporting flame. Thus a small candle burned 80 seconds in a closed jar full of common air. When the same receiver contained one part of fixed air to 19 of common air, the candle burned 51 seconds. When the fixed air was $\frac{3}{40}$ths of the whole mixture, it burned 23 seconds, and when the fixed air was $\frac{1}{10}$th of the whole, 11 seconds. It was extinguished immediately when the air contained less than $\frac{1}{9}$th of its bulk of fixed air. Cavendish draws

* *Phil. Trans.*, 1784, p. 122.

attention to the circumstance that the size of the candle is an element of importance in such trials, and that it must bear a certain proportion to the capacity of the gas-jar, because "large flaming bodies will burn in a fouler air than small ones." This, however, did not affect the validity of his conclusion, "that the power which common air has of keeping fire alive, is very much diminished by a small mixture of fixed air." Later inquirers have confirmed and extended this conclusion, which still remains, as Cavendish left it, without any theory having been offered in explanation of it.

The last series of experiments which Cavendish undertook in connection with this inquiry, had for its object the determination of "the quantity of fixed air in alkaline substances." As may be anticipated, his quantitative analyses are far from accurate, but they are interesting, from the period at which they were made; and the principles on which they were conducted are in many respects identical with those followed at the present day. Marble was analysed by finding the loss of weight which it underwent when dissolved in hydrochloric acid, contained in a weighed flask provided with a drying tube, which was filled with shreds of filter-paper to arrest moisture, instead of pearl-ashes; "for," says Cavendish, "pearl-ashes would have absorbed the fixed air that passed through them." Carbonate of ammonia effervesced too violently to be examined in this way. For its analysis, three vials were taken, and weighed with their contents in the same scale. One contained weak muriatic acid; the second held some lumps of carbonate of ammonia, and was corked to prevent evaporation of the salt; the third, in which the contents of the other two were to be gradually mixed, contained a little water, and had a paper-cap to arrest the small jets of liquid thrown up during the effervescence. When it had ceased, the three vials were again weighed, and the loss appears to have been set down as carbonic acid, without any deduction for the accompanying moisture. The general conclusion was, that in proportion to the quantity of acid it can saturate, carbonate of ammonia contains much more carbonic acid than marble does; but that different specimens of the ammoniacal salt differ considerably in composition. Cavendish then applies this observation to explain a phenomenon which had greatly perplexed him; namely, the occurrence of effervescence when a neutral solution of chloride of calcium was added to a solution of carbonate of ammonia. This effervescence he explains by a reference to the fact, that as lime requires less carbonic acid to saturate it, than is present in the salt of ammonia, the excess of gas which the lime cannot absorb flies off in an elastic form. He refers, in like manner, the non-precipitation of a salt of magnesia by carbonate of ammonia, to the alkaline earth being held in solution by the large amount of carbonic acid in the ammoniacal salt.

Pearl-ashes were analysed in the same way as carbonate of ammonia, with the substitution of diluted sulphuric for muriatic acid. When the effervescence was over, the neutral liquid was tested for free carbonic acid, as it was in the other analyses also, by the addition of lime-water. The precipitate which was produced in the case of the pearl-ashes, was collected, dried, and weighed; the proportion of carbonic acid in it, calculated on the assumption that the precipitate was identical in composition with marble, and added to that represented by the loss of weight.

The last carbonate which Cavendish analysed was bicarbonate of potash. Availing himself of a suggestion of Dr. Black's, he prepared this salt (probably for the first time), by slowly forcing carbonic acid from a bladder communicating with a gas-bottle, into a solution of pearl-ashes. Crystals gradually formed, which were analysed in the same way as the carbonate of ammonia had been. In preparing them, Cavendish was led to suspect that the carbonic acid from marble is not homogeneous in composition, but consists of portions not equally soluble in caustic alkalis, as he had formerly supposed it to be a mixture of gases which were not equally soluble in water. On this point, however, he speaks hesitatingly.

The large proportion of carbonic acid which he found in bicarbonate of potash, led him to anticipate that it would resemble carbonate of ammonia in its action on salts of lime and magnesia. He found, accordingly, that the bicarbonate precipitated chloride of calcium with effervescence, and that it gave no precipitate with sulphate of magnesia in the cold, but when heat was applied to the mixture, "a great deal of air was discharged, and the magnesia was precipitated."

The following table represents the composition which Cavendish assigned to the carbonates he analysed:—

Marble....................	1000 grs. contained	408	of fixed air.
Carb. amm.............	1000 „	533	„
Pearl-ashes	1000 „	284	„
Bicarb. potash........	1000 „	423	„

None of those numbers are accurate. It is impossible, however, to be certain what variety of carbonate of ammonia Cavendish anaylsed; and it can scarcely be doubted that the pearl-ashes were very impure. If all the salts had been pure, and quite accurately analysed, the numbers would have been as follow:—

Marble (CaO,CO^2).......	1000 grs. contain	440	of carb. acid.
Carb. amm. ($2NH^4O,3CO^2$)........	1000 „	559·32	„
Pearl-ashes (KO,CO^2)	1000 „	318·84	„
Bicarb. potash ($KOCO^2,HOCO^2$)	1000 „	400	„

It is curious to notice, in connection with these determinations of the quantities of carbonic acid necessary to saturate different bases, how long it was before its possession of acid characters, even when free, was detected. Black held, on theoretical grounds, that fixed air was an acid. Brownrigg attributed to it the peculiar taste of Spa water.* Cavendish determined its saturating power. None of those observers, however, made direct trial of its acid properties. Bergmann, who called it the aerial acid, was the first who discovered that it reddened vegetable blues.† He communicated this observation to Priestley, who mentions the fact.‡

The third part of Cavendish's paper details experiments "On the Air produced by Fermentation and Putrefaction." Macbride, following out a suggestion of Black's, had shown that these processes yield carbonic acid, and, as he conceived, only that elastic fluid.§ Cavendish confirmed this result, so far as the vinous fermentation of sugar and apple-juice was concerned. The gas these evolved, he found to be entirely absorbed by caustic potash, and to have the same solubility in water, action on flame, and specific gravity, as the fixed air from marble. He further showed that the common air which had remained in contact with the fermenting liquid suffered no change during the process, but detonated as sharply with hydrogen as that of the atmosphere.

* *Phil. Trans.*, 1765, p. 219.

† Hoefer, *Hist. de la Chim.*, ii., 444.

‡ *Experiments on Air*, (1775) i., p. 31.

§ Black's *Lectures*, vol. ii., p. 89.

The gaseous products of putrefaction were examined by keeping "gravy-broth" contained in a gas-bottle, at a temperature of about 96° as long as it gave off an elastic fluid. This was received in a bottle filled with solution of caustic potash, which absorbed the carbonic acid, and left a mixture of common air (transferred from the gas-bottle) and inflammable air (derived from the gravy), in the proportion, as Cavendish estimated, of one volume of the former to 4·7 of the latter. He ascertained the specific gravity of this mixture by filling with it "a piece of ox-gut furnished with a small brass cock," which he found more convenient than a bladder, for determining the density of small quantities of gas. He afterwards filled the ox-gut with a mixture of 4·7 volumes of hydrogen and one of common air, and found that it weighed less than the gas from the gravy, in the proportion of $4\frac{1}{2}$ to $4\frac{3}{4}$. He drew the conclusion, accordingly, that "this sort of inflammable air is nearly of the same kind as that produced from metals. It should seem, however, either to be not exactly the same, or else to be mixed with some air heavier than it, and which has in some degree the property of extinguishing flame, like fixed air." Raw meat was also found to yield inflammable air when it putrefied, but in smaller quantity than the gravy. It was not very minutely examined, but appeared to be of the same kind as that already described. A fuller reference to the difference between hydrogen and other inflammable airs, will be found in the 4th series of Experiments on Air, printed from Cavendish's MS. *Brit. Assoc. Rep.* 1839, p. 60.

Experiments on Rathbone Place Water*.

This paper may be regarded as to a great extent a continuation of an inquiry into the properties of fixed air, but it is also interesting as detailing one of the earliest tolerably accurate analyses of a mineral water. The experiments described were made about the same time as those detailed in the Researches on Factitious Air, but were recorded separately, as they included an examination of the solid as well as the gaseous contents of the water.

* *Phil. Trans.*, 1767, p. 92. Read to Royal Society Feb. 19, 1767.

It had long been noticed, as Cavendish observes, that "most waters, though ever so transparent, contain some calcareous earth, which is separated from them by boiling, and which seems to be dissolved in them without being neutralised by any acid, and may, therefore, not improperly be called unneutralised earth." The cause of the suspension of this earth was unknown, and with a view to discover it, the Rathbone Place water was selected for examination, "as it contains more unneutralised earth than most others."

The water in question was the produce of a large spring at the end of Rathbone Place, and at one time was raised by an engine to supply the neighbouring parts of London. Its fixed ingredients were first determined. To ascertain their amount, a measured quantity of the water was distilled, till between a third and a fourth had been drawn off. The earth which precipitated during the distillation was collected and dried. It was quite soluble in hydrochloric acid, and was, therefore, according to the canons of analysis of the day, "an absorbent earth," *i. e.*, carbonate of lime, or of magnesia, or a mixture of both.* To determine its nature more minutely, a second and larger quantity of the precipitate was saturated with oil of vitriol, which converted it in greater part into insoluble selenite, or sulphate of lime. The clear liquor strained from off the selenite, yielded on evaporation a small quantity of sulphate of magnesia, so that the precipitate contained both of the absorbent, or, as we should now call them, alkaline earths, with which chemists were then familiar.

The water in the still was then evaporated, first in a silver pan, and afterwards in a glass cup, to about three ounces. In the course of concentration, it deposited a little sulphate of lime; and it was found, to contain another sulphate, apparently sulphate of potash, and, in addition, chloride of sodium.

Much attention has lately been directed to the presence of nitrates in natural waters, but Cavendish and his contemporaries were well aware of what has been regarded as a recent discovery. He sought for nitric acid in the Rathbone Place water, because "many waters contain a good deal of neutral

* In the ordinary language of the day, clay, or rather perhaps alumina, was also an absorbent earth; but Cavendish appears to limit the term to lime and magnesia.

salt composed of the nitrous acid, united to a calcareous earth." He found no nitrate present, however; and he remarks, in reference to this point, "as I have heard of no other London water that has been examined with this view, but what has been found to contain a considerable proportion of nitrous salt, it seems very remarkable that this should be entirely destitute of it."

The water which distilled over, precipitated lime-water, sugar of lead, and corrosive sublimate, and changed vegetable blues to green. When mixed with a little sulphuric acid, and evaporated to dryness, it left a brownish salt, which gave off the odour of volatile alkali when lime was added to it. Cavendish determined approximatively the proportion of ammonia present in the water, by adding to a measured quantity of the distilled fluid, a slight excess of sulphuric acid, which was afterwards neutralised by the addition of a known weight of carbonate of ammonia. He appears, however, to have made no allowance for the loss of carbonic acid which attends the conversion of carbonate of ammonia into sulphate, for he deducts the whole weight of the former salt which he added to neutralise the excess of sulphuric acid, from the entire residue of sulphate of ammonia, produced; thus: "The volatile sal-ammoniac (carbonate of ammonia) contained in sixty-six grains of vitriolic ammoniacal salt (sulphate of ammonia) is 58½ grains." His estimate, therefore, must have been far wrong. It is singular, that after having determined the proportion of carbonic acid which carbonate of ammonia loses when dissolved in an acid (ante, p. 206), Cavendish should have omitted to allow for this loss in making his calculation.

He now proceeded to investigate the nature of the gases present in Rathbone Place water, prepared to expect that it would yield carbonic acid, from the investigations of Dr. Brownrigg, who had found "that a great deal of fixed air is contained in Spa water," and ready to connect the solubility of the calcareous earth found in the former water, with the presence of the fixed air expected to occur in it.

With this view a considerable quantity of the Rathbone Place water was introduced into a tin pan, occupied by a dome-shaped funnel, with its narrow end uppermost, and wide enough below, and laterally, to fill nearly the whole circumference of the pan. The water rose above the neck of the funnel, over which a bottle filled with the water under examination was placed, with its mouth downwards. The contents of the pan were raised to the boiling point, and when the bottle was filled by the air which had risen from the water, "it was removed by putting a small ladle under its mouth," a convenient substitute for a cork or tray, when the necessary manipulations had to be performed in boiling water. A second bottle full of the cold Rathbone Place water was in the same way substituted for the first, and that in its turn by others so long as the gas was freely evolved. The gas was analysed by allowing it to stand for a day over water, when "much the greatest part of the air was absorbed," and the water acquired the power of precipitating lime-water, from which it was inferred that what had been absorbed was fixed air. The unabsorbed air was then transferred to another bottle standing over a solution of "sope-leys," or caustic alkali, which reduced it considerably in bulk, and the residue was tested as to its identity with common air, by detonating it with hydrogen. It must be recollected that nitric oxide was still unknown, as well as oxygen and nitrogen, and that no eudiometer had been devised, or any test proposed for common air. Cavendish's instrument for analysing the latter was quite unique, and differed from all later instruments in being an *acoustic* eudiometer. He applied it, as we have seen in reviewing his former paper, to the identification both of inflammable and of common air. A gas supposed to be one of these, was mixed with a certain volume of the other, and exploded; the loudness of the explosion was carefully noted, and compared with the sound produced by the detonation of a mixture in the same proportions of hydrogen and common air. If the sound were the same, then the gas under examination, if inflammable, was inferred to be hydrogen—if uninflammable, it was inferred to be common air, as we have seen already in referring to the analysis of the air confined over fermented sugar and to that of the inflammable air from putrefying meat and gravy (ante, p. 209). In the case before us a small vial being filled with equal quantities of the unabsorbed air from the Rathbone Place water and inflammable air, and a piece of lighted paper applied to its mouth, "it went off with as loud a bounce as when a small vial was filled with equal quantities of common air and inflammable air." This singular and uncertain method of identifying atmospheric air did not satisfy Cavendish. He determined, in addition, the specific gravity of the unabsorbed gas of the water, and found it the same as that of common air.

It was possible that the fixed air which had arisen from the water had been generated during the boiling. Cavendish, however, satisfied himself that fixed air *pre-existed* in the water, by adding to it lime-water, which gave an abundant precipitate. He found that in this way he could throw down the whole of the calcareous earth, so that the water ceased to deposit on boiling, and was not troubled by the addition of "fixed alkali."*

From the amount of lime-water needed to precipitate a measured quantity of Rathbone Place water, and the weight of calcareous earth which in its natnral state it deposits on boiling, Cavendish inferred that it contained "near 2⅓ times as much fixed air as is sufficient to saturate the unneutralised earth in it." From his whole experiments he drew the following conclusion as to the relation of the fixed air to the solubility of the earthy carbonates in water. "It seems likely from hence, that the suspension of the earth in the Rathbone Place water is owing merely to its being united to more than its natural proportion of fixed air; as we have shown that this earth is actually united to more than double its natural proportion of fixed air, and also that it is immediately precipitated, either by driving off the superfluous fixed air by heat, or by absorbing it by the addition of a proper quantity of lime-water."

Cavendish then proceeds to comment on the strangeness of the fact, that the total abstraction of carbonic acid from lime, and the addition to it of a great excess of that gas, should equally render it soluble in water, although in its natural, intermediate condition of calcareous earth it is insoluble. To lessen the objections to his conclusions, which their strangeness in this respect might occasion, he resolved to make a direct trial, as to the possibility of suspending a calcareous earth in water,

* The term signifies here, carbonate of potash or soda, either or both; Cavendish distinguishes caustic alkali by the name "sope-leys."

"by furnishing it with more than its natural proportion of fixed air." For this purpose he placed in a bottle, a weighed quantity of carbonate of potash dissolved in rain-water, and poured into it a solution of chloride of calcium, mixed with a portion of free hydrochloric acid, less than sufficient to neutralise the whole of the alkaline carbonate. The bottle was quickly stopped and well shaken. At first the mixture was turbid, but it soon became transparent. On heating it the liquid again became turbid, discharged a good deal of air, and yielded an earthy precipitate. In this experiment the proportions were adjusted so as to correspond to the Rathbone Place water. The carbonate of potash precipitated carbonate of lime from the chloride of calcium, but simultaneously supplied carbonic acid to dissolve the precipitate.

To demonstrate that the carbonic acid was the solvent of the calcareous earth, Cavendish repeated the experiment, with the same proportion of materials, but added the alkaline carbonate to the hydrochloric acid, and allowed the effervescence to be past before pouring in the solution of chloride of calcium. The precipitate which was produced in this case "could not be redissolved on shaking," because, as Cavendish inferred, the carbonic acid which would have held it in solution had been allowed to escape. Lastly, lest any should imagine that the chloride of calcium or other salts, assisted in suspending the calcareous earth, Cavendish saturated rain water with carbonic acid, and added 11 ounces of the solution to 6½ of lime-water. "The mixture became turbid on first mixing, but quickly recovered its transparency on shaking, and has remained so for upwards for a year." When this experiment was repeated with about two-thirds of the carbonic acid water, a permanent precipitate was produced.

Three other London pump-waters were found to give a precipitate of calcareous earth with lime-water, and to yield a similar residue by evaporation. From his examination of them, along with the Rathbone Place water, Cavendish thought it "reasonable to conclude that the unneutralised earth in all waters, is suspended merely by being united to more than its natural proportion of fixed air."

He finishes his paper with a summary of his analytical results. That of the Rathbone Place is subjoined, as an example of the quantitative analysis of a natural water in 1766.

One pint or 7315 grains of Rathbone Place Water leaves of solid residue	17·5	gr.
Carbonate of ammonia		0·9
Carbonate of lime and a little carbonate of magnesia		8·4
Free carbonic acid		4·65
Sulphate of lime		1·2
Chloride of sodium and sulphate of magnesia		7·9

In reference to the preceding analysis, it may be noticed, that Cavendish appears to have regarded the ammonia as present in the caustic state, which it could not be in a water containing free carbonic acid. His first item is, "as much volatile alkali as is equivalent to about $\frac{9}{10}$ grain of volatile sal-ammoniac." The carbonic acid he gives as, "as much fixed air, including that in the unneutralised earth, as is contained in $19\frac{8}{10}$ grains of calcareous earth." Only the carbonic acid not present in the 8·4 carbonate of lime is placed in the table as free. It is calculated from Cavendish's datum, that carbonate of lime contains in 1000 parts, 408 of carbonic acid

An Account of a New Eudiometer.*

The seventeen years which elapsed between the publication of Cavendish's two first papers and the one which we are now to consider, did more to alter and enlarge the boundaries of pneumatic chemistry than any seventeen years have done before or since. During that long interval, Bergmann, Scheele, Lavoisier, but, above all, Priestley, besides others, had been engaged in researches on the gases.† The great majority of these had been discovered, and though almost nothing had been done towards their analysis, the properties of the chief among them had been carefully studied and were well known.

The discovery of nitrogen and oxygen had naturally directed much attention towards the atmosphere, as a compound or mixture of chemical substances, and a convenient as well as accurate

* *Phil. Trans.*, 1783, p. 106. This paper was read to the Royal Society January 16, 1783.

† Five out of Priestley's six volumes on air were published before the end of 1781.

method o analysing it was now an object of desire to every chemist. Priestley's discovery in 1772 of nitrous gas or nitric oxide, which Hales had prepared, but had not recognised as a distinct elastic fluid, supplied a method of determining the amount of oxygen present in any gaseous mixture. As nitric oxide, when it meets oxygen, combines with it to form a compound soluble in water, the latter rises within the vessel in which the gases are permitted to mingle, as it dissolves the compound which they form by their union. If we suppose a slight excess of nitric oxide made use of in every case, then, *cæteris paribus*, the water will rise higher the greater the proportion of oxygen present, and by the degree of its elevation will measure the relative quantities of that gas contained in different gaseous mixtures, analysed in the same way. In actual practice, a difficulty occurs in the use of this gas, which only Cavendish's successful employment of it prevents us from calling insurmountable. The same volume of oxygen can combine with very different volumes of nitric oxide, according to circumstances, and occasion a corresponding difference in amount of contraction; so that equal contractions cannot be taken as implying the presence of equal volumes of oxygen.

Two methods of employing nitric oxide were in use before Cavendish published his paper. The theoretically simpler process was to add nitric oxide to a measured volume of respirable air standing over water, in small successive quantities, so long as it occasioned diminution in the bulk of the air. The space through which the water rose, corresponded in this case, exactly to the volume of oxygen which the nitric oxide had withdrawn from the air. Experimenting in this way, Priestley showed that "about $\frac{1}{5}$th" of common air combined with the nitric oxide, and was absorbed by the water.*

It was found in practice, however, a very difficult matter to adjust the proportion of the nitric oxide, so that not a bubble too much or too little of that gas should be mixed with the air under examination. Priestley accordingly substituted for the method described, another, in which an excess of nitric oxide was at once added to the air, and the diminution in bulk which followed was noted. Two specimens of respirable air which

* *Experiments and Observations on Air* (1775), vol. i., p. 111.

suffered the same contraction in these circumstances, were assumed to be equally pure; but the absolute amount of oxygen contained in the airs was not ascertained by this mode of using the nitric oxide. Priestley's standard, accordingly, was quite arbitrary. He mixed equal volumes of nitric oxide and air in a small wide jar, and after contraction had ceased, transferred the residue to a narrow graduated tube, in which he measured the diminution of bulk that had occurred. He expressed this diminution by the number of parts remaining. Thus, if one measure of air, added to one of nitric oxide, diminished from 2 measures to 1·06, Priestley called the purity of the air 1·06.*

An arbitrary standard such as this would never have been followed, had the impression not been universal, that no two specimens of air had exactly the same composition. This belief, or rather notion, would very soon have been corrected as methods of analysis improved, had it not been connected with an hypothesis, which, as it was ultimately understood, was to the effect, that as oxygen supported respiration and combustion much better than common air did, the salubrity of the latter might be reasonably assumed to depend on the proportion of oxygen present in it. This hypothesis quickly became a theory in the hands of those who tested its truth by analyses of air, although Priestley, who was indirectly its originator and its great supporter, confessed that air hypothetically bad, was often not to be distinguished from what was reputed the best. The difference, for example, between the most unwholesome air from the workshops of Birmingham, and the "very best air in this county (Wiltshire), which is esteemed to be very good," "was very trifling."† Others were more successful in finding the difference which they wished to find, and all the philosophers of the day sanctioned the general belief by the name *Eudiometer*, which they gave to their instruments for gaseous analysis. In reality, however, their view as to the salubrity of different portions of the atmosphere, was not exactly as is generally represented, or as has been stated above. They connected the purity of the air, not so much with the presence of oxygen,

* *Experiments and Observations on Air* (1775), vol. i., introduction, p. xx; and vol. iv. (1779), introduction, p. xxix., also p. 280.

† *Experiments and Observations* (1779), vol. iv., p. 269.

as the absence of *phlogiston*. Their object, as stated by themselves, was to ascertain, not the degree of oxygenation, but the degree of phlogistication of the atmosphere. This fact must not be overlooked. It is difficult to guard successfully against the tendency to represent the chemists of a former time as holding our views exactly as we hold them. We are apt to think of Priestley and his contemporaries as apprehending as distinctly as we do, that air consisted, in greater part, of unequal measures of two unlike gases. For some ten years, however, after the discovery of nitric oxide, and its application to the analysis of the atmosphere, chemists explained its composition otherwise. The early conception of air as essentially one and indivisible, was not easily thrown aside, nor did it seem necessary that it should be. Dephlogisticated air (oxygen) was air *minus* phlogiston; phlogisticated air (nitrogen) was air *plus* phlogiston. Both were equally air; and the principle, which by its presence or absence altered their properties, was imponderable, intangible, and unknown.* The atmosphere could thus be represented as consisting, not of two unlike airs, but of one air, and of phlogiston. The latter term was used in a wider sense than when first introduced by Stahl, and signified the principle common to all those bodies which, when left in contact with the air, lessened its respirability, and its power of sustaining animal life and flame, whether this vitiation of the air was accompanied by the combustion of the phlogisticating body or not. Nitrogen was specially distinguished by the name *Phlogisticated Air*. We are apt on this account to conceive that the phlogistication of air must always be synonymous with the abstraction from it of oxygen, so as to leave nitrogen. Paradoxical, however, though it may seem, *air phlogisticated* was not necessarily nitrogen, although *phlogisticated air* was. The former might, besides being nitrogen, be air with its oxygen replaced in whole, or in part by carbonic or sulphurous acid, as well as by other bodies; or air with its normal amount of oxygen and nitrogen, but containing the gases named above, or any other irrespirable or poisonous gases diffused through it in such quantity as to render it noxious to life, and unfit to support

* Cavendish, for example, in another paper, speaks of "the dephlogisticated *part* of common air." *Phil. Trans.*, 1784, p. 123.

combustion. Pure nitrogen; a mixture of nitrogen with irrespirable gases; and a mixture of common air with these, were thus all *air phlogisticated.* To ascertain the extent of this phlogistication, or vitiation, was the object of the early analysts of the atmosphere, who did not at first propose to themselves the task of determining the relative volume of constituent gases in it; although in the end their inquiry unavoidably merged in such a research. They did not accordingly name their instruments Pneumatometers, Aerometers, Gasometers, or the like. They should have named them Phlogistometers: "Measurers of the *badness* of the Air." They preferred, however, the more euphonious title of Eudiometers, or measurers of its *goodness;* a title still retained, and curious as the only fragment of the Phlogiston Nomenclature which has survived to the present day.*

These instruments were constructed on the assumption that the atmosphere was liable to the greatest variations, as to its degree of phlogistication, or amount of impurity. When air, moreover, previously respirable, was phlogisticated, it was observed to undergo a diminution of bulk, which was great in proportion to its original purity. Thus, liver of sulphur, solutions of the alkaline persulphurets, a mixture of sulphur and iron filings, and nitric oxide, as well as other substances, were known to diminish the bulk of air while they phlogisticated it. We should err greatly, however, if we assumed that the chemists who first employed eudiometers, distinctly apprehended that this reduction of volume resulted from the conversion of the oxygen of the air, into a liquid or soluble compound. Cavendish's paper, recounting experiments which "were made principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated," was not read to the Royal Society till a year after his communication on the eudiometer.† Scheele's important treatise on Air and Fire, which discussed the same question, did not, according to Hoefer, appear in its original form till 1777, and was not generally known in France or England till 1781.‡

* The name Eudiometer appears to have been introduced by Landriani. Black's *Lectures,* vol. ii., p. 523.

† *Experiments on Air. Phil. Trans.,* 1784, p. 119.

‡ *Histoire de la Chimie,* t. ii., p. 460.

The majority of chemists of the period accepted as an ultimate, or for the time, inexplicable fact, the diminution in bulk of air, when it was phlogisticated, or as we should now say, de-oxidised. A theory on the subject was not essential to the employment of the eudiometer. It was enough that air containing no phlogiston (oxygen), suffered a great reduction in volume when it was phlogisticated; and that air, saturated with phlogiston (nitrogen), suffered a much smaller reduction, or none at all. It could thus be assumed, that air diminished in volume when it united to phlogiston, and that the less of that principle it contained before it was phlogisticated, the more of it would it combine with, and the greater would be the reduction of volume which occurred.

It must further be noticed, before discussing Cavendish's paper at greater length, that the opinion, natural enough, that a body exposed to so many vitiating influences as the atmosphere is, could not be uniform in composition, appeared to the early observers completely confirmed by their analyses. No precise endeavour, accordingly, was made to ascertain even the average quality of air, although hundreds of analyses were performed, the mean of which would have given at least an approximation towards it. Priestley had no fixed standard to which he referred different specimens of air, when he was analysing them. His habit was to examine two specimens at once, the one *ex hypothesi* good, the other bad, and to mark the difference between them. In this way, as we have seen, he compared the foul Birmingham air with the "very good" air of Wiltshire; but the quality even of the latter was assumed to be variable, and was not reckoned as a constant quantity. How great the variation in quality seemed to be, when tested by those who expected variation, and employed an imperfect apparatus to measure it, will appear from the statement of one of the analysts of the period. Signor M. Landriani, after making an eudiometrical tour through Italy, writes to Priestley in November, 1766: "I have had the satisfaction of convincing myself, that the air of all those places which, from the long experience of the inhabitants, has been reputed unwholesome, *is found to be so, to a very great degree of exactness, by this instrument of mine;* so that the theory seems to correspond very well to observation. In the mountains near Pisa I made trial of the air at different heights, beginning on the plain, and proceeding to the highest summits; and found a remarkable difference in the state of the air, every stratum being purer in proportion as I ascended.* . . . . . The air of the Pontine Lakes, that of the Sciroccho at Rome (so very unwholesome), that of the Campagna Romana, of the Grotto del Cane, of the Zolfatara at Naples, of the Baths of Nero at Baja, of the sea-coast of Tuscany, were all examined by me, and found to be in such a state as daily experience led me to expect."† No one who reads this will feel surprised that Landriani should have been the person who introduced the word Eudiometer.

Cavendish begins his paper by observing that "Dr. Priestley's discovery of the method of determining *the degree of phlogistication of air* by means of nitrous air (nitric oxide), has occasioned many instruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé Fontana is by much the most accurate of any hitherto published."‡

The great improvement in Fontana's eudiometer over previous instruments, consisted in the graduated tube in which the diminution of the mixed gases was measured, being long and narrow, and provided with a wide-necked funnel, through which the air and nitric oxide were rapidly passed. The gases rose in one continuous column: "so that," adds Cavendish, "there

* Saussure, on the other hand, inferred from his experiments, "that the air of the valleys among the Alps and at Geneva is better than that on the tops of high mountains." *Voyages dans les Alpes* (1779), t. i., p. 517.

† *Experiments and Observations on Air,* vol. iii., appendix, p. 380. Similar statements will be found from other correspondents in the appendix to Priestley's fourth volume. One of these writers is Dr. Dobson, who found "marine air to be one-eighth of a measure better than common air." (P. 469.) The air here referred to was that procured from sea-water, raised to the temperature of 212° F., and doubtless contained more oxygen than atmospheric air does. It was naturally enough assumed that the air above the sea would be identical in composition with that found dissolved in it, and in this way the salubrity of marine districts was accounted for.

‡ Besides Fontana and Priestley; Magellan, Dobson, and Landriani are referred to by the second as devisers of nitric oxide eudiometers. At a later period, A. Humboldt endeavoured to improve them, as well as Thomson, Dalton, and Davy (Thomson's *System of Chemistry,* 6th ed., vol. iii., p. 167); but they have long been abandoned by all chemists.

is time to take the tube off the funnel, and to shake it before the airs come quite in contact; by which means the diminution is much greater and much more certain than it would otherwise be." The diminution in volume, also, reached a maximum, in the short time during which the mixture was shaken, so that the latter was not sensibly altered in bulk subsequently, however long it was left over water.

Cavendish referred those phenomena to the opportunity which was afforded by Fontana's instrument for each small portion of the nitrous air being in contact with water, either at the instant it mixes with the common air, or at least immediately after. He thought it, accordingly, worth while to try whether the diminution would not be still more certain and regular, "if one of the two kinds of air was added slowly to the other in small bubbles, while the vessel containing the latter was kept continually shaking;" and finding his anticipations fulfilled, he constructed an instrument by means of which the gases might be mixed in the way which yielded the most accurate and constant results. His eudiometer consisted essentially of three parts: 1. A small glass jar with a handle, which served as a measure; 2. A hollow glass globe with a wide neck, in which the combination of the gases took place. This was suspended in a trough of water, with its mouth downwards, so that it could be readily shaken backwards and forwards, and one of the gases (air or nitric oxide) was measured into it, at the commencement of the experiment; 3. A glass cylinder, provided above with a cap and stopcock, and open below, but made to fit a brass socket or stand, with a small aperture in its centre. A measured portion of the other gas was introduced into this vessel, which was then placed in its socket, with the nozzle of the stopcock within the neck of the suspended globe. When the stopcock was opened, water entered by the aperture in the socket of the cylinder (3), and the gas contained in it slowly ascended into the globe which was kept constantly agitated.

In using this apparatus, it was in the option of the experimenter to add the common air slowly to the nitric oxide, or the nitric oxide slowly to the common air. Cavendish generally did the former. He was not satisfied with the measurement of the gases, as errors were occasioned by more water "sticking to the sides of the measure and tube at one time than at another." He preferred, accordingly, to determine the quantity of air and nitric oxide used, and the diminution which followed their mixture, by weighing the containing vessels under water. It is not necessary to enter minutely into a consideration of this process. The final weighings gave the weight of a volume of water exactly equal to the volume of mixed gases which had been expended, and likewise the weight of a volume of water, representing the space through which the gases had contracted.

When air was in the cylinder, and nitric oxide in the suspended globe, a measure of the former was taken to $1\frac{1}{4}$ of the latter. Equal measures would have sufficed, but it seemed well to take a slight excess of the nitric oxide, lest it should be impure. In the case supposed, the air was slowly added to the nitric oxide, and the $2\frac{1}{4}$ measures suffered a contraction of 1·08, which number was what Cavendish called the *test* of air tried in this way.

When the nitric oxide was added to the common air, a measure of each was taken, and the diminution was only 0·89. The cause of this difference will be considered presently.

Priestley, it will be remembered, marked the purity of the air by the volume of mixed gases *remaining after* contraction had ceased; Cavendish, on the other hand, noted the volume which *disappeared during* contraction.

A point neglected by all previous experimenters, was the quality of the water with which their eudiometers were filled. Cavendish made trial with water of different degrees of purity, from distilled water to "water fouled by oak shavings," and found that though the other conditions of the experiment were the same, the result varied materially according to the quality of the water employed. He remarks in reference to it, "this difference in the diminution, according to the nature of the water, is a very great inconvenience, and seems to be the chief cause of uncertainty in trying the purity of air. . . . . It shows plainly, how little all the experiments which have hitherto been made for determining the variations in the purity of the atmosphere can be relied on, as I do not know that any one before has been attentive to the nature of the water he has used, and the difference proceeding from the difference of waters is much greater than any I have yet found, in the purity of air." He recommends accordingly, as the best way of obviating the inconvenience complained of, the employment in all cases of distilled water. This water, however, he found to absorb different quantities of nitric oxide at different times, partly as it appeared, owing to its temperature not being the same at each experiment, partly in consequence of the proportion of oxygen present in the water varying, so that different quantities of nitric oxide were withdrawn by different specimens of water.

The greater number of these and of the other eudiometrical experiments were made by adding air to nitric oxide; the order of mixture which Cavendish preferred. Many, however, were tried with the gases mixed in the reverse order, when it always appeared, as already mentioned, that the contraction in bulk was much less than when air was added to nitric oxide.

It is curious to notice Cavendish's explanation of this phenomenon as illustrative of the difficulty which he and his contemporaries experienced, in accounting for the diminution in bulk which attended the deoxidation (or phlogistication) of air. "When nitrous and common air," says he, "are mixed together, the nitrous air is robbed of part of its phlogiston, and is thereby turned into phlogisticated nitrous acid, and is absorbed by water in that state, and *besides that*, the common air is phlogisticated and thereby diminished." Here it will be seen, two causes are assigned for the contraction of the air.—1. The nitric oxide, an insoluble gas, by losing phlogiston which it communicates to the air, becomes a substance soluble in water, which absorbs it.—2. The phlogiston, transferred to the air, compels it, in virtue of an unexplained power which it possesses, to diminish in bulk. Cavendish's own words, in continuation of those already quoted, are—"The whole diminution in mixing is equal to the bulk of nitrous air, which is turned into acid, added to the diminution which the common air suffers by being phlogisticated." The diminution owing to the latter cause was, as Cavendish knew, a constant quantity, but not that, as he believed, owing to the former. Nitric oxide, according to our chemist, could part with variable quantities of phlogiston to air according to the relative proportion of the two gases. When a small quantity of nitric oxide was added to a large volume of air, it parted with a larger amount of phlogiston to the air, than it did when the nitric oxide was in excess. A smaller volume accordingly, of nitrous air sufficed to effect the maximum contraction of common air, if it were added in successive small quantities to the latter, till it ceased to contract, than was sufficient for that purpose if the common air were let up bubble by bubble into the nitric oxide.

Cavendish details a long series of experiments demonstrating the truth of these statements, but they do not call for minute reference. It is important, however, to notice that his views concerning the variable phlogisticating (*i. e.* deoxidising) power of nitric oxide over common air, are in exact accordance with our modern views, provided only (as in translating the statements of a writer of the phlogiston school we are generally justified in doing) we always substitute for *addition of phlogiston* to air, *abstraction of oxygen* from it. We may then understand Cavendish as imperfectly teaching, what Dalton, Gay Lussac, and Humboldt afterwards announced more fully, viz., that the same volume of oxygen, according to circumstances, can combine with one or more volumes of nitric oxide, occasioning in each case a different amount of contraction, although the volume of oxygen withdrawn is the same*.

Having settled this point, Cavendish proceeds to record a series of experiments made to prove the superiority of his method to Fontana's, which need not be detailed.

After giving reasons for assigning to his own the preference, he enters on the consideration of the question, how far the accuracy of the "nitrous test" is affected by the quality of the nitric oxide employed. In discussing this he makes a distinction, at first sight not very intelligible, as to two modes in which the gas may differ. "First, it may vary in purity, that is, in being more or less mixed with phlogisticated or other air; and, secondly, it is possible, that out of two parcels equally pure, one may contain more phlogiston than the other." The first cause of difference, unless the impurity were fixed air, which could, however, be excluded, Cavendish did not think of much importance, as the presence of foreign matters would only in-

* Dalton's paper is contained in *Phil. Mag.*, xxviii., 351; quoted in Thomson's *System of Chemistry*, iii., 169.

crease the proportion of nitric oxide required to produce full contraction, and he always employed an excess sufficient to cover any probable amount of impurity.

The second cause of difference would, if considerable, destroy the whole value of the test, as two different specimens of nitric oxide would give different results, though employed in exactly the same way. To determine this, he prepared nitric oxide from quicksilver, from copper, from brass, and from iron, and tested common air with each. He found no appreciable difference between the three first, but the "air from iron" occasioned a greater diminution of bulk than the other specimens of nitric oxide did, when added to a nearly equal volume of common air, and a smaller diminution than they occasioned, when mixed with four measures of air. From this Cavendish concluded that the nitric oxide prepared from iron was both impure, and contained "rather less phlogiston than the others," so that more of it was expended in condensing the same amount of oxygen; or, what came to the same thing, a given bulk of this nitric oxide caused less contraction in a large volume of air, than the same measure of the other specimens of the gas did. It is not unlikely that the gas from iron contained hydrogen, and perhaps also nitrous oxide, so that its deoxidising effect on air, would be less, bulk for bulk, than that of pure nitric oxide. Cavendish recommended the gas prepared with copper, as constant in properties, and easily procured.

Having by those careful trials certified the value of his eudiometer, Cavendish proceeded to apply it to the determination of the important question, Is the atmosphere constant in composition? The following is his account of the result of his researches on this subject. "During the last half of the year 1781, I tried the air of near sixty different days, in order to find whether it was sensibly more phlogisticated at one time than another, but found no difference that I could be sure of, though the wind and weather on those days were very various, some of them being very fair and clear, others very wet, and others very foggy." This conclusion was founded on a very extensive series of experiments, for seven or eight analyses were made in different ways of the air of each day. From first to last, indeed, Cavendish cannot have made fewer than 500 quantitative determinations of the composition of atmospheric air. The result of his protracted observations he stated thus—"On the whole, there is great reason to think that the air was in reality not sensibly more dephlogisticated on any one of the sixty days on which I tried it, than the rest. The highest test I ever observed was 1·000, the lowest 1·068, the mean 1·082."*

This was the result of researches into the quality of the air from day to day. Cavendish made other experiments "to try whether the air was sensibly more dephlogisticated at one time of the day than at another, but could not find any difference." Trials were also made "with a view to examine whether there was any difference between the air of London and the country." Slight differences appeared sometimes in favour of the purity of the London air, sometimes in favour of that of Kensington; "but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was twenty, and a great part of them taken in winter, when there are a great number of fires, and on days when there was very little wind to blow away the smoke."

The settlement, by those ample trials, of the uniform composition of the atmosphere, enabled Cavendish to suggest what till then was wanting, viz., a common scale of graduation applicable to all nitric oxide eudiometers. Atmospheric air he proposed to call 1·00. Nitrogen supplied the zero, or was 0·00; and for those who agreed with Scheele and Lavoisier in supposing that common air "consists of a mixture of dephlogisticated and phlogisticated air," oxygen was the maximum, and was marked by Cavendish 4·8.† Those numbers he refers to as the *standards* of the several gases mentioned, in contradistinction to their *tests,* which were the numbers representing the

* This number, it will be remembered, signifies that a measure of air being slowly added to a measure and a quarter of nitric oxide, contraction occurred through a space equal to one measure and $\frac{82}{100}$ths, or the two measures and a quarter of mixed gas and air left 9·18 parts of one measure (of nitric oxide and nitrogen) unabsorbed.

† Cavendish does not imply in this paper, an undoubting agreement with Scheele and Lavoisier, as to the distinct nature of dephlogisticated, and phlogisticated air. His concurrence in this opinion is much more explicitly announced in his *Experiments on Air.* (*Phil. Trans.*, 1784, p. 141.)

contraction which occurred when those gases were mixed with nitric oxide in the eudiometer. Thus the *standard* of air was 1·00, but its *test*, as we have seen, was 1·086.* The standards for specimens of air less oxygenated than common air, were found by taking the *test* of the air under examination, and then making an artificial mixture of similar composition, of common air and nitrogen. In adjusting this mixture, one measure of the former was taken, and variable quantities of the latter, till a mixture was obtained which suffered the same amount of contraction in the eudiometer, or had the same test as the air under examination. If, *ex. gr.*, "its test was the same as that of a mixture of 1 part of common air and x of phlogisticated air (nitrogen), its standard was $\frac{1}{1+x}$." If the specimen were more oxygenated than atmospheric air, then the quantity (x) of nitrogen which must be mixed with it to reduce it to the purity of common air, determined its standard, which was $1 + x$. Oxygen would thus, in round numbers, be $1 + 4 = 5$.

I have described the principle of Cavendish's graduation, because he does not directly give in this paper his estimate of the relative quantities of oxygen and of nitrogen in atmospheric air. As he called it unity, or 1, and assumed it as a constant quantity, he made no reference to the factors of this unit, although it was a middle point in his scale. The standard, however, of oxygen was a number obtained thus:—Let O parts of oxygen added to N parts of nitrogen, form a mixture identical with common air, then the standard of oxygen is $\frac{O+N}{O}$. Cavendish gives 4·8 as the standard for oxygen. If we divide 100 by this, we shall obtain the per-centage by volume of oxygen in air, or 20·83.† Air, therefore, according to him, had the composition by volume:—

* A somewhat similar method of graduation was followed by Dr. Dobson, of Liverpool, in 1799. He made "good common air" the middle point of his scale, but called it 0. From this he counted upwards 22 degrees to oxygen, and downwards 20 to nitrogen. The numbers above 0 represented degrees of goodness, or of superiority in purity to common air; those below 0 were degrees of badness. Priestley's *Experiments and Observations on Air*, vol. iii., app., p. 470.

† Taking 100 volumes of air $O + N = 100$. By Cavendish's formula $\frac{O+N}{O} = 4{\cdot}8$; therefore substituting the value of $O + N$; $\frac{100}{O} = 4{\cdot}8$ and $O = \frac{100}{4{\cdot}8} = 20{\cdot}83$.

Oxygen	20·833
Nitrogen	79·167
	100·000

According to Dumas' recent analysis, the numbers are:—

Oxygen	20·90*
Nitrogen	79·10
	100·00

The approximation is very close. Scheele made the amount of oxygen 25 per cent.;† Lavoisier made it 27 per cent.;‡ Saussure 22 per cent. § Cavendish's analysis, therefore, was much more accurate than those of his illustrious contemporaries. In his later essay ("Experiments on Air") he announces the result of his analysis of air more fully. Referring to an experiment, he says, "some dephlogisticated air was reduced by liver of sulphur to $\frac{1}{30}$th of its original bulk; the standard of this air was 4·8, and consequently the standard of perfectly pure dephlogisticated air should be very nearly 5, which is a confirmation of the foregoing opinion, for if the standard of pure dephlogisticated air is 5, common air must, according to this opinion, contain $\frac{1}{5}$th of it, and therefore ought to lose $\frac{1}{5}$th of its bulk by phlogistication, which is what it is actually found to lose."‖

The part of the paper immediately succeeding that last discussed, is occupied with references to the best mode of preparing nitrogen for the graduation of the eudiometer, and in explaining the cause of the slight contraction which attends the addition of nitric oxide to pure nitrogen. Cavendish refers this to the solution of the nitric oxide in the water, but it was

* Regnault, by a number of determinations made from the 24th to the 31st of December, 1847, found the proportion of oxygen in the atmosphere to vary between 20·90 and 21·00 vol. per cent. In January, 1848, it varied between 20·89 and 20·99; the results obtained in the analysis of air at various hours of the same day were found to oscillate between the same limits. R. F. Marchand found the quantity of oxygen in the air in ten experiments to vary from 20·90 to 21·03, the mean being 20·97 vol. per cent. Liebig and Kopp's *Annual Report of the Progress of Chemistry*, 1847-48, part ii., pp. 298, 299.

† Hoefer, *Traité de Chimie*, t. ii., p. 463.

‡ *Elements of Chemistry*, translated by Kerr, p. 86.

§ Black's *Lectures*, ii., p. 524.

‖ *Phil. Trans.*, 1784, p. 141.

probably also owing to the nitrogen being mixed with a little oxygen derived from atmospheric air displaced from the water. It was so appreciable, that although the standard of nitrogen was 0, its test was 0·7.

The paper concludes with an estimate of the nature and extent of the information supplied by the eudiometer, of great value. Cavendish shows that etymologically the name had no significance, and this in a twofold way: for, 1. In so far as the instrument takes cognizance of the degree of phlogistication, or impurity of the atmosphere, it betrays no difference between one specimen of air and another, so that apparently there are no degrees of goodness to be measured; 2. Even when the atmosphere is certainly phlogisticated, as by the addition of some ounce measures of nitric oxide to the air of a large room, their "effect in phlogisticating the air must be utterly insensible to the nicest eudiometer." "In like manner, it is certain that putrefying animal and vegetable substances, paint mixed with oil, and flowers, have a great tendency to phlogisticate the air; and yet it has been found" that such air "was not sensibly more phlogisticated than common air." The general inference from this is, "that our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity of the air than can be perceived by the nitrous test."

The nitric oxide eudiometer has long been abandoned, but the constant results which Cavendish alone among chemists obtained with it, remain a lasting monument to his unique skill, which converted a most imperfect analytical instrument into a delicate and accurate recorder of the relative proportions of the more abundant constituents of the atmosphere.

It need scarcely be noticed, that we are still as much in need of an eudiometer, properly so called, as the contemporaries of Priestley and Cavendish were. There cannot be two opinions as to the atmosphere being as little entitled to be considered a perfectly homogeneous mixture, as the ocean is; nor does any other obstacle stand in the way of the analysis of the air, than that presented by the comparatively small quantity of many of the substances which must be sought for in it. Liebig, however, has taught us how to overcome this difficulty, at least in part, by analysing rain and snow, which bring down to the earth the soluble substances of the atmosphere which they have encountered in their fall; and Dumas and others have shown how much may be done by forcing large volumes of air through solutions of substances which combine with, and detain certain of its ingredients. Medicine, as well as meteorology and chemistry, have the deepest interest in such inquiries, and we may anticipate the period when a laboratory will form an essential part of our meteorological observatories, and systematic and continuous analyses will be made of all the accessible constituents of the atmosphere.

Experiments on Air.*

The paper now to be considered contains the record of experiments made in part in the summer of 1781, before those on the analysis of air which have just been commented on. The earlier researches, however, could not be successfully prosecuted without a knowledge of the composition of the atmosphere, and Cavendish, accordingly, interrupted the original inquiry, after it had made some progress, till he had completed the protracted eudiometrical investigation, which was made public a year before the "Experiments on Air." That title conveys a very imperfect idea of the nature of the researches which were carried on by its author. Its most important section, according to our modern estimation, is that which treats of the synthesis of water from its elements. The production, however, of water, or the determination of its composition, was not the special object of the inquiry, which was undertaken with a view to ascertain what were the products of the deoxidation of atmospheric air by the ordinary combustibles, and some other bodies having a great affinity for oxygen. Little notice has been taken, even by the professed historians of chemistry, of the general scope of the paper, but much criticism has been expended on those parts of it which relate to the Water Controversy. It is desirable, accordingly, in order to avoid repetition, to limit the present abstract to an unpolemical analysis of the contents of the "Experiments on Air," the disputed portions of which will be considered in detail, when the claims of Watt

* *Phil. Trans.*, 1784, p. 119; read to the Royal Society January 15, 1784.

and Cavendish, as the discoverers of the composition of water, are under discussion.

Cavendish begins by observing that "the following experiments were made principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." He then mentions that many have supposed that "fixed air is either generated or separated from atmospheric air by phlogistication, and that the observed diminution is owing to this cause." His first experiments, therefore, were made with a view to ascertain the truth of this opinion; and he began by excluding from consideration all animal and vegetable substances, which, as they "contain fixed air,"—or as we should now say, yield it by combustion—could not be employed to phlogisticate the air in such *experimenta crucis* as he desired to make. He then proceeds to detail the only methods known to him which are not liable to objection, viz., "the calcination of metals, the burning of sulphur or phosphorus, the mixture of nitrous air, and the explosion of inflammable air." He doubts about the electric spark, which he thinks may phlogisticate the air only by igniting some combustible matter present in the containing vessel, so that he does not deem it necessary to experiment with it. The unexceptionable methods of investigation are then discussed *seriatim*. He states, in the first place, that "there is no reason to think that any fixed air is produced during the calcination of metals." Priestley, he observes, found none, and Lavoisier only a very slight and scarcely perceptible turbid appearance, when lime-water was shaken in a glass vessel full of the air in which lead had been calcined. A statement of Priestley's, that impure quicksilver is changed by agitation and exposure to the air, into a powder containing fixed air, Cavendish sets aside as not unexceptionable in reference to the question he was considering, because this gas may have been contained in the impure mercury before its agitation with the air was commenced. "I never heard," he continues, "of any fixed air being produced by burning sulphur or phosphorus; but it has been asserted, and commonly believed, that lime-water is rendered cloudy by a mixture of common and nitrous air, which, if true, would be a convincing proof that on mixing those two substances, some fixed air is either generated or separated." He showed, however, that if the gases are washed with lime-water before being mingled, no carbonic acid can be detected after mixture.

Cavendish then tried whether fixed air was produced by the explosion of inflammable air from metals (hydrogen), with either common or dephlogisticated air (oxygen). The gases were washed with lime-water before the electric spark was passed, and "the event was, that not the least cloud was produced in the lime-water when the inflammable air was mixed with common air, and only a very slight one, or rather diminution of transparency, when it was combined with dephlogisticated air." The general conclusion from the whole experiments was, that "on the whole, though it is not improbable that fixed air may be generated in some chymical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it."

Having thus disposed of carbonic acid as the constant product of the phlogistication or deoxidation of air, Cavendish proceeded to try whether, as some of Priestley's experiments seemed to render probable, "the dephlogisticated part of common air might not by phlogistication be changed into nitrous or vitriolic acid." For this purpose, he burned sulphur in air over milk of lime, filtered and evaporated the resulting solution, and found that "it yielded no nitrous salt, nor any other substance except selenite; so that no sensible quantity of the air was changed into nitrous acid." He tried also "whether any nitrous acid was produced by phlogisticating common air with liver of sulphur." For this purpose he boiled sulphur with milk of lime, and then shook the solution with large quantities of air, till the liquid lost its yellow colour, "a sign that all the sulphur was, by the loss of its phlogiston, turned into vitriolic acid and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the least nitrous salt."

Cavendish calls the salt of lime produced in both these experiments, "Selenite." In the first it must have been sulphite, in the second, hyposulphite of lime. It did not escape his observation, however, that the salt he encountered in both these trials differed from "Common Selenite" (sulphate of lime). Unlike it, the salt "was very soluble, and even crystallized readily, and was intensely bitter." By exposure to the air, however, and evaporation to dryness, it lost its great solubility, and ceased to interfere, as it did at first, with the search for a nitrous salt.*

The same negative result was obtained with sulphur and milk of lime, when oxygen was substituted for common air. Cavendish then proceeded to try whether any vitriolic acid was produced during the phlogistication of air. For this purpose he caused a large quantity of nitric oxide (the phlogisticating agent) to combine with the oxygen of common air confined over distilled water. The acidulated water was then distilled, saturated with salt of tartar, and evaporated to dryness. He procured in this way $87\frac{1}{2}$ grains of nitre, which was unmixed with vitriolated tartar (sulphate of potash), "consequently no sensible quantity of the common air with which the nitrous air was mixed, was turned into vitriolic acid." He then records an erroneous conclusion as to the relative acidity of nitric oxide and nitric acid, which he founded upon the experiments last detailed; and thereafter proceeds thus: "Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceed to some experiments, which serve really to explain the matter."

The account which follows is so brief and clear, and the passage is so important, that it does not admit of condensation. "In Dr. Priestley's last volume of experiments is related an experiment of Mr. Warltire's, in which it is said that on firing a mixture of common and inflammable air by electricity in a close copper vessel holding about three pints, a loss of weight was always perceived, on an average about two grains, though the vessel was stopped in such a manner that no air could escape

* Although Cavendish did not distinguish between the salt of the first experiment (CaO,SO^2) and that of the second (CaO,S^2O^2), he spoke of them considered as one, as owing their peculiarity to the sulphur-acid which they contained, "being very much phlogisticated." In his nomenclature, phlogisticated vitriolic acid was our sulphurous acid; and vitriolic acid "very much phlogisticated" was a body such as hyposulphurous acid.

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

In the experiments detailed, the hydrogen (generally from zinc) and the common air were mingled in known quantities, and the amount of diminution in bulk which followed explosion, ascertained in each case. The *test* of the air, including its *standard*, in other words the amount of oxygen (if any) remaining after detonation, was also observed. The mode in which this was done is not described, but it cannot be doubted that it was by means of the nitric oxide eudiometer, as the nomenclature made use of is that explained by Cavendish in describing that instrument. (Ante, p. 227.)* These quantitative results are given fully in a table, of which the fourth entry is the following.

Common Air.	Inflammable Air.	Diminution.	Air remaining after the explosion.	Test of this Air in first method.	Standard.
1	·423	·612	·811	·097	·03†

* *Phil. Trans.*, 1783, p. 131.

† The fifth column records the amount of contraction which occurred when the air uncondensed by the explosion was mixed with a little more than an equal

On this entry Cavendish makes the following important comment, which contains his earliest formal announcement of the synthetical determination of the composition of water: "From the fourth experiment it appears that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude that when they are mixed in this proportion, and exploded, *almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.*" This remarkable passage shows how clear and precise were the views of Cavendish. It distinctly intimates, not a hesitating supposition concerning the fate of the common air and hydrogen which had disappeared, but an assured conviction that they were converted *into* the dew, *i. e.*, that the dew was the very gases in the liquid state.

Having ascertained in this way the connection between the disappearance of the gases and the appearance of the dew, Cavendish proceeded to investigate the nature of this dew. For this purpose, he so arranged as to burn together 500,000 grain measures of inflammable air with $2\frac{1}{2}$ times that quantity of common air, within a glass cylinder, and collected the resulting liquid. "By this means, upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; *in short, it seemed pure water.*"

A short unimportant paragraph then occurs, which is followed by his conclusion from both sets of experiments, namely, those with the globe, and those with the cylinder. "By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity, and are condensed into dew.

measure of nitric oxide; and the sixth the amount of oxygen in that air according to a eudiometrical scale, which made nitrogen 0, air 1, and oxygen 4·8. *Phil. Trans.*, 1783, p. 131. *Ante*, p. 227.

And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water."

Having thus assured himself that the gases had changed during the phlogistication of the air into a liquid, and that the liquid was pure water, Cavendish proceeded to make similar experiments with oxygen and hydrogen, in the proportion of 19,500 grain measures of the former to 37,000 of the latter, or rather less than two volumes of hydrogen to one of oxygen. An exhausted glass globe was filled with the mixture, "and the included air fired by electricity, by which means almost all of it lost its elasticity." As Cavendish wished, however, to examine the liquid product of this combustion also, he replenished the globe with fresh supplies of the mixture, and "by this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe." By an ingenious contrivance, "the whole quantity of the burnt air was found to be 2,950 grain measures; its standard was 1·85." In other words, the residual uncondensed gas contained more oxygen than common air, in the proportion of 1·85 to 1·00. On proceeding to examine the product of combustion, it was found that the "liquor condensed in the globe, in weight about 30 grains, was sensibly acid to the taste, and by saturation with alkali and evaporation, yielded near two grains of nitre; so that it consisted of water united to a small quantity of nitrous acid. No sooty matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate." This is the first mention of the appearance of nitric acid, which compelled Cavendish to undertake an additional and difficult inquiry, and delayed the publication of his observations on the synthesis of the elements of water.

At first, Cavendish "suspected that the acid contained in the condensed liquor was no essential part of the dephlogisticated air," but was derived from the basic nitrate of mercury contained in the red precipitate from which the oxygen was prepared. He accordingly repeated the experiment, with oxygen from the same source, but which had been well agitated with water. The product of combustion, however, was still acid. The experiment was also tried with oxygen from red lead and oil of vitriol, but with the same result, only the nature of the acid was not ascertained. Then oxygen was procured from the leaves of plants, and exploded with inflammable air as before; the "condensed liquor still continued acid and of the nitrous kind."

There appeared thus to be no difference in the action of the oxygen depending on its mode of preparation. Could the production or appearance of acid depend on the proportions in which the gases were mixed? Cavendish proceeded to try this. He observes: "In these experiments the proportion of inflammable air was such, that the burnt air was not much phlogisticated; and it was observed, that the less phlogisticated it was, the more acid was the condensed liquor." In other words hydrogen was exploded with more than a combining measure of oxygen, in the experiments where acid appeared; and the greater the excess of oxygen, the larger the proportion of acid produced. The latter proposition, it need scarcely be noticed, can only be true within certain limits. Cavendish then repeated the experiment with oxygen from plants, and a large proportion of hydrogen, so that "the burnt air was almost completely phlogisticated," *i. e.*, a very slight residue of oxygen remained uncombined, and "the condensed liquor was then not at all acid, but seemed pure water; so that it appears that with this kind of dephlogisticated air, the condensed liquor is not at all acid when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but it is considerably so when it is not much phlogisticated." Cavendish then tried whether a difference in the proportions would in the same way affect the production of acid when the oxygen made use of was procured from red precipitate. He found the same law to hold: the more oxygen the more acid; and he adds, "there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air."

Oxygen procured from Turbith mineral (basic sulphate of mercury $3HgO + SO^3$) was then made use of, along with less than a combining volume of hydrogen, with a view to ascertain whether the acid produced would still be the nitrous, which it was found to be. Cavendish further showed that the non-appearance of acid, when common air was detonated with hydrogen, in such proportions as to condense nearly all the oxygen, was quite consistent with the results he had procured when the latter gas was substituted for air. For when a mixture of oxygen and nitrogen, in the proportions in which they form common air, was employed along with less than a combining measure of hydrogen, no nitrous acid appeared.

From the experiments recorded, Cavendish drew four conclusions, which, as stated by himself, are in effect as follows:— 1. When hydrogen and oxygen are exploded, with the latter in excess, a small quantity of acid is developed. 2. From whatever source the oxygen is procured, "the acid is always of the nitrous kind." 3. If the hydrogen and oxygen are in such proportions as to leave, after combination, only a slight residue of the latter, "the condensed liquor is not at all acid, but seems pure water, without any addition whatever." 4. It follows from 3, that "almost the whole of the hydrogen and oxygen is converted into pure water."

Cavendish, it will be observed, with the accuracy which as much characterizes his statements, as his experiments, contents himself with saying, that "*almost the whole*" of the mixture of gases became water. The uncombined residue, however, he goes on to show, was very small, "not more than $\frac{1}{17}$ of the dephlogisticated air employed, or $\frac{1}{50}$th of the mixture." The existence of a residue, which he did not attempt to reduce to a minimum or absolute zero, he refers to the impurities present in the hydrogen and oxygen; "consequently," he adds, "if those airs could be obtained perfectly pure, the whole would be condensed." The absence of acid from the liquid obtained by detonating hydrogen with air, or with a mixture in atmospheric proportions of oxygen and nitrogen, had still to be explained. Cavendish thought it probable, that when the other conditions for the production of acid were secured, namely, a small volume of hydrogen to a large one of air, "the explosion is too weak, and not accompanied with sufficient heat." Modern chemists have acquiesced with its originator in the justness of this view. A parenthetical passage then occurs, which it will afterwards appear was added to the paper after it was read, and before it was printed.* In this addition, Cavendish states that "all the foregoing experiments on the explosion of inflammable air with common and dephlogisticated air, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. Priestley, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions.† During the last summer also, a friend of mine‡ gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time, so far was M. Lavoisier from thinking any such opinion warranted, that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable that neither of these gentlemen found any acid in the water produced by the combustion, which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it."

Without anticipating what will afterwards be considered, it may be noticed here that Lavoisier was of opinion, as he himself tells us, "que l'air inflammable en brûlant devoit donner de l'acide vitriolique, ou de l'acide sulfureux."§

He acknowledges his acquaintance with Cavendish's experiments in the following terms:—"Ce fut le 24 Juin, 1783, que nous fîmes cette expérience, M. de la Place et moi, en présence de MM. le Roi, de Vandermonde, de plusieurs autres Acadé-

* The passage in question is the paragraph commencing, "*All the foregoing experiments,*" and ending "*a greater proportion of it.*" *Phil. Trans.*, 1784, pp. 134–135. It is placed within brackets, in Mr. Muirhead's reprint of the "Experiments on Air." *Watt Corr.*, p. 129.

† For 1783, pp. 426 and 434.

‡ Dr., afterwards Sir C. Blagden.

§ *Mémoires de l'Academie des Sciences pour* 1781 (printed in 1784), p. 473, reprinted by Mr. Muirhead in the *Watt Corr.*, p. 173. The pages of the original memoirs of Lavoisier, Meusnier, and Monge, on the composition, &c., of water, published in the *Mémoires de l'Académie,* 1784 and 1786, are copied here and elsewhere from Mr. Muirhead's convenient reprint, for the sake of those to whom the French work may be more accessible.

miciens, et de M. Blagden, aujourd'hui Secrétaire de la Société Royale de Londres; ce dernier nous apprit que M. Cavendish avoit déjà essayé, à Londres, de brûler de l'air inflammable dans des vaisseaux fermés, et qu'il avoit obtenu une quantité d'eau très sensible."*

Sir Charles (then Dr.) Blagden protested against this account of matters, as concealing a part of the truth. His statement is quoted here, as it shows who the friend was to whom Cavendish referred, and what were the grounds on which he made the reference to Lavoisier's knowledge of his experiments and conclusions; but to avoid repetition, the discussion of the question of priority between Cavendish and Lavoisier is reserved for another place. "He (Lavoisier) should likewise have stated in his publication, not only that Mr. Cavendish had obtained 'une quantité d'eau très sensible,' but that the water was equal to the weight of the two airs added together. Moreover, he should have added, that I had made him acquainted with Messrs. Cavendish and Watt's conclusions; namely, that water, and not an acid or any other substance, arose from the combustion of the inflammable and dephlogisticated airs."†

From this episode, Cavendish returns to the consideration of the subject which mainly interested him. Before doing so, however, he thinks it "proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air." This view was quite consistent with the phlogiston doctrines. When nitre, which contained nitric or nitrous acid, was heated alone, it gave off dephlogisticated air, *i. e.*, oxygen; when heated with charcoal, it yielded phlogisticated air, *i. e.*, nitrogen. Cavendish was well aware, as he states lower down, that "a small part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal." As he did not, however, know the composition of carbonic acid, or the nature of the inflammable gas which charcoal yielded when heated, his mere

* *Mém. de l'Acad.* for 1781, p. 472, reprinted in *Watt Corr.*, p. 176.

† Letter from Dr. Blagden to Dr. Crell, published in Crell's *Chemische Annalen*, 1786, translated by Mr. Muirhead in *Watt Corr.*, p. 73.

acquaintance with the fact of their production, gave him no assistance in explaining the changes which occurred, and he appears to have conceived that the nitrous acid obtained phlogiston from the carbon, and so became phlogisticated air. He explains this more fully in the subsequent paragraphs, which are too important from their bearing both on his views in reference to the nature of water, and the production of nitric acid by the action of the electric spark on air, to be omitted here. He observes that "it is well known that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a considerable analogy between that and the vitriolic acid." The analogy is then illustrated in a way which for brevity's sake I omit, and he proceeds :—"In like manner the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air, which readily quit their phlogiston to common air; but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shews no signs of acidity, and is still less disposed to part with its phlogiston than sulphur."

In these explanations Cavendish, true to the guiding principle of the phlogiston school, supposes an addition of phlogiston, where the present chemistry teaches that there is a loss of oxygen. Vitriolic or sulphuric acid by combining with a certain amount of phlogiston forms sulphurous acid, and by taking up still more phlogiston constitutes sulphur. Nitric acid united to a certain quantity of phlogiston forms nitrous acid or nitric oxide; united to still more phlogiston, it forms nitrogen.

This being premised, Cavendish proceeds to apply the views suggested, to the explanation of the acidity of the water, obtained by exploding apparently pure hydrogen and oxygen together. He indicates "two ways by which the phenomena of the acid found in the condensed liquor may be explained; first by supposing that dephlogisticated air contains a little nitrous acid, which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion; and secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air; but that, when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased, is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston and turned into nitrous acid; whereas, when the dephlogisticated air is not more than sufficient to consume the inflammable air, none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid."

It depended upon which of these views was adopted, what opinion should be held concerning the nature of water as well as of oxygen. The inseparability of the questions; what was the source of the nitrous acid? and what the composition of the water which it accompanied? has generally been overlooked in the criticisms which have been published of Cavendish's views, but in his apprehension it was essential that they should be studied together. In this spirit he proceeds to say—"If the latter explanation be true, I think we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston; since according to this supposition, these two substances united together form pure water." It has already been noticed that when Cavendish experimented on hydrogen in 1766, he regarded that body as phlogiston (see p. 197). He now gives Priestley and Kirwan the credit of this opinion which he abandons, in favour of the view that hydrogen is, what we should now call a hydrate of phlogiston. In a note he assigns as his reason for this change of belief, the observation, "that common or dephlogisticated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them." According to this view, hydrogen could not be a simple substance because it did not combine at ordinary temperatures with air or oxygen although they have a great affinity for it, but must be regarded as a compound of phlogiston more difficult of decomposition by oxygen, than nitric oxide or the alkaline sulphurets were. The question is further enlarged on by Cavendish, but its consideration is reserved till the discussion of the water controversy is reached.

Such was the view of the nature of oxygen, hydrogen and water, if the second supposition were adopted, as in fact it was by Cavendish. "On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid, and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole, as it is found that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air."

The second of these explanations Cavendish adopts as "much the most likely," and he assigns three significant reasons for the preference.

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

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

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

experiments." Cavendish then gives the details of two experiments, in each of which the same amount of oxygen and hydrogen from the same specimens was employed, but in the second trial a certain volume of nitrogen was added, which had the effect of increasing the proportion of nitric acid, as ascertained by a quantitative analysis of the liquid in both cases. "It must be observed," he remarks, "that all circumstances were the same in these two experiments, except that in the latter the air to be exploded was mixed with some phlogisticated air, and that in consequence the burnt air was more phlogisticated than in the former; and from what has been before said [*i. e.*, in the first explanation, as to the presence *ex hypothesi* of nitrous acid ready formed in the oxygen], it appears that this latter circumstance ought rather to have made the condensed liquor less acid; and yet it was found to be much more so, which shows strongly that it was the phlogisticated air which furnished the acid."

Another pair of trials leading to the same inference is then given, but it does not call for minute consideration, and the general conclusion from the whole is stated thus: "From what has been said, there seems the utmost reason to think that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former."

This is the last direct reference to the composition of water which the paper contains, but in the interval between its communication to the Royal Society and its publication, an essay on the same subject by Mr. Watt was read at one of the public meetings of that body. Cavendish in consequence added the following passage, which, as it has been much animadverted on, is here given entire.*

"As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few

* The passage in question is the paragraph beginning "*As Mr. Watt,*" and ending "*than it is worth.*" (*Phil. Trans.* for 1784, pp. 140, 141.) It is marked by brackets in Mr. Muirhead's reprint. (*Watt Corr.*, pp. 135, 136.)

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

Cavendish's views on the nature of heat, which he did not regard as a material entity, will appear when his papers on Freezing Mixtures are under review. In practice, his successors have for the greater part agreed with him in discarding Watt's special reference to the evolution of latent heat, as not more called for in the case of hydrogen and oxygen, than in that of other combining substances. But no theory of the synthesis of water can be complete, which does not account for the evolution of heat which accompanies the union of its elements. In so far, therefore, as Mr. Watt attempted to explain this, his view is more perfect, but it is now-a-days confessedly insufficient to account for the phenomena, which still await a satisfactory explanation.

The remainder of Cavendish's paper is chiefly employed with speculations on the nature of common air and oxygen, to which his discoveries led him. He begins by stating, more fully than he had done in his paper on the eudiometer, his conviction that Scheele and Lavoisier are right in supposing that "dephlogisticated and phlogisticated air are quite distinct substances, and not differing only in their degree of phlogistication; and that common air is a mixture of the two." The proof of this which he adduces, is one which they did not give, and could not have given; namely, "if the dephlogisticated air is pretty pure, almost the whole of it loses its elasticity by phlogistication, and, as appears by the foregoing experiment, is turned into water, instead of being converted into phlogisticated air." He goes on to say, that though in his experiments the whole of the mixture of oxygen and hydrogen was not turned into water, at least $\frac{18}{17}$ths were; and that by liver of sulphur he had reduced oxygen to less than $\frac{1}{30}$th of its bulk, and other parties had reduced it further; "so that there seems the utmost reason to suppose that the small residuum which remains after its phlogistication proceeds only from the impurities mixed with it." From this it should seem that Cavendish believed the diminution of air by liver of sulphur to result from the oxygen of the former combining with hydrogen derived from the alkaline sulphuret, and so changing into water. He then introduces a passage quoted at length in the notice of the paper on the eudiometer (p. 229), as to the proportion of oxygen present in air, which he states to be "one-fifth of its bulk;" and continues: "From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say it is deprived of, or receives, an addition of dephlogisticated air." This passage is interesting, as an approach to the language of the Lavoisierian school, which teaches that oxygen is added, where the believers in phlogiston held that the latter was removed, but Cavendish clung too closely to his faith in phlogiston, to abandon willingly its nomenclature, and he immediately excuses himself for making the proposed change, "as the other expression is convenient, and can scarcely be called improper, I shall (says he) still frequently make use of it in the remainder of this paper." This remainder will not require so full an abstract of its contents, as the portions already considered.

The light which Cavendish had now obtained on the true composition both of water and of air, changed of necessity his views concerning the theory of the methods in use for preparing oxygen. Priestley had supposed that both the vitriolic and nitrous acids were convertible into oxygen, as this gas could be prepared in the greatest quantity from substances containing those acids, especially the nitrous. Cavendish, however, thought that his researches seemed "to show that no part of the acid is converted into dephlogisticated air." In corroboration of this view, he refers to experiments proving that red precipitate, though it yields oxygen abundantly, contains no nitrous acid, "and consequently that, in procuring dephlogisticated air from it, no acid is converted into air; and it is reasonable to conclude, therefore, that no such change is produced in procuring it from any other substance."

He then considers in what manner those acids act in producing dephlogisticated air, and after adducing certain considerations which seem to warrant the inference, states it as his conclusion, "that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or *as quicksilver united to dephlogisticated air;* after which, on further increasing the heat, the water in it rises deprived of its phlogiston, that is, in the form of dephlogisticated air, and at the same time the quicksilver distils over in its metallic form." The passage marked above in italics, shows how clearly Cavendish apprehended the possibility of reversing the form of explanation current among believers in phlogiston, but he showed no preference for this view. In a note he observes, "It would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver; all that we can say is, that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston." This passage is important as showing, what many other passages in the papers of Cavendish and his contemporaries also show, that the discovery of the composition of water would not, in the hands of the disciples of the phlogiston school, have materially altered the aspect of chemistry. The difference it introduced was little more than this, that where formerly transferences of phlogiston, from one body to another, were assumed to take place, now water instead of phlogiston was shifted backwards and forwards, and decomposed and recomposed as the exigencies of theory required. The whole of the concluding part of Cavendish's paper, and of Watt's "Thoughts on the Constituent Parts of Water," amply illustrate this, and the former distinctly enounces it in the continuation of his remarks: "*Mercurius Calcinatus*," he observes, "appears to be only quicksilver, which has absorbed dephlogisticated air from the atmosphere during its preparation; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form; . . . . . but yet, as *uniting dephlogisticated air to a metal comes to the same thing as depriving it of part of its phlogiston, and adding water to it*, the quicksilver may still be considered as deprived of its phlogiston." After the same manner he accounts for the evolution of oxygen from nitre by the assumption, "that the acid absorbs phlogiston from the water in the nitre, and becomes phlogisticated, while the water is thereby turned into dephlogisticated air."* A little further on, however, he suggests it as not unlikely "that part of the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part."

A good deal of space is then devoted to the theory of the evolution of oxygen from the sulphate of the red oxide of mercury, and turbith mineral, which need not be minutely dwelt on, as the general conclusion is "that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar." Some remarks follow on the mode in which vitriolic acid acts in the production of dephlogisticated air, which do not call for special notice, and Cavendish proceeds, "There is another way by which dephlogisticated air has been found to be produced in great quantities, namely, the growth of vegetables exposed to the sun or daylight; the rationale of which in all probability is, that plants, when assisted by the light, deprive part of the water sucked up by their roots, of its phlogiston, and turn it into dephlogisticated air, while the phlogiston unites to, and forms part of, the substance of the plant." It need scarcely be mentioned, that the view suggested here by Cavendish, that plants can decompose water and retain its hydrogen is probably well founded, but that the chief source of the oxygen which plants so largely evolve, is not decomposed water, but decomposed carbonic acid. In confirmation of his view, Cavendish adduces

* Watt held an exactly similar view. *Phil. Trans.* for 1784, p. 336.

many proofs "that light has a remarkable power in enabling one body to absorb phlogiston from another." He cites as illustrations of this: 1. the bleaching by sunlight of a spirituous tincture of green leaves, when contained in bottles only partially filled, so as to contain, besides the liquid, a portion of air; 2. the yellow tint which colourless nitric acid acquires by exposure to light; and 3. the effect of the same agent in reducing to the metallic state moist salts of silver and gold. In reference to the last example, he adds, "There is the utmost reason to think, that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water."

Having in this way shown the probability of plants possessing the power he attributes to them, he observes that "vegetables seem to consist almost entirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water, since, by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere, and forms water, they seem to be reduced almost entirely to water, and those two kinds of air. Now plants growing in water without earth, can receive nourishment only from the water and air, and must, therefore, in all probability, absorb their phlogiston from the water." The use of light in promoting the growth of plants, Cavendish thus refers to its enabling them to absorb phlogiston from the water. He alludes also to the fact that plants yield more oxygen when growing in water impregnated with fixed air, than they do in plain distilled water, but he did not understand the function of the carbonic acid, of the composition of which he was ignorant, nor did he suspect that it was decomposed. His explanation of its action was that "as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance than in other water."

This is the concluding passage of the paper as it was originally sent to the Society, but its author added a passage between the reading and the printing of his essay, which is of great importance, as containing a comparison of the merits of the phlogiston hypothesis of which he was certainly the ablest English advocate, and the antiphlogiston theory which the great French chemist was now enforcing by irresistible arguments.* "There are several memoirs," he observes, "of Mr. Lavoisier, published by the Academy of Sciences, in which he intirely discards phlogiston, and explains those phenomena which have been usually attributed to the loss or attraction of that substance by the absorption or expulsion of dephlogisticated air; and as not only the foregoing experiments, but most other phenomena of nature, *seem explicable as well, or nearly as well*, upon this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other." Cavendish then gives a short but very perspicuous sketch of Lavoisier's views, which it is not necessary to quote at length, but one statement demands notice. "According to this [Lavoisier's] hypothesis, we must suppose that water consists of inflammable air united to dephlogisticated air." It seems at first sight singular that Cavendish should use these words to express Lavoisier's opinion, for they are identical with those in which the former expressed his own view. The difference, therefore, must have lain in the meaning attached to the words by the French and English chemist, and it is important, in reference to the assignment of the due share of merit to all parties, to notice that Cavendish imputed a different opinion to Lavoisier from that which he held himself. The difference lay solely in the view entertained concerning inflammable air. According to the former, it was a principle common to many combustibles, and either alone, or in combination with water, assumed the form of an elastic fluid. According to the latter it was a substance *sui generis*, present in some of the compound combustibles, but not contained in those which were simple, such as sulphur, carbon, phosphorus, and the metals. The difference of opinion went for little, in reference to water considered alone, but it was of the greatest importance in reference to the theory of chemical changes in which combustibles and oxygen took a part.

* The added passage begins, "*There are several memoirs*," and ends '*those three substances*." (*Phil. Trans.* for 1784, pp. 150—153.) It is inclosed in brackets in Mr. Muirhead's reprint. (*Watt Corr.*, pp. 147—150.)

After stating Lavoisier's view of the composition of water, the oxyacids, and the metallic calces, and his rationale of the evolution of oxygen from red precipitate and nitre, as well as from growing plants, Cavendish comments upon it thus:—" It seems, therefore, from what has been said, as if the phenomena of nature might be explained very well on this principle without the help of phlogiston; and, indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston, and adding water to it, and as there are perhaps no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phenomena, at least as well as Mr. Lavoisier's, I have adhered to that."

This view of Cavendish's was referred to before. It shows strikingly the necessity under which a false theory lies of multiplying falsities, when contradicted by an *experimentum crucis.* Instead of an appeal to the Balance, which would have at once shown that only one of the theories consisted with truth, or an endeavour to collect and exhibit the water, which according to Cavendish was produced at every oxidation, the necessary experiments were declined on the score of their difficulty, and the production of water assumed, instead of demonstrated. At a later period, a similar line of defence was adopted by those who advocated the doctrine of the composite nature of chlorine, and named it Oxymuriatic Acid, but it was brought to the test of direct trial, and water was shown not to be produced. In truth, Cavendish's defence of his antiphlogiston views, has all the appearance of an after-thought, and was added after his opinions had been made public, as a justification of what could not then be withdrawn. He is more successful in disputing the truth of Lavoisier's doctrine "that dephlogisticated air is the acidifying principle," which he acknowledges to be true if no more be meant than that the addition of phlogiston (*i. e.*, the loss of oxygen) deprives certain acids of their acidity. This, however, he did not think was a universal phenomenon, for "as to the marine acid and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston," or as Lavoisier would have stated the fact, by any loss of oxygen. This shrewd observation, it need not be said, has been confirmed, so far at least as marine or hydrochloric acid is concerned, but there is no reason to suppose that Cavendish was aware of the true nature of that acid.

The "Experiments on Air" were immediately subjected to criticism, in so far as they were adduced by their author, as disproving the doctrine that the phlogistication of air was always attended by the production of carbonic acid.

Richard Kirwan, one of the most accomplished chemists of the time, held this view strongly, and published "Remarks on Mr. Cavendish's Experiments on Air." The paper was read to the Royal Society on February 5th, 1784, about three weeks after that on which it commented, and it follows Cavendish's in the vol. of Transactions for that year.* Kirwan endeavours to prove that during the calcination and amalgamation of metals, and by the action of nitric oxide, and of the electric spark on atmospheric air, fixed air (carbonic acid) is produced. It is unnecessary to discuss the paper at length, as its views, for the greater part, are now discredited, and the objections adduced against Cavendish's conclusions are known to be unfounded. For the same reason, it is unnecessary to give Cavendish's "Answer to Mr. Kirwan's Remarks upon the Experiments on Air," which was read to the Royal Society, March 4th, 1784, and follows Kirwan's paper in the Transactions.† An extract from the conclusion (p. 175) will sufficiently illustrate the nature of Cavendish's reply. "There are five methods of phlogistication considered by me in my paper on air, namely: first, the calcination of metals, either by themselves, or when amalgamated with quicksilver; secondly, the burning of sulphur or phosphorus; thirdly, the mixture of nitrous air; fourthly, the explosion of inflammable air; and fifthly, the electric spark. . . . . . . As to the first method, or the calcination of metals, there is not the least proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper insinuate that we had. The same thing may be said of the burning of sulphur

* Vol. lxxiv., p. 154. † Ibid., p. 171.

and phosphorus. As to the mixture of nitrous air and the combustion of inflammable air, it is proved that if any fixed air is generated, it is so small as to elude the nicest test we have. . . . . . So that out of the five methods enumerated, it has been shown, that in two no sensible quantity is generated, and not the least proof has been assigned that any is in two of the others; and as to the last [the electric spark] good reasons have been assigned for thinking it inconclusive, and therefore the conclusion drawn by me in the above-mentioned paper seems sufficiently justified; namely, that though it is not impossible that fixed air may be generated in some chemical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air by phlogistication is by no means owing to the generation or separation of fixed air from it."

This paper was followed by a "Reply to Mr. Cavendish's Answer," read to the Royal Society, March 18th, 1784,* in which Kirwan contests some of Cavendish's opinions, but very briefly, because he considers, as he states at the beginning, the greater part of his reply to Cavendish as still unanswered. The latter, however, entered into no further discussion as to the general question, what causes the diminution of air during its deoxidation.

In one respect, Cavendish's answer was incomplete. He acknowledged that he could not give any proof of the truth of his opinion, that the diminution of the air by the electric spark, "is owing to the burning or calcination of some substance contained in the apparatus." He proceeded, however, soon after, to test the justice of this theory, and found that it was only partially true, and that the production of nitric acid was the chief cause of the diminution of air by the electric spark. This leads directly to his next paper which contains the announcement of his great discovery of the composition of nitric acid. It bore the same title as his previous communication.

* *Phil. Trans.*, vol. lxxiv., p. 178.

Experiments on Air.

Second Series.

This paper* is professedly a continuation of that recording the first series of Experiments on Air. In it Cavendish had conjectured, as he now repeats, that the diminution in bulk which air undergoes, when phlogisticated by the electric spark, "was owing to the burning of some inflammable matter in the apparatus; and that the fixed air supposed to be produced in that process, was only separated from that inflammable matter by the burning." He now records experiments made with a view to test the truth of this conjecture, which showed that the real cause of the diminution was very different from what he suspected, and depended "upon the conversion of phlogisticated air into nitrous acid." The experiments are then related which led to the important discovery of the nature of that acid. The first part of the paper is occupied with an account of the apparatus made use of, which need not be minutely described. It consisted essentially of a glass syphon filled with quicksilver and inverted, so that each of the limbs stood in a glass of the same fluid. Air was then passed up into the syphon through the quicksilver, and soap-lees, or any other liquid employed during the experiment, was introduced in the same way. To transmit the electric spark, an insulated ball connected with the quicksilver in one glass, was placed at such a distance from the conductor of a Friction Electrical Machine, as to receive a spark, whilst the quicksilver in the other glass communicated with the ground.

In the first experiment, "When the electric spark was made to pass through common air included between short columns of a solution of litmus, the solution acquired a red colour and the air was diminished, conformably to what was observed by Dr. Priestley." Lime-water was then substituted for the litmus, and the spark passed till the air could be no further diminished. No cloudiness appeared in the liquid, but the air was reduced to two-thirds of its original bulk, "which is a greater diminution than it could have suffered by mere phlo-

* Read June 2nd, 1785, *Phil. Trans.*, 1785, p. 372.

gistication, as that is very little more than one-fifth of the whole." The experiment was then repeated with impure oxygen, but no cloudiness of the lime-water appeared, and it was not precipitated by the addition of carbonic acid, but yielded a brown sediment on the further addition of ammonia. From these phenomena, Cavendish inferred that "the lime-water was saturated by some acid formed during the operation," but that no fixed air was produced. Soap-lees, however, (solution of caustic soda or perhaps potash) were found to cause a more rapid diminution of air than lime-water did, so that they were employed in the subsequent experiments. The first of these was made with a view to determine "what degree of purity the air should be of, in order to be diminished most readily, and to the greatest degree." Oxygen suffered a slight diminution, doubtless owing to the presence of impurities, such as nitrogen and traces of combustible matter. When nitrogen was used, "no sensible diminution took place; but when five parts of pure dephlogisticated air were mixed with three parts of common air, [or, according to Cavendish, three volumes of nitrogen with seven of oxygen,] almost the whole of the air was made to disappear."* This ascertained, a mixture of nitrogen and oxygen, in the proportions stated above, was exposed to the electric spark whilst confined over soap-lees. As fast as the air was diminished, fresh quantities were supplied, till no further diminution took place. The soap-lees were then poured out of the tube, separated from the quicksilver, and examined as to the change they had undergone. They "seemed to be perfectly neutralised, as they did not at all discolour paper tinged with the juice of blue flowers. Being evaporated to dryness, they left a small quantity of salt, which

* Cavendish's calculation of the relative amount of oxygen and nitrogen present in the mixture of these gases, which he entirely converted into nitric acid, is erroneous. The mistake must have been accidental, for he states, as the basis of his calculation, "that common air consists of one part of dephlogisticated air (oxygen), mixed with four of phlogisticated (nitrogen)." Three measures, therefore, of air, could contain only six-tenths of a measure of oxygen, instead of two measures, which he assumed to be present. The actual composition of his mixture of 5 parts of oxygen with 3 parts of air, was—oxygen 5·6 volumes, nitrogen 2·4. He should have taken 5 measures of air to 9 of oxygen, or what is the same proportion, 2 measures of nitrogen to 5 of oxygen. The mixture he did employ nearly approached in composition to this.

was evidently nitre, as appeared by the manner in which paper impregnated with a solution of it burned." The experiment was repeated on a larger scale, and with still more decisive results. "The liquor, when poured out of the tube, smelled evidently of phlogisticated nitrous acid, and being evaporated to dryness, yielded $1\frac{4}{10}$ gr. of salt, which is pretty exactly equal in weight to the nitre which that quantity of soap-lees would have afforded, if saturated with nitrous acid. This salt was found, by the manner in which paper dipped into a solution of it burned, to be true nitre." It contained a trace of vitriolic acid, but not more than the soap-lees originally did, and "there is no reason to think that any other acid entered into it, except the nitrous." At first, indeed, it appeared that some hydrochloric acid had been produced, for the saturated soap-lees precipitated nitrate of silver; but Cavendish set this apparent evidence of hydrochloric acid aside, referring the precipitation to the acid produced having been much phlogisticated, in other words, to its having been one of the lower oxides of nitrogen.

The production of nitric acid being thus beyond doubt, Cavendish proceeded to form a theory as to its generation. In his previous paper, detailing "Experiments on Air," he had referred to the results obtained when charcoal is detonated with nitre as proving that "phlogisticated air [nitrogen] is nothing else than nitrous acid united to phlogiston." He now considers what are the logical consequences of this view, if it be followed out. The statement is best given in his own words. "According to this conclusion, phlogisticated air ought to be reduced to nitrous acid by being deprived of its phlogiston. But as dephlogisticated air is only water deprived of phlogiston, it is plain, that adding dephlogisticated air to a body, is equivalent to depriving it of phlogiston, and adding water to it; and therefore, phlogisticated air ought also to be reduced to nitrous acid, by being made to unite to, or form a chemical combination with, dephlogisticated air; only the acid formed this way will be more dilute than if the phlogisticated air was simply deprived of phlogiston." In other words, nitrogen is a compound of nitrous acid and phlogiston, which is a hydrate of inflammable air, and when oxygen is added to nitrogen, it unites with the phlogiston of the latter, producing water, which dilutes the nitrous acid separated or set free at the same time*. Cavendish then continues, "This being premised, we may safely conclude, that in the present experiments the phlogisticated air was enabled, by means of the electrical spark, to unite to, or form a chemical combination with, the dephlogisticated air, and was thereby reduced to nitrous acid, which united to the soap-lees and formed a solution of nitre; for in these experiments those two airs actually disappeared, and nitrous acid was actually formed in their room; and as, moreover, it has just been shown, from other circumstances [namely, from the action of the charcoal on nitre referred to in the previous paper, *Ante*, p. 241], that phlogisticated air must form nitrous acid, when combined with dephlogisticated air, the above-mentioned opinion seems to be sufficiently established. A further confirmation of it is, that, as far as I can perceive, no diminution of air is produced when the electric spark is passed either through pure dephlogisticated air, or through perfectly phlogisticated air; which indicates the necessity of a combination of these two airs to produce the acid. Moreover, it was found in the last experiment that the quantity of nitre procured was the same that the soap-lees would have produced if saturated with nitrous acid; which shows, that the production of the nitre was not owing to any decomposition of the soap-lees."

Nothing can be clearer than the interpretation here given of the immediate phenomena concerned in the production of nitrous acid, although the more remote reactions were obscured by Cavendish's false notions as to the nature of inflammable air which have frequently been referred to. The bearing of these results on those obtained in the previous experiments, on the occasional production of acid along with water, when hydrogen and oxygen were detonated together is evident, and was fully recognised by Cavendish. In reference to his earlier researches, he remarks, " I also gave my reasons for thinking, that the small

* Nitrogen would thus have been styled, according to our present nomenclature, *a hydrated nitrate of hydrogen*. When burned along with oxygen, the hydrogen was oxidised, and the resulting water combined with the nitric acid, which was set free.

quantity of nitrous acid, produced by the explosion of dephlogisticated and inflammable air, proceeded from a portion of phlogisticated air mixed with the dephlogisticated, which I supposed was deprived of its phlogiston, and turned into nitrous acid by the action of the dephlogisticated air on it, assisted by the heat of the explosion. This opinion, as must appear to every one, is confirmed in a remarkable manner by the foregoing experiments; as from them it is evident, that dephlogisticated air is able to deprive phlogisticated air of its phlogiston, and reduce it into acid, when assisted by the electric spark; and therefore it is not extraordinary that it should do so, when assisted by the heat of the explosion."

The discovery of the power of hydrogen to produce water when detonated with oxygen, led directly to speculations on the nature of hydrogen and oxygen; and the derivation of nitric acid from nitrogen, led in like manner to theories concerning its intrinsic qualities. Cavendish, accordingly, followed up his discovery of the composition of nitric acid by an inquiry into the nature of nitrogen. He points out in the sequel of his paper that the known properties of this gas are all negative, and that it might fairly be doubted "whether there are not, in reality, many different substances confounded by us under the name of phlogisticated air." He "therefore made an experiment to determine, whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the rest, which would refuse to undergo that change." On trial, he found that so small a quantity of nitrogen escaped conversion into nitric acid, "that, if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude that it is not more than $\frac{1}{120}$th part of the whole."

The remainder of the paper is occupied with a detail of experiments, instituted with a view to ascertain whether, "when any liquor, containing inflammable matter, was in contact with the air in the tube, some of this matter might be burned by the spark, and thereby diminish the air." To determine this point, oxygen was confined over distilled water, soap-lees, and infusion of litmus respectively; so that while the conditions essential to the production of nitric acid were wanting, those conducing to combustion were most fully realised. Very slight diminution occurred with the first two liquids, owing, doubtless, to the presence of a little air, and therefore of nitrogen. When a solution of litmus was employed, it became first red, and, by-and-by, as successive sparks passed, paler and paler, till it was quite colourless and transparent. When lime-water was let up into the tube, it became cloudy; "therefore the litmus was, if not burnt, at least decompounded, so as to lose entirely its purple colour, and to yield fixed air."

The account which Cavendish gave of his experiments was so full and explicit, that apparently no one could fail of success who repeated them. It was otherwise, however; and in consequence he published the following paper, nearly three years after the first:—

*On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitrous Acid by the Electric Spark.**

After a reference to the nature of the experiments recorded in the first communication on nitric acid to the Royal Society, Cavendish says: "As this experiment has since been tried by some persons of distinguished ability in such pursuits without success, I thought it right to take some measures to authenticate the truth of it. For this purpose I requested Mr. Gilpin, clerk to the Royal Society, to repeat the experiment, and desired some of the gentlemen most conversant with these subjects to be present at putting the materials together, and at the examination of the produce."

From a later part of the paper (p. 271), it appears that the parties "who have endeavoured to repeat this experiment are, M. Van Marum, assisted by M. Paets Van Trootswyk; M. Lavoisier, in conjunction with M. Hassenfratz; and M. Monge." "I am not acquainted," adds Cavendish, "with the method which the three latter gentlemen employed, and am at a loss to conceive what could prevent such able philosophers from succeeding, except want of patience."

As later chemists have found no difficulty in repeating Cavendish's experiments on the production of nitric acid by

* Read to the Royal Society, April 17, 1788; *Phil. Trans.*, lxxviii., p. 261, 1788.

the synthesis of nitrogen and oxygen, it is not necessary to analyse this paper minutely.

Two repetitions were made with the same apparatus as Cavendish himself employed. The tedious transmission of electric sparks was managed by Mr. Gilpin alone, but the commencement and conclusion of the experiment were witnessed by others. "On December 6, 1787, in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. J. Hunter, and Mr. Macie, the materials were put together."

"On January 28 and 29, the produce of this experiment was examined in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson." The result of this repetition was, in the words of Cavendish, "that the mixture of the two airs was actually converted into nitrous acid; only the experiment was continued too long, so that the quantity of air absorbed was greater than in my experiments, and the acid produced was sufficient, not only to saturate the soap-lees, but also to dissolve some of the mercury." A second repetition accordingly was commenced on February 29, 1788; and "on March 19, the produce was examined, in the presence of Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson." It yielded the same results as before; only less mercury was dissolved, so that the characteristic properties of the nitre were more easily perceived.

It may be noticed here, that the great test on which Cavendish relied as a proof of the conversion of the soap-lees into nitre, was the deflagration of paper dipped into a solution of the latter. The presence, however, of nitrate of mercury interfered with the characteristic combustion of the touch-paper, and rendered the demonstration of the production of nitric acid less satisfactory. It would have been better had mercury been dispensed with in the experiments, and the spark passed over the surface of the soap-lees, which could have been introduced into a syphon or Volta's eudiometer, with its wires so arranged as to be a little above the liquid. Faraday has shown that, if a piece of paper dipped in a solution of caustic potash be stretched a little below and between two brass balls, from one of which electric sparks are passing to the other, the alkali will be rapidly converted into nitrate of potass, and the paper become touch-paper. Van Marum, who employed in his repetition of the nitric acid experiments the celebrated Teylerian electric machine, failed, notwithstanding—at least as he supposed—in obtaining the same results as Cavendish. He "used a glass tube, the upper end of which was stopped by cork, through which an iron wire was passed and secured by cement, and the lower end was immersed into mercury; so that the electric spark passed from the iron wire to the soap-lees." At the conclusion of the experiment, the alkali seemed unsaturated. Cavendish made two objections to the mode in which Van Marum and Trootswyk experimented: the first, that there was a risk "of the iron wire being calcined by the electric spark, and absorbing the dephlogisticated air;" the second, "that the only circumstance from which they concluded that the alkali was not saturated, was the imperfect marks of deflagration that the paper dipped into it exhibited in burning, which, as we have seen, might proceed as well from some of the mercury having been dissolved, as from the alkali not being saturated."

Van Marum supposed that the difference between his results and those of Cavendish arose from the latter having used oxygen prepared from the impure black oxide of mercury, and reproached him with having declined to explain how this oxide was prepared. Cavendish, however, made no mystery of the process, which simply consisted in agitating mercury mixed with lead, as he informed Van Marum in a letter printed in his paper. In the letter he also points out that he had likewise employed oxygen from turbith mineral, and that there was no reason for supposing that the source of the oxygen made any difference as to the success of the experiment. Van Marum, after all, however, only doubted whether the alkali could be entirely saturated. As to the possibility of this, he and Trootswyk would not speak positively: "Nous contentant pour le présent d'avoir vu, que l'union du principe d'air pur et de la mofette produit de l'acide nitreux, suivant la decouverte de M. Cavendish."*

* Von Marum's account is quoted by Cavendish in his paper, and the answer which the latter sent to a letter from the former requesting information is given in full.

The failures in repeating the nitric acid experiment were soon forgotten, and the value of Cavendish's discovery was universally acknowledged. Dr. Black says of it: "This discovery by Mr. Cavendish is one of the most important in the whole science of chemistry."* Black here referred chiefly to the new light which it threw on the theory of chemistry; and the unanimous opinions of later authorities on this point need not be detailed. Neither Cavendish, however, nor any of his contemporaries, anticipated the importance which his discovery would be found to possess in relation to natural phenomena. We now perceive, that not only must every lightning flash convert a certain quantity of atmospheric air into nitric acid, but that this compound, either in its free state, or as nitrate of ammonia, must be brought to the earth by the rains that accompany or follow thunder-storms. Much discussion has been carried on, as to the importance of this atmospheric nitric acid in furnishing plants with nitrogen, especially in tropical regions, where thunder-storms are frequent. The subject cannot be considered at length here: it is fully discussed in the last editions of Liebig's "Chemistry of Agriculture," and of Johnston's "Agricultural Chemistry."

Cavendish's discovery must also be considered as supplying the explanation of the production of nitrates in the soil, which was so long a perplexity to chemists, inasmuch as it demonstrated that, although nitrogen appears, when carelessly examined, to be a quite neutral or indifferent substance, it is in reality a combustible body. It was left to the ingenuity and skill of Cavendish's successors, and especially to Liebig, to show in what way exactly nitrogen undergoes combustion, during the generation of nitre in a mixture of animal matter and alkali, and to point out the great probability of ammonia derived from the decaying animal matter being the compound of nitrogen which is burned by the oxygen of the air during nitrification. When Cavendish, in 1781 and afterwards, burned a mixture of hydrogen, oxygen, and nitrogen into water and nitric acid, he established a truth which, more than half a century later, was

* Lectures, vol. ii., p. 108.

to furnish the rationale of the process by which nitre is generated wherever moist organic matter containing nitrogen, along with an alkaline, or other powerful base, and free oxygen, meet under the influence of a suitable temperature. It would not be easy, accordingly, at the present day, to over-estimate the importance of Cavendish's discovery of a method by which common air may be converted into nitric acid.

The paper of 1788 was the last of Cavendish's published chemical researches. I now, accordingly, consider in detail his claims as the discoverer of the composition of water. These have already been referred to in the "Life," but only dogmatically. I here enter upon a critical inquiry into the relative claims of all the alleged authors of that discovery. This cannot be prosecuted without some repetition of what has been stated already; but as far as possible I shall avoid going over ground already traversed. The statements in the Life are addressed to general readers. What follows is intended for students of science.

A CRITICAL INQUIRY INTO THE CLAIMS OF ALL THE ALLEGED AUTHORS OF THE DISCOVERY OF THE COMPOSITION OF WATER.

PRELIMINARY DISCUSSION.

THE remarkable discovery which Cavendish announced in his first "Experiments on Air," that a mixture of hydrogen and oxygen can be burned into its own weight of water, had scarcely been made public in January, 1784, before James Watt claimed the announcement as having been previously made by him; and Lavoisier declared that he had discovered the compound nature of water before, and independently of either. A controversy accordingly arose, in which Cavendish and Watt disputed with each other the priority of the discovery, whilst they were at one in disallowing Lavoisier's claim to take precedence of either. The dispute was not settled during the lifetime of the claimants, at least so far as the English rivals were concerned, and for a long period after their deaths the controversy excited no public interest. In 1839, however, it revived, and from that period to the present it has occasioned the keenest discussion among some of the most eminent literary and scientific men of England, Scotland, and France. I shall refer to it in future as the WATER CONTROVERSY. It will take its place in the history of science side by side with the discussion between Newton and Leibnitz, concerning the invention of the Differential Calculus; and that between the friends of Adams and Leverrier, in reference to the discovery of the planet Neptune.

For the sake of perspicuity I shall divide the following discussion into several sections, and first consider those who have taken part in it.

1. *Disputants in the Water Controversy.*

The Water Controversy commenced in March, 1784, when De Luc made known to Watt the contents of Cavendish's "Experiments on Air," read to the Royal Society on the 15th of January of that year. Watt in consequence transmitted to the Society his "Thoughts on the constituent parts of Water, &c." which formed the subject of three communications read to that body in April and May, 1784; and in the first of these his claim to the disputed discovery was carried back to April 26th of the preceding year. Lavoisier in the meanwhile laid before the French Academy of Sciences his alleged discoveries on the same subject, sharing the merit to some extent with his countrymen, La Place, Meusnier, and Monge, but awarding no credit to Watt, and scarcely any to Cavendish. An interval, however, of several months elapsed between the reading and publication of the "Experiments on Air," during which Cavendish made three additions to his paper, which have been already referred to, and will be noticed again. In one of these, which alone concerns us at present, he implicitly asserts a claim to priority over Watt, by stating that many of his experiments had been performed in 1781, and communicated to Priestley, before the latter made certain researches whose latest date is April 21st, 1783; and in the same paragraph he explicitly declares, that Lavoisier was made acquainted with his views in the summer of the same year, before the French chemist had formed any opinion of his own as to the composition of water. Here, so far as the principals were concerned, the controversy ended. Cavendish and Watt contented themselves with putting on permanent record the evidence which seemed necessary to justify their claims to the discovery in dispute, but neither made any public attack on his rival, although each in private asserted priority over the other. No long time, moreover, elapsed before these illustrious men were sufficiently reconciled to cultivate each other's acquaintance. I have conversed with more than one scientific man, who remembers to have seen them together at Sir Josephs Banks' conversaziones, and we have it on the authority of Mr. James Watt in reference to his father, that "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."*

It was not likely in these circumstances that the dispute would be revived by those whom it most concerned. Neither Cavendish nor Watt accordingly appears to have directly or openly stirred in the matter again, although the friends of each were careful to maintain his claims, when opportunity for so doing offered. One such interference only calls for notice. In 1786 Blagden, the intimate friend of Cavendish, addressed a letter to Crell, the editor of a well known German scientific journal, in which he accuses Lavoisier of having misstated the nature and amount of the information which Blagden communicated to him in reference to Cavendish's experiments. This letter, which will be fully discussed further on, avoided any consideration of the question of priority between the English philosophers, and is the last public allusion to the Water Controversy which was made during their lifetime. Lavoisier made no reply to the serious charges preferred against him, and the question had ceased to be one of general interest long before the close of last century.

Cavendish died in 1810, Watt in 1819, and the next year witnessed the decease of Sir Joseph Banks and Sir Charles Blagden, the last survivors of those who had taken part in the controversy of 1784. From 1820 till 1839 nothing occurred to revive the dispute, nor did any one during the interval systematically endeavour to adjust the merits of the several claimants, although various opportunities offered for attempting this. Many references however, were made in scientific treatises to the great discovery of the composition of water, but generally of a dogmatic kind, and consisting chiefly of ascriptions of the merit of the discovery entirely to one or other of the claimants, though sometimes they recommended a division of the merit, in different ways among them. I do not detail those imperfect notices of the Water Controversy for two reasons. In the first place, the question under discussion is not one which can be settled by an appeal to authorities, or the balancing of great names, such as those of Berzelius and Davy, Arago and Brewster, Dumas and Peacock, against each other, according to the different views which each of them has

* *Watt Corr.* p. iv. Ante, p. 161, where Cavendish's visit, in 1785, to Watt at Birmingham is noticed. To prevent confusion, I shall hereafter refer to the celebrated engineer by his surname as Watt, and to his son, who died recently, as Mr. James Watt.

taken. The question is one of evidence, which these and the other distinguished men who have taken part in the dispute, can assist in deciding only by their skill in the analysis and exposition of facts, the value of which may be appreciated by persons of ordinary intelligence. In the second place, it is only since 1839, that the documents essential to the settlement of the controversy have been even partially before the public, and only since 1846, when the *Watt Correspondence* was published in full, that the means of coming to a satisfactory conclusion have been in the hands of all. It may be doubted, indeed, whether we are yet in a condition to judge fairly of Lavoisier's claims, but we know enough to assure us that they are of posterior date to those of Cavendish and Watt, and the question I have chiefly to consider affects the rival claims of the English philosophers.

I pass therefore to 1839, when the vexed question, who first discovered that water is a compound of hydrogen and oxygen, was at length, and for the first time, deliberately discussed, and pressed to a settlement. Mr. James Watt, who had been associated with his father in his studies and was familiar with his views on the composition of water, never lost sight of his claims as a discoverer in chemistry, and from time to time sought to interest various distinguished philosophers in a matter which he had naturally much at heart. He did not succeed, however, in inducing any of his countrymen to come forward publicly to assert Watt's priority to Cavendish as the discoverer of the true nature of water, but he ultimately found in one of the most accomplished French philosophers an able and willing advocate of his father's claims.

Watt enjoyed the honour of being one of the Foreign Members of the French Institute, and in 1833, Arago was called upon, in his capacity of Perpetual Secretary of the Academy of Sciences, to write the *éloge* of the great engineer. Arago accordingly came to this country in the autumn of 1834, to obtain materials for the projected memoir, and having satisfied himself that Watt, and not Cavendish, was the discoverer of the compound nature of water, he announced this in his "Historical Eloge of James Watt," which was read to the Academy in December, 1834, but not published till 1838. Lord Brougham, who had been requested by Mr. James Watt to pass judgment on the validity of the evidence adduced in support of his father's claims, was present at the public meeting of the Institute at which the *éloge* was read. On his return to England he drew up a "Historical Note on the discovery of the theory of the Composition of Water," to which Mr. James Watt appended some notes, and the double document was published along with Arago's Eloge in the *Annuaire du Bureau des Longitudes*, for 1839; as well as in the *Mémoires de l'Académie des Sciences*, for the same year.

Cavendish had no near relative alive to watch over his interests, as James Watt had watched over those of his father; but his memory was too sacred in the eyes of his countrymen, to allow of his being long left without zealous defenders. The Rev. William Vernon Harcourt was President of the British Association for the Advancement of Science, for 1839, and conceiving that Arago had done great injustice to Cavendish, devoted a considerable portion of his eloquent inaugural address to the refutation of what he regarded as unjust and erroneous views.* To these animadversions Arago replied at a meeting of the French Academy in 1840, and the distinguished chemist Dumas put on record his conviction

* *Report of Brit. Assoc. for* 1839. President's Address, p. 6 to 15.

that his colleague's history of the discovery of the composition of water was complete in all respects.*

Sir David Brewster sought meanwhile to mediate between the contending parties, but did not succeed in inducing the advocates of the claims of Watt and Cavendish to moderate the extreme views urged on both sides.† The controversy in consequence proceeded without any abatement of its onesidedness. When the report, accordingly, of the British Association for 1839 was published, nearly a year after Mr. Harcourt's address was delivered, he added a postscript, containing a lengthened reply to Arago, Dumas, Brougham, and Brewster, along with an appendix containing various documents bearing on Cavendish's claim, as the only party entitled to the honour of being styled the discoverer of the composition of water. In 1841, Berzelius published a conditional judgment in favour of Watt's claims.‡ In 1845, Lord Brougham animadverted on Mr. Harcourt's Postscript, questioning the validity of certain of the proofs he adduced of Cavendish's priority.§ In the same year the Dean of Ely (Dr. Peacock) reviewed his Lordship's work, assailing his conclusions and asserting anew the claims of Cavendish.‖ In 1846, Mr. Harcourt likewise replied at great length to Lord Brougham, and reiterated his previous declarations.¶ And the year after, Dr Whewell reasserted his conviction that Cavendish was entitled to the honour of the disputed discovery.**

The friends of Watt were not slow in reasserting his claims. A most important addition was made to the literature of the Water Controversy in 1846, by the publication of the "Correspondence of the late James Watt on his discovery of the Theory of the Composition of Water." This was accompanied by a letter from his son, and an introduction by the editor, Mr. Muirhead, a kinsman of Watt's, in which the right of their illustrious relative to the entire merit of the discovery was urged in the strongest terms. Finally, the *Watt Correspondence* was discussed in 1847 by Sir David Brewster in the North British Review, and in 1848 by Lord Jeffrey in the Edinburgh Review, both of whom prefer the claims of Watt to those of Cavendish. Lord Jeffrey's paper likewise contains an authorised statement from M. Dumas that he held unchanged the opinion in favour of Watt, "which he put upon record nearly seven years ago."††

The preceding list does not include the names of all who have taken part in the Water Controversy, but defines sufficiently accurately those by whom it has been chiefly conducted. Its discussion will be facilitated if I add the exact titles of the works alluded to, with a brief notice of the nature of each.

* *Comptes Rendus de l'Académie des Sciences,* 20 Jan. 1840, pp. 109 to 111.

† *Edinburgh Review,* January, 1840.

‡ *Jahres Bericht,* 1841. II Heft pp. 43—51. The advocates of Watt refer very confidently to Berzelius, as on their side. In reality, however, his opinion is very guarded, and, in 1843, he assigns scarcely any merit to Watt, and little to Cavendish, between whom, however, and Lavoisier, who receives much the larger share, he divides the honour of the disputed discovery. (*Lehrbuch der Chemie,* 1843, pp. 370—2.)

§ *Lives of Men of Letters.* See Life of Watt, p. 400.

‖ *Quarterly Review,* 1845, p. 105.

¶ *Lond. & Edinr. Phil. Mag.* Feb. 1846.

** *History of Inductive Sciences,* 2nd edition, pp. 206—207.

†† *Edinburgh Review,* Jan. 1848, p. 85.

2. *Bibliography of the Water Controversy.*

The *Philosophical Transactions*, and the *Mémoires de l'Académie des Sciences*, are the chief repositories of the early literature of the Water Controversy. Mr. Muirhead has done a great service, accordingly, by printing, in the Appendix to the *Watt Correspondence*, the chief papers of Cavendish and Watt referring to the disputed discovery, and those of Lavoisier, Meusnier, and Monge, which are more conveniently consulted in that gentleman's accurate reprints than in their original issues. I shall therefore give the page of Mr. Muirhead's volume, as well as that of the *Mémoires de l'Académie*, when citing the papers already referred to.

1775 to 1786.

Experiments and Observations on different kinds of Air: in six volumes. By Joseph Priestley, LL.D., &c.

1790.

Experiments and Observations on different kinds of Air, &c., being the former six volumes abridged and methodized. By Joseph Priestley, LL.D., &c.

Priestley's experiments, and those of Warltire, which the former records, confessedly led to the discovery under discussion. He was appealed to, moreover, by Cavendish and Watt, as the umpire between them, so that all his statements concerning the original investigations into the composition of water, are of the utmost importance.

The later edition of the Experiments on Air, though professedly an abridgment of the original work, contains new and important matter, so that it occasionally demands separate consultation. I shall refer to the first as *Priestley on Air*, and to the second as *Abridgment of Priestley on Air.*

1784.

Experiments on Air, by Henry Cavendish, Esq., F.R.S. & S.A. Phil. Trans., Vol. lxxiv., p. 119; or Appendix to Watt Correspondence, p. 111.

Cavendish's other papers of which abstracts have been given, especially those on Hydrogen and the production of Nitric Acid, must also be referred to, but it is unnecessary to repeat their titles here.

1784.

Thoughts on the constituent parts of Water and of Dephlogisticated Air; with an Account of some Experiments on that subject. In a letter from Mr. James Watt, Engineer, to Mr. De Luc. Phil. Trans., Vol. lxxiv., p. 329, or Appendix to Watt Corr., p. 77.

Sequel to the Thoughts on the constituent parts of Water and Dephlogisticated Air. In a subsequent letter from Mr. James Watt, Engineer, to Mr. De Luc, F.R.S. Phil. Trans., Vol. lxxiv., p. 354, or Appendix to Watt Corr., p. 106.

1784.

Mémoire où l'on prouve par la décomposition de l'eau que ce fluide n'est point une substance simple, et qu'il y a plusieurs moyens d'obtenir en grand l'air inflammable qui y entre comme principe constituant. Par

MM. Meusnier et Lavoisier. Mémoires de l'Académie des Sciences for 1781 (printed in 1784), p. 269, or Appendix to Watt Corr., p. 151.

1784.

Mémoire dans lequel on a pour objet de prouver que l'eau n'est point une substance simple, un élément proprement dit, mais qu'elle est susceptible de décomposition et de recomposition. Par M. Lavoisier. Mémoires de l'Académie des Sciences pour 1781 (printed in 1784), p. 468; or Appendix to Watt Corr., p. 171.

1786.

Mémoire sur le résultat de l'inflammation du gaz inflammable et de l'air déphlogistiqué, dans des vaisseaux clos. Par M. Monge. Mémoires de l'Académie des Sciences pour 1783 (printed in 1786); or Appendix to Watt Corr., p. 205.

1786.

Letter from Dr. Blagden, Sec. R. S., to Dr. Lorenz Crell. Chemische Annalen, &c., von Dr. Lorenz Crell, p. 58; or translation by Mr. Muirhead in Watt Corr., p. 71.

1839.

Eloge Historique de James Watt; par M. Arago, Secrétaire Perpétuel de l'Académie des Sciences. Paris. Annuaire du Bureau des Longitudes; or, Mémoires de l'Académie des Sciences, pour l'an 1839.

Two English translations of this work have appeared, namely:—

1. *Historical Eloge of* James Watt; *by M. Arago, &c. Translated from the French, with additional Notes and an Appendix, by James Patrick Muirhead, Esq., M.A., of Balliol College, Oxford, Advocate.* London, 1839.
2. *Life of James Watt, by M. Arago, &c.; reprinted from the Edinburgh New Philosophical Journal for October*, 1839. Edinburgh, 1839.

Mr. Muirhead's work is the more complete of the two, but the "Life" is probably more generally accessible. I shall cite the former, however, as the portion of it relating to the History of the discovery of the Composition of Water is reprinted in the Appendix to the *Watt Correspondence,* where it can be conveniently consulted, side by side with other important documents bearing on the Water Controversy. Both the translations, as well as the original Eloge, contain Lord Brougham's important Historical Note with Mr. James Watt's annotations; and Mr. Muirhead has added to his work some useful comments in his own name, as well as a reply to Mr. Harcourt's first discussion of the claims of Watt and Cavendish, which was made public in the interval between the appearance of the "Eloge Historique," and the publication of the English translation, so that in order of time, Mr. Harcourt's address comes before the latter.

Arago's Eloge is unquestionably the most important publication which the revival of the Water Controversy has called forth; and when it is considered how much the tone of a discussion is determined by the temper in which it is commenced, it cannot but be deeply lamented that the Secretary of the French Academy should have opened the controversy in so onesided a spirit as he showed, and should have urged the claims of

Watt rather as an advocate than as a judge. The charges which he brought by implication against the good faith of Cavendish, whom his countrymen universally regarded as a man of spotless integrity, could not but provoke a retaliation of harsh dealing towards Watt. The results have been equally detrimental to the interests of both claimants, on each of whom judgments have been passed so partial, that if we credited them we should be compelled to acknowledge that Cavendish and Watt, instead of being among the most remarkable men of their age for genius and virtue, were wanting alike in generosity and intellectual capacity. Had Arago been more just to Cavendish, his opponents would have been more just to Watt, and the claims of both would have been more speedily adjusted.

It is necessary, however painful be the task, to point this out at present, as the dogmatic and partisan spirit which so painfully characterises the greater part of the literature of the Water Controversy, must be largely referred to the extreme views which were urged by Arago in his Eloge of Watt.

1840.

Report of the Ninth Meeting of the British Association for the Advancement of Science, held at Birmingham in August, 1839. *Address by the Rev. W. Vernon Harcourt, President for the Year.*

A portion only of Mr. Harcourt's eloquent address refers to the Water Controversy. It was published in the *Athenæum* and other journals at the time of its delivery, and was in consequence noticed by Arago and Dumas before its official publication in the report cited above.

Their comments are contained in the *Comptes rendus Hebdomadaires des Séances de l'Académie des Sciences, 20 Janvier*, 1840, p. 109, of which a reprint is given in the Appendix to the *Watt Correspondence*, p. 260.

Sir David Brewster also noticed Mr. Harcourt's address in an article on Arago's Eloge of Watt, in the *Edinburgh Review, No.* 142, 1840.

In consequence of those criticisms, Mr. Harcourt added a lengthened postscript, which follows the address in the authoritative report of it, and is followed in its turn by an appendix containing extracts from unpublished papers by Cavendish, and a lithographic fac-simile of the "records in his Note-book of all his experiments relating directly to the composition of water." The last is a most important document. Mr. Harcourt's entire paper is the most valuable contribution to the literature of the Water Controversy, which has appeared on the Cavendish side, and is not less learned and forcible, than eloquent. It is greatly to be regretted, however, that its accomplished author should have formed so low an estimate of Watt as a chemist. I cannot believe that impartial critics, putting the Water Controversy aside, will ratify Mr. Harcourt's judgment on this point. I owe it at least to my own convictions to say, that a careful study of Watt's chemical papers, has satisfied me that the sagacity, originality, and perspicuity of conception, which he displayed in his purely physical inquiries, did not forsake him when he entered on chemical research. The denial of this by Mr. Harcourt has led to a corresponding depreciation of Cavendish's merits, and is one of the causes why the controversy has been so greatly prolonged, and has exhibited so much of a partisan character throughout.

1840.

A Few Notes on the History of the Discovery of the Composition of Water. By J. O. Halliwell, Esq., F.R.S.

This is a pamphlet of three pages, "intended as a supplement to a paper on the same subject, by Lord Brougham." It is important chiefly as containing a part of Watt's letter to Priestley, of 26th April, 1783, which is not printed in the former's "Thoughts on the Constituent Parts of Water, &c. &c."

1845.

Lives of Men of Letters and Science, who flourished in the time of Geo. III. By Henry, Lord Brougham.

In connection with the lives of Black, Watt, Cavendish, and Priestley, which are contained in this volume, Lord Brougham has republished his "Historical Note on the Discovery of the Composition of Water;" and has added an appendix containing a brief reply to Mr. Harcourt's address and postscript.

His lordship prefers the claim of Watt, but urges it temperately, and disclaims all doubt as to Cavendish's good faith in the transactions connected with the early researches into the composition of water. His work is remarkable also for the impartiality and, as I believe, justice, with which it insists on Lavoisier, as well as Cavendish and Watt, being recognised as an important contributor to the discovery of the true nature of Water. The Historical Note is more accurate in its statements than Arago's Eloge, and we are indebted to Lord Brougham for the explanation of certain apparent anachronisms in Cavendish's "Experiments on Air," (1784), which, till his lordship consulted the manuscript of this paper in the R. S. Archives, were inexplicable. The least satisfactory part of his statement is the reference to Blagden's share in the transactions which led to the original controversy.

1845.

Article in Quarterly Review, December, 1845, entitled "Arago & Brougham on Black, Cavendish, Priestley, and Watt."

This paper is interesting as the production of the learned and accomplished Dean of Ely, the Rev. Professor Peacock. (*Edinr. Rev. Jan.* 1848, p. 83.) It is unnecessary, however, to refer to it at length, as it avowedly takes up the same ground as Mr. Harcourt occupies in his address and postscript, and is obnoxious to the charges preferred against these productions, of being too unfavourable to Watt. Dr. Peacock's views on the relation of heat to chemical combination, which are strongly expressed in this article, differ from those entertained by the majority of chemists at the present day.

1846.

Letter to Henry, Lord Brougham, F.R.S. Containing Remarks on certain Statements in his Lives of Black, Watt, and Cavendish. By the Rev. W. V. Harcourt. Lond. Ed. & Dub. Phil. Mag. 1846.

This is a very learned and interesting paper, which will be welcome to all students of the history of science for the information which it supplies in reference to many important chemical discoveries. It is chiefly occupied, however, not with the Water Controversy, (on which Mr. Harcourt contents himself with reiterating his former opinions), but with criticisms of many of the statements made by Lord Brougham, in his lives of the chemists named above. It is foreign to my present purpose to refer to those, except to say that the Water Controversy has indirectly led in this, and other productions, to much discussion concerning the respective merits of the founders of pneumatic chemistry, which has been of essential service to the cause of science.

1846.

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.

I have already referred to the valuable nature of the reprints and other contents of the Appendix to this work, and cannot speak in too high terms of the painstaking and conscientious way in which Mr. Muirhead has edited the whole volume. The infirmities of advanced life prevented Mr. James Watt from superintending the publication of his father's correspondence, but he has contributed an introductory letter of much interest. The correspondence extends from the close of 1782 to 1786; the more important letters belonging to 1783 and 1784. The extracts are partly taken from letters written by Watt to Dr. Priestley, Dr. Joseph Black, J. De Luc, Mr. Gilbert Hamilton, Mr. Smeaton, Mr. Fry, Mr. Kirwan, Sir Joseph Banks, and Dr. Blagden, partly from certain of those gentlemen's letters to Watt, chiefly replies. The latter are printed from the autograph originals which, at the period of their publication, were in the possession of Mr. James Watt; the former from copies taken by Watt himself by means of his ingenious copying machine.

It would have prevented much discussion, if this correspondence had been published in 1839 along with Arago's Eloge; but no blame can be attached to Watt's relatives for the delay which has attended its appearance. For some six years, however, it was accessible only to the advocates of Watt, who tantalised the public with partial quotations from it, whilst the defenders of Cavendish had no means of effectually replying to the masked battery brought against them, and were even taunted with their ignorance of unpublished facts which they could not possibly know.

The publication of the correspondence has done as much service to the cause of Cavendish as to that of Watt, and has greatly enlarged our means of bringing the Water Controversy to a satisfactory conclusion.

The correspondence is prefaced by a series of lengthened introductory remarks from Mr. Muirhead, of which I wish I could write in as favourable terms as I have done of his labours as an editor. That he has overpraised Watt, who is so deserving of praise, is a fault for which no one will greatly blame him, seeing that he is a kinsman of the great engineer's, and wrote in the name of his son. But there was no one from whom depreciation of Cavendish's merit could come with a worse grace, than from a relative of his rival. Mr. Muirhead, moreover, disparages not only Cavendish, but nearly all who have advocated his claims, and is seldom content with the evidence adduced by the defenders of Watt, which he qualifies or extenuates till it accords with his own view of the deserts of his great client. The perusal, accordingly, of his introductory remarks is a painful task, which is not lightened by the discovery that he has expended so much of his zeal in attacking Cavendish and his friends, that he has not perceived what was essential to the defence of Watt, so that a considerable part of Lord Jeffrey's discussion of the Water Controversy is devoted to supplementing Mr. Muirhead's argument. It is only justice, however, to the latter, to acknowledge that his work everywhere exhibits proofs of an earnestness and sincerity in his estimate of Watt's merits, which deserve all praise.

1847.

Dr. Whewell's History of the Inductive Sciences, 2nd edition.

In this well known work Dr. Whewell reiterates on grounds similar to those urged by Mr. Harcourt and Dr. Peacock, the opinion he had before expressed in favour of Cavendish.

1847.

North British Review, article entitled, *Watt and Cavendish, Controversy respecting the Composition of Water.*

This vigorous and eloquent essay is from the pen of Sir David Brewster, as appears from the *Edinburgh Review* for January, 1848. The claims of Watt are much more strongly asserted than in Sir David's paper on the same subject, published in 1840, and a very unfavourable view is taken of the good faith of Blagden, as well as of the generosity of Cavendish, and the fair dealing of the office-bearers of the Royal Society in 1783 and 1784.

1848.

Edinburgh Review, article entitled, *The Discoverer of the Composition of Water; Watt or Cavendish?*

This contribution to the literature of the Water Controversy, from the pen of Lord Jeffrey, is without question the ablest analysis of the discussion which has been given to the public. It may be read with equal pleasure by the friends of Cavendish and of Watt, for it treats both throughout as men of great intellectual power and blameless integrity. His Lordship prefers the claims of Watt, and urges them with great earnestness, but he does not conceal what has been so unwisely denied by previous advocates of the claims of both candidates, that there are difficulties in the way of an exact settlement of merit, and room therefore for difference of opinion between equally impartial critics. It is a remarkable circumstance, that ten years should have elapsed before the claims of Watt as a discoverer of the true nature of water, were discussed in such a way as to enable even his well-wishers to make out a logically consistent case for him. Down to 1848, one-half of the question in dispute, namely, Did Watt hold that hydrogen was one of the elements of water? was either assumed or asserted on grounds that involved a *petitio principii.* Lord Jeffrey, for the first time, meets the difficulty, and at least offers the proof which had been so long demanded. His paper accordingly contains the ablest defence of Watt's claims which has appeared, and as such will be frequently referred to in the following pages.

1848.

A History of the Royal Society, &c. &c. By Charles Richard Weld, Esq.

In this interesting work, Mr. Weld has given a brief account of the Water Controversy (vol. ii., p. 170), and has contributed from the manuscripts in the archives of the Society, several references, especially facsimiles of one of the interpolated passages in the MS. of the 'Experiments on Air' (1784), and of Cavendish's and Blagden's handwriting, which add to our means of deciding the questions in dispute.

In concluding this bibliographical notice, I would refer to two matters which can be best disposed of here. Mr. Harcourt has been censured for bringing the question of the rival claims of Watt and Cavendish before the British Association, in his inaugural address, without apprising M. Arago of his intention to call in question his statements; to which moreover Arago, even if present, could not have been permitted to reply. I cannot see the justice of the accusation. M. Arago might as fairly be blamed for not informing Cavendish's relatives that he intended to bring certain charges against him before the Academy of Sciences, where no answer could be given to the Secretary's statement. It has always been the practice of the President of the British Association to review the important scientific events of the past year, and Mr. Harcourt did not exceed the prerogative of his office in regarding the revival of the Water Controversy as one of them. Neither party in truth deserves any censure. The Secretary of the French Academy would have done a great wrong if he had concealed his conviction that Cavendish had acted unjustly to Watt; and the President of the British Association did not err in claiming a right to vindicate in the most public way the honour of the accused. It is another question, which of the officials was in the right. That, however, does not concern us at present, and I shall be glad if I can persuade the reader, before entering on its discussion, to dismiss from his mind any doubt he may have entertained as to the fairness of the interference of Arago and Harcourt in the Water Controversy.

The other remark I have to make refers to a more important matter. It is an interesting feature in the controversy under discussion, that, on both sides, manuscripts unpublished during the lifetime of their authors have been adduced in support of the claims of Cavendish and Watt. On the one side we have Cavendish's laboratory Note-book, on the other the greater part of the *Watt Correspondence.* This fact will have much weight in settling the disputed point, how far manuscripts are admissible as evidence in deciding questions of priority. Arago and Sir David Brewster have objected to the validity of MSS. in reference to Cavendish's claims. It is curious that such an objection should have come from the Watt side, for his case would suffer more than that of Cavendish, if manuscript evidence were refused on both sides. There seems no valid reason, however, why it should not be accepted in favour of all the candidates. The genuineness, authenticity, and integrity of the Cavendish and Watt MSS., which have been adduced in evidence during the controversy, are unimpeachable. In other words, they certainly are the productions of the parties to whom they are attributed; they were written at the periods indicated by the dates affixed to them; and they have not been altered by their original writers, or by others, or in any way tampered with, since. Documents of such a character cannot be passed over as inadmissible on the plea of their informality as *private papers* of the claimants. On the other hand, the fact that they were not intended for publication, greatly adds to their value. I shall accordingly consider the Watt Letters and the Cavendish Note-book as unquestionable authorities, and quote from them unreservedly.

In connexion with this point, it seems desirable also to notice that as the law of patents has been held by Mr. Muirhead to determine the way in which the Water Controversy should be decided; and as other statutes have been referred to as of authority in the matter, it may save any lengthened defence of the way in which the case is argued in the following pages, if I adduce here the striking and authoritative statement of Lord Jeffrey, whose decision is of peculiar value in a case like the present, where the one party demands the application of the formal rules of courts of justice, and the other refuses to be bound by them.

"We can by no means adopt," says his Lordship, "those narrow and jealous canons of evidence, derived from the rigid maxims of law, or the precedents in cases of patent, by which both M. Arago and Sir D. Brewster seem anxious to limit the inquiry. Courts of law must proceed upon inflexible rules, and can make no distinction of persons; and are forced therefore peremptorily to reject all evidence proceeding from the parties concerned, or from those having interest in the issue; though it is certain that by so doing they must occasionally decide against the truth and the conviction of all unprofessional observers. The question in a court of law, in short, is never really what the truth of a case is, according to the actual and conscientious belief of the judges (or jury), after considering every atom of producible evidence that is in existence, but merely what the import is of the evidence that *is legally admissible.* But in a case like the present, where the only judge and jury is the intelligent public, and where there is neither any motive for excluding *any* proof which can at all affect the ultimate conviction of that multitudinous tribunal, nor any power by which the parties and their advocates can be restrained or limited in the production of it, it would evidently be mere affectation and absurdity in those who may assume the office of summing up for their assistance, to leave out of view anything that is so produced or producible; or to pretend to shut their eyes upon those parts of the evidence which, though strictly inadmissible in a court of law, they yet know to be producing, and *to be entitled to produce,* the greatest possible effect upon the honest and intelligent minds from which the decision must proceed. In all questions before the public, in short, *no evidence is inadmissible,* nor can there ever be any question, except as to *the credit* to which it is entitled; while the court is always open for as many appeals and new trials as the parties may choose to venture on.

"The analogy of the law of patents is still more palpably inapplicable. A patentee gets his monopoly as a reward for being the first *to make the public participant* of a useful discovery; and therefore it is most just that he should not be excluded from this reward by any rival claimant, who only offers to prove that he had previously made the same discovery, but admits that he had never disclosed it. But where the competition is merely for the intellectual glory of the discovery itself, it is plain that no such principle can apply, and that the palm of priority in the invention may be justly awarded to one who has been forestalled in the publication; whilst that of original and independent invention may be shared between both."*

In this decisive opinion, most persons I think will concur, and the remark may once for all be made, that it is vain to attempt to lay down stringent rules for the adjustment of disputes as to priority between discoverers in science. We cannot settle for others, nor can they for themselves, the way in which discoveries shall be effected, so that our canons shall at once suffice for disposing of disputes. All that we can do, is to enforce certain rules which may *prevent* collisions occurring; but if they do occur, our prospective rules will seldom afford the means of adjusting the claims of rival competitors, and we must dispose of each case upon its own merits. The scientific societies of Europe have recently adopted

* *Edinburgh Review*, Jan. 1848, p. 87.

expedient after expedient to prevent disputes, but with a success so partial, that the discovery of the Planet Neptune, and of the anæsthetic properties of sulphuric ether, have already led to much keener controversies than that which originally attended the discovery of the composition of water; and these, I need not say, are not the only contested discoveries of the present day. When we have learned all the laws in obedience to which Genius unconsciously works, we shall be able to bind it in fetters, which it will not refuse; but for the present, at least, we must regard each great discovery as teaching some of these laws, not as supplying a case which is to be decided by them. New truths will often come to light, as new stars appear in the heavens, when no one is expecting them, and we shall search in vain in the records of the past for any precedent by which to dispose of the difficulties that attend their recognition. The discovery of the compound nature of water was such a truth, and will be studied to most advantage, if all prepossessions as to how it should have been made, or should have been announced, are thrown aside.

3. *Questions in dispute between the Principals in the Water Controversy.*

It cannot, I think, but strike every dispassionate reader, who has glanced at the literature of the Water Controversy, as something not a little singular, that a matter, to appearance, so simple as the question who first made a single chemical discovery, should have been found so difficult of decision. The announcement of the discovery in the original papers of Cavendish and Watt does not occupy many lines, and both of these able reasoners were very clear and perspicuous writers. Their principal commentators, too, have been men universally admired for their genius and talents, and referred to as famous alike in literature and science. How then has it happened, that a question which apparently any twelve moderately intelligent men were competent to decide, should have been discussed with so indecisive result, so far as unanimity is concerned, by the special jury who have had it before them? Whatever be the explanation of the conflicting judgments which have been passed on the subject before us, I think it is impossible to avoid the conclusion, that a problem which ten years' discussion has not solved, cannot be one very easy of solution. The prolongation of the Controversy has, no doubt, been owing in part to the absence, at its commencement, of all the data requisite for its conclusion, and partly also to the polemical spirit which has been displayed on both sides, but chiefly to the real, but generally unconfessed and perhaps unperceived difficulties which are inseparable from the whole question. The love of justice and fair dealing which induced so many distinguished men to come forward spontaneously to defend the rights of the several claimants was too deep-seated to have permitted them to conceal their convictions, had the evidence adduced shown them that their views were erroneous. Sir David Brewster's frank acknowledgment of the change in view which the publication of the *Watt Correspondence* induced in him, was worthy of so distinguished a philosopher, and would assuredly have been followed by others, had their views undergone a similar change. As it is, MM. Arago and Dumas' unaltered confidence in the opinions they published in favour of Watt in 1839, and Harcourt, Whewell, and Peacock's equally unchanged advocacy of Cavendish, show that the evidence to which both parties make their common appeal, instead of compelling a unanimous judgment, is held to justify two exactly opposite conclusions.

In truth, it must be conceded by all dispassionate inquirers, that it is impossible to base the claims of either Cavendish or Watt on evidence which is at once direct and unexceptionable, in reference to every contested point. Certain links in the chain of proof on both sides have been beyond cavil from the first; but others can be supplied only conjecturally from indirect and sometimes vague evidence, to which critics, equally candid and impartial, will attach very different values, and no two will perhaps attach the same. Thus, it is now acknowledged, that Cavendish's experiments on the production of water from its elements were of earlier date than the observations from which Watt drew his conclusions. But the period at which Cavendish inferred that water is a compound of hydrogen and oxygen, is not recorded in any existing document, more precisely than as lying between the summer of 1781 and that of 1783, and we must be guided in our choice of one or other of three years by considerations which will weigh differently with different persons. So also the date at which Watt drew his conclusion concerning the composition of water is quite certain, but what that conclusion was is by no means certain; for it is not precisely stated in any paper hitherto made public; and its import must be gathered from the comparison of many passages in his writings, and those of Priestley, which admit of more than one interpretation.

Where such is the state of matters it would be unwise in any author to seek to compel his readers to concur in all his conclusions. I wish on the other hand to urge on all interested in the Water Controversy, that unanimity of opinion in reference to many of the disputed points, cannot be expected and should not be demanded. I shall be content, therefore, to state my own opinion, without insisting at great length on its justness; and shall devote myself chiefly to as impartial and unpolemical a statement as the circumstances of the case will permit, of the grounds on which Cavendish, Watt, and Lavoisier, claimed the discovery of the composition of water.

These claims, as they were originally urged, were not founded upon an analysis of water into its elements, which was first effected at a later period by Lavoisier; but rested on the discovery that two gases could be made to combine and produce water. The common claim, however, of the three rivals, if reduced to its simplest elements, involved two points. Each affirmed (1) that he had discovered for himself the composition of water; and further (2), that he had made the discovery before the others did. The question of *priority* of discovery is the one which has chiefly been discussed, especially by the advocates on the Watt side; but the question of *reality* of discovery is of as much importance, and must be considered first. There are in addition certain accusations of plagiarism against Cavendish and Lavoisier; but these are best reserved till the claims asserted by each for himself have been disposed of. It must further be noticed, that although the original claims were publicly founded on similar experiments, Cavendish, Watt, and Lavoisier arrived at their conclusions whilst pursuing very different trains of research. Cavendish was investigating the products of combustion; Watt was speculating on the changes which a vapour would undergo, if all its latent heat became sensible; and Lavoisier was seeking in the combustion of inflammable gases for additional proofs of the truth of his view, that oxygen is the great acidifying agent. Those researches in their turn sprang out of earlier investigations, and the whole ultimately converged to one line of inquiry, which led to the discovery under discussion. A brief reference, accordingly, to these preliminary observations and to some others, will make the whole argument more easily followed.

4. *Researches which led to the discovery of the Composition of Water.*

From the earliest dawn of scientific speculation, through ages of intellectual light and darkness, down to the days of the first French Revolution, the simple, uncompounded, or elementary nature of water had been regarded as an unquestionable fact. Physical philosophy had for centuries busied herself in dictating to Nature a simplicity which she disowned. Air, earth, fire, and water, were the elements of all things, and the dogma had been repeated so frequently, that its very echo was mistaken for a new utterance and confirmation of its justness, and no one doubted its truth. Yet if it ever were a wise thing to spend much time in wondering that a discovery was not made more speedily than it was made, we might wonder here. The phenomena of vegetation, if they had been watched with attention, would have shown what a questionable doctrine that of the elementary character of water was. Nor did it escape the sagacity of Van Helmont and other observers, that the development of a tree transferred from earth to water, implied the derivation of even the most solid constituents of plants, from the so-called indivisible liquid. But this inference lost all its value by being extended too far, and ended in the implicit assertion, that all the elements of vegetables are contained in water. Such a conclusion did nothing to further the progress of science, but rather retarded it, for it was founded on inaccurate experiments, which left totally out of consideration the influence of the atmosphere, and of the substances dissolved in water, on the growth of plants. Nor did the observation of the functions of animal life—to the maintenance of which endless decompositions and recompositions of water are so essential—prove more instructive to the earlier chemists than the study of the simpler vegetable life had done. Not a weed grew but was in the secret of the composite nature of water! The smallest animalcule knew how to decompose the so-called element, and daily divided the indivisible that it might change it into its own substance! No one, however, understood their language, or tried to interpret it, and hieroglyphics which seem to us pictures which tell their own story, revealed nothing to those who had already decided that they had no meaning.

The mere observation of natural phenomena thus taught nothing, and as little did direct experiment. From the earliest times speculative and practical chemists, whether engaged in economical processes such as chiefly occupied the Greeks and Romans, and the ancient Egyptians, or in mystical investigations like the Arabian and mediæval Alchemists, must have formed and decomposed water hundreds of times, but they were not aware of the feat they had performed. Nor can we wonder at their blindness, for long after their later successors were warned that water was probably not simple—and after they had its elements in their hands for years—they failed to detect the significance of phenomena, which we are apt to conceive carry their interpretation with them. Water accordingly was reputed for some thousands of years a simple substance.

We may begin the modern history of its decomposition, with Newton's celebrated inference, from its optical characters, that water consisted of ingredients which were unlike each other, and one of them (or one class of them) inflammable. This conclusion is often referred to, as if it had been greatly more precise than it certainly was, and popular authors write as if Newton had predicted, in so many words, that water would be found to consist of two gases, one of them inflammable. His own words, however, certainly do not warrant any such inference. In the course of his observations on the refractive indices of various bodies, he noticed that whilst transparent, uninflammable substances refracted light more powerfully the denser they were, there was an exception in favour of combustibles, such as camphor, the oils, turpentine, &c., whose refractive indices were much higher than their density would account for. "Water," he goes on to observe, "has a refractive power in a middle degree between those two sorts of substances, which consist as well of sulphureous, fat, and inflammable parts, as of earthy, lean, and alcalizate ones."*

It is impossible to gather from a statement so general as this, what Newton's precise opinion was as to the nature of water, and though we may look back at it as a prediction that one of the constituents of that liquid would prove to be inflammable, it may be doubted whether Newton intended to affirm this; and it seems quite certain that his contemporaries, and immediate successors, did not put that interpretation on his words. His prediction, in truth, was not called to mind till long after the detection of hydrogen as a constituent of water had amply fulfilled it. It went for nothing, accordingly, in leading to the discovery of the composition of water.

Newton's observation was made in the beginning of last century. Some fifty years passed on, and water was still an element. At the end of this period, men had become familiar with a powerful new engine for effecting chemical decomposition in the Friction Electric Machine and the Leyden Jar. Beccaria, taking advantage of this, exposed water to the electric spark, but though he resolved the reputed element into its constituent gases, he was not aware of what he had done.†

Some ten years later, viz. in 1766, Cavendish gave the first detailed account of the properties of hydrogen, but though, in the course of his experiments, he must have burned that gas into water many times, he does not, as already noticed, once mention that he so much as saw a liquid produced.

Ten years more passed away, and at length the threshold of the discovery was reached. In 1776 John Warltire, an English natural philosopher, observed that when a jet of hydrogen is allowed to burn under a bell jar, closed below and containing air, till the flame goes out, "immediately after the flame is extinguished there appears through almost the whole of the receiver *a fine powdery substance like a whitish cloud*, and the air in the glass is left perfectly noxious."‡ In the same year Macquer, a

* *Optics*, 1704. Book Second, p. 75.

† Beccaria's observations are contained in his *Lettere dell' Elettricismo*, published at Bologna in 1758. Beccaria's experiments, which must not be confounded with those made nearly half a century later with the voltaic battery, were repeated in England by Pearson (*Phil. Trans.* 1797, p. 142), and in Holland by Trootswyk, after the compound nature of water had been discovered. In this century they have again been carefully repeated by Wollaston, Faraday (*Electr. Res.* series 3, par. 328), and Grove (*Phil. Trans.* 1846). See also *Chem. Soc. Mem.* vol. iii. p. 340.

‡ *Priestley on Air*, vol. iii. 1777, App. p. 367. Warltire calls the gas he used "inflammable air," but from the description he gives of the mode in which he prepared

French chemist, without any knowledge of Warltire's observations, and anxious only to ascertain whether the flame of hydrogen evolved smoke or soot, made an experiment similar to his, but to better purpose, and saw the uncertain "whitish cloud" which the English observer mistook for a fine powder, condense into visible drops of a clear liquid like water, which accordingly he assumed it to be.*

Macquer prosecuted the matter no further, nor did he draw any conclusion as to the origin of the water he saw deposited; and we must pass on to 1781 before we find anything additional worthy of notice. In that year, Dr. Priestley made, what he was fond of making, "a random experiment," as, with characteristic candour, he calls it. It proved in the end, like the chance shot of an uncertain marksman, of more real service to the progress of science, than many of its performer's carefully aimed experiments have done.

The random experiment consisted in exploding a mixture of inflammable air (apparently hydrogen) and common air, in a close glass vessel by means of the electric spark, in the way first practised by Volta in 1776.† When the spark had passed and the explosion was over, the sides of the glass were observed to be bedewed with moisture, but to this latter phenomenon Priestley paid no attention. Warltire, however, who, as I have mentioned already, had had his attention previously directed to the appearance of a powdery deposit, or "whitish cloud," as attending the combustion of inflammable and common air, was now struck by the appearance of moisture, and said to Priestley, whose experiments he witnessed and repeated, that it confirmed an opinion he had long entertained that common air deposited moisture when phlogisticated.‡ Similar experiments were made with oxygen and inflammable air in glass vessels, and the appearance of moisture was probably noticed, although this is not mentioned. The only remark Priestley makes, is, that "little is to be expected from the firing of inflammable air, in comparison with the effects of gunpowder."

Warltire, however, though he drew the conclusion mentioned above, paid little attention to the appearance of moisture. The experiments, which he and Priestley performed, interested him chiefly, because he "had long entertained an opinion, that it might be determined *whether heat is heavy, or not*, by firing inflammable air mixed with common air, and applying them to a nice balance." To avoid the risk of injury from the explosion, he

it, it must have been hydrogen. His account of the experiment, which he considered "very curious," is dated Jan. 3, 1777. It has been overlooked by the historians of the Water Controversy.

* *Dictionnaire de Chymie*, t. ii. p. 314, quoted in *Watt. Corr.* p. xxviii.

† Arago ascribes to Warltire the merit of first passing the electric spark through gaseous mixtures confined in glass vessels. (*Annuaire du Bureau des Longitudes pour* 1839, or *Watt Corr.* p. 225.) Warltire, however, states that he borrowed the device from Priestley (*Priestley on Air*, vol. v. 1781. Appendix, p. 395); and the latter, in his turn, refers to Volta as having kindled inflammable air by the electric spark before he did. (*Op. cit.* vol. iii. 1777, p. 382.) Arago must have awarded this honour to Warltire inadvertently, for in his *Eloge Historique d'Alexandre Volta*, read to the French Academy in 1833, after mentioning that the electric spark had been employed to light combustibles, such as alcohol and hydrogen, in the open air, he continues— "Volta was the first who repeated such experiments in close vessels. (1777.) To him therefore belongs the apparatus which Cavendish employed in 1781 to effect the synthesis of water." (*Annales de Chimie et de Physique*, t. liv. (1833). p. 402.)

‡ The account of Warltire's experiments "on the firing of inflammable air in close vessels," with Priestley's observations on the conclusions they warrant, will be found in the latter's *Expts. and Obs. on Air*, vol. v. 1781, App. p. 395. The account is reprinted by Mr. Muirhead, *Watt Corr.* p. xxx.

employed a copper flask, which he filled with the mixture of common and inflammable air, and then weighed. When an electric spark was passed through the contents of the flask, and the mixture of gases exploded, great heat was evolved as the hydrogen and oxygen combined. The flask was then cooled, and weighed again, to ascertain whether it had become lighter by the loss of the heat which had been given off; and in several trials the vessel appeared on the second weighing to have lost weight; from which Warltire seems to have concluded that heat is a ponderable body.

Warltire and Priestley's experiments were made before the 18th of April, 1781, and it was their repetition in the summer of that year by Cavendish, which led to the discovery of the composition of water. Their consideration, therefore, brings us to the point where the controversy commences, and I now enter on the disputed ground. It involves two questions, namely: 1. What is the discovery which Cavendish, Watt, and Lavoisier claim to have made? 2. When was that discovery made? It will prevent confusion if these questions are discussed apart; at least at first, and so far as their separate consideration is practicable.

It may seem at first sight unnecessary to discuss formally the nature of the discovery claimed to have been made by the rivals in this controversy: and it would be needless, if it were certain that both held the same view; but as it has been affirmed that they did not, it is impossible to discuss the question of priority till the reality and nature of the contested discovery have been determined.

QUESTION OF REALITY. NATURE OF THE DISCOVERY CLAIMED BY CAVENDISH, WATT, AND LAVOISIER, AND IMPUTED TO MONGE.

5. *Cavendish's Experiments and Conclusions concerning the Composition of Water.*

In the abstract of the "Experiments on Air," Cavendish's observations and conclusions have been fully considered, so that a brief and dogmatic reference to their nature will be sufficient here.

The experiments, it will be remembered, were undertaken in the course of an inquiry into the products of combustion in air; whilst the special trials which led to the discovery of the composition of water, were professed repetitions of Warltire's process for demonstrating the ponderability of heat. The determination of that question, however, was not the *motive* for the repetition, although this has been strongly asserted by some of the advocates of Watt's claims. Cavendish's own statement is, that it was the appearance of moisture incidentally observed by Priestley and Warltire, which seemed to him "likely to throw great light on the subject he had in view," and, accordingly, "he thought it well worth examining more closely." The alleged loss of weight he also thought, "if there was no mistake in it, would be very extraordinary and curious." He arranged matters accordingly, so that the same experiment should test the truth of Priestley's statement, that a deposition of moisture followed the detonation of hydrogen and air in a close vessel, and the truth of Warltire's statement that the explosion was attended by a loss of weight. To secure the means of making the double observation, it was only necessary that the vessel should consist of *glass*, so that the deposition of liquid within it might be visible; and that it should be weighed before and after every explosion. Two sets of experiments were made with this apparatus; the one with hydrogen and air, the other with hydrogen and oxygen. When air was used, it was mingled with hydrogen, in the proportion (in the decisive trials) of 1000 measures of the former to 423 of the latter; and the mixture was introduced into a glass globe, (provided with a stop-cock and wires for passing the electric spark), which had been previously emptied at the air pump, and its weight ascertained. The spark was then passed so as to burn the gases, and the globe was weighed again, to ascertain whether or not it had lost weight. No certain alteration in weight occurred in Cavendish's trials. "I could never," says he, "perceive a loss of weight of more than one fifth of a grain, and commonly none at all." "In all the experiments," he further observes, "the inside of the glass globe became dewy," and when this dew was subjected to what appeared to him a sufficient number of decisive tests, "it seemed pure water." Other trials were likewise made by simple combustion, without the aid of the electric spark. In these hydrogen and air, in the proportion of one volume of the former to two volumes and a half of the latter, were burned together, as they issued from separate tubes lying side by side, and opening into a common canal in which the resulting water condensed. The object of these trials was, to collect a larger quantity of water for analysis, than the globe experiments furnished.

The conclusion which he drew in full, was as follows: "By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one fifth of the common air, lose their elasticity and are condensed into dew. And by this experiment [that is, by analysis of the larger quantity of water obtained by simple combustion,] it appears that this dew is plain water, and consequently that almost all the inflammable air, and about one fifth of the common air, are turned into pure water." *

The experiments with hydrogen and oxygen were made in the same way. The gases were mingled in the proportion of "19,500 grain measures of dephlogisticated air and 37,000 of inflammable air," or one volume of oxygen to less than two of hydrogen. "The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity." Sometimes the resulting liquid was sour to the taste, and contained nitric acid, a phenomenon which Cavendish showed to result from the oxidation of the nitrogen of atmospheric air, not entirely removed by the air pump when the globe was exhausted. If excess of oxygen were used, the liquid was acid; but if this gas was in such a proportion as exactly, or nearly exactly, to oxidise all the hydrogen, then "the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows that almost the whole of the inflammable and dephlogisticated air is converted into pure water." † Cavendish's most comprehensive conclusion is summed up in the following sentence. "I think we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists

* *Phil. Trans.* 1784, p. 129. † *Op. cit.* p. 133.

of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston; since, according to this supposition, these two substances united together form pure water."*

Two remarks, only, seem called for here, in reference to Cavendish's experiments and conclusions. 1. The combustible gas which he employed, he calls *inflammable air*. It is of importance, therefore, to be certain, that by this title he denoted hydrogen. That he did, there can be no question. "In these experiments," says he, "the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed."† There is no expression 'otherwise' in reference to the trials, from which the conclusions quoted above were drawn. Cavendish's "inflammable air" then was hydrogen, and it is worth while to notice that it was so, not merely in the experiments specially referred to, but even in those which he excepts; only in the latter it was prepared by the solution of iron, not zinc, in diluted oil of vitriol.‡ 2. The mode in which he obtained his results was singularly beautiful and simple, but the elegance of the process may easily be overlooked; and more than one critic of the Water Controversy has altogether misapprehended it. It is necessary, therefore, to refer to it a little more at length, and I select for illustration the case of hydrogen and oxygen as simpler than that of hydrogen and air. Cavendish, then, ascertained by preliminary trials, that when about two volumes of hydrogen and one of oxygen were fired by the electric spark, they disappeared as gases, and left, (with the exception of a small amount of incondensible elastic fluid, which he considered as impurity), no residue of any kind but a little water. Having ascertained this, he filled the globe with a mixture of hydrogen and oxygen in the proportions mentioned, weighed the vessel with its contents, and fired the gases by the electric spark. When the vessel cooled, it was hung up again to the balance without being opened, and found not to have changed in weight. Nothing ponderable, then, was lost. All the gas (a trace of impurity excepted) had ceased to be gas, and in its place was so much liquid, occupying a space or volume immensely smaller than the gas had occupied, but possessing a weight exactly identical. Weight for weight, then, the liquid had replaced the gas, so that all the ponderable matter of the one was contained in the other. In other words, the gas was "*turned into*" the liquid, as Cavendish himself phrases it; and it only remained to repeat the globe experiment several times, and to burn considerable volumes of the gases by direct combustion, so as to procure a sufficient quantity of the liquid, to admit of its being analysed, and shown to be pure water.

The refinements of modern chemistry have not devised a more elegant or demonstrative process. As the whole operation was carried on in the same vessel, not the fraction of a drop of the liquid could be lost. It was saved alike for weighing and analysis. The method, which it will presently appear was practised by Priestley, of sucking up the moisture by blotting paper, required the sacrifice of the liquid without analysis, and made accurate weighing impossible. Lavoisier's process gave results which fell far short of demonstrating that the gases burned, and the water produced, were identical in weight. Only Cavendish could show identity between the weight of liquid which had appeared, and that of gas which had disappeared.§

* *Op. cit.* p. 137. † Ibid. p. 127. ‡ *Op. et loc. cit.*

§ The identity was of course not *absolute*, in the strict sense of that term, but, deducting the small amount of incondensible impurity, approached as nearly to it, I know, in truth, but one objection to Cavendish's process. He did not *dry* the gases he employed. That he should not have done so is curious, for he knew and employed, in 1766, (as he records in his paper on hydrogen,) the process for drying gases by passing them through a tube containing a hygrometric salt, which is followed at the present day.* Had he endeavoured to determine the *quantitative* composition of water by *weight*, this neglect would have involved him in error, but it did not vitiate his conclusion as to its qualitative constitution, and would not have sensibly affected an inference as to the quantitative composition of water by *volume*.

The French experiments were no better in this respect than Cavendish's, for the gases were not dried by Lavoisier, Meusnier, or Monge; and Priestley led Watt and himself into a serious error, by the process which he adopted for rendering "inflammable air" anhydrous.

Cavendish's results, then, are unexceptionable, so far as the materials made use of and the mode of experimenting are concerned; and the inference was warranted and was just, that water consists of dephlogisticated air (or oxygen), and of the inflammable air of the metals (or hydrogen). Whether the substitution of the word *phlogiston* for inflammable air, in the more general statement of his results which Cavendish gave, rendered his statement more vague and uncertain than it was in its more simple original shape, I shall consider in another section.

6. *Priestley's Experiments, and Watt's Conclusions from them concerning the Composition of Water.*

Watt's claims to be considered a discoverer of the composition of water, are based by his advocates on a twofold ground. He is reported, 1. to have long entertained the belief, that water was convertible into air or gas; and 2. to have inferred, from certain experiments of Priestley's, which their performer did not understand, that water consists of the particular gases, hydrogen and oxygen. His more general views will be considered in the sequel; his special conclusion demands careful consideration here; and first, of the experiments from which it was drawn.

In the earlier half of March, 1783, Priestley repeated Cavendish's globe experiments on the convertibility of a given weight of inflammable and dephlogisticated air into the same weight of water. With the date of Priestley's experiments, and the fact that they were a repetition of Cavendish's, we are not specially concerned at present; but this passing reference to these points will render more intelligible many of the allusions in the succeeding statements.

The earliest account hitherto published of Priestley's experiments, is contained in a letter from Watt to Gilbert Hamilton, of date 26th March, 1783, in which this passage occurs:—"He (Priestley) puts dry dephlogisticated 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."†

probably, as the limits of accuracy in experiment permit; at all events, it came nearer to it than the results obtained by any of Cavendish's rivals.

* Ante, p. 201.

† *Watt Corr.* p. 17. An earlier reference is given, ante, p. 94.

The exact nature, object, and value of the experiment thus described, have been matters of the keenest discussion between the friends of Watt and Cavendish, and particularly in reference to the *kind* of inflammable air which Priestley employed. Watt describes the experiments at greater length in various of his letters, and more fully in his "Thoughts on the Constituent Parts of Water, &c.;" but in none of his writings has he stated what kind of inflammable air Priestley employed. The latter, however, is more explicit in his own account of his experiments, and as it confessedly contains the only *direct* reference to the quality of the combustible gas which Watt held to be one of the elements of water, I quote Priestley's statement in full. It is contained in his paper entitled, "*Experiments relating to phlogiston, and the seeming conversion of water into air*," which was originally accompanied by a commentary from the pen of Watt, in the shape of a letter to Priestley, containing an exposition of the views of the former concerning the composition of water. For reasons which will be afterwards considered, that commentary was withdrawn before Priestley's paper was read to the Royal Society (June 26, 1783); so that Watt's conclusions, as they were ultimately published in the succeeding year (1784), appear quite disconnected from Priestley's own account of his experiments. It is necessary, therefore, to notice that the following quotation is from one of the most important parts of the text on which Watt commented.

"Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the re-conversion of air into water, by decomposing it in conjunction with inflammable air. And in the first place, in order to be sure that the water I might find in the air was really a constituent part of it, and not what it might have imbibed after its formation, I made a quantity of both dephlogisticated and inflammable air in such a manner as that neither of them should ever come into contact with water, receiving them, as they were produced, in mercury; the former from nitre, and in the middle of the process (long after the water of crystallization was come over), and the latter from perfectly-made charcoal. The two kinds of air thus produced I decomposed, by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance, in the same quantity as if both the kinds of air had been previously confined by water.

"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 nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper.

"As there is a source of deception in this experiment, in the small globules of mercury, which are apt to adhere to the inside of the glass vessel, and to be taken up by the paper with which it is wiped, I sometimes weighed the paper with the moisture and the mercury adhering to it, and then exposing it in a warm place, where the water would evaporate, but not the mercury, weighed it again, and still found, as nearly as I could pretend to weigh so small a matter, a loss of weight equal to that of the air.

"I wished, however, to have had a nicer balance for this purpose; the result was such as to afford a strong presumption that the air was reconverted into water, and therefore that the origin of it had been water."* The account which Priestley gives of his experiments, (here spoken of as one,) is exceedingly defective. It is explicit enough, however, to show, that in two important points, his mode of experimenting differed from that of Cavendish. 1. The inflammable air he employed was *not* hydrogen, but a gas procured by heating perfectly-made charcoal. 2. He removed the water from the vessel in which it was produced, before he ascertained its weight. The first of these variations on Cavendish's process was introduced with the laudable purpose of employing *anhydrous* inflammable air, so that no ready-formed water might be present (at least in a state of mechanical mixture) in the gas which was to *produce* water by its union with dephlogisticated air. Priestley manifestly regarded the inflammable air *from charcoal* as identical with the inflammable air *from metals*, and preferred the former because it was freer from moisture: in truth, as it was procured from red-hot charcoal, it was naturally enough assumed to be anhydrous. His great anxiety to procure dry gases, probably arose from his recollection of the explanation which Warltire had given of the source of the water which appeared when inflammable and dephlogisticated air were exploded together. The latter, as mentioned previously, held that air deposits its moisture when phlogisticated—an opinion which Priestley recorded as at least worth notice. According to this view there was no "conversion of air (gas or gases) into water," but a mere precipitation in the liquid form of the aqueous vapour, which was diffused through the gases before their combustion. If, however, they could be deprived of this aqueous vapour, or prepared *ab initio* without it before they were burned, Warltire's explanation would plainly be inapplicable, and the proof that conversion of gas into water had occurred, would be rendered more complete.

In this way Priestley escaped one fallacy, only however to fall into a more serious one. We are indebted to Mr. Harcourt for first pointing out the important fact, that the inflammable air which the former employed was not hydrogen. Its more abundant constituent (by weight) must have been carbon, but its exact composition cannot be ascertained, for Priestley merely states that he prepared the gas from perfectly-made charcoal.

There can be little doubt, however, that he heated the charcoal in an earthen retort. This at least may be inferred, from the following passage which occurs in the paper containing the account of his repetition of Cavendish's experiments. "Wood or charcoal is even perfectly destructible, that is, resolvable into inflammable air, in a good earthen retort and a fire that would about melt iron. In these circumstances, after all the fixed air had come over, I have several times continued the process during a whole day, in all which time, inflammable air has been produced equally, and without any appearance of a termination."† Perfectly-made charcoal, if it were in reality pure carbon, could yield nothing in an air-tight retort, free from moisture, but a small quantity of carbonic oxide, and a little carbonic acid, formed by the combination of the charcoal with the oxygen of the air which filled the unoccupied spaces in the retort. The best charcoal, however, frequently retains in combination a little hydrogen derived from the wood from which it was prepared, and always contains, if it has been exposed to the air, water-vapour and other gases,

* *Phil. Trans.* 1783, pp. 426—427; or *Priestley on Air*, vol. vi. (1786) p. 50.
† *Phil. Trans.* 1783, p. 412.

which it absorbs with great avidity from the atmosphere. Priestley's charcoal, accordingly, would probably yield hydrogen, or carburetted hydrogen, or both, as well as carbonic oxide and carbonic acid, when heated in an air-tight retort.

From the way, however, in which that ingenious chemist refers to the evolution of inflammable air from heated charcoal continuing for hours "without any appearance of termination," it seems exceedingly doubtful whether he employed a close vessel. It is much more probable, if not certain, that the retorts he made use of, were Wedgwood-ware vessels, of the kind he constantly refers to in the paper from which I have been quoting. These were, or appeared to be, air-tight at ordinary temperatures, but when made red-hot became sensibly porous, so as to permit the diffusion of gases through them.* If charcoal were exposed to a high temperature in such retorts, the gases which it evolved would, to a certain extent, exchange places with those resulting from the burning fuel by which the retorts were heated. In this way the carbon, even if pure, would be exposed to carbonic acid and steam, besides other gases, which penetrating the porous walls of the retort, would become converted in whole or in part into carbonic oxide, hydrogen, and carburetted hydrogen. Except on this supposition, it seems impossible to account for the endless evolution of inflammable air from charcoal to which Priestley refers; and it can scarcely be doubted that his so-called *dry* gas contained ready-formed water.†

It is not necessary, however, to insist at length on this point. It is not denied by any party, and does not admit of dispute, that Priestley's charcoal-gas was not hydrogen. It must have contained as large a volume of carbonic oxide as of hydrogen, on the view most favourable to the account which he gives of the products which it yielded on combustion; but it probably also contained carburetted hydrogen, carbonic acid, and water vapour. On either view it could not, when detonated with oxygen, afford the results which Priestley conceived that it yielded. In two important particulars his statement is irreconcilable with the account of his experiments which he furnishes himself. 1. There is no proportion in which the charcoal-gas can be burned along with oxygen, so as to yield a quantity of water equal to the weight of the gases consumed. 2. The product of the combustion of the charcoal-gas is not water alone, but water and carbonic acid. It is further to be noticed, that Priestley does not explain how he ascertained that the weight of water was equal to that of the gases burned. Besides employing a

* See, in illustration of this, Priestley's "experiments relating to the seeming conversion of water into air." *Phil. Trans.* 1783, p. 414; and Prof. Graham's *Elements of Chemistry*, 2nd edit. vol. i. p. 85.

† Mr. Harcourt, assuming that Priestley's retorts were airtight, has calculated the probable composition of the latter's charcoal-gas. (*Rep. Brit. Assoc. for* 1839. Pres. Address, p. 27.) For the reasons, however, given above, I question the value of any calculation founded on such an assumption; and I think it needless to offer another in its place, when the data are so imperfect. The quality of the gas, *ex. gr.*, would be affected by the shape and size of the retort, the mode in which it was heated, the configuration and dimensions of the pieces of charcoal, and the extent to which the retort was occupied with them, or filled with air at the commencement of the process. The gas, moreover, would progressively alter in quality, as the charcoal diminished in quantity, and permitted a greater part of the cavity of the retort to be filled by elastic fluid. When all those variable points are unrecorded, a hypothetical calculation of the quantitative composition of the charcoal-gas, could be of no value; and it is not called for by the demands of any of the parties in the Water Controversy.

different gas from that which Cavendish employed, he adopted another method of proving that the whole burnt gas was converted into water.

This required that the absolute weight of the mixed gases fired by the electric spark, should be ascertained at the beginning of the experiment, and the weight of the resulting water at the end; but Priestley makes no reference to the preliminary weighing of the gases. To make his account consistent (not to say credible), we must suppose that he ascertained or calculated the weight of a globeful of the mixed charcoal-gas and oxygen before he passed the spark. The resulting water, we have seen, he absorbed by blotting-paper, which he weighed whilst wet, and again after it was dry. The loss in weight gave the amount of water which the gases had yielded.

I need not expatiate on the rudeness of this process, which could not have given accurate results in the hands even of a Berzelius or a Faraday, much less in those of Priestley, who, though singularly ingenious and inventive, was far from an accurate observer in quantitative investigations. Defective as his method essentially was, it was rendered additionally imperfect, as Priestley himself acknowledges, by the absence of a delicate balance, and by the mixture with the water of globules of mercury, with which, it should seem, the globe had been filled, before the gases were introduced into it. Priestley, moreover, implicitly acknowledges that the weight of the gases, and that of the water, were not identical; though to what extent the one fell short of the other, is concealed from us by the total absence of numerical statements which characterises the whole account of this important experiment.

Lastly, it is to be noticed, that Priestley makes no reference to any examination of the liquid which resulted from the combustion of the gases. He cannot have analysed it carefully, or he would have found carbonic acid in it, which no one knew better how to detect than he did; and he probably did not analyse it at all, for his method of procedure (unlike Cavendish's) dissipated the water in the process of weighing.

It appears, then, that Priestley cannot have obtained the results he professed to have got. He employed the *wrong* gas; he must have weighed inaccurately; and he either did not analyse, or failed to analyse sufficiently. the so-called water. In this way he unconsciously deceived himself; and for a long period he undesignedly led others astray.

Into the general consideration of the result of these errors, I have not occasion to enter; but it is manifestly of the greatest importance to determine whether Priestley's charcoal-gas experiments were those on which Watt founded his conclusions.

When Arago revived the Water Controversy, he assumed that Priestley's experiments were made with hydrogen and oxygen, and that they were unexceptionable. Those, on the other hand, who disallowed Watt's claims, referred to Priestley's account of his own researches, as proving that they were made with improper materials, and were not trustworthy. Mr. Muirhead left this important point almost entirely unconsidered, although the testimony of the *Watt Correspondence* in reference to it called for special notice. His elaborate defence of Watt's claims was, accordingly, pronounced defective in a most important particular; and Lord Jeffrey, acknowledging the force of the objections which Harcourt and others raised against the validity of inferences drawn from the charcoal-gas experiments, sought at great length to show that other *unrecorded* experiments were made by Priestley with the proper materials, and in a trustworthy way, and that these were the foundation of Watt's conclusions. His Lordship is thus the only advocate of Watt's claims, who has endeavoured to show that the latter was entitled to his conclusions in virtue of the sufficiency and significancy of the experiments which gave birth to them, and as his discussion of the question is as complete as it is forcible, I shall consider it at some length. If Lord Jeffrey's view, indeed, is well-founded, he has succeeded in establishing a claim for Watt such as none of his other advocates have been able to sustain. What that claim implies, will be best understood if Watt's conclusion be stated before any criticism is offered on the experiments from which it was drawn. In the letter of April 26, 1783, which he addressed to Priestley, the following passage occurs :—"Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These two kinds of air unite with violence, they become red hot, and upon cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed.

"This water is then the only remaining product of the process; and *water*, *light*, and *heat* are all the products, unless there be some other matter set free which escapes our senses.

"*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, &c.?*"* The concluding portion of the paragraph, which refers to the nature of dephlogisticated air, and the relation of latent heat to the gases, will be considered in another section. In the passage quoted, Watt, it will be observed, infers from Priestley's experiments that water consists of dephlogisticated air or oxygen, and of another substance which he names inflammable air or phlogiston. He does not limit the term *inflammable air* (with which alone we are at present concerned) in the paragraph under notice (nor, as will afterwards appear, in any other of his published writings), so that, if his conclusions were founded on Priestley's *recorded* experiments, he must have intended by inflammable air that from charcoal, and his inference was, that *the components of water are oxygen and the charcoal-gas,* not oxygen and hydrogen.

It is not a little singular, that the serious difficulty which the charcoal-gas experiments throw in the way of Watt's claim to be the discoverer of the true composition of water, should have been passed over so lightly by nearly all his advocates. Lord Jeffrey meets the difficulty in the following way. "Watt himself," says he, "makes no mention of this charcoal-gas; and nowhere refers to this paper of Priestley's as containing tne experiments on which he proceeded, but states *these* for himself in very minute and particular detail. There is not an atom of evidence, indeed, to show that, before writing his letter, he had ever seen this paper, which was not read in London till 25th May, 1783, nor printed till the very end of that year; and we think it by far most probable that he knew nothing of its particular contents till after that publication. It is a great and fundamental mistake, also, to suppose that the main object and subject of *that* paper was the same, or even very much connected with that of Watt's letter. Its first and longest division consists of a dissertation on the nature of Phlogiston generally; and the other on 'the supposed convertibility of water into air,' by which he explains he means into the common air of the atmosphere. Almost the whole of the experiments detailed in it, accordingly, are referable to this *analytical* process; and there is but a slight notice, extending in all to little more than a page, of

* *Phil. Trans.* 1784, p. 333.

the *synthetical* proceedings of Cavendish, and the few experiments he had himself made in connexion with them; not so much, we think, to test or confirm the general results reported by Cavendish, as to eliminate one particular source of possible error. Watt's letter, on the other hand, professed only to embody his own 'thoughts on the constituent parts of water;' and had, therefore, no material bearing on the general disquisition of Priestley's. He had plainly received a full and complete account of all the experiments on which his own conclusions were founded some time before the 26th of March, and therefore before either his own letter or Priestley's paper of 21st April were written; and had no occasion, therefore, to look to that paper, or concern himself about its contents, in order to prepare that exposition of his important theory which he then proposed to make public. He had, of course, long before communicated largely with his learned friend and neighbour on the nature of that theory, and made himself minutely acquainted with every material particular of the experiments on the faith of which it was grounded; and our own firm conviction is, that he had been distinctly told, and told truly, that all those experiments in which the quantity of missing air was carefully measured, and the freedom of the water produced from acid ascertained, were made with the *inflammable air from the metals,* or the *hydrogen* of our modern technology; and that if any mention at all was made between them of the employment in later experiments of the gas from charcoal, it was only for the purpose of showing that the (supposed) *perfect dryness* of that air did not interfere with the general success of the processes." *

Before making any remarks on Lord Jeffrey's argument, I think it of great importance to notice that his Lordship, as will be seen from the quotation, implicitly acknowledges that neither Priestley nor Watt has *described* experiments with hydrogen, and that no *direct* evidence of any kind can be produced to show that they employed that gas. The proof that they did, if it can be supplied at all, must be gathered from the comparison of many separate passages in the writings of both.

From such a collation of passages, Lord Jeffrey seeks to show: 1. That Watt's original exposition of his views concerning the composition of water had little or no reference to Priestley's paper, in which the charcoal-gas experiments are recorded; so that Watt is relieved from any share in the errors into which Priestley fell. 2. That, from various statements and allusions, it must be inferred that Priestley performed experiments with hydrogen, and that these formed the groundwork of Watt's conclusions. It is thus held that Watt did *not* signify, by *inflammable air,* the charcoal-gas, and that he *did* signify hydrogen.

With neither of these conclusions can I agree, for the following reasons:—It seems to me to admit of direct proof, that the connexion between Priestley's paper and Watt's original letter was of the most intimate and essential kind; so that the former was the text, on which the latter was the commentary. Priestley, it will be remembered, drew up, in March, 1783, a paper for the Royal Society, entitled "Experiments relating to Phlogiston, and the seeming Conversion of Water into Air," which was read to that body on June 26, 1783. Watt, who was aware of his intention, sent him a letter dated 26th April, of the same year, with the request that Priestley would present it to the Society, if he thought proper. Priestley, accordingly, brought it before the Society, and it was eventually read publicly, April 22, 1784. A second version of it also,

* *Edinburgh Review*, Jan. 1848, pp. 99, 100.

containing large parts of the first repeated *verbatim*, was addressed in the form of a letter to De Luc, and read to the Royal Society, April 29, 1784. This later letter forms, with some additions, the paper printed in the *Phil. Trans.* for 1784, with the title "Thoughts on the Constituent Parts of Water," &c.* It is not to this triple document that we must turn for evidence to show what the relation was that subsisted between Priestley's paper and Watt's first letter; for the omitted portions of the latter are exactly those which are most important in reference to the particular question immediately before us. The MS. of Watt's letter to Priestley of April 26, 1783, is preserved in the archives of the Royal Society; and through the courtesy of Mr. Weld, Assistant Secretary, I have obtained an authenticated copy of it. It begins thus: "On considering your very curious and important discoveries on the nature of phlogiston and dephlogisticated air, and on the conversion of water into air, and *vice versa*, some thoughts have occurred on the probable causes of these phenomena, which, though they are mere conjectures, seem to me more plausible than any I have heard on the subject, and in that view I have taken the liberty to communicate them to you."†

In this passage it will be seen that Watt, so far from confining his attention solely to Priestley's experiments on the convertibility of a mixture of inflammable air and oxygen into water, announces his intention of commenting on various of his friend's recent discoveries, of which he proceeds to enumerate four:—1. The nature of phlogiston. 2. The nature of dephlogisticated air. 3. The conversion of water into air. 4. The conversion of air into water. To each of those topics Watt refers in his letter, which concludes with the following passage, which I quote here because of its importance as proving that Watt was aware that Priestley's paper, which his letter was to accompany, would contain an account of the discoveries on which that letter commented:—"If my deductions have any merit, it is to be attributed principally to the perspicuity, attention, and industry with which you have pursued the experiments which gave birth to them, and to the candour with which you receive the communications of your friends. If you shall think that a hypothesis so hastily compiled deserves to have the honour of being communicated to the Royal Society, or published in any other way, *along with the account of your experiments*, I will be obliged to you to present it to the Society, or to the public, as you shall see proper."

The passage I have marked by italics shows most plainly that Watt relied on Priestley's account of his experiments, and wished his letter to be published along with it. It seems impossible, therefore, to contend that the former was ignorant of the contents of Priestley's paper, and is

* Watt himself gives the following account of the fortunes of his first letter:—"This letter [April 26, 1783] Dr. Priestley received at London; and after showing it to several members of the Royal Society, he delivered it to Sir Joseph Banks, the President, with a request that it might be read at some of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr. Priestley's new experiments, begged that the reading might be delayed. The letter, therefore, was reserved until the 22nd of April last, when at the author's request it was read before the Society. It has been judged unnecessary to print that letter, as the essential parts of it are repeated, almost *verbatim*, in this letter to M. De Luc; but, to authenticate the date of the author's ideas, the parts of it which are contained in the present letter are marked with double commas." (*Phil. Trans.* 1784, note, p. 330.)

† The first part of this sentence (down to "*phenomena*") has already appeared in print, in *A Few Notes on the History of the Discovery of the Composition of Water*, by J. O. Halliwell, p. 1. (Ante, p. 269.)

not to be considered cognisant of the description which it contained of the experiments relating to the conversion of air into water. I do not wish to be understood as implying that Watt had read the MS. of Priestley. It seems, on the other hand, certain, from the date of Priestley's letter to Sir Joseph Banks (April 21, 1783), which accompanied his paper, as well as from internal evidence, that Watt cannot have read the latter part at least of Priestley's MS. when he wrote his letter.

The internal evidence to which I refer, is the fact that, at the date of his letter (26th April, 1783), Watt believed, on the authority of Priestley, that by distilling water in clay retorts raised to a high temperature it could be converted into air. Priestley, however, before he *completed* his paper, discovered that he had been mistaken in this conclusion, and that the apparent conversion of water into air was owing to the steam of the boiling liquid, and the elastic fluids of the atmosphere, changing places through the porous walls of the retort. He did not inform Watt of his mistake, however, till April 29th, 1783, three days after Watt had written his letter; so that, whilst he refers to the conversion of water into air as a certainty, Priestley alludes to it as a mistake, entitling the part of his paper which refers to it, "On the *seeming* conversion of water into air." It is impossible, therefore, that Watt can have read the MS. of at least the latter part of Priestley's paper, and he probably did not peruse any part of it.

Let it then be conceded that Watt was not acquainted with the *ipsissima verba*, or minute details of Priestley's paper; nevertheless he must be held to have been generally aware of the account which Priestley has given of his experiments, for this is implied in the passages of his letter which I have marked in italics; and, in truth, the statements about to be made by Priestley to the Royal Society were only repetitions of statements already made orally or in writing to Watt. And further, it must not be overlooked that Priestley, although informed by Watt that the latter left to him the task of describing his experiments minutely, referred to the charcoal-gas, and *to it alone*, as the inflammable air which he exploded along with oxygen, in his experiments on the conversion of air into water. Neither did Watt at any later period disclaim Priestley's account, or object to the reference to the charcoal-gas, or affirm that hydrogen should have been named instead of that mixture of elastic fluids.

The concluding sentence of Watt's letter to Priestley has not been published before, so far as I am aware; and the advocates of Watt, therefore, may not have had its contents brought under their notice. There are passages, however, as pertinent, in the *Watt Correspondence*. On 21st April, 1783, Watt writes to Dr. Black: "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*."* Here Watt refers to the conversion of water into air, as "the subject" to which his letter and Priestley's paper alike referred; the one recording experiments proving (or apparently proving) the conversion, the other pointing out its cause.

To the same effect he writes to Mr. Gilbert Hamilton: "Dr. Priestley has made many discoveries lately in relation to the conversion of water

* *Watt Corr.* p. 18.

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.*"*

Similar statements occur in letters from Watt to Smeaton (April 27), and to Fry (April 28);† and in truth, till the extent to which Watt's claim was perilled by the connexion of his conclusions with Priestley's erroneous charcoal-gas experiments had been forced upon the attention of Watt's advocates, they did not deny that Priestley's paper contained the account of the researches on which Watt's conclusions were chiefly based. Mr. Muirhead, *ex. gr.*, says, "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.*"‡ Mr. Muirhead's testimony on this point is peculiarly valuable, as he is the most zealous of all Watt's advocates, and his statement may satisfy the reader that I have only put a just construction on the passages I have quoted from Watt's letters. It seems to me impossible, therefore, to acknowledge the justice of Lord Jeffrey's statements, already quoted, that "it is a great and fundamental mistake to suppose that the main object of Priestley's paper was the same, or even very much connected with that of Watt's letter;" and that "Watt's letter, on the other hand, professed only to embody his own 'thoughts on the constituent parts of water,' and had, therefore, no material bearing on the general disquisition of Priestley's." Those statements are at variance with Watt's own acknowledgments, for the passages I have adduced from his correspondence show that he intended his letter to be a commentary on the *whole* of Priestley's paper; and that, instead of limiting himself to the consideration of the constituent parts of water, he has enumerated four topics; namely, the nature of phlogiston; the nature of dephlogisticated air; the conversion of water into air; and the conversion of air into water; to all of which he refers. The title (*Thoughts on the constituent parts of Water*) which his letter ultimately bore, no doubt conveys a different impression, and is referred to by Lord Jeffrey as proving the limitation of Watt's speculations to one branch of Priestley's experiments which Watt took care himself to describe fully. That title, however, was not added to the paper till May, 1784, more than a year after the letter was written,§ and after that portion of it had been withdrawn, which treated of the conversion of water into air. The first letter, in truth, had no title, and the fact is significant, for it needed none if it were to be read after a paper of Priestley's, on which it was a commentary.

It is unnecessary, however, to dwell at greater length upon this; for the extent to which Priestley's paper and Watt's letter go over the same ground, can be determined by any one who will compare the two documents. The agreement is very close, notwithstanding the alteration which Priestley made in his paper, after he discovered that he was mistaken in his supposition, that water can be distilled into air. Thus Priestley enters at great length into the properties of phlogiston, and details a long series of experiments demonstrating that "pure inflammable air" can reduce metallic and other calces or oxides, and revive the metal or the combustible (such as sulphur or phosphorus) which they contain. Then he records the experiments made by heating

* *Watt Corr.* p. 20. † *Watt Corr.* pp. 23—24. ‡ *Watt Corr.* p. 21.

§ *Watt Corr.* p. 63. Letter from Watt to Blagden, May 27th, 1784.

water in porous clay retorts, which appeared to him, when he communicated his observations to Watt, to prove that water can be converted into air; and thereafter he describes the repetition of Cavendish's experiments,* which seemed to him to establish with more or less certainty the converse of his own (supposed) discovery, namely that air (gas or elastic fluid) can be converted into water. Such is a very brief analysis of Priestley's paper. Watt's (MS.) letter of April 26, 1783, will show to what extent it is occupied with topics similar to those discussed by Priestley. Addressing Priestley, Watt says: "1st. You have shown by the experiment of reducing the calces of metals in inflammable air, that the latter is either phlogiston itself, or that it contains a very small quantity of any other matter. 2nd. You have informed me, that when you mix together quite dry inflammable air and quite dry dephlogisticated air, and fire them by means of the electric spark in a close vessel, you find that a quantity of water very nearly or quite equal in weight to the whole air, is deposited on the sides of the vessel . . . . . 3rd. That when you expose to heat porous earthen vessels previously soaked with water, or make steam pass slowly through a red-hot tobacco pipe, that the water or steam is converted into air, &c."† This it will be seen is an abstract of Priestley's paper. I have taken it from the original letter as it exists in the archives of the Royal Society, because it is the document on which Watt's claim to priority over Cavendish is mainly based. That paper, however, is not generally accessible, and for the present may seem less authoritative than those already before the public. It seems well, therefore, to notice that abstracts of Priestley's paper nearly identical, will be found in a letter from Watt to Black, April 21, 1783, and in one to Gilbert Hamilton of April 22, of the same year.‡ I make but one further remark on this topic. In Watt's paper as it was ultimately published, with the title, *Thoughts on the constituent parts of Water*, the connexion between his views and Priestley's is less apparent than in the original letter, but the very difference, in the amended paper, only adds to the force of the conclusion I am urging. For the difference mainly consists in the omission of all reference to the power of porous clay vessels to convert water into air, which Priestley had discovered to be a delusion, and which fell to the ground along with all that Watt had founded upon it. That it should have been referred to, however, at all, shows how unwarranted are the statements which represent Watt as only interested in Priestley's experiments on the synthesis of the elements of water. Priestley himself, an unexceptionable authority in the present case, thought very differently; for, assured of the importance which Watt attached to the conversion of water into air, Priestley wrote to him, informing him of the mistake he had made, in the following terms:—"Behold with *surprise and with indignation* the figure of an apparatus that has utterly ruined your beautiful hypothesis."§ To this Watt replied as if he cared little for the new observations of Priestley: "I deny

* Quoted in full, ante, p. 284.

† The remainder of the letter, which is occupied with Watt's conclusions concerning the composition of water; the relation of elementary heat to the production of that liquid; the nature of oxygen; the mode in which a porous clay vessel acts when it (apparently) converts water into air, &c. will be referred to in another place. They form the commentary on the text of which Watt has given the abstract, and are not at present under discussion.

‡ *Watt Corr.* pp. 18—21.

§ *Watt Corr.* p. 25. Priestley to Watt, 29th April, 1783.

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."* But that Priestley did not overrate the importance which Watt attached to the porous clay experiments, is evident from two things: 1. In his original letter the latter refers to them thus, "On considering the last and *most remarkable* production of air from water imbibed by porous earthen vessels, (the only case wherein it appears almost incontrovertibly that nothing was concerned in the production except water and heat,) I think," &c.† 2. It was the information supplied by Priestley, that water was *not* convertible into air by porous clay heated, that induced Watt to withdraw his entire letter from the Royal Society. Writing to Dr. Black on 23rd June (1783), he says, "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 theory useless in so far as relates to the change of water into air by means of porous earthen vessels."‡ This fact supplies the best proof, that the whole of Priestley's paper, and not merely the section of it referring to inflammable air and oxygen, was before Watt's mind when he wrote his letter. Had he concerned himself only about the conversion *of inflammable air and oxygen* into water, he would not have withdrawn his paper because water was not convertible into *atmospheric air*. Had his letter accordingly been read to the Royal Society at the time of its receipt, the Water Controversy would either not have arisen, or would have exhibited a very different aspect from that it has shown. As it is, that controversy is a standing record of the intimate connexion that subsisted between the whole of Priestley's paper and the whole of Watt's letter.

It should seem, then, that unless very distinct and explicit proof can be afforded, that Priestley did perform experiments with *hydrogen* and oxygen, and that on these Watt's conclusions were founded, it will be impossible to exculpate him from a participation in Priestley's erroneous preference of charcoal-gas to hydrogen, or to understand Watt as signifying, by inflammable air, the latter gas. Yet if he did not, he cannot be considered as having taught that water consists of hydrogen and oxygen.

Lord Jeffrey, as already implied, is the only one of Watt's advocates, who has seen and acknowledged the necessity of proving that by inflammable air Watt signified hydrogen. But even he does not profess to have discovered a direct statement in any production of Priestley's or Watt's, that hydrogen was employed in the experiments of the former; and he only says that he "cannot but believe that there were other experiments made with hydrogen; and this for a great variety of reasons."§ These I shall presently notice; but before doing so, the reader will not fail to observe that in discussing them we necessarily abandon the direct documentary evidence, on which hitherto all our conclusions have rested. The friends of Watt have been fond of contrasting the gaps which occur in the early chain of evidence in favour of Cavendish's priority, with the direct and unbroken succession of proofs which they allege can be adduced in support of Watt's claims; but it now appears by the acknowledgment of the ablest of Watt's defenders, that there exists no docu-

* *Watt Corr.* p. 27. Watt to Priestley, 2nd May, 1783.
† MS. Letter, April 26th, 1783.
‡ *Watt Corr.* p. 30.
§ *Edinburgh Review*, January, 1848, p. 94.

ment directly affirming that he believed, or taught, that hydrogen is one of the elements of water. He called the combustible ingredient of water *inflammable air* or *phlogiston*, and he has in none of his writings limited either of these terms to hydrogen. If it can be shown that he did signify hydrogen by the titles in question, it is only by a lengthened and circuitous process, involving the comparison of many passages in the writings of Watt and Priestley, and which does not, even in the hands of Lord Jeffrey, yield a decisive result. The result it does yield, however, must be ascertained as the only means of doing justice to Watt.

His theory of the composition of water he implicitly announced, as Cavendish also did, in two ways:—1. As a conclusion from certain experiments. 2. As a formula more general in its character, founded upon that conclusion. The particular conclusion was, that water consisted of inflammable air and oxygen. The general formula was, that since inflammable air is phlogiston, water may be defined to be a compound of phlogiston and oxygen. I reserve the full discussion of Watt's views concerning phlogiston, as I have done those of Cavendish on the same subject, to another section; and limit myself here to the consideration of his inference from Priestley's experiments.

I have already quoted Watt's conclusion, as given in his "Thoughts on the constituent parts of Water," &c.; I state it here again in a more compendious form from his letter to Black (21st April, 1783), as the text of the following remarks: "When quite dry pure inflammable air and quite dry pure dephlogisticated air are fired by the electric spark in a close glass vessel, he [Priestley] 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. . . . . . . Are we not then authorised to conclude that water is composed of dephlogisticated and inflammable air?"*

One, then, of the elements of water, according to Watt, was dephlogisticated air, by which it is not disputed that he signified what we now name oxygen. The other was "inflammable air," and we are now to consider whether he denoted by that gas, hydrogen. As this question, however, is of the greatest importance, and cannot be discussed without digressing from the direct path in which our main inquiry lies, I shall devote a separate section to its consideration.

7. *On the signification of the term Inflammable Air as used by Watt, to denote the combustible element of Water.*

When Arago revived the Water Controversy, he thought it so certain that Watt signified by inflammable air, hydrogen, that he considered himself at liberty to substitute the one term for the other, and did so in his Eloge of Watt. A large part, accordingly, of Mr. W. V. Harcourt's inaugural address at the meeting of the British Association in 1839, was a protest against the liberty thus taken as not consistent with the facts of the case. In 1840, Arago sought to vindicate himself from the charge, by pointing out that he had not given Watt any unfair advantage over Cavendish by the change of words he had made, inasmuch as he had substituted 'hydrogen' for 'inflammable air,' when referring to the latter's views, as well as when discussing those of the former. This explanation, however, was manifestly insufficient, and involved a *petitio principii;* for

* *Watt Corr.* p. 19.

an important part of the question in dispute was, "Did Watt use the word 'inflammable air' in the sense in which Cavendish employed it?" The advocates of the latter showed that he defined his inflammable air as that from zinc, and in effect they asked at the hands of the advocates of his rival for evidence that Watt employed the word in the same sense. Arago thought it a sufficient reply to this request, to refer to an addition which he had made to Lord Brougham's historical note which was printed along with the Eloge of James Watt.

The statement was to the following effect. "In 1784, the preparation of two permanent and very dissimilar gases was known. Some called these gases, pure air and inflammable air; others, dephlogisticated air and phlogiston; and lastly, others, oxygen and hydrogen."* If Arago's opinion, as stated in the quotation, be just, there is an end to the Water Controversy; but his view cannot be substantiated. The term "hydrogen" was not used even by Lavoisier at the period when Watt and Cavendish read their papers to the Royal Society, and could not precede in time the discovery of the compound nature of water. I shall set that term therefore aside as irrelevant to the present discussion, and the signification of the word "phlogiston," in its widest acceptation, has already been adjourned to a succeeding section; so that I am now to inquire, first, whether Arago is right in affirming that inflammable air, and phlogiston in the sense of inflammable air, so certainly signified hydrogen in 1783 and 1784, that Watt must be understood to refer by these titles to that gas; and secondly, whether, as Lord Jeffrey urges, Watt did so limit his use of phlogiston and inflammable air when describing his conclusions or the experiments from which they were drawn, that it cannot be doubted that he denoted by both terms hydrogen.

It was not till the very close of last century, that chemists thoroughly awoke to the conviction that difference of property indicated radical difference of substance, simple or compound, and became satisfied that the various gases were not modifications of one air or gas, but specifically distinct bodies. This length, however, they had not got in 1783; and, accordingly, they included the whole of the combustible gases known at that period under the one title of inflammable air, which Arago conceives them to have applied *only* to hydrogen. That they did not, however, is not difficult to prove. Priestley prided himself on having clearer views as to the essential differences between the gases than any of his predecessors, not excepting "even Mr. Cavendish."† Yet his definition of inflammable air, so late as 1790, was as follows:—The term inflammable air "sufficiently characterises and distinguishes that kind of air which takes fire, and explodes on the approach of flame."‡ I quote this passage on account of its precision and brevity, and because Priestley's "Observations on Air" were regarded as a storehouse of facts, to which Cavendish, and especially Watt, but in truth all the chemists of Europe, had recourse for information; however cautious the wiser amongst them were, in discriminating between the value of the ingenious author's observations and his conclusions.

* *Watt Corr.* p. 252, and 263; or, *Historical Note by Lord Brougham*, appended to French and English editions of Watt's Eloge; also *Comptes Rendus*, Jan. 1840, pp. 109—111.

† *Priestley on Air, abridged.* Published in 1790. Vol. i. Introduction, p. 6. The whole of this introduction is worth perusal, in reference to the subject under discussion.

‡ *Op. cit.* p. 8.

In the fifth of his six *original* volumes on air, which was published in 1781, Priestley gives summaries of all the facts he had collected from his own observations and those of others, in reference to the different airs. Pages 335, 6, 7, and 8, are devoted to "Facts relating to Inflammable Air." These pages, consulted in their double character of index and summary, supply the most comprehensive account with which I am acquainted, of what the substances were to which the chemists of the eighteenth century gave the name of inflammable air. From Priestley's "Facts" we learn that that title was applied, 1. to hydrogen; 2. to sulphuretted hydrogen; 3. to various definite compounds of carbon and hydrogen, such as marsh gas and olefiant gas; 4. to combustible vapours, such as those of ether and turpentine; 5. to mixtures of combustible gases and vapours, such as coal gas; 6. to mixtures of combustible and incombustible gases, which contained so much of the former as to be inflammable, such as the gas from heated charcoal, consisting of carbonic oxide, carbonic acid, and carburetted hydrogen. To all those elastic fluids the name of "inflammable air" was given, and by Priestley's contemporaries as well as himself. He did not introduce the name; on the other hand he tells us, "I found the terms *common or atmospherical air*, *fixed air*, and *inflammable air*, used by all philosophers, and no person whatever had objected to them."*

I have quoted chiefly from Priestley in illustration of the meaning of the word inflammable air. Testimony to the same effect might be produced from his contemporaries. It will be enough, however, if I show that Watt, Cavendish, Lavoisier, and Priestley, who were the parties chiefly connected with the Water Controversy, did not consider the word inflammable air as necessarily synonymous with hydrogen. Watt and Cavendish's views are fully stated a little further on. Of Lavoisier I have only to say, that he most certainly did not, as Arago implies, identify hydrogen with inflammable air, but only with one combustible gas, which he distinguished by the name of "aqueous inflammable air" (air inflammable aqueux).†

It appears, then, that every known gas, vapour, or mixture of gases or vapours (in a word, every elastic fluid), which was combustible in the atmosphere, was called, in 1783, inflammable air. When the word, therefore, occurs unqualified in writings of that period, it signifies neither more nor less than combustible gas. Hydrogen unquestionably was called by the chemists of the latest Phlogiston School, "inflammable air;" but the words are not convertible. The one was a generic, the other a specific term. Hydrogen was inflammable air, but inflammable air was not necessarily hydrogen. When, accordingly, the word inflammable air occurs in a writing of the last century, the canon of interpretation is not to settle summarily that it signifies hydrogen, but by a study of the context to discover what combustible gas it does denote.

The chemists of the closing half of the preceding century were day by day realising more clearly that there were different *kinds* of inflammable air. But in 1783, not one of those various airs had been subjected to analysis, so that the views of philosophers as to the nature of the differences among them were necessarily very vague. They seem, indeed, to have oscillated between the conception of one elementary air, modified variously by impregnation and mixture so as to become inflammable, or

* *Priestley on Air*, vol. ii. 1776, p. 334.

† *Mémoires de l'Académie des Sciences pour* 1781, p. 468 (printed in 1784). Reprinted in *Watt Corr.* p. 171.

rather perhaps of one inflammable air, *sui generis*, but in like manner liable to alteration in properties; and the idea of specifically distinct bodies.* The last view, however, is nowhere unequivocally expressed, and was not clearly apprehended, nor could it be, for the mind has no satisfaction in dwelling upon differences, when it can do no more than doubtfully realise that there are differences without apprehending their nature or extent.†

Although the chemists of the Phlogiston School, however, waived precision of definition as to the kinds of inflammable air with which they were acquainted, they nevertheless distinguished certain of them by distinctive names. These had not relation, as our titles at the present day generally have, to the properties or composition of the gases; but to their *sources*. Thus the carburetted hydrogen which rises from stagnant pools, was "*the inflammable air of marshes*,"—a name still retained.‡ The mixture of gases obtained by heating charcoal, was "*the inflammable air of* or *from charcoal*." Hydrogen, the body that concerns us most, was "*the inflammable air of* or *from the metals*." It received this name because, according to the views of chemists before Lavoisier's time, when iron or zinc dissolved in acidulated water, and hydrogen was given off, it was the metal, and not, as we declare, the liquid which yielded the gas. This name was introduced after Cavendish's paper on hydrogen in 1766. He uses the term frequently; Priestley employs it constantly. Watt was familiar with it, and introduces it in the paper containing his views concerning the composition of water thus: "According to Dr. Priestley's experiments, dephlogisticated air unites completely with about twice its bulk of *the inflammable air from metals*."§

It does not seem necessary to adduce further evidence. The chemists of 1783 and 1784 certainly did not, as Arago supposes, appropriate the term "inflammable air" to hydrogen; and Watt knew that they did not.

It remains to inquire in what sense he used the word "inflammable air." Before doing so, I lay down the following rules for my own and the reader's guidance:—1. When a chemist of the last century employs the term "inflammable air" without qualification or restriction, or any reference, in the context or otherwise, to its source or mode of preparation, he must be understood to include under the title each of the combustible gases or elastic fluids which he can be shown to have called inflammable air. 2. When a chemist of the last century is describing an experiment with inflammable air, though he does not define the latter, its nature may generally be learned by his account of the process by which he prepared it. We shall know, for example, if it were hydrogen which he made use of, by his stating that the inflammable air was obtained by dissolving iron or zinc in diluted sulphuric or muriatic acid.

* See, in illustration of this, a very interesting letter from Volta to Priestley (1776), published by the latter, in his 3rd volume on Air (1777), App. p. 381.

† The justice of this remark will be appreciated, if it be remembered that the combustible gases are analysed by *oxidising* them, whilst, at the period of which I am writing, the products of combustion in oxygen were unknown. Till water was demonstrated to be the oxide of hydrogen, there existed no means of proving that an inflammable gas contained hydrogen; till carbonic acid was shown to be an oxide of carbon, the presence of carbon in the majority of the combustible gases could not be proved. So far, indeed, was a distinct recognition of hydrogen as a specific substance from preceding the discovery of the composition of water, that it was this discovery that furnished the means of distinguishing hydrogen from other combustible gases.

‡ *Priestley on Air*, vol. iii. (1777), Appendix, p. 382.

§ *Phil. Trans.* 1784, p. 349.

3. If he desire to limit his observations to a single kind of inflammable air, he will do so by stating its source. Thus he will describe or define hydrogen as the inflammable air of or from the metals.

To what substances Watt applied the title of inflammable air, will be understood from the following quotations. In a letter to Dr. Black (3rd February, 1783), he mentions "that olive-oil, or oil of turpentine in that earthen retort, produces very pure inflammable air."* In a letter to Priestley (2nd May, 1783), he refers to "the inflammable air produced from spirit of wine and oils."† In a letter to Gilbert Hamilton he states, that "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."‡

The reference here is to certain experiments of Priestley's, in which he conceived, that by heating pure iron filings he could make them yield inflammable air. He describes his experiments thus in the paper ("Experiments relating to Phlogiston, and the seeming Conversion of Water into Air"), on which Watt's letter was a commentary. "The second article that I shall now mention, affords an indisputable proof of the generation of fixed air from dephlogisticated air, and phlogiston or inflammable air. . . . . . I was firing some shavings of iron in dephlogisticated air confined by mercury, by means of a burning lens. In this way I quickly fired the iron, and it burnt away in a very pleasing manner. But what struck me most was, that of the air that remained, a considerable portion was fixed air, though in the receiver I had nothing but the purest dephlogisticated air, together with the iron which could only give inflammable air. . . . . . Afterwards to put this hypothesis concerning the constituent principles of fixed air, to a more direct proof, I mixed iron filings, which gave only inflammable air, with red precipitate, which I found to give nothing but the purest dephlogisticated; and when I heated them in a coated glass retort, they gave a great quantity of fixed air, in some portions of which, nineteen-twentieths were absorbed by lime-water; but the residuum was inflammable."§ From this account it appears that the iron must have contained carbon, for it yielded carbonic acid when heated with red oxide of mercury; and in all probability the inflammable air referred to, consisted of a mixture of combustible gases (hydrogen, carburetted hydrogen, and carbonic oxide), resulting from the action of the heated carburet of iron on moisture entangled among the metallic filings. At all events, it could not be pure hydrogen.||

* *Watt Corr.* p. 14. † *Watt Corr.* p. 27.
‡ *Watt Corr.* p. 17. § *Phil. Trans.* 1783, pp. 412, 413.

|| Cavendish refers thus, to what he calls "Dr. Priestley's experiment of expelling inflammable air from iron by heat alone."—"I am not," he continues, "sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it much more likely that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated." (*Phil. Trans.* 1784, p. 137, note.)

Priestley adopted Cavendish's views. In the 6th vol. of his *Expts. and Obs. on Air*, published in 1786, he refers to his having *previously* observed that "iron filings in a gun-barrel, and a gun-barrel itself, had always given inflammable air whenever I tried the experiment." (p. 88.) Induced, however, by what he says, Cavendish told him, he observed matters more carefully, and noticed that "iron filings are seldom so dry as not to have moisture enough adhering to them, capable of enabling them to give a considerable quantity of inflammable air . . . . Being thus apprised of the influence

In addition to the passages I have quoted, in which the term *inflammable air* is employed by Watt, there are other places in which the word phlogiston is used as synonymous with that term. Although, accordingly, the full signification of phlogiston is not at present under discussion, it is necessary to refer to it where it signifies inflammable air. Thus, in a quaint letter addressed by Watt to Mr. Fry of Bristol, after referring to the composition of air and water, he says, "I will add the receipt below for making both those 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, &c. &c."*

Here, it cannot be doubted, that Watt signified by phlogiston, inflammable air; and that he always uses the words as synonymous and interchangeable, is most strongly held as a cardinal point by the advocates of Watt, from Arago to Muirhead. I have already quoted the statement of the former. The latter, when defending Arago from the criticisms of Berzelius, says, "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 again, "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."†

The italics in the preceding quotation are Mr. Muirhead's, and from both passages, as well as from Arago's statement, it will appear, that the advocates of Watt themselves insist upon their client being understood *always* to signify by phlogiston, inflammable air. Were I writing as a partisan, I might content myself with their statement, and insist upon the conclusion which I am about to urge on the reader. It is not my object, however, to avail myself of concessions made by the advocates of any of the parties in the Water Controversy, unless those are consistent with my own convictions, and, as Mr. Muirhead did not see the danger to his client of his concession, and was not alive to the necessity of establishing that Watt signified by phlogiston or inflammable air, hydrogen, but supposed that the three names were identical in meaning, I cannot build upon his admission; especially since Lord Jeffrey acknowledges and seeks to supply the defect in Mr. Muirhead's argument. After all, however, Lord Jeffrey is at one with Mr. Muirhead, in holding that Watt signified by phlogiston inflammable air, only he seeks to limit Watt's employment of it to the one inflammable air, hydrogen. The whole, then, of the

of unperceived moisture in the production of inflammable air, and willing to ascertain it to my present satisfaction, I began with filling a gun-barrel with iron-filings in their common state, without taking any particular precaution to dry them, and I found that they gave air, as they had been used to do, and continued to do so many hours. I even got ten ounce measures of inflammable air from two ounces of iron filings in a coated glass retort. At length, however, the production of inflammable air from the gun-barrel ceased; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston." (p. 90.) Watt refers to those experiments, as they were originally tried, in the letter to Gilbert Hamilton. (*Watt Corr.* p. 17.)

* *Watt Corr.* p. 24.

† *Op. cit.* pp. cxiii and cxiv.

advocates of Watt contend that he used phlogiston as a synonyme for at least *combustible gas.* And in this view I believe that they are correct, at least in interpreting Watt's direct references to the composition of water, although such is not Mr. Harcourt's view. One consideration, however, is sufficient to show that the opinion attributed by Mr. Muirhead to Watt, was really held by him. His conclusion was certainly drawn from experiments in which inflammable air and dephlogisticated air were employed, and it represented water as consisting of those two gases. When, therefore, he used the word phlogiston to indicate one of the gases of water, he must have signified by that term either inflammable or dephlogisticated air, and as he certainly did not intend the latter, for which his synonyme was pure air, he must have intended the former.

Thus much, then, premised, I quote the following passage, which is one of the most important statements in Watt's writings in reference to the combustible elements of water. It has not in general been referred to by his own advocates. He is discussing in the close of his "Thoughts on the constituent parts of Water," the question, how much heat is evolved when phlogiston is combined with dephlogisticated air, and refers in illustration to certain experiments of Lavoisier and La Place made with sulphur, phosphorus, and charcoal. After stating the nature of their trials, he continues, "The weight of the ashes of an ounce of charcoal is very inconsiderable; and by some experiments of Dr. Priestley's, charcoal, when freed from fixed air, and other air which it imbibes from the atmosphere, is *almost wholly convertible into phlogiston.* . . . . . . It is also worthy of inquiry, whether all the amazing quantity of heat let loose in these experiments was contained in the dephlogisticated air; or whether the greatest portion of it was not contained in the *phlogiston or inflammable air.*"* From this passage, I think it indisputably appears that Watt applied the term phlogiston in the sense of inflammable air, to a combustible gas into which Priestley had shown (or appeared to have shown), charcoal could be converted. How important this reference of Watt's is, in connexion with the much criticised charcoal-gas experiments of Priestley, from which the former is alleged by the advocates of Cavendish to have drawn his conclusion, I need not insist. To that I shall recur.

At present, it seems only necessary to notice, that although Watt does not specially refer to the nature of the experiments by which Priestley effected the conversion referred to, there cannot be any reasonable doubt as to what the experiments were. They have already been noticed incidentally as contained in Priestley's paper on "Phlogiston and the seeming conversion of water into air," which Watt's "Thoughts," in their original epistolary form, were intended to accompany. The following passage from the first section of Priestley's paper, which is solely occupied with experiments illustrating that phlogiston and inflammable air are the same thing, will sufficiently demonstrate what the process was by which charcoal was converted into phlogiston: "I shall conclude these observations on phlogiston with two articles, one of which seems to contradict an established maxim among chemists, and the other a former opinion of my own.

"It is generally said that charcoal is indestructible, except by a red heat in contact with air. But I find that it is perfectly destructible, or decomposed *in vacuo,* and by the heat of a burning lens almost wholly converted into inflammable air; so that nothing remains besides an ex-

* *Phil. Trans.* 1784, pp. 351, 352.

ceedingly small quantity of white ashes, which are seldom visible, except when in very small particles they happen to cross the sunbeam, as they fly about within the receiver. It would be impossible to collect or weigh them; but, according to appearance, the ashes thus produced from many pounds of wood, could not be supposed to weigh a grain. The great weight of ashes produced by burning wood in the open air, arises from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without the least particle of fixed air in it. But in order to this the charcoal must be perfectly well made, or with such a heat as would expel all the fixed air which the wood contains, and it must be continued till it yield inflammable air only, which in an earthen retort is soon produced."* The succeeding paragraph, which refers at length to the heating of charcoal in retorts, has already been given.

Priestley republished his account of these experiments in 1786, but he added the following important note, as a qualification of his original statements. "Notwithstanding these *facts*, it will appear from my subsequent experiments, that water was necessary to the formation of this inflammable air from *wood* as well as of that from *iron*."†

Altogether, then, it appears, that Watt applied the term inflammable air, or phlogiston as a synonyme of it, to 1. Oil gas; 2. Light carburetted hydrogen; 3. A gas obtained by heating moist iron containing carbon; 4. A gas obtained by heating charcoal; and it may be noticed further that in the only passage of his paper in which he directly refers to hydrogen, he defines it as "the inflammable air from metals."‡

There were thus five gases or gaseous mixtures, which were severally styled by Watt, inflammable air or phlogiston, and there was one of those five, which, like his contemporaries, he distinguished as the inflammable air from metals. It cannot, therefore, be conceded to his advocates, that when he uses the term "inflammable air" without any qualification, he signifies only that one among the five which we now name hydrogen.

I may now, therefore, take for granted, that it is incumbent on the advocates of Watt to show which of his five inflammable airs he regarded as the combustible element of water; if in reality he assigned to one a preference over the rest, as an ingredient of that liquid. I pass, therefore, to the arguments by which Lord Jeffrey seeks to show that the inflammable air specially referred to by Watt as an element of water was hydrogen. "It seems admitted," says his Lordship, "that if Priestley had made, and shown, or reported to Watt, other experiments with the proper hydrogen, which might certainly have given the results which he specifies, there would have been nothing to say against the accuracy, any more than against the originality of that conclusion. Now, looking at the whole of the evidence before us, we have come to be satisfied that Priestley *had*, in point of fact, made and shown, or reported to Watt, such other experiments; and that, though it may be somewhat difficult to account for some expressions which he uses in speaking of his charcoal experiments in that paper, it would be *immeasurably more difficult* to believe that there were no other experiments with hydrogen, and that those two gifted individuals

* *Phil. Trans.* 1783, p. 411.

† *Priestley on Air*, vol. vi. (1786), p. 24. The italics are the author's own. He gives a full account of the experiments (tried at Cavendish's suggestion), which led him to change his opinion, in the same vol. pp. 87—90.

‡ *Phil. Trans.* 1784, p. 349.

were the dupes and victims of an *hallucination* without parallel or precedent in the history of the human understanding."*

Lord Jeffrey then proceeds to adduce various reasons for believing that there were other experiments made with hydrogen. "First of all, the whole series was professedly entered on as a mere repetition of those of Cavendish, which were made exclusively with that substance ; and it seems inconceivable that, when the main object was to test their accuracy, he should not have *begun* at least, with the same materials."† It might suffice as reply to this argument, to notice, that it is not easy to see why, if Priestley made preliminary trials with hydrogen, he should not have mentioned the fact ; whilst on the other hand it cannot surprise us, that one who thought inflammable air from charcoal preferable to that from metals, because it was drier, should have thought it needless to use the latter. But it is of more importance to notice that Priestley did not try the experiments under consideration, *to test Cavendish's accuracy*, but to convince himself that air or gas (gases) could be converted into water ; and that neither Priestley nor Cavendish refers to the former's experiments as *mere repetitions* of the trials of the latter. Priestley explicitly announces a device of his own (namely, the substitution of the charcoal-gas for hydrogen), which was intended to render the experiment more crucial.‡ And Cavendish does not refer to Priestley's experiments as identical with his own, but only as "of the same kind," and points out as important, the fact of Priestley's "having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it." There seems then, no antecedent probability that unrecorded experiments with hydrogen were made by Priestley. It is possible, notwithstanding, that such may have been made, and the following are the reasons assigned by Lord Jeffrey for believing that they were. I quote his Lordship's six arguments in full, before commenting on any of them. "First, as early as March 26th, 1783, Priestley had told him [Watt] that 'he put dry dephlogisticated air and dry inflammable air into a close vessel, and fired them by electricity, *that no air remained when both were pure*, but that he found on the sides of the vessel a quantity of *water*, equal in weight to the air consumed.' Now, this is the very experiment, shortly recited, from which, a few weeks after, Watt intimated that he had drawn his famous conclusion, and we have now only to ask whether these results, or *anything at all like them*, could have been produced if the gas from charcoal, or anything but hydrogen, had been the inflammable air employed ?

"Secondly, on the 21st April, Watt writes to Dr. Black *and to Priestley himself*, informing the one and *reminding* the other, that he [Priestley], after firing the dephlogisticated and inflammable air as above, and opening the close vessel over mercury, found that the mercury rose and filled the vessel '*to within one two-hundredth part of its whole contents*,' and that there was a quantity of water equal or nearly to the weight of the whole air employed.

"Thirdly, that sometime before 28th April, Priestley had also told him that, in order to form water, 'you should take of pure or dephlogisticated air *one part*, and of phlogiston (or inflammable air) *two parts by measure*, and fire them by the electric spark.'

* *Edinr. Rev.* Jan. 1848, p. 94.

† *Op. et loc. cit.*

‡ See, in illustration, Priestley's account of his experiments, quoted in full, p. 284.

"Fourthly, that before 18th May, Priestley had also told him 'that the water remaining after the explosion *is not in the least acid.*'

"Fifthly, that Priestley had also told him before the 21st April, that by heating the *calces* of metals with 'inflammable air,' they were reduced to the metallic state, the air being absorbed (or disappearing) so completely, '*that only two parts out of one hundred and one remained at the end of the operation;*' from which he had inferred that this 'inflammable air *was the thing called phlogiston.*' Now, *it is certain*, from the detailed account of this very experiment, given by Priestley himself, in his paper read to the Royal Society, that the inflammable air there used was the *proper hydrogen;* he expressly describing it as '*air extracted from iron by oil of vitriol;* and it was *this* inflammable air, therefore, and nothing else, that he and Watt were led by this very experiment to consider as 'the thing called phlogiston.'

"But, sixthly, in Watt's own paper, given into the Society in November, 1783, and subsequently printed in their Transactions, he distinctly states that 'according to *Dr. Priestley's experiments* dephlogisticated air *unites completely* with about twice its bulk of *the inflammable air from metals*—the inflammable air being supposed to be *wholly phlogiston.*' Now this is a separate and distinct experiment from that in which the *calces* of metals were reduced by the same agent; and is, therefore, a second and additional proof *what sort* of inflammable air both these philosophers considered as identical with *phlogiston,* and on account of what properties they so considered it." A passage follows which for brevity's sake I omit, in which Lord Jeffrey concedes that in Priestley's experiments on the reduction of metallic oxides by hydrogen, he did not observe that water was produced. "But," continues his Lordship, "in the *second* experiment where *both* airs had been carefully put together in *the proper proportions for forming water*, and were found to 'unite completely,' or be mutually absorbed, it seems impossible to doubt that the formation of water must have been expected, and consequently observed: and, accordingly, though very briefly recorded, we find that it was so; for, in the very same paragraph, and at the distance of only four lines from the words we have cited, the learned author proceeds:—"*Therefore* one ounce of dephlogisticated air will require 120 grains of inflammable air, or phlogiston (that is, unequivocally, of *hydrogen*) *to convert it into water.*"*

In the preceding quotations Lord Jeffrey has, for brevity's sake, in one or two places given the signification of Priestley and Watt's statements, without adducing their very words. If taken, however, exactly as they occur in their writings, none of the passages adduced will be found to contain an explicit reference to hydrogen as the substance which Watt intended to be understood when he spoke of inflammable air, or phlogiston, as a constituent of water. Neither do the whole taken together warrant this inference. The argument based on the first and second passages referred to, amounts simply to this; that since Priestley's experiments are incredible unless we suppose him to have used hydrogen, it should be conceded that he did employ this gas. This argument might have some weight if Priestley had given no account whatever of his experiments, and we had no means of judging of their nature except by learning their alleged phenomenal results. But when he deliberately tells us that he used charcoal-gas, and in effect reiterates the statement years after the Water Controversy commenced; and when he even repeats the

* *Edinr. Rev.* Jan. 1848, pp. 95, 96.

experiment, as I shall presently show he did, with the inflammable air from charcoal,* instead of that from metals, in spite of all that Cavendish, Watt, and Lavoisier had published in reference to the composition of water, it is impossible, in the face of his own statements, to affirm that he must have used hydrogen. In truth, even if it were certain that he had used that gas, it would only render the charcoal-gas experiments the less credible; for it would be still more difficult to believe that one who had accurately observed the results of detonating a mixture of oxygen and hydrogen should err as Priestley did with the charcoal-gas, than it is to understand the mistakes which he committed whilst limiting his attention solely to the latter.

Lord Jeffrey attaches importance to the fact, that Watt twice states that if the vessel in which the gases were burned, was opened under mercury after the explosion, the liquid rose into the vessel, and almost entirely filled it. That the account of an experiment, however, which is certainly erroneous in three particulars,† should err in a fourth, cannot much amaze us. If Priestley could deceive himself, as he certainly did, in reference to the three points of most importance in his repetition of Cavendish's experiments, he might well be misled in a particular which he did not deem of such interest as to deserve mention by himself at all.

Moreover, it is not impossible that he may have obtained certain of the results he describes with the charcoal-gas. In the passages to which Lord Jeffrey refers, Watt states that "if the vessel was opened with its mouth immersed *in water* or mercury, so much of these liquids entered as was sufficient to fill the glass within about $\frac{1}{200}$dth part of its whole contents."‡

The phenomenon here described might occur with the charcoal-gas so far as water is concerned, if the gas were detonated with a volume of oxygen, exactly or very nearly sufficient to convert it into a mixture of carbonic acid and water, both of which would dissolve in the water admitted into the vessel, and allow the liquid to fill it completely. With mercury this could not happen in any conceivable circumstances, so that we must either discredit the account altogether, or may suppose the experiments to have been made with hydrogen. I am willing to accept either alternative. It is of the greatest importance, however, to notice that the experiments in which the vessel was opened under the surface of a liquid, must have been distinct from those in which the water produced was examined as to its purity, and as to its equality in weight with the mixture of gases which had been burned. Priestley's method of procedure, unlike Cavendish's, rendered three distinct sets of experiments necessary. (1). One in which the water was absorbed by blotting-paper, and its weight ascertained; (2) a second set (which perhaps was not made) in which the water was analysed as to its purity; and (3) a third, in which the total conversion of the burned gases into liquid was ascertained by opening the mouth of the vessel in which the explosion had taken place, under water or mercury.

It is possible that experiments of the three different kinds indicated, were performed with inflammable air prepared in the same way. In

* *Priestley on Air*, vol. vi. p. 126.

† Namely, in asserting that charcoal-gas and oxygen (1) yield water only when burned together; (2) yield a weight of water equal to that of the gases burned; (3) and do not yield carbonic acid.

‡ *Phil. Trans.* 1784, p. 332; also *Watt Corr.* p. 19.

Priestley's own account, however, only the first set of experiments is described, and we are left to conjecture, and to Watt's allusions, to discover how the second and third series were performed. It is not improbable that the same inflammable air may not have been used in each series of experiments. We have Priestley's authority for affirming that the inflammable air from charcoal was preferred for the experiments, by means of which he sought to establish that there was equality of weight between the amount of gases burned and of water produced. On the other hand we have no information whatever as to what inflammable air was used when the production of nothing but pure water seemed to be proved. Even, therefore, if we should concede, on the ground of the incredibility of the account on any other supposition, that hydrogen was employed in the experiments where the vessel was opened under mercury and under water, it does not follow that the much more important experiments in reference to the weight and purity of the water, on which Priestley chiefly relied, and which alone he published, were also made with that gas. Without further comment, therefore, I leave the reader to decide for himself, whether the alleged rise of the mercury so as completely to fill the globe after each explosion, is an additional inaccurate statement in an account otherwise in many respects incredible, or a trustworthy report of a collateral experiment, in which hydrogen was employed.

Lord Jeffrey's third argument is founded on the quaint recipe "to make water," given by Watt to Mr. Fry, of Bristol, and already quoted in greater part (ante, p. 300.)* In this he tells his correspondent to take "of phlogiston, in a fluid form, two parts by measure," a direction which his Lordship thinks Watt himself received from Priestley. If this supposition be well founded, we must inquire, not what Watt but what Priestley signified by phlogiston, and it is quite certain that the latter did not restrict the term to hydrogen (ante, p. 97), but used it as synonymous with inflammable air, his definition of which as *a gas* (*any gas or gaseous mixture*) *combustible in air*, has already been given. I apprehend, however, that Watt's recipe is rather the general expression, in the shape of a formula, of his widest view concerning the nature of water, than the mere report of information given him by Priestley. If this be the true view, the passage is of no importance to the question under discussion, for it contains no statement or allusion by which we can discover that Watt signified by phlogiston one combustible elastic fluid rather than another, or that he specially intended hydrogen.

Lord Jeffrey's fourth reason for affirming that Priestley made experiments with hydrogen, is, that Watt reports that the water, which resulted from the combustion of hydrogen, was not in the least acid.† His Lordship frequently refers to this, and even urges that it proves that Priestley's experiments were better than Cavendish's, since the latter was seldom able to get pure water by the combustion of hydrogen and oxygen.‡ Cavendish, nevertheless, knew quite well how to obtain pure water from hydrogen and oxygen, and has told us how to secure its production.§ It is quite true, however, that in the majority of his experiments on the detonation of mixtures of pure hydrogen and oxygen, the water was acid; and the fact is one of great importance in reference to the question before us. Paradoxical as it may appear, the alleged

* *Watt Corr.* p. 24. † Watt's letter to De Luc: *Watt Corr.* p. 30.
‡ *Edinr. Rev.* p. 100. § *Phil. Trans.* 1784, p. 138.

purity of the produced water in Priestley's experiments, instead of justifying the inference that he employed hydrogen, leads to the very opposite conclusion. For although pure hydrogen and oxygen can yield nothing but pure water when they combine, it is very difficult to exclude from one or both gases a little atmospheric air, the nitrogen of which burns along with the hydrogen, when the latter is detonated with oxygen, (at least if the oxygen is present in a quantity exceeding its combining measure), and produces nitric acid. Priestley seems to have detonated the gases in a glass tube with wires inserted into its sides, resembling the electric pistol or Volta's eudiometer.* He does not appear to have emptied this tube at the air-pump as Cavendish did his globe, but to have filled it with mercury which he displaced by the gases, afterwards detonated in the eudiometer. This method of procedure, as well as Cavendish's, rendered inevitable the presence of nitrogen in the apparently pure hydrogen and oxygen. And accordingly we find that $\frac{1}{200}$dth part of the gases escaped condensation; "which remainder," observes Watt, "is phlogisticated air [nitrogen] probably contained as an impurity in the other airs."†

The conditions necessary to the production of nitric acid were thus certainly realised in Priestley's experiments, and although it is not absolutely impossible that in a solitary trial pure water may have been obtained, it is in the highest degree improbable that a series of experiments can have been made with hydrogen and oxygen, in the way Priestley operated, without nitric acid being detected. It appears, accordingly, that so soon as Priestley's attention was directed to the fact that pure water is not the invariable product of the combustion of hydrogen and oxygen, he not only confirmed Cavendish's results, but went the length of affirming that acid water is always produced by the combination of the two gases. The following quotations from his later writings will speak for themselves. The italics are his own. "Having never failed, when the experiments were conducted with due attention to procure some *acid* whenever I decomposed dephlogisticated and inflammable air in close vessels, I concluded that an acid was the necessary result of the union of those two kinds of air, and not water only."‡

"There is, therefore, no source of the *nitrous acid* which I find on the decomposition of dephlogisticated and inflammable air, besides the union of those two kinds of air, which therefore do not make *mere water*, as the antiphlogistians suppose."§ These statements were criticised by Berthollet and others, who did not deny the (apparent) facts, but objected to the conclusions drawn from them. Their objections, however, made no impression on Priestley, who when he republished his observations in 1790, reiterated still more strongly his previous statements. "I must say, as I did when I was myself a believer in the decomposition of water, that I have never been able to find the full weight of the air decomposed in the water produced by the decomposition; and that now I apprehend

* *Phil. Trans.* 1784, p. 331. † *Watt Corr.* p. 19.

‡ *Abridg. of Priestley on Air*, vol. iii. (1790), p. 54. The paper is entitled, "On the Composition of Spirit of Nitre from dephlogisticated and inflammable air," and is reprinted from *Phil. Trans.* vol. lxxviii. It contains a lengthened account of experiments, like those of Cavendish, but chiefly made in a copper vessel, which was corroded by the acid, and yielded, after several explosions within it, a marked quantity of nitrate of copper. The inflammable air used in those experiments was hydrogen, prepared by passing steam over red-hot iron.

§ *Op. cit.* p. 63. See also the extracts from the *Wedgwood Correspondence*, ante, pp. 97–103.

it will not be denied, that the produce of this decomposition is not mere water, but always some acid."*

The non-appearance of acid, then, in Priestley's original experiments, instead of justifying the inference that he employed hydrogen, forbids the belief that he did. Nor must it be forgotten that Cavendish explicitly pointed out that the probable cause of Priestley's finding no acid, was his "having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it."† Priestley was thus taxed with having used the wrong materials when he found no acid, and never denied the justice of the accusation; whilst, on the other hand, in the only experiments where he certainly employed the right materials, he found abundance of acid produced, as he was the first himself to declare.‡ It will appear in the sequel, that Monge and Lavoisier also procured acid water when they experimented as Cavendish did.

Lord Jeffrey's fifth argument refers to certain experiments of Priestley's, in which he reduced metallic oxides by heating them in an atmosphere of hydrogen by means of a burning glass, which concentrated the sun's rays on the oxide. As hydrogen was certainly employed in these experiments, and as both Priestley and Watt call it phlogiston, his Lordship seeks to show that "it was *this* inflammable air, therefore, and nothing else, that he and Watt were led by this very experiment to consider as 'the thing called phlogiston.'" I might repeat, in reference to this opinion, that whatever was Watt's view concerning phlogiston, it is quite certain that Priestley applied that term to many substances besides hydrogen. It is enough, however, to determine what Watt's employment of the word was; and I have but to remark here, that the passages adduced only show that he termed hydrogen, *phlogiston*, not that he confined that term to the single gas in question. In truth, if Watt's own words are taken, they will be found at variance with Lord Jeffrey's interpretation of them. Watt does not say that he inferred "*this* inflammable air," (namely, a special combustible gas, which he defined)—to be phlogiston; nor does he even refer to it as "*the* inflammable air," so as to limit his inference to the particular gas which Priestley employed. His words are: "He [Priestley] found, that by exposing the calces of metals to the solar rays, concentrated by a lens, in a vessel containing *inflammable air* only, the calces of the softer metals were reduced to their metallic state."§ The words, *inflammable air* are three times repeated without limitation or definition in the succeeding sentences, and the final conclusion is "that inflammable air must be the pure phlogiston, or the matter which reduced the calces to metals."‖ To the same effect Watt writes to Black that "by reducing metals in inflammable air, he [Priestley] 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 [not *this* inflammable air] is the thing called phlogiston."¶

* *Abridg. of Priestley on Air*, vol. iii. p. 555.

† *Phil. Trans.* 1784, p. 135.

‡ Priestley's charcoal-gas, when detonated with oxygen, would yield, as we have already seen, carbonic acid. As water, however, dissolves only its own volume of that gas, the quantity of the latter present in the liquid produced at each explosion, would be much too small to give it an acid taste, or to invest it with the power of reddening vegetable blues. Had Priestley suspected the presence of fixed air, he would have tested for it with lime-water.

§ *Phil. Trans.* 1784, p. 331.

‖ *Op. et loc. cit.*

¶ *Watt Corr.* p. 19.

It is quite true, that on consulting Priestley's account of his reduction experiments, we find that hydrogen was the gas employed; but so little importance does Watt attach to this fact, that he never once mentions it in his paper, or throughout his correspondence. It seems to me therefore, that Watt's statements warrant a conclusion exactly the opposite of that which Lord Jeffrey thinks they justify, viz. that provided pure inflammable air was employed, it was unimportant from what source it was obtained. On this, however, I do not insist. It is enough to affirm that the passages quoted do not prove a limitation by Watt of the term phlogiston to hydrogen, but imply a use of it, as synonymous with inflammable air.

Lord Jeffrey's last argument is based upon the only passage in which Watt explicitly refers to hydrogen in connexion with the production of water. The allusion, however, as his Lordship acknowledges, is only incidental, and the passage does not occur in Watt's original letter. It is, nevertheless, the most important statement in his writings referring to hydrogen as an element of water. I shall therefore quote it in full.

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

The passage just quoted, which occurs near the close of the latest version of Watt's "Thoughts," is not the record of observations made by Priestley, except so far as the combining measure of hydrogen and oxygen is concerned. Lord Jeffrey seeks to show that that could not have been discovered without the production of water being simultaneously observed, but this cannot be conceded. I have sought for Priestley's own earliest reference to the fact that hydrogen combines with half its bulk of oxygen; but I have not been able to find it, and I cannot in consequence offer any precise criticism as to the nature of his experiments.

There are several references, however, in Priestley's later papers to the combination of hydrogen and oxygen in the proportions mentioned by Watt; but, singularly enough, he refers to his experiments not as original, but as resembling those of Lavoisier; and he further affirms that water was not always produced when hydrogen and oxygen were detonated together. "I also," says he, "procured water when I decomposed dephlogisticated and inflammable air from iron, by the electric spark in a close vessel, which is an experiment similar to those that were made by Mr. Lavoisier at Paris.† I put 3.75 ounce measures of a mixture of air, of which one-third was dephlogisticated, and two-thirds inflammable from iron, in the close vessel; and after the explosion I found in it one grain of moisture. * * * * But repeating this experiment with half as much dephlogisticated as inflammable air, *I could not perceive any water* after the ex-

* *Phil. Trans.* 1784, p. 349—350.

† This allusion is rather obscure. Lavoisier's recorded experiments were made by direct kindling of the gases, which burned tranquilly together, not by exploding them by the electric spark. This was the method of Cavendish and Monge, and indeed had been employed by Priestley himself, before either of those observers used it.

periment."* Here, then, is exactly the case Lord Jeffrey supposes. The proper materials for the production of water were mingled in the proper proportion, and the appearance of water was watched for, and yet no water was seen. Nor is this a solitary statement of Priestley's. (Ante, p. 97.) In the same paper he asserts that he had never "been able to procure any water when he revived mercury from red precipitate in inflammable air, or at least not more than may be supposed to have been contained in the inflammable air as an extraneous substance;"† and he makes similar statements in reference to the reduction by hydrogen of the black oxide of mercury, and the oxide of lead.

It is quite certain, moreover, as Lord Jeffrey acknowledges, that in Priestley's earlier reductions of metallic oxides by hydrogen (1783), where much larger quantities of the gas were employed than are likely to have been used in his detonations, he altogether overlooked the production of water, nor did he attach any importance to its appearance in his experiments along with Warltire in 1781. From the *Wedgwood Correspondence* it also appears (ante, p. 97), that Priestley deliberately asserted that hydrogen and oxygen may be exploded together in their combining proportions, and yet not produce water. I feel it impossible, therefore, to concede that when he detonated two measures of hydrogen and one of oxygen, he must certainly have witnessed the formation of water. The eudiometers he describes in his "Experiments and Observations on Air," were small, and could contain only a few cubic inches of the mixed gases, which would yield on detonation but one or two grains of water. This might easily escape observation, even if the experiment were made in a shut vessel, as the steam produced would not condense till the vessel cooled. In truth no one, I think, who has perused the endless contradictory statements regarding the products of the combustion of inflammable air, which are scattered through Priestley's volumes, will feel disposed to credit him with having seen anything which he does not explicitly affirm that he did see.

If Watt's statement, moreover, be looked into closely, it will be found to be quite hypothetical, except in reference to the combining measure of hydrogen. His object was to calculate the amount of heat evolved during the combustion of inflammable air, on the assumption that it is equal to that "extricated by the burning of phosphorus." To make this calculation it was necessary to know the relative weights of equal volumes of inflammable air and oxygen, so as to determine what weight of the former would unite with an ounce (by weight) of the latter, when the two gases produced water by their combination. The inflammable air from the metals, or hydrogen, however, was the only combustible gas whose density had been ascertained,‡ and was the only one, accordingly, which could be made the basis of such a calculation as Watt pursued. For this reason he refers specially on this solitary occasion to a particular inflammable air as the only one he had in view, and that not with direct reference to the production of water, but as rendering probable the conclusion "that the union of 120 grains of inflammable air with 576 grains of dephlogisticated air, extricates 9265° of heat."

That the known (or supposed) density of hydrogen was the main cause of Watt's special reference to it, appears further from the continuation of his argument. He is discussing the question whether the phlo-

* *Priestley on Air*, vol. vi. (1786) pp. 126, 127.
† *Op. cit.* p. 128.
‡ By Cavendish, *Phil. Trans.* 1766.

giston of charcoal gives out as much heat as phlogiston in the form of inflammable air does? The facts on which he reasoned seemed to show "that the union of phlogiston in different proportions with dephlogisticated air does not extricate proportional quantities of heat." For this he accounts as follows :—" This difference may arise from a mistake in supposing the specific gravity of the inflammable air Dr. Priestley employed to have been only $\frac{1}{9.6}$ of that of dephlogisticated air; for if it be supposed that its specific gravity was a little more than one-eighth of that of the dephlogisticated air, then equal additions of phlogiston would have produced equal quantities of heat :* this matter should therefore be put to the test of experiment, by deflagrating dephlogisticated air with inflammable air of a known specific gravity," &c.† The concluding reference shows that in defect of better data, Watt referred to the inflammable air from metals, as the combustible gas whose density was best known, and the only one therefore in reference to which a calculation founded on weights could be based.

The only passage, then, in which Watt names hydrogen, is secondary to the main subject of his " Thoughts," to which it was an addition made some seven months after their first issue. It does not profess to record experiments on the production of water performed with hydrogen, but founds on its supposed density an inference as to the amount of heat it should evolve when it burns. The statement, accordingly, may most justly be referred to as proving that hydrogen was *a* gas (one of the gases) which Watt denoted by inflammable air, but it cannot serve to demonstrate that it was *the* only gas to which he gave that name. The continuation of his paper, in the passage quoted, is the best evidence that he gave other gases the same title, for he proceeds without interruption to refer to charcoal, and in the course of his argument states, as already mentioned, that *it* is almost wholly convertible into phlogiston, or *inflammable air*.

I have now to refer to evidence of another kind, illustrating the nature of the materials which were made use of in the experiments from which Watt's conclusions were drawn. Priestley, Cavendish, and Watt, all had occasion to reconsider the data upon which their inferences concerning the composition of water were founded. The two first, some time after they had given their opinions to the world, commented upon the original publication of their views; and the third added notes to his " Thoughts" before they were printed. Something, accordingly, may be learned, both from what they said and from what they left unsaid, in illustration of the point under discussion. I begin with Priestley. His statement is of great importance; for whatever view may be held as to Watt having read Priestley's paper before he addressed his first letter to him, there is not and cannot be any dispute as to Priestley having read Watt's letter, and knowing exactly to what extent the letter referred to his experiments. Yet when Priestley returned to the subject of the com-

* Where Watt or Priestley got those numbers does not appear. The want of any precise statement on this point, adds another element of vagueness to the imperfect record we possess of Priestley's experiments. If we are to understand that the latter had ascertained, by direct trial, that the inflammable air he employed in his repetition of Cavendish's experiments was only 8 or 9 times lighter than oxygen, we could have no better proof that hydrogen was not one of the gases made use of, for its density is only $\frac{1}{16}$th of that of oxygen.

† *Phil. Trans.* 1784, pp. 350—351.

position of water, long after the publication of Cavendish and Watt's papers, he declared in effect, that he had published fully to the world the experiments which were the ground-work of Watt's conclusion. "In the experiments," says he, "of which I shall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. Cavendish, Mr. Watt, and M. Lavoisier. Mr. Cavendish was of opinion that when air is decomposed water only is produced; and Mr. Watt concluded from some experiments, *of which I gave an account to the* [Royal] *Society,* and also from some observations of his own, that water consists of dephlogisticated and inflammable air," &c.* The passage I have marked in italics seems of itself sufficient to negative the supposition, that Watt's conclusion was based upon experiments which Priestley did not publish, although he made them known to Watt. Nothing can be more explicit than the declaration of Priestley, that not unreported trials, but the very experiments which he had detailed to the Society, were the occasion of his friend's "Thoughts on the constituent parts of Water."† Besides this reference to the intimate connexion between his experiments and Watt's conclusions, Priestley published various disavowals of his original assertions concerning the production of water. I have already quoted passages from his later writings, in which he retracted his affirmation in 1783, that *pure* water was the only product of the detonation of inflammable air and oxygen (ante, pp. 97—103). In certain of these passages, he also retracts his early assertion that the weight of water produced, equalled that of the gases burned. The one retractation indeed necessitated the other, for it would have been a contradiction in terms to have affirmed that the burned gases changed entirely into water, and yet partly into acid. The conclusion, accordingly, to which Priestley came when he repeated his experiments was the following—"That water in great quantities is sometimes produced from burning inflammable and dephlogisticated air is evident from the experiments of Mr. Cavendish and Mr. Lavoisier. I have also frequently collected considerable quantities of water in this way, though never quite so much as the weight of the two kinds of air decomposed."‡

Mr. Harcourt drew the attention of Arago to these retractations of Priestley, as neutralising the force of his earlier and opposite declarations. Arago in reply contended that the latest date in the history of the Water Controversy is 1784, and that he was not bound to consider statements which had not been made till 1786 or 1788.§ I must urge, however, that no critic of the Water Controversy can excuse himself from giving every attention to Priestley's affirmations. His earlier statements regarding the synthesis of the elements of water are inexplicable and incredible; and therefore worthless as the foundation of any conclusion. This is in effect the opinion of Lord Jeffrey, who holds that if Priestley did in reality employ the charcoal-gas, he and Watt were "the dupes and

* *Priestley on Air,* vol. vi. (1786) p. 71. The paper is reprinted from *Phil. Trans.* vol. lxxv. p. 279.

† In the quotation given in the text, Priestley refers to observations of Watt's own. It seems needless to demonstrate at length, that these observations were not experiments with inflammable air (of any kind) and oxygen, as all the disputants in the Water Controversy are agreed in holding that Watt made no such trials. The experiments which he did make are recorded in his "Thoughts," and consisted chiefly of observations as to the evolution of oxygen from various nitrates when raised in temperature, and on the conversion of latent into sensible heat.

‡ *Priestley on Air,* vol. vi. p. 138.

§ *Comptes Rendus,* 20 Jan. 1840.

victims of an hallucination without parallel in the history of the human understanding."

To refuse in these circumstances to listen to an explanation which acknowledges the hallucination, and to insist on asserting that Priestley must have obtained impossible results, although he took great pains to explain that he had been mistaken in thinking he had obtained those results, might possibly be the duty of a partisan, but would be a great fault in a historian. Had Priestley's original trials been made with materials which could have yielded the results reported, we might have supposed that he succeeded once, though he failed ever after; but when we find his early statements inconsistent and inexplicable, and his later statements consistent and quite credible, there surely cannot be two opinions, as to which are to be believed.

A statement of Priestley's not less important than those already adduced, remains to be given. His charcoal-gas experiments have been represented by Lord Jeffrey as secondary and subsidiary to more perfect trials made with hydrogen. Yet long after Cavendish, Watt, and Lavoisier had published their views upon the composition of water, Priestley repeated his charcoal-gas experiments, with a view to test the theory which they had published. In his sixth volume on air, published three years after the date of Watt's first letter, he details the following observation :—

" Using more precautions to exclude all water from either of the two kinds of air before the experiment, (both the dephlogisticated air, which was from nitre, and the inflammable air, *which was from charcoal*, being from the first received in mercury, and always confined by it,) I still found a little water after the explosion.

" I varied this experiment by producing the inflammable air in the dephlogisticated air as follows. Into a vessel containing dephlogisticated air confined by mercury, I introduced a piece of perfect charcoal, as hot from the fire as I could bear to handle it, and threw upon it the focus of the lens, so that a quantity of the air was imbibed; but I could not perceive that any moisture was formed. . . . . . The phlogiston the charcoal contained uniting with the deplogisticated air, free from moisture, formed, I presume, the fixed air that was found after this process."*

This passage is remarkable, first, as showing how deliberate Priestley's selection of the charcoal-gas was, and why he preferred it to hydrogen, namely, because it was drier; and secondly, how defective his early analysis of the product of combustion of the charcoal-gas must have been, for he had no difficulty now in detecting carbonic acid.

It thus appears, (1) that Priestley explicitly affirmed that he had published to the Royal Society the experiments from which Watt drew his conclusion; (2) that he retracted his original declaration that he had obtained a weight of pure water equal to the weight of the inflammable air and oxygen he exploded together; (3) that he regarded the charcoal-gas as positively preferable to hydrogen, when the production of water by synthesis of its elements was the object of experiment; and (4) that he believed fixed air, or carbonic acid, to be an oxide of phlogiston, which last term, as synonymous with inflammable air, he applied to the charcoal-gas as well as to hydrogen. Priestley thus testifies against Watt having drawn conclusions from experiments made by him with hydrogen. I turn now to Cavendish.

* *Priestley on Air*, vol. vi. (1786) pp. 127—128.

The criticism which Cavendish published on Priestley's experiments has been quoted more than once already. It is contained in one of the additions which he made to his paper between its being read and printed, in consequence of the public reading of Watt's paper. In this he refers to Priestley "having used a different kind of inflammable air, namely, that from charcoal."*

The interest of this reference lies in the fact that Cavendish taxed Priestley with having used the charcoal-gas; and as the passage implies *only* that gas, and not hydrogen in repeating the experiments of the former, and Priestley, as we have seen, not merely acknowledged the justice of the statement, but used the same gas again in testing the truth of Cavendish's fully published views.

It remains to inquire whether Watt himself ever detected the fallacy of the charcoal-gas experiments, or added anything to his paper after he became acquainted with the views of Cavendish and Lavoisier, to qualify the vagueness of his original references to phlogiston and inflammable air, and limit these terms to hydrogen. It appears that he did not introduce any such qualifications, and the fact is of great importance, for his "Thoughts on the constituent parts of Water, &c.," was not, as some have affirmed, a hastily written letter, but (at least as it was ultimately published) a document which had been very carefully considered and frequently revised. Thus the first version of the "Thoughts" was a letter to Priestley, of date, 26th April, 1783. Two days later, however, he recalled this letter and forwarded another copy, assigning as his reason for so doing that he had "discovered some inaccuracies in language, and some inconsistencies in the theoretical essay" he had previously sent his friend.†

This second letter was not publicly read till the succeeding year; before this, however, viz. in November, Watt drew up a third version of his views, which he addressed as a letter to De Luc, of date, November 26, 1783.‡ This letter, nevertheless, satisfied its author no better than the first to Priestley, and on April 17th, he writes to De Luc,§ stating that he had made certain alterations on what he had sent him. On the same day he writes also to Sir Joseph Banks, "I have, however, revised the letter itself, and by this post send a corrected copy to him [De Luc], which he will deliver to you, ... I have also added some notes, &c."‖ A postscript, also, which he had not been able to finish in time to add to this letter, was sent as a separate communication to De Luc, of date, April 30, 1784.¶

Finally, after the letter to Priestley, and the letter and postscript to De Luc had been read to the Royal Society, Blagden wrote to Watt to know in what shape these papers should be published.** Watt desired in reply that the letter to Priestley, and that to De Luc, should be incorporated in a way which he pointed out, and at the same time he furnished a title to the double document to which he also added an explanatory note.†† There were thus no fewer than five versions of Watt's "Thoughts," besides a postscript or sequel, which he himself styles "an explanatory letter."‡‡ Between the issuing of the first and last of those versions, more than a year elapsed, during which the subject which they discussed was

* *Phil. Trans.* 1784, p. 135. † *Watt Corr.* p. 23.
‡ *Watt Corr.* p. 32. § *Watt Corr.* p. 54. ‖ *Watt Corr.* p. 56.
¶ As it appears in the *Phil. Trans.* 1784, p. 354, it is entitled, *Sequel to the Thoughts on the Constituent Parts of Water and Dephlogisticated Air. In a subsequent Letter from Mr. James Watt, Engineer, to Mr. De Luc, F.R.S.*
** *Watt Corr.* p. 62. †† *Op. cit.* p. 63. ‡‡ *Op. cit.* p. 56.

thought over again and again by Watt, and various amendments were made in the statement of his views.* Yet in the last version, as well as in the first, he is satisfied with calling the combustible element of water inflammable air or phlogiston, and nowhere informs the reader that he desires him to understand by these titles only the inflammable air from metals, *i. e.* hydrogen. In this respect, there is a great contrast between him and his rivals, for both Cavendish and Lavoisier were careful to define that their conclusions had reference to experiments made solely with hydrogen.

Again, the *Watt Correspondence* embodies some seven separate accounts of Watt's views on the composition of water, addressed by him to different friends:† and in truth there are few of the letters which do not more or less refer to the subject. But in none of them is there a more precise definition of the combustible element of water, than that it is phlogiston or inflammable air. Can it be imagined that if Watt had intended hydrogen to be understood by these terms, he would not have said so? or is it conceivable that throughout a whole year he should have been meditating on the nature of the elements of water, and discussing it among his ablest friends, and yet never once signify to one of them, that of all the inflammable airs he referred to in the course of his remarks, he desired to be understood as intending only the one from metals, when he described the constituents of water? The total absence of any limitation of inflammable air or phlogiston, is irreconcilable with the supposition that such was his intention.

That I do not wrong Watt in saying this, may be proved by a very simple, yet sufficient and fair test. Let us suppose that a contemporary of Watt, anxious to verify the truth of his theory, and ignorant of all that Cavendish and Lavoisier had published on the subject of water, consulted Priestley's paper "On the seeming conversion of Water into Air, &c.," and Watt's "Thoughts," as well as his Correspondence, with a view to discover what two substances he should fire together by the electric spark in order to produce water. In Watt's writings, he would see one of the two substances precisely enough defined as dephlogisticated air or oxygen. The other he would find called "phlogiston in the fluid form," or inflammable air; but without any directions as to the source from which it should be prepared, or special reference to its qualities, except that it should be dry and pure. If he sought through Watt's writings to ascertain whether he limited the word inflammable air to one gas, or to several, he would find him applying it to five different combustible gases; and if he turned to Priestley's paper he would find charcoal-gas the only inflammable air specified. Would such a student come to the conclusion that Watt connected his inference merely with hydrogen, or guaranteed the verification of his theory, only if that gas were employed? Would he not rather, either take the charcoal-gas on the authority of Priestley, or following Watt alone, consider himself free to use any of *his* five inflammable airs, and among others the "very pure inflammable air" obtained

* In referring to these revisions, I have no purpose of disputing Watt's claims, as based even upon the earliest version of his "Thoughts." In truth, the changes which he made were of no great importance, except in so far as he omitted the reference to Priestley's alleged transmutations of water into atmospheric air, and were rather for the worse than the better, in some respects. All that I wish to show, is, that if there be any want of precision in his definitions, it cannot have been the result of haste, or inadvertence, as he had many times conned over the record of his views before he gave it to the public.

† *Watt Corr.* pp. 18—58.

from oil, or the phlogiston in a fluid form, which could be procured by heating charcoal? In truth, five different students of Watt's writings, might each have used a different inflammable air, and yet have justified his choice by a reference to Watt; nor could an umpire, to whom they might have appealed, have found in Watt's papers the means of deciding which gas alone deserved approval.

Another point deserves a moment's attention. The advocates of Watt are naturally solicitous to disconnect his conclusion from Priestley's inexplicable charcoal-gas experiments, and have laboured strenuously to represent him as either ignorant of their nature, or indifferent to them. I have said enough already in abatement of the plea of ignorance; I would now refer to that of indifference. No reason can be given why Watt should have been indifferent to the charcoal-gas experiments; and reasons can be given why he should have preferred them to those with hydrogen. He relied entirely upon Priestley's account of his experiments, and he must have possessed a spirit of divination to have discovered that his friend had deceived himself and misled him, when he declared that the charcoal-gas yielded the results which Priestley affirmed that it did. *A priori*, charcoal-gas was quite as likely as hydrogen, to produce water when detonated with oxygen. Nothing but direct trial could determine whether the one or the other was a water-producer, or true hydrogen. When Priestley, therefore, asserted that charcoal-gas, when it united with oxygen, produced nothing but water, he was as much entitled to credit, so far as the mere assertion was concerned, as if he had stated that our hydrogen is the only body whose oxide is water.

But further, the very considerations which induced Priestley to substitute the charcoal-gas for hydrogen, in repeating Cavendish's experiments, are likely to have had equal weight with Watt. The former gas had the character of being dry or *anhydrous*, so that any water resulting from its combustion, or at least any water exceeding in weight that of the combustible gas burned, must have been *produced*, not merely deposited from pre-existent vapour (at least in the charcoal-gas); whereas hydrogen, as prepared by the solution of iron or zinc in a diluted acid, was certain to have diffused through it much aqueous vapour; and the water which appeared after its combustion might be supposed to be only that which accompanied the gas, from the acid which yielded it. That Watt was alive to those considerations, is evident from all his writings on the composition of water. In the MS. of his letter to Priestley, he says, "You have informed me that when you mix together *quite dry* inflammable air and dephlogisticated air, &c."* In the printed version this is changed into "*pure dry* inflammable air."† He informs G. Hamilton that Priestley "puts dry dephlogisticated air and *dry inflammable air* into a close vessel, &c."‡ To the same gentleman he writes again, "Pure dry dephlogisticated air, and *pure dry* inflammable air fired together, &c."§ To Dr. Black he repeats the phrase of his first letter, "*quite dry* pure inflammable air."|| All these references occur in descriptions of Priestley's experiments, and warrant the conclusion that Watt, unaware, as he along with all his contemporaries certainly was, of the difference between the inflammable air from charcoal and that from metals, would indubitably have preferred the former, as "quite dry pure inflammable air."

The conclusions, then, to which the entire discussion prosecuted in this section conducts us, are:—1. That the experiments on the synthesis

* Ante, p. 290. † *Phil. Trans.* 1784, p. 331.
‡ *Watt Corr.* p. 17. § Ibid. p. 20. || Ibid. p. 19.

of the elements of water, from which Watt drew his conclusion concerning the nature of the latter, were made by Priestley.

2. That Priestley stated that he published to the Royal Society these experiments; and that Watt did not object to the statement; whilst from the published account it appears that the gases burned together to produce water, were oxygen and the inflammable air from charcoal.

3. That Cavendish drew the attention of his readers to the fact that Priestley had employed the charcoal-gas and not hydrogen, and that neither Priestley nor Watt found fault with the statement.

4. That Watt was acquainted with Priestley's experiments, and has himself recorded his belief in the latter's statement that charcoal can be converted into phlogiston or inflammable air by heat.

5. That Watt nowhere describes experiments on the synthesis of the elements of water, made with hydrogen, or in any of his statements of the composition of water defines its combustible element otherwise than as phlogiston or inflammable air, which titles he applied to five different combustible gases.

6. That Watt, like his contemporaries, had a special name for hydrogen, viz. inflammable air of the metals; and would have used that term if he had wished to specify hydrogen as the inflammable air which was an element of water.

When, therefore, Watt stated his opinion concerning the nature of water, as a conclusion from Priestley's experiments, in the words, "Are we not then authorised to conclude that water is composed of dephlogisticated and inflammable air?"* he must be understood to refer to the 'inflammable air from charcoal,' *if he be held to signify one inflammable air more than another*. But as he generalised his conclusion, and announced that "pure inflammable air is phlogiston itself," and that "water is dephlogisticated air deprived of part of its latent heat, and united to a large dose of phlogiston,"† he cannot be held to have limited his reference to the charcoal-gas, but must be considered as including under the titles phlogiston or inflammable air, at least all the combustible gases to which he gave either or both of these names. Whatever, therefore, be the merit of Watt, a question which I shall afterwards consider, he has not the merit of having inferred or announced, either before or after Cavendish and Lavoisier, that water is a compound of the gases we now name oxygen and hydrogen. In other words, he was not *a* discoverer, and *a fortiori*, not *the* discoverer of the true composition of water.

8. *On the full signification of the term Phlogiston, as employed by Cavendish and Watt.*

In the two preceding sections it has been shown that Cavendish and Watt both employed the term phlogiston, as a title for the combustible element of water, and so far, therefore, as a synonyme for 'inflammable air.' Their precise opinion, however, concerning the nature of water, cannot be learned without a further inquiry into their views concerning that mystical entity, phlogiston. To ascertain this, nevertheless, is a somewhat difficult matter. We are too prone at the present day to speak as if all the chemists of the Phlogiston School, from Stahl to Priestley, held precisely the same doctrine concerning phlogiston; whereas, in reality, the later disciples held nothing, almost, in common with their predecessors,

* *Watt Corr.* p. 19. † *Watt Corr.* p. 21.

except the name. On this point M. Dumas, referring to the period when the Water Controversy arose, says, "At this epoch Macquer, Baumé, and many other chemists, had each contrived, in order to meet the new exigencies of the science, a phlogiston of such a kind as best suited himself. Lavoisier had no longer to deal with the phlogiston of Stahl, but with a crowd of entities of that name, which had no quality in common, unless that of being intangible by every known method."* To the same effect Lavoisier himself declares that "the chemists have made a vague principle of phlogiston which is not strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed; sometimes this principle is heavy, and sometimes it is not; sometimes it is free fire, and sometimes it is fire combined with the earthly element; sometimes it passes through the pores of vessels, and sometimes they are impenetrable to it. It explains at once causticity and non-causticity, transparency and opacity, colours and the absence of colours. It is a veritable Proteus which changes its form every moment."†

In short, every orthodox chemist of the 18th century, not favouring the Lavoisierian schism, considered it his duty to make confession of his belief in phlogiston; but what it was he believed in, he was by no means so particular in declaring.

From this charge neither Cavendish nor Watt can be exempted. Both avoided giving any definition of phlogiston, yet both imputed to it properties the very reverse of those which were ascribed to it by Stahl, who certainly would not have understood many of the references to it, contained in the later writings of the so-called disciples of his own school. Thus, vague though his description of phlogiston was, he defined it with a certain precision as a combustible principle, or essence of inflammability, present in all combustibles, which parted with it when they burned, and lost in consequence their combustibility. According to this view, the calx or oxide which is left as the residue of ordinary combustion, is the combustible *minus* the phlogiston, which it has given off. In consistence with this doctrine, Stahl, had he been aware of Cavendish's results, would have affirmed that hydrogen consisted of the water which appeared when the gas was burned, and of phlogiston, which during the combustion passed away. Cavendish and Watt, on the other hand, held that the water (a certain minimum hypothetically present in the inflammable air excepted) did not pre-exist in the gas, but had been *produced*, and that the phlogiston was all present in the water, and was essential to its existence. Their phlogiston, therefore, was the very opposite to that of Stahl.

Watt departed still further from the ancient faith, for he dispensed with phlogiston as a means of explaining combustion, accounting for it by the theory of an "elementary heat" present in all bodies, and given out when they burned. Thus although inflammable air was, according to him, phlogiston, it needed an addition of elementary heat, before the phenomena of combustion could be explained. Watt's phlogiston came in this way to contain Stahl's phlogiston, for the functions of the "elementary" or "latent heat," were exactly those which Stahl attributed to his phantom, and the original fire-essence was for a moment saved from its approaching extinction by Lavoisier, by being made the depositary of an inner quintessence of fire.‡

* *Leçons sur la Philosophie Chimique*, par M. Dumas, p. 161.

† Ibid. Ibid. p. 162.

‡ Watt's views were peculiar in another respect. The more ancient phlogistians held that phlogiston was a "principle of levity," and conferred positive lightness on

In this singular way, both Cavendish and Watt marred the force of their own conclusions, and whilst using language which was irreconcilable with a belief in phlogiston, kept using the word, as if they were the orthodox representatives and successors of Stahl. After all, however, the historian will not wonder much at a phenomenon which so constantly appears in the history of mankind. So marvellous is the fascination that names exert, and so deeply are we all imbued with a conservative spirit, that even the greatest reformers in politics, arts, literature, and science, are found tenaciously clinging to a word long after they have counted it their highest triumph to have swept away the reality which it represented. It was so with Cavendish, Watt, Priestley, Kirwan, Scheele, and in truth nearly all the chemists of the last century. They took the greatest liberties with the entity phlogiston, but with the name, no one before Lavoisier would meddle. Nay, they even strove to the very last to frame a nomenclature for the gases, and, as far as possible, for all bodies, which should include only references to the one charmed word, although they never were able to construct more than three derivatives from it, viz. *phlogisticated*, *dephlogisticated*, and *super-phlogisticated*.

A nomenclature so scanty and so barbarous, compelled its employers to deal in perplexing and often contradictory statements. Neither Cavendish nor Watt, for example, could find better terms in which to state his view of the composition of water, than that it consisted of dephlogisticated air, united to phlogiston; in other words, of dephlogisticated phlogisticated air. A *plus* B, *minus* B, is equal, it should seem, to A; but according to the literal interpretation of this statement, it is equal to A B? A substance, apparently, could not be both phlogisticated and dephlogisticated; possessed of, and deprived of phlogiston, at the same time; yet water, according to the formula of Cavendish and Watt, was in this predicament.

The defects of their nomenclature, however, were not the only sources of obscurity in the writings of these philosophers, when they alluded to phlogiston. We are too apt to represent them, as the extreme advocates on both sides have done, as regarding phlogiston in the same light as we do hydrogen, viz. as a special ponderable substance. This, however, was not exactly the view of either Cavendish or Watt. From a period even earlier than that of Stahl, an endeavour had been constantly making to identify the supposed common cause of inflammability, with some one combustible. In the writings of Beccher (Stahl's teacher), of Sir Isaac Newton, and Stephen Hales, there are many indications of a disposition to embody the principle of combustibility, (which with them was nameless), in sulphur.* After 1766, however, the date of Cavendish's exposition of the properties of hydrogen, and in consequence, apparently, of a suggestion of his† in reference to hydrogen, (although he did not after-

bodies. Watt, on the other hand, says, "It appears to me very probable, that fixed air contains a *greater* quantity of phlogiston than phlogisticated air does, because it has *a greater specific gravity*," &c. (*Phil. Trans.* 1784, p. 335); and again, "if we reason by analogy, the attraction of the particles of matter to one another in other cases is increased by phlogiston, and bodies are thereby rendered *specifically heavier*." (*Ibid.* p. 352.)

* The statement in the text must be taken with an important qualification. The founders of modern chemistry, especially Boyle, Hooke, Mayow, as well as Rey, understood the true nature of combustion much better than their immediate successors did. Stahl's interregnum was the dark middle-age of the science, which intervened between the imperfect announcement of a consistent theory of combustion by the early disciples of Bacon, and its revival, extension, and verification by Lavoisier. (*British Quarterly Review* (February, 1849, p. 239), and Brande's *History of Chemistry*, pp. 61—67.)

† Ante, p. 197.

wards take the credit of it,) inflammable air in the widest sense of the term, with its appropriate qualities of elasticity, levity, and combustibility, was fixed upon as the more probable embodiment of the subtle fire-essence or phlogiston. In spite, however, of its identification with inflammable air, phlogiston never entirely lost its phantom character. It assumed, indeed, new relations as a material entity; but it retained its old attributes as an ideal existence. It was, at once, an 'airy nothing' as phlogiston; and a ponderable something as inflammable air.

This double view of the nature of phlogiston, which was common to Cavendish and Watt, has not been recognised by the advocates of either. Much needless discussion, accordingly, has been carried on regarding the relative superiority of their views, in so far as those relate to the presence of water as an essential ingredient in phlogiston or inflammable air; and much difficulty has been experienced in accounting for either philosopher entertaining so singular an opinion. Before attempting to dispose of this difficulty, I have to remind the reader that it as much embarrasses Watt's exposition of his views, as it does that of Cavendish. The advocates of the former do not deny that he ultimately taught that one of the elements of water, contained water as one of *its* elements; but they claim for him that at least he originally taught that inflammable air without water is pure phlogiston, whereas Cavendish always held the less precise view, that inflammable air is a hydrate of phlogiston.* Mr. Muirhead has even gone the unwise length of affirming, that the Dean of Ely "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."† It is Mr. Muirhead, however, who is in error, not Dr. Peacock. I have already pointed out that it is a mistake to imagine that Watt's view differed from Cavendish's, in so far as the presence of water in inflammable air was concerned. If any preference, in truth, is to be given to the views of the one over those of the other, it must be assigned to the statement of Cavendish. Watt and he alike commenced by holding that anhydrous inflammable air is phlogiston, and ended by believing that the latter, when free, should rather be regarded as a compound of inflammable air and water. Cavendish's original opinion, however, goes back to 1766, and was founded on his own researches; whilst Watt's earlier view dates only from 1783, and was based upon the observations of Priestley and Kirwan. Cavendish, moreover, did not commit himself absolutely to either view, but only gave the one a preference. Thus after stating that "inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston," he adds in a note, "Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely."‡

In reality, then, Cavendish and Watt were at one in their final belief, as to the necessity of water to the existence of phlogiston "in the aërial form," whatever difference there might be in their views, in reference to the *kind* of inflammable air of which phlogiston was the hydrate. To this conclusion they appear to have come independently of each other, although this has been denied; and Cavendish induced Priestley to adopt the same view.

Thus much then premised, it remains to determine what were the motives which induced those observers to hold a view which only added

* *Edinburgh Review*, Jan. 1848, p. 103.

† *Watt Corr.* Introd. Rem. p. ciii.

‡ *Phil. Trans.* 1784, p. 137.

obscurity and contradiction to their enunciation of the nature of water. Nothing, we may be certain, but a strong conviction of its truth, or at least of its great probability, can have led such clear thinkers as Cavendish and Watt to a conclusion, the awkwardness of which was apparent to themselves. On this last point, Priestley, commenting on their views, writes thus,—"Inflammable air seems now to consist of water and inflammable air, which however seems extraordinary, as the two substances are hereby made to involve each other, one of the constituent parts of water being inflammable air, and one of the constituent parts of inflammable air being water; and, therefore, if the experiments would favour it (but I do not see that they do so,) it would be more natural to suppose that water, like fixed air, consists of phlogiston and dephlogisticated air, in some different mode of combination."*

The reason assigned by Cavendish himself, for preferring the view which represented phlogiston as a compound of hydrogen and water, has been referred to in the abstract of his "Experiments on Air" *(first series)*. He thought that if hydrogen were pure phlogiston, it would spontaneously combine with oxygen, whereas it required to be made red-hot before it united with it; unlike liver of sulphur and nitric oxide, the phlogiston (hypothetically present) in which needed no elevation of temperature to cause its union with oxygen. It thus seemed as if pure phlogiston, when free, phlogisticated (deoxidised) air with more difficulty than the phlogiston which was locked up or contained in a state of combination in nitric oxide and the alkaline sulphurets. Swayed by this consideration, Cavendish thought that, in all probability, there was something present in hydrogen besides phlogiston, which prevented the latter exhibiting the intense affinity for oxygen which it would have shown had it been free.†

Having come to this conclusion, Cavendish at once fixed upon water as the substance which prevented phlogiston from exhibiting its characteristic affinities, and this for a very significant reason, which seems, however, to have escaped the notice of the critics of the Water Controversy. When phlogiston (hydrogen) and oxygen were exploded together, they condensed

* *Priestley on Air*, vol. vi. p. 406.

† Kirwan criticised this opinion in the following terms: "Mr. Cavendish is inclined to think that pure inflammable air is not pure phlogiston, because it does not immediately unite with dephlogisticated air, when both airs are simply mixed with each other. This reason seems to me of no moment, because I see several other substances, that have the strongest affinity to each other, refuse to unite suddenly, or even at all, through the very same cause that dephlogisticated and inflammable airs refuse to unite, viz. on account of the specific fire which they contain, and must lose, before such union can take place." (*Phil. Trans.* 1784, p. 168.) This criticism, however just in the sense in which we now understand it, was no reply to Cavendish's statement, and had in truth no meaning in the mouth of a phlogistian, unless, like Watt, he supplemented phlogiston by Black's latent or elementary heat. To tell Cavendish, who disbelieved in the materiality and latency of heat, that phlogiston must part with specific fire (*i.e.* an entity identical in functions with phlogiston) before it would unite with oxygen, was only to say, that phlogiston must part with phlogiston, before it can exhibit the affinities of phlogiston. This suggestion, moreover, left unexplained why the (hypothetical) phlogiston of liver of sulphur, and nitric oxide, so readily gave off its specific fire, whilst the phlogiston in (or identical with) hydrogen retained its elementary heat so obstinately. It is not surprising, therefore, that Cavendish should have attached no weight to Kirwan's arguments.

Priestley gave the true explanation of the indifference of free hydrogen and oxygen to each other, at ordinary temperatures. His opinion is quoted by Watt in the following passage, which is remarkable as containing one of the earliest examples of the word *nascent* being used in that peculiar and ill-defined but expressive sense, in which it is into water. Had any non-aqueous body, however, been united with phlogiston in the hydrogen, it would have been found in the condensed water; but the water was quite pure, so that unless a portion of it had pre-existed, and been united to the phlogiston, the latter alone must have formed the inflammable air. That one or other of these alternatives must be accepted by every interpreter of his experiments, was the belief of Cavendish, as his own words, I think, unquestionably show: "inflammable air is either pure phlogiston, ... or else water united to phlogiston *since* . . . . . . *these two substances united together form pure water*," and for the reasons already given he preferred the latter alternative.

In this conclusion Watt practically concurred, probably in consequence of pursuing a similar train of reasoning. On this point, however, we have no information from himself or others.* In the earlier version of his "Thoughts" (April, 1783) he stated that Dr. Priestley had found by some experiments made lately, that inflammable air "is either wholly pure phlogiston, or at least that it contains no apparent mixture of any other matter."† In the later version (November, 1783) he adds, "In my opinion, however, it contains *a small quantity of water* and much elementary heat."‡

So far, then, all is certain enough, but something more is needed to account for the tenacity with which Cavendish and Watt, as well as Priestley, held by the belief that inflammable air contains water as an essential ingredient. Watt has left us in the dark as to the grounds of his faith in the doctrine; and the motive which Cavendish states to have induced him to adopt it, he refers to as the *principal*, not as the only, reason which weighed with him. His principal reason, moreover, was a mere hypothesis, which evaded rather than solved the difficulty. There were many other ways of accounting for the refusal of hydrogen to combine with oxygen at ordinary temperatures, besides the supposition that the former gas was united to water. This hypothesis, in truth, involved a second quite gratuitous assumption, viz. that water resisted the escape of phlogiston from it. Every theorist, moreover, on phlogiston, which was "vox et preterea nihil," felt at liberty to attribute new properties to it, whenever difficulties arose in the explanation of phenomena, in which it was supposed to take part. Cavendish, accordingly, had many hypotheses to choose among, and must have had some forcible reason for selecting that one which he preferred; and so must Watt, who was equally free to invest phlogiston with whatever properties the exigencies of his theory required.

The true explanation of their peculiar views may be found, I think, in that double view of the nature of phlogiston which has already been re-

still employed. Watt mentions that "a mixture of dephlogisticated and inflammable air, will remain for years in close vessels, in the common heat of the atmosphere, without suffering any change," . . . and then adds, "These facts the Doctor [Priestley] accounts for, by supposing that the two kinds of air, when formed at the same time and in the same vessel, can unite in their *nascent* state; but that, when fully formed, they are incapable of acting upon one another, unless they are first set in motion by external heat." (*Phil. Trans.* 1784, p. 334.)

* It is proper to notice, that Watt was as much perplexed as Cavendish, to account for the refusal of hydrogen and oxygen to unite at ordinary temperatures, and was led in consequence to peculiar views concerning phlogiston, which will be noticed a little further on. He does not include among these, however, his opinion that inflammable air contained water.

† *Phil. Trans.* 1784, p. 330. ‡ *Ibid. ibid.*

ferred to, as held by its later advocates. Cavendish, Priestley, Kirwan, and Watt, all spoke freely, and even confidently, of inflammable air as phlogiston; thus transforming the latter from the ethereal, imponderable entity which it was in the eyes of the older chemists, into a ponderable substantiality. It is evident, however, that to conceive it a substance possessing the ordinary properties of matter, was more than they could succeed in doing. To this I ascribe the addition to the gas, of water, which should embody the phlogiston and act as a *vehicle,* or medium by means of which that entity, in itself insusceptible of isolation, might be transferred from substance to substance. Cavendish and Watt demanded only "a little water," but could not dispense with that little. It might be an infinitesimal minimum, but that minimum must not be wanting, otherwise the subtle fire-essence would vanish into nothing!* The phlogistians, in truth, were accustomed to consider phlogiston as an ethereal entity, which might be transferred from one substance to another; but they as little expected to see it isolated, as the ancient believers in the transmigration of spirits expected to witness the disembodied soul in the course of its metempsychoses. Thus it happened, that no sooner had the chemists been led by certain appearances to believe that they had obtained the disembodied phlogiston, than their old prejudices set them to embody it again; and no one of them, so far as I have read, appears to have, with entire good faith, identified phlogiston with inflammable air. The latter, in reality, only furnished it with "a local habitation and a name."

Having thus disposed of what was common to Cavendish and Watt in their conception of the nature of phlogiston, I have to consider in what respect their views regarding it differed. I consider this, however, only in so far as it affects their exposition of the nature of water.

So far as Cavendish is concerned, I have little to add to what has been stated already in the abstracts of his paper on hydrogen (p. 198), and in that of his first "Experiments on Air" (p. 231), as well as in the 5th section of the Water Controversy (p. 282). The sum of the matter may be stated thus. In 1766 Cavendish taught that when iron, zinc, and tin are dissolved in diluted sulphuric, or muriatic acid, "their phlogiston flies off without having its nature changed by the acid, *and forms the inflammable air;*" so that for some fifteen years before he experimented on the union of hydrogen and oxygen, he regarded the former as phlogiston. When he undertook, in 1781, an inquiry into the nature of phlogistication, he employed hydrogen as an unexceptionable phlogisticating agent, and in entire consistence with the professed object of his research, which was not to discover the nature of water, or merely to ascertain the product of the combustion of hydrogen, but to determine the change which air underwent when it was vitiated (phlogisticated) by combustibles, he declared water (which he believed to be the invariable product of phlogistication) to contain phlogiston as an element. In reference to these views, what I seek to insist on most strongly is, that Cavendish held phlogiston to be a substance identical with his inflammable air from the metals (our hydrogen), and that he believed it to have but one oxide, namely, water. I cannot, however, unreservedly concur in Mr. Harcourt's statement that Cavendish's phlogiston "was hydrogen *and nothing else.*"† No chemist, in 1783, was in a condition to say whether the inflam-

* In the statement in the text, I assume, for argument's sake, that Priestley's experiments were unexceptionable, as Watt believed them to be, and that the former, as well as Cavendish, obtained pure water alone, as the result of his detonations.

† *Brit. Assoc. Rep.* 1839. Pres. Address, p. 28.

mable air from the metals was identical with other inflammable airs or not, and Cavendish and Lavoisier were careful to pronounce this an open question, contenting themselves with pointing out that their conclusions, *as conclusions,* had reference to one inflammable air only, and leaving it for time to determine whether other inflammable airs were identical with that one. If any were, their conclusions might be generalised to the extent of that identity. To have asserted more, they must have possessed the power of divination; for, as I have urged already, the distinction of the combustible gases from each other was the result, not the precursor, of the discovery of the composition of water.

It must, however, be added, that though Cavendish called no combustible gas but hydrogen phlogiston, when recounting his own experiments, nevertheless he generalised much too widely, and assumed hydrogen to be present in many bodies which do not contain it. He seems, in truth, to have been a more faithful disciple of Stahl than is generally supposed; and as the latter held that all combustibles contained phlogiston, so Cavendish taught that they all contained hydrogen. The disputants in the Water Controversy appear to have overlooked this curious fact, which the advocates of Cavendish might not wish to insist upon, and his detractors do not seem to have discovered; although in truth an allusion to it could not much have served their client, for Watt generalised still more unwisely than Cavendish did.

The views of the latter are stated too distinctly by himself, to leave it at all doubtful what they were. When he commenced his "Experiments on Air," he found the majority of chemists believing, that when combustibles are burned in air, and when the latter is deoxidised by such bodies as nitric oxide and liver of sulphur, or, as they phrased it, when air is phlogisticated, the *invariable* product is fixed air, or carbonic acid. He quickly proved that this was a mistake; but he erred in one sense as widely as those he corrected had done, for he affirmed that the invariable product of phlogistication is water. He was quite aware that most organic combustibles yield carbonic acid when burned, but he conceived this to have pre-existed in them, and excluded them on that account from his trials as not suitable for an *experimentum crucis.* All other phlogisticating (combustible or oxidable) bodies, including nitric oxide, liver of sulphur, and the metals, he believed to contain hydrogen, (as Stahl believed them to contain phlogiston,) and all of them he thought yielded water, as one of the products of what we should now call their oxidation. The passages in his paper which prove this, will be found specially referred to in the abstract of his "Experiments on Air." Two of the most pertinent may be noted here. After describing experiments which proved that neither carbonic, nitric, nor sulphuric acid was the constant product of the phlogistication of air, he adds, "Having now mentioned the unsuccessful attempts made to find out what becomes of the air lost by phlogistication, I proceed to some experiments which serve really to explain the matter." (Ante, p. 234.) The experiments thereafter recorded are those with hydrogen, which Cavendish plainly considered as explaining phlogistication *in every case.* In other words, the phlogistication of air always diminished its bulk, because it was always attended by the production of water. To this he constantly refers throughout his paper, and he explicitly announces it in his criticism of Lavoisier's views, in which he affirms that, "Adding dephlogisticated air to a body, comes to the same thing as depriving it of phlogiston, and adding water to it," which was equivalent to saying, that the universal result of oxidation is the separation of hydrogen from the body oxidised, and the addition of water to the oxide.

From another quarter, moreover, proof, if possible more decisive, can be obtained concerning Cavendish's opinions. Kirwan, it will be remembered, published "Remarks on Cavendish's Experiments on Air." In these the following passage occurs in reference to "phlogistic processes." "I selected, as least liable to objection, the calcination of metals, the decomposition of nitrous by mixture with respirable air, the phogistication of respirable air by the electric spark, and lastly, that effected by amalgamation. *In each of these instances*, Mr. Cavendish is of opinion that the diminution of respirable air is *owing to the production of water*, which, according to him, is formed by the union of the phlogiston disengaged in those processes with the dephlogisticated part of common air."* To Kirwan's remarks Cavendish replied, qualifying his reference to the electric spark, but finding no fault with the opinion concerning the production of water attributed to him.†

Cavendish cannot then be vindicated from the charge of having generalised too widely in his speculations on the nature of hydrogen and the production of water. But after this has been fully conceded, we must guard against depriving him of the great merit that truly belongs to him. And what was meritorious in his researches may be easily ascertained by inquiring how much of his views the progress of science has shown to have been false. Tried by this test, we shall find that Cavendish's errors lay in his hypotheses, and that time has only confirmed what he based upon his observations. We no longer believe that every oxidable body contains hydrogen, and yields water when it is oxidised, which Cavendish *imagined*, but never pretended to demonstrate, was the case. But we still hold that hydrogen unites with half its volume of oxygen, and that the resulting oxide is water. And we further concur with Cavendish in believing, that all bodies which contain hydrogen, yield water when oxidised. We differ from him, in truth, but in two particulars. We disbelieve that the simple combustibles contain hydrogen; and we think it unnecessary to assume that hydrogen contains water. This latter doctrine, however, was but an opinion liable to correction— not a settled conviction on Cavendish's part; neither can we, as Arago has justly urged, demonstrate that there is no water present in hydrogen.‡ The substitution, accordingly, by Cavendish, of phlogiston for hydrogen, as the title of the combustible element of water, did not in any way alter the signification of his statement; for he called hydrogen, phlogiston, from the earliest period of his acquaintance with the gas; and it was the only substance which he professed to have "turned" into water, by uniting it with oxygen.§

* *Phil. Trans.* 1784, p. 154.

† *Phil. Trans.* 1784, p. 176. Further illustration of the same fact will be found in a letter of Kirwan to Crell, referred to in a subsequent section, and in a statement by Priestley.

‡ Note by M. Arago to *Lord Brougham's Historical Note*, reprinted in *Sequel to Watt Corr.* p. 253.

§ The advocates of Watt build much upon the fact, that Cavendish accepted Watt's conclusion as of equal value with his own, although he drew attention to the fact, that Priestley's charcoal-gas differed from the inflammable air from the metals. This point will be discussed at length in another section. It may suffice, therefore, to notice here, that all that Cavendish did, was to accept Watt's conclusion as identical with his own, *provided Priestley's experiments, from which it was drawn, were trustworthy*. That they were accurate he doubted, and he afterwards demonstrated that they were not.

I have illustrated Cavendish's views the more fully, that they have been misapprehended by Berzelius. The great Swedish chemist was so excellent a critic of the labours of others, that I feel myself peculiarly liable to the charge of presumption in disputing the accuracy of the account he gives of the opinions of Cavendish (as well as of Watt) concerning the nature of water. The following extract from the *Lehrbuch* (1843) will show what interpretation he puts upon the language of the English chemists. He is discussing the relative merit of Cavendish, Watt, and Lavoisier, as alleged discoverers of the composition of water—a question which does not at present concern us; and in reference to the first of those philosophers he mentions, that he stated his views thus:—"Oxygen is water deprived of phlogiston; hydrogen is water supersaturated with the same hypothetical substance. By their mutual combination, water is obtained in its original condition. According to this explanation, water was still regarded as a simple substance, which constituted the ponderable matter in hydrogen and in oxygen."*

The view imputed in the above quotation to Cavendish, is founded upon a misconstruction of his language. When Cavendish called oxygen water deprived of, or minus phlogiston, or dephlogisticated water, he did not intend to teach that it was water in any but a negative sense. The phrases were equivalent to those we should employ at the present day, if we chose to say that oxygen is water deprived of, or minus hydrogen, or dehydrogenated water. Those terms refer to the derivation of oxygen from water, not to its identity with it, and were natural at a period when no substances but those which contained water were supposed capable of yielding oxygen. They seem to us at the present day awkward and circuitous, when we know that oxygen can be prepared from many sources, and seldom prepare it from water. They were, nevertheless, in an intelligible and significant sense, quite accurate; and though we are apt to forget it, we employ exactly equivalent phrases in our present nomenclature. The word *aldehyde*, for example, a contraction for *alcohol dehydrogenatus*, by which one of the acetyle compounds (C^4H^3O,HO) is known, is a term referring to source or derivation, exactly corresponding to the aqua dephlogisticata (dehydrogenata) of Cavendish. No one interprets aldehyde, *i. e.* dehydrogenated alcohol, as signifying alcohol in any other than a negative sense, namely, as alcohol which, by the loss of so much hydrogen, has become a body distinct in all its properties from the substance which yielded it. In like manner Cavendish's dephlogisticated or dehydrogenated water was, by its very definition, *not* water, but chemically a moiety of it; the half of it, not the whole; the substance which was left when the combustible ingredient of water was removed from it. The whole of Cavendish's references to oxygen are in keeping with this view. I do not know a single passage in his writings which even remotely hints, still less one which directly asserts that oxygen is, in a positive sense, water, or contains it as an essential constituent, and as its only ponderable ingredient. On the other hand, he contrasts dephlogisticated with phlogisticated air; and observes, that there is the utmost reason to think that they are distinct substances, and not differing in their degree of phlogistication.†

As for hydrogen, Cavendish, we have seen, thought it highly probable that it contained water; but not because it was impossible that this

* *Lehrbuch der Chemie* Von J. J. Berzelius. *Fünfte umgearbeitete Original-Auflage.* 1843. Erster Band, p. 370—2.

† *Phil. Trans.* 1784, p. 141.

gas could exist in an anhydrous condition, but because the indifference of hydrogen to oxygen at ordinary temperatures seemed to imply the presence of some substance in the former, which lessened the intensity of its affinity for oxygen, and this substance he conceived could only be water, since it was the sole residue of the combustion of hydrogen and oxygen. So far, however, was Cavendish from holding that the non-aqueous portion of hydrogen is imponderable, that in 1766, years before he thought it necessary to affirm that water is probably present in hydrogen, he determined its specific gravity; and whilst referring to it simply as phlogiston, ranked it among ponderable bodies.

In consistence with the views expressed in the quotation, Berzelius represents Lavoisier as the first who spoke of water as a compound. We have seen however already, that Cavendish uses such expressions as these :—" Almost the whole of the inflammable and dephlogisticated air is converted into pure water." " Water consists of dephlogisticated air *united* to phlogiston." " These two substances united together form pure water."* In these passages, especially the last two, we have the compound nature of water as consisting of two things—phlogiston and dephlogisticated air, explicitly announced. Berzelius thinks that we put an interpretation on these and similar passages at the present day, which was not intended by their author, and did not occur to any one till after Lavoisier had published his views. But if we consult Kirwan's "Remarks on Cavendish's Experiments on Air," which were read to the Royal Society before Lavoisier's papers reached this country, we shall find him declaring, that " Mr. Cavendish is of opinion that the diminution of respirable air is owing to the production of water, which according to him *is formed by the union of the phlogiston* disengaged in those processes, *with the dephlogisticated part* of common air."† To this representation of his views, Cavendish, as we have already seen, made no objection, although he replied to other statements of Kirwan's.

Blagden is still more precise, for he charges Lavoisier with having been most reluctant to believe that " water was dephlogisticated air united with phlogiston," when informed that such was the view entertained in England. His prepossessions led him to expect, that hydrogen and oxygen would produce an acid when burned together; and the statement that water was a compound of these gases was so unwelcome to him, that, according to Blagden, he insisted that the water was not "formed or produced out of the two kinds of air, but was already contained in, and united with the airs, and deposited in their combustion."‡ We have Blagden thus, in the name of Cavendish, representing Lavoisier as protesting against a doctrine almost identical with that which Berzelius represents Cavendish as holding; and Lavoisier receiving, and at first discrediting, instead of originating the doctrine of the compound nature of water.

It may also be noticed here, that Watt regarded Cavendish's views as identical with his own; and so also did De Luc, as will appear more fully in another section. It is certain, however, that whatever were the defects of Watt's views, he was most explicit in his declaration that water is a compound. " Air and water," says he, " are not simple elements." " I have found out . . . . . what water is made of." " The ingredients of water are pure air and phlogiston."§

* *Phil. Trans.* 1784, pp. 133 & 137. † *Phil. Trans.* 1784, p. 155.

‡ Blagden's Letter to Crell, *Watt Corr.* p. 72.

§ *Watt Corr.* pp. 24, 25.

Cavendish, again, accepted Watt's views as to a great extent identical with his own; the points on which they differed having no reference to the question of the elementary or compound nature of water. Blagden was of the same mind; so that Watt, De Luc, and Blagden, as well as Kirwan, testify to having understood Cavendish to teach that water is a compound; and he was aware that they imputed this doctrine to him, but found no fault with the imputation.

Further, Cavendish, Watt, and Blagden unhesitatingly accused Lavoisier of plagiarism from the two first,—a charge they never would have preferred, had the doctrine of the compound nature of water been a novelty to them. This charge has the more weight, that Cavendish and Blagden at least were alive to the peculiarity of Lavoisier's views in reference to hydrogen. "Lavoisier's present theory," says Blagden, "perfectly agrees with that of Mr. Cavendish; only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston."*

The considerations adduced above will suffice to show that Berzelius is mistaken in thinking that Cavendish was not understood by his contemporaries to teach that water is a compound body. Great as Lavoisier's merits are, it was not left to him to deny for the first time the elementary nature of water; or to teach, that two gases could be burned together into their joint weight of this liquid. To Lavoisier, however, I shall return.

I have now to consider what Watt's opinions concerning phlogiston were. That he held it, in its state of greatest isolation from matter, to consist of inflammable air, along with a little water and much elementary heat, and that inflammable air was a term applied by him to other gases besides hydrogen, has already been pointed out. My present object is to inquire whether, on a more full investigation of his views concerning phlogiston, it can be shown that he did not identify it with hydrogen, which Cavendish did. As the case now stands with Watt, the only way in which a claim can be established for him as a discoverer of the composition of water, is by showing that his conclusions had a primary and special reference to hydrogen. Could that be shown, it might with justice be argued that, in accepting Priestley's charcoal-gas as identical with the phlogiston, or combustible element of water, he erred only to the extent of believing that charcoal, when heated, evolves hydrogen. It is not a little curious, accordingly, that none of Watt's advocates should have attempted to demonstrate this, and so to make out a logically consistent case for him. I shall assume the possibility of such a view being substantiated, and avoid doing injustice to Watt, by inquiring if it can.

In pursuing this inquiry, the first point demanding attention is the important one that, for years before he published his "Thoughts on the Constituent Parts of Water," Watt had anticipated the probability of water being convertible into air. "For many years," says he, "I have entertained an opinion that air was a modification of water, which was originally founded on the facts that, in most cases wherein air was actually made, which should be distinguished from those wherein it is only extricated from substances containing it in their pores, or otherwise united to them in the state of air, the substances were such as were known to contain water as one of their constituent parts, yet no water was obtained in the processes, except what was known to be

* Letter to Crell, *Watt Corr.* p. 73.

only loosely connected with them, such as the water of the crystallisation of salts.* This opinion arose from a discovery that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased; or, in other words, that the latent heat of steam was less when it was produced under a greater pressure, or in a more dense state, and greater when it was produced under a less pressure, or in a less dense state; which led me to conclude that, when a very great degree of heat was necessary for the production of the steam, the latent heat would be wholly changed into sensible heat; and that, in such cases, the steam itself might suffer some remarkable change. I now abandon this opinion, in so far as relates to the change of water into air, as I think that may be accounted for on better principles."†

It thus appears that Watt conceived that, at a high temperature, steam would undergo a remarkable change, which, judging from the supposed fact that only bodies containing the elements of water evolve "air," he thought (as we learn from a reference of Priestley's)‡ would consist in the water-vapour becoming converted into a permanently elastic fluid. §

It is difficult to be quite certain in what sense Watt used the term "air" in the passages quoted. It is manifest, however, that he employed it either as synonymous with atmospheric air, or as identical with gas or elastic fluid, in the widest sense of these terms. From his referring in the next paragraph, in which his remarks are continued, to the evolution of dephlogisticated air from melted nitre, I am inclined to think that he used the word "air" in its widest sense. After all, however, it is quite possible that Watt himself would have been puzzled to reply, had he been asked in what sense he employed the ambiguous word. Atmospheric air, as I have pointed out, in the abstract of Cavendish's paper on the eudiometer, was not regarded as constant in composition, but might, *ex hypothesi*, vary in respirability through a very wide range, of which the extreme limits, in opposite directions, were oxygen and nitrogen. Anything, therefore, short of pure nitrogen, was *pro tanto* respirable air; and even nitrogen was frequently referred to, not as a special gas, but as the phlogisticated *part* of atmospheric air. It is of less importance, however, to decide the absolute signification of the term *air*, as used by Watt, than it is to show that his early hypothesis of the convertibility of water into air implied no precise anticipation of the nature of the air, or elastic fluid, which water should yield, and still less any expectation that it would consist of two unlike gases, one of which should prove to be inflammable. The opposite has been argued by Mr. James Watt, who, in qualification of a statement of Lord Brougham, adduces the passage on which I have been commenting, as prov-

* It need scarcely be noticed that Watt was in error in this notion, and that nitre, *ex. gr.* which he supposed to yield oxygen because it contains water, is an anhydrous salt. He as freely and as unwarrantably assumed the presence of water in bodies as Cavendish did.

† *Phil. Trans.* 1784, p. 335.

‡ *Phil. Trans.* 1783, pp. 415, 416. The passage is quoted at p. 330.

§ Watt abandons this opinion at the close of his remarks, quoted in the text. Nevertheless, we may justly regard Mr. Grove's beautiful discovery of the power of white-hot platina to decompose water, as the unexpected fulfilment of Watt's sagacious conjecture. (*Chemical Society's Memoirs*, 1847, p. 332.) He seems to have changed his opinion in consequence of the conversion of air (inflammable air and oxygen) into water, in Priestley's experiments, being attended with the change of latent into sensible heat, whereas, according to his view, this change should have occurred in exactly the opposite circumstances, namely, when the liquid was undergoing conversion into gas or gases.

ing that "the idea existed in his [Watt's] mind previously" to the repetition of Cavendish's experiments by Priestley.* Sir David Brewster also seems to a certain extent to sanction this view, as in his advocacy of Watt's claims he observes (without, however, naming him), "that to conjecture even the very improbable fact that water is formed of two different kinds of air, was a bold and an original idea."† Watt assuredly would deserve the highest praise if he had entertained so sagacious a thought; but neither he, nor any other of the claimants of the disputed discovery, predicted, or professed to have predicted, that water would prove a combination of two dissimilar gases. All of them, alike in France and England, reached the discovery as the *unexpected* result of an *à posteriori* investigation. Whilst, therefore, it would be doing Watt great injustice not to acknowledge that his beautiful researches into the relation of heat to steam led him to watch for any indications of its becoming a permanent gas, with an interest and a keenness shared by none of his contemporaries, and prepared him to turn to the best account anything bearing on the convertibility of water into air,—it would be doing others equal injustice to affirm that, before Priestley repeated Cavendish's experiments, Watt had done more than anticipated that water might be transformed into gas, without having come to any conclusion as to the probable chemical composition of the elastic fluid which should be produced.

For several years, as the last quotation from his writings shows, Watt entertained this idea, but he did not endeavour to realise it by experiment. Priestley, however, who entertained a somewhat similar, though less precise notion, made it an object of investigation. "I imagined," says he, "that when substances consisting of parts so volatile as to fly off before they had attained any considerable degree of heat in the usual pressure of the atmosphere were compelled to bear great heats under a greater pressure, they might assume new forms, and undergo remarkable changes; . . . . . but I had no particular expectation concerning the nature of that change. . . . . . I was mentioning these ideas to Mr. Watt, in whose neighbourhood I have the happiness to be situated, when he mentioned a similar idea of his, viz. that of the possibility of the conversion of water, or steam, into permanent air; saying that some appearances in the working of his fire-engine had led him to expect this. He thought that if steam could be made red-hot, so that all its latent heat should be converted into sensible heat, either this or some other change would probably take place in its constitution. The idea was new to me, and led me to attend more particularly to my former projects of a similar nature," &c. &c.‡ Encouraged in this way by the similarity of his views and those of Watt, Priestley instituted those experiments with porous clay retorts already frequently referred to, in which water was apparently, by simple distillation, converted into air. This air was sometimes a little purer, sometimes less pure, than atmospheric air, but always respirable. In the detail, for example, of one experiment which did not materially differ from the rest, Priestley says: "This air was never much less pure than that of the atmosphere. Sometimes it could not be distinguished at all from it at all [sic in orig.] by the test of nitrous air, and once or twice I thought it even purer than that of the atmosphere."§ In like manner, he observes that "another pre-

* Note by Mr. James Watt to Lord Brougham's *Historical Note*, appended to Arago's *Eloge* of Watt, reprinted in *Watt Corr.* p. 257.

† *North Brit. Rev.* Feb. 1847, p. 474.

‡ *Phil. Trans.* 1783, pp. 415, 416.

§ *Op. cit.* p. 423.

sumption in favour of the generation of our atmosphere from water was, that the purity of the air that I produced from it is so very nearly the same with that of the atmosphere."* These results were speedily communicated to Watt, who welcomed them as the probable verification of his hypothesis. On the 13th December, 1782, he writes to De Luc:—"Dr. Priestley has made a most surprising discovery, which seems to confirm my theory of water undergoing some very remarkable change at the point where all its latent heat would be changed into sensible heat." He then describes one of Priestley's distillations, in which lime and water were heated in an earthen retort; and adds—"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."† On the 26th of the same month, Priestley writes to Watt: "I now convert water into air without combining it with lime or anything else. . . . . The air is of the purity of that of the atmosphere, and, I think, without any mixture of fixed air."‡ The final conclusion, then, of Priestley, before he discovered the fallacious nature of his experiments, was, that water is convertible into *atmospheric air*. In this Watt acquiesced; for when Priestley told him that he had unexpectedly discovered that respirable air was only obtained when the porous retorts were surrounded by pure atmospheric air, Watt replied conjecturally: "If, after all, this should account for the production of *common air* from water," &c.;§ implying that the experiments had formerly seemed to demonstrate such a change. When Priestley's experiments appeared unexceptionable, Watt made the statement still more deliberately. In the letter to De Luc, already quoted, he says: "I now believe air is generated from water. . . . . If this process contains no deception, here is an effectual account of many phenomena, and one element dismissed from the list."‖ In other words, water and (atmospheric) air are not distinct elements, but two forms of one substance, and the liquid can, by change of temperature, be converted into the gas. It was free to one who held such a view, to consider neither of the two bodies, which were mutually convertible, as more elementary than the other, or to prefer one of them as the radical or base of the other. Watt preferred the latter view, and held that air was "generated from water," which was thus more elementary than air. Watt would not have argued thus at a later period, but as yet (December, 1782) he had no evidence that air of any kind was convertible into water, nor does he make any reference to Priestley's explosions of inflammable air and oxygen till some three months (March, 1783) after he had declared his belief that air is generated from water.

I have dwelt at length upon this apparently secondary point, because it is of great importance to notice, that whereas Cavendish reached his conclusions concerning the nature of water, *solely* from observations on the synthesis of oxygen and hydrogen, Watt's earliest opinion concerning the nature of that liquid, was founded neither upon synthetical nor analytical researches into the quality of its constituents, but upon observations in which it seemed to undergo direct transmutation (so far at least as its ponderable matter was concerned) into atmospheric air. This erroneous notion, based upon Priestley's delusive experiments, was not entirely abandoned by Watt, even when he published the latest version of his "Thoughts on the Constituent Parts of Water." It confused his specu-

* *Phil. Trans.* 1783, p. 428. † *Watt Corr.* p. 4. ‡ *Op. cit.* p. 8.
§ *Watt Corr.* p. 27. ‖ *Watt Corr.* p. 4.

lations concerning the nature of water, and seems to have been the source of his belief that phlogisticated air or nitrogen consisted of the same ingredients as water.

Meanwhile Priestley proceeded with his researches, and led Watt astray in another particular. The experiments I have been considering were performed in December, 1782. On 26th March of the same year the latter writes to G. Hamilton—"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." Thereafter Watt refers for the *first* time to the production of water by the direct explosion of the gases which he had already stated produced fixed air.* In this way, and at as early a period as that at which he came to the conclusion that water consisted of inflammable air and oxygen, he inferred that carbonic acid was identical with water, as it was also with nitrogen, in qualitative composition. Nor was Priestley's repetition of Cavendish's experiments undertaken as a special inquiry into the product of the combustion of inflammable air and oxygen, but as an incidental justification of his own theory that water is convertible into air. "Still hearing," says Priestley (in a passage already quoted in full), "of many objections to the conversion of water into air, I now give particular attention to an experiment of Mr. Cavendish's *concerning the reconversion of air into water*, by decomposing it in conjunction with inflammable air."†

It is of importance to notice the mode in which Priestley thus refers to his experiments with inflammable air. The title of Watt's paper, which was not added till a year after the paper was written, and its general tenor in its latest version, naturally convey the impression that the main object of Priestley's researches and Watt's conclusions was the demonstration of the power of inflammable air and oxygen to produce water. Whereas Priestley's paper of 1783, studied as a whole, and the several versions of Watt's Thoughts, as well as the *Watt Correspondence*, show that the primary object of both observers was the demonstration of the transmutability of water into atmospheric air; whilst the interest which Cavendish's experiments had for Priestley, lay almost entirely in the fact that they assisted him in establishing the general proposition that a gas may become a liquid, and therefore a liquid a gas, and increased the probability of his particular assertion that water may be transformed into common air. To Watt again, Cavendish's experiments, as they reached him through Priestley's repetition of them, were objects of independent interest : but he did not originally think the conclusion they warranted of more importance than the one he had agreed with Priestley in drawing from the latter's experiments with porous earthen retorts; and the breaking down of the proof that water might be converted into common air, led him to withhold his *whole* paper, and decline to have it publicly read, although the evidence in favour of the convertibility of a mixture of inflammable air and oxygen into water, remained unaffected by the detection of a fallacy in the experiments made with the earthen retorts.

Discoverers are fond of insisting on their discoveries being studied by others in the order in which they were made, and love to record even the irrelevant matters which interested themselves during their researches. Watt was not superior to this foible, but has been at pains to inform us

* *Watt Corr.* p. 17. † On Phlogiston, &c. *Phil. Trans.* 1783, p. 398.

in the commencement of the latest version of his "Thoughts," "I *first* thought of *this* way of solving the phenomena in endeavouring to account for an experiment of Dr. Priestley's, wherein water appeared to be converted into air;"* so that he continued to the last to hanker after giving the precedence to those experiments which agreed most with his own *à priori* hypothesis. When this is considered, and all that has been stated already, it seems impossible to accept as tenable the assertion that Watt throughout his paper specially referred to hydrogen when he used the word phlogiston. It would be more easy to prove the very converse of this for the following reasons :—(1) Watt's views regarding the nature of water commenced with a speculation which did not even remotely contemplate the resolution of water into oxygen and *an* inflammable air, much less into oxygen, and the inflammable air, hydrogen. (2) He accepted Priestley's porous retort experiments, in which water appeared to undergo conversion into common air, as the full realisation of his hypothesis, and he gave the following rationale of the process, which first appeared in Priestley's paper. "Since pure external air was necessary in order to procure good air, it was concluded by several of my friends, and especially Mr. Watt, that the operation of the earthen retort was to transmit phlogiston from the water contained in the clay to the external air; and that the water thus dephlogisticated was capable of being converted in respirable [*not dephlogisticated*] air by the intimate union of the principle of heat."† What became of the phlogiston lost by the water appears more clearly from Watt's own account.

"On considering the last and most remarkable production of air from water imbibed by porous earthen vessels, (the only case wherein it appears almost incontrovertibly that nothing was concerned in the production except water and heat,) I think that the earth of the vessel attracts the phlogiston from the water, and gradually conveys it from particle to particle, until it transmits it to the external air, which it probably phlogisticates; and that, therefore, the same substances moistened with water, and heated in glass or metalline vessels, can produce only limited quantities of air, because the earth comes to be saturated with phlogiston, which it cannot transmit to the external air, and consequently will decompose no more of the water than it can retain the phlogiston of, united to itself. . . . . I omitted to mention in its proper place that clay when made hot has a very powerful attraction for phlogiston, and in some circumstances becomes quite black with it, but readily parts with it to pure air, and becomes white again."‡

On these views of Watt, Mr. Harcourt comments as follows :—"Here inflammable gas or hydrogen is obviously out of the question; the *phlogiston* of the water, which passing through the retort, is presumed to phlogisticate and vitiate the external air, is *nitrogen;* and the dephlogisticated air of the water is supposed *to retain sufficient phlogiston* to make, with the assistance of heat, *good air, of the same purity as the atmosphere.*"§

In the first part of this criticism I cannot concur, for it would represent Watt as considering phlogiston and phlogisticated air (nitrogen) as the same

* *Phil. Trans.* 1784, p. 329. † *Phil. Trans.* 1783, p. 431.

‡ The quotations in the text are from the unpublished part of Watt's letter to Priestley (April 26, 1783) belonging to the Royal Society, and already referred to, p. 292. The first and last passages have been published by Mr. Harcourt. (*Brit. Assoc. Rep.* 1839, p. 24.) The concluding sentence forms a postscript to the letter.

§ *Brit. Assoc. Rep.* 1839, p. 25.

thing, whereas he has himself told us that he regarded nitrogen as "a composition of phlogiston and dephlogisticated air." His view plainly was, that the phlogiston passed through the retort as phlogiston till it reached the external air, when it combined with oxygen and converted it into nitrogen.

The remainder of Mr. Harcourt's criticism is quite to the point. No property of hydrogen is imputed by Watt to phlogiston, but rather those of carbon, which Watt believed to be transmutable into that entity; for the whitening of black clay, which is so familiar a phenomenon in the potter's kiln, results from the oxidation of the carbon of vegetable matter, which a lower temperature renders black by charring. If, then, Watt's ideal phlogiston is to be identified with one ponderable substance rather than another, it must be with carbon rather than with hydrogen, so far as we have yet proceeded. On this, however, I am not anxious to dwell. It is enough if Watt can be shown not to have signified hydrogen by phlogiston, and that he did not, seems most manifest from this, that according to his view the walls of the clay retort deprived the water of *part* of its phlogiston, which transuded through the vessel, and passed off from its outer surface into the air. Yet though the retort was surrounded by burning fuel, and the escaping phlogiston had the little water and the much heat necessary to convert it into inflammable air, it did not appear as a combustible gas, neither did it burn nor produce water, but it phlogisticated the air, *i. e.* produced nitrogen.

(3) Watt believed that "dephlogisticated air can unite in certain degrees with phlogiston without being changed into water."* One of the products which it then yielded was carbonic acid, as Watt inferred from Priestley's delusive experiment with iron filings and red oxide of mercury, already described.† (4) According to Watt another oxide of phlogiston, as we should now call it, was nitrogen. "Phlogisticated air," says he, "seems also to be another composition of phlogiston and dephlogisticated air; but in what proportions they are united, or by what means, is still unknown. It appears to me to be very probable that fixed air contains a greater quantity of phlogiston than phlogisticated air does, because it has a greater specific gravity, and because it has more affinity with water."

Such then were the contradictory views of Watt, whose caution and clearness as a thinker were less than a match for Priestley's extraordinary blunders as an experimenter. Cavendish, who founded only on his own admirable experiments, avoided the errors into which Watt fell, and differed from him in opinion concerning nearly all the gases. The former regarded carbonic acid and nitrogen as peculiar bodies. The composition of the first he did not know, but he seems to have considered it as a simple body, as he was justified in doing before it was analysed. Nitrogen he believed to be a compound of nitrous (nitric) acid and phlogiston, in perfect consistence with the tenets of the phlogistians, and in conformity with all the phenomena which were witnessed, so long as the balance was not employed. Phlogiston, or the inflammable air from the metals, he represented as a substance whose only oxide was water. According to Watt on the other hand, phlogiston had four oxides, viz. atmospheric air, nitrogen, carbonic acid, and water. The two latter contained the greater amount of phlogiston, water the most, and atmospheric air the least. In the language of our modern chemistry, therefore, air would be the peroxide of phlogiston, and water the suboxide, whilst car-

* *Phil. Trans.* 1784, p. 334. † *Op. et loc. cit.* Ante, p. 301. ‡ *Ibid.* p. 335.

bonic acid might be the protoxide, and nitrogen the deutoxide. Any one of these substances, therefore, might be changed into the other, by the loss or gain of oxygen or phlogiston; and carbonic acid, nitrogen, and atmospheric air only required the union of so much phlogiston to convert each into water. Water, therefore, might with as much propriety be regarded as a compound of phlogiston and carbonic acid; or of phlogiston and nitrogen; or of phlogiston and atmospheric air; as of phlogiston and dephlogisticated air.

These erroneous opinions were not offshoots from an originally just conclusion, that water was the oxide of hydrogen. Some of them preceded the view that water is an oxide of inflammable air: none were derived from it, nor did it abolish faith in them, although the notion that atmospheric air is an oxide of phlogiston was placed in abeyance.

It cannot be held, then, that Watt's wider speculations concerning phlogiston, remove the difficulty that attends the interpretation of that term as used in his conclusion from Priestley's explosion experiments. If phlogiston could not be held to have specially signified hydrogen, when applying to experiments made with another gas, still less can it be limited to that substance, when connected with such speculations as I have considered.

I come, therefore, to the conclusion, that when Watt, in stating his views on the nature of water, substituted the word phlogiston for inflammable air, he put it beyond question, that he signified by neither phrase the gas hydrogen.

9. *Experiments and Conclusions of Lavoisier concerning the production of Water from its Elements.*

In conformity with the method followed in discussing the opinions of Cavendish and Watt, I shall avoid at present, as much as possible, all reference to the claim to priority of discovery, contested between the French and English chemists, and limit myself in this section to a consideration of the nature of Lavoisier's observations on the synthesis of hydrogen and oxygen. To him, in association with La Place, belongs the entire merit of first consciously *analysing* water; nor has any one claimed this great discovery from him. It forms no part, however, of the present inquiry, to discuss the particulars of Lavoisier's famous analysis, although in another section, devoted to the consideration of the relative merits of the claimants in the Water Controversy, I shall endeavour to do justice to the genius and labours of the great French chemist. At present, I confine myself solely to his synthetical researches, which in their general nature resemble those of Cavendish.

Lavoisier's views are contained in two papers, the exact titles of which have been given in the bibliographical section, (ante, p. 268). The one is solely by him, in the other he had the co-operation of Meusnier. Both were printed in 1784, but their contents, or at least those of Lavoisier's own paper, were known in part in England, before their publication in the Mémoires de l'Académie des Sciences, and were referred to by Cavendish and Watt, in the final versions of their views concerning the nature of water.

The paper of which Lavoisier was sole author, is of most importance to the present inquiry. It is entitled "Mémoire dans lequel on a pour objet de prouver que l'eau n'est point une substance simple," &c. It was read, as its author informs us, "à la Rentrée publique de la Saint Martin, 1783; *depuis on y a fait quelques additions relatives au travail fait en commun avec M. Meusnier,* sur le même objet. Il auroit dû se trouver placé avant celui lu par M. Meusnier, à la Séance publique de Pâques, 1734. Voyez, p. 269.*

The paper here referred to, is that entitled "Mémoire où l'on prouve, par la decomposition de l'eau, que ce fluide n'est point une substance simple, &c., par MM. Meusnier et Lavoisier. Lu le 21 Avril, 1784."† In this communication, which is chiefly occupied with the analysis of water, Lavoisier refers thus to his memoir on the synthesis of hydrogen and oxygen, "Ce mémoire se trouve dans ce même volume. C'est par erreur, qu'il a été imprimé postérieurement à celui ci."‡

It thus appears that Lavoisier was anxious to have his experiments on the synthesis of hydrogen and oxygen, considered as anterior in time to those on the analysis of water. As Mr. Muirhead, however, justly observes, "although M. Lavoisier's paper was in part read before that by him and M. Meusnier, yet much of it contains express allusions to that other, and was therefore written later in order of time, and we have in the Mémoires, as printed, no means of determining precisely the extent of the additions."§

The reader, therefore, must bear in mind, that though the question of priority between the French and English chemists, is not under consideration in this section, Lavoisier himself acknowledged having altered and added to his earlier paper after it was read, so that his statements as they now appear in the Mémoires, must not be regarded as written without a certain acquaintance with Cavendish and Watt's views. What the extent of that acquaintance was, is one of the problems in the Water Controversy, but Lavoisier did not himself deny that he had obtained some information from Blagden concerning Cavendish's researches to which he refers in this paper. And without desiring in any way to prejudge the question of Lavoisier's originality or fair dealing, I must draw attention to the fact, that he acknowledges that his priority had been called in question, and though he does not say who had claimed precedence over him, he refers pointedly in the course of his narrative to Cavendish, and to him alone, as having through Blagden asserted, that he had at least observed the production of water by the combustion of hydrogen and oxygen before Lavoisier did, so that it cannot be doubted that Cavendish was the party he was most anxious to show had not anticipated him. In order, accordingly, to vindicate his priority, he gives a brief historical account of the researches which conducted him to his experiments on the synthesis of hydrogen and oxygen, the contents of which are important in reference to the merits of all the claimants in the Water Controversy. I give a brief abstract of his account, accordingly.‖ From this account it appears, that before 1777 Lavoisier was of opinion that inflammable air, in burning, would yield sulphuric or sulphurous acid. M. Bucquet, on the other hand, thought that fixed air would be the product of this combustion. To de-

* *Watt Corr.* p. 171. All my quotations of these French papers are, taken as stated already, from Mr. Muirhead's reprints appended to the *Watt Correspondence.* I borrow from him also the paging of the *Memoirs,* for the sake of those to whom the originals are more accessible than the reprints. Thus, the quotation in the text is from *Watt Corr.* p. 171; *Mém. de l'Acad. pour* 1781, p. 468.

† *Watt Corr.* p. 151; or *Mém. de l'Acad. pour* 1781, p. 269.

‡ *Watt Corr.* p. 152; or *Mém. de l'Acad. pour* 1781, p. 269.

§ *Watt Corr.* p. 152.

‖ *Watt Corr.* p. 173; or *Mém. de l'Acad. pour* 1781, p. 472.

termine this point, Lavoisier and Bucquet set fire to hydrogen, and whilst it was burning at the mouth of a large bottle, poured lime-water through the flame into the vessel, but without obtaining any precipitation of the lime, or evidence of the production of fixed air. This experiment disproved Bucquet's view, but did not establish Lavoisier's. The latter, accordingly, in the winter of 1781–1782, made another ingenious experiment of the same kind. A large bottleful of hydrogen was kindled, whilst lime-water was being poured in, and the mouth of the bottle was immediately thereafter closed by a cork, through which passed a copper tube, drawn to a small aperture, and conveying oxygen from a gasholder. When the cork was introduced, the surface of the inflammable air ceased to burn, but at the end of the copper tube within the bottle, a beautiful jet of very brilliant flame appeared, "and we saw," says Lavoisier, "with much pleasure, the vital air [oxygen] burn in the inflammable air [hydrogen], in the same manner, and in the same circumstances, as inflammable air burns in vital air."* During this combustion the bottle was constantly agitated, but no change occurred in the transparency of the lime-water, and when the experiment was repeated with pure water instead of lime-water, no acid appeared, nor was a weak solution of an alkali neutralised when substituted for those liquids.†

These negative results, Lavoisier tells us, surprised him the more, that he had already observed that in every combustion an acid was formed. Sulphur when burned, yielded sulphuric acid; phosphorus, phosphoric acid; charcoal, fixed air; and analogy had led him to conclude "that the combustion of inflammable air should equally produce an acid."‡ He returned to the inquiry in 1783, but before he recommenced his experiments on 24th June of that year, Blagden made the communication to him concerning Cavendish's having obtained water by the combustion of hydrogen in close vessels, which Lavoisier acknowledges in the following terms: "M. Blagden . . . . . . nous apprit que M. Cavendish avoit déjà essayé, à Londres, de brûler de l'air inflammable dans des vaisseaux fermés, et qu'il avoit obtenu une quantité d'eau très sensible."§

The apparatus which Lavoisier employed in June, 1783, was nearly identical with that made use of at the present day for the oxyhydrogen blowpipe. Separate gasholders were employed to contain the oxygen and hydrogen, which were conducted by flexible tubes of leather to a jet shaped like the letter Y. The stalk of this drawn to a point, formed the nozzle at which the mixed gases burned, whilst the two limbs in which the tubes from the gas-holders terminated, were furnished with stopcocks by means of which the flow of each gas could be regulated. In adjusting the relative proportion of oxygen and hydrogen, Lavoisier did not attempt to measure them out by any numerical scale, but proceeded, as he says himself, "par voie de tâtonnement," and guided himself as to the extent to which either stopcock should be opened, by the colour and brightness of the flame of the mixed gases, which appeared at the end of the nozzle. "La juste proportion," he tells us, "des deux airs donnoit la

* *Watt Corr.* p. 175; or *Mém. de l'Acad. pour* 1781, p. 471.

† The beautiful experiment recorded in the text, in which oxygen is made to burn in hydrogen, has generally, I believe, been attributed to other and later observers than Lavoisier, as its first performers; but the merit belongs entirely to him, and the whole account shows how broad was the view which he took of combustion as a phenomenon, in which each of the opposite bodies essential to its occurrence is equally concerned, and may with equal propriety be termed the combustible, or the supporter of combustion.

‡ *Watt Corr.* p. 175; or *Mém. de l'Acad. pour* 1781, p. 471.

§ *Watt Corr.* p. 176; *Mém. de l'Acad. pour* 1781, p. 472.

flamme la plus lumineuse, et la plus belle." This point having been settled, and the jet set fire to, the nozzle was inserted, so as to fit air-tight, into the upper tubulure of a glass bell-jar standing over mercury, and the gases were allowed to burn till the supply was exhausted. The first phenomenon observed was the clouding of the bell-jar by vapour; speedily drops of liquid appeared and ran down the sides of the vessel, so that in fifteen or twenty minutes the surface of the mercury was covered by a lighter fluid. To obtain this, a plate was passed under the bell-jar, without permitting the mercury in it to escape, and its contents were transferred to a glass funnel. The mercury was then allowed to run off, and the water "remained in the tube of the funnel. It weighed a little less than five drachms."*

The liquid thus obtained, "cette eau," was submitted to "every test that could be thought of," but appeared as pure as distilled water. How many tests were tried does not appear. Lavoisier mentions only three. The water did not redden turnsol, nor render syrup of violets green, nor precipitate lime-water. "In short," he adds, "none of the known reagents gave the least indication of impurity." He does not say, however, that he employed any other reagents than those mentioned above.

The conclusion which these results warranted is then stated. Lavoisier begins by acknowledging that as the flexible (leather) tubes which conveyed the gases were not absolutely air-tight, it was impossible to be certain what was the exact quantity of the two gases burned; but as the whole is equal to its parts, and as pure water alone was produced, he thought himself entitled to conclude that the weight of water was equal to that of the two gases which had served to produce it.† To this conclusion, however, he was plainly not entitled, unless he could show by other experiments (which he could not) that oxygen and hydrogen were certainly simple substances, not admitting of decomposition. In the absence of certainty on this point, it was manifestly quite possible, that one or both gases might be compounds, which when they seemed merely to unite and produce water were decomposed, so that certain only of their constituent elements were contained in that liquid. In all Lavoisier's experiments, as appears in the continuation of his paper, the weight of water was less than that of the gases burned. Till, however, this deficiency in weight could be shown to result from the unavoidable imperfection of the process, it could not fail to suggest the possibility of some ingredient of one or other, or of both the gases having escaped, the absence of which necessitated that the water should differ from the unburned gases, both in weight and in composition.

Lavoisier was alive to the necessity of establishing identity of weight between the gases burned and the water produced; and, by his own showing, he should have suspended his judgment till he had repeated his experiment several times. He was exceedingly desirous, however, to carry back his conclusions to the earliest possible date, and hence, apparently, his anxiety to show that the single, imperfect, and insufficient experiment on which I have been commenting, entitled him to the

* Lavoisier appears to have shown less than his usual ingenuity in this process. It would have been much better to have dispensed with mercury, and burned the mixed gases within a dry glass vessel kept very cool. At the end of the combustion, this vessel could have been closed and weighed, and the whole product of water ascertained. In the process actually followed, a sensible quantity of water must have been left adhering to the bell-jar, the plate, the mercury, and the funnel.

† *Watt Corr.* p. 177; or *Mém. de l'Acad. pour* 1781, p. 473.

inference which he drew. The arguments, however, by which he seeks to justify this, are of no value whatever. The axiom that the whole is equal to its parts, did not necessitate that these should be oxygen and hydrogen; and as little did the purity of the water, so long as the whole weight of gases expended was not accounted for. The only objection, nevertheless, which Lavoisier acknowledges, is the possibility of the heat and light evolved during the combustion being ponderable. That they were not, he had ascertained, as he tells us, by an independent inquiry into their ponderability, an unpublished memoir on which had for some months been deposited with the Secretary of the Academy.* The certainty, however, thus gained, that the deficiency in weight of the water, as compared with that of the gases which yielded it, could not be accounted for by a reference to the escape of ponderable matter in the form of heat and light, rendered it all the more necessary to explain what had become of the lost gas.†

It must further be noticed, that Lavoisier's method of experimenting was less accurate than Cavendish's, in many respects.

1. It does not appear that his gas-holders were graduated, so that he could ascertain how much gas he expended, and a knowledge of the capacity of his gas-holders, when full, was not sufficient to determine this, for he was compelled by his plan of procedure to keep the gases burning for some time before he could begin to collect the product of their combustion.

2. He adjusted the proportion of the gases by the imperfect test of the colour and brilliancy of their flame when burning together, a criterion too uncertain to be relied upon as a proof that neither gas was in excess, or escaping, to that extent, combustion.

3. As already noticed, he cannot have collected all the water produced, but only the surplus which did not adhere to the bell-jar, the mercury, and the plate, and perhaps also to the neck of the funnel.

4. He tested the water imperfectly. The employment of lime-water was superfluous, for he had ascertained previously that when hydrogen and oxygen burned together their product did not affect that reagent. All, accordingly, that he observed, was that the water did not contain a free acid or a free alkali. It might, however, have contained one or more neutral salts, and fixed organic matter. That it did not, in his experiments, Cavendish established by proving that it left no sensible residue when evaporated to dryness, besides trying Lavoisier's three tests, and in addition showing that the water was tasteless‡ and odourless. I refer to these points because Cavendish has been accused of imperfectly analysing the water produced in his experiments, but in reality his analysis was much more complete than those of his rivals.

* *Watt Corr.* p. 178; *Mém. de l'Acad. pour* 1781, p. 473.

† The recognition by Lavoisier of the necessity of establishing that heat and light are imponderable, before it could be inferred that water is the oxide of hydrogen, furnishes the best reply to those critics who have affirmed that Cavendish's determinations, before and after each explosion, of the weight of the globe in which he detonated hydrogen and oxygen, and his demonstration that the weight did not alter, proved nothing of importance to the inquiry, except that his globe was air-tight. Lavoisier would not have said so, for he instituted a separate inquiry to determine this point, which Cavendish settled without special research, by the same experiments which furnished the basis of his other conclusions concerning the composition of water.

‡ Lavoisier, however, although he does not mention it himself, appears to have been aware that the water was tasteless, for his colleague, La Place, states that it had this character. (La Place to De Luc, *Watt Corr.* p. 41.)

It is impossible, indeed, to read Lavoisier's account of his experiments without surprise. He was in general so sagacious, so cautious, so ingenious, so accurate and painstaking, that one is startled to find him summarily coming to a conclusion, after trying a single imperfect and insufficient experiment. He laid it down himself as a maxim, "C'est au reste la multitude des faits, bien plutôt que le raisonnement, qui doit établir toute espèce de théorie nouvelle;" and he implicitly obeyed it in his elaborate researches into the methods of analysing water, in his paper on which he announces this maxim. It is not a little strange, therefore, that he should have forgotten it, in his not less important observations on the synthesis of the elements of water. Nevertheless, although by his own showing, not entitled to give any better account of his experiments than he gave of Cavendish's, namely, that "he had tried to burn inflammable air in close vessels, and had obtained a very appreciable quantity of water:" he reported his solitary trial of the 24th June (1783), to the Academy on the 25th, in these terms:—"We did not hesitate to conclude that water is not a simple substance, and that it is composed weight for weight of inflammable and vital air."* Why Lavoisier should thus have hastened to publish a conclusion, which by his own confession was based, in one important particular, upon assumed or hypothetical premises, will be considered when discussing the question of priority. He repeated the experiment along with Meusnier many times before he finally and formally published it.† In the record of these repetitions, he states that the proportion by volume in which the elements of water combine as gases, is 12 parts of oxygen to 22·924345 (or in round numbers 23) of hydrogen. He mentions however, that there is "quelque incertitude sur l'exactitude de cette proportion;"‡ and I need not say that they are not accurate. 12 volumes of oxygen combine exactly with 24 of hydrogen. Lavoisier's numbers approximate less closely to accuracy than those (?) given by Cavendish. Priestley also, as reported by Watt, had anticipated Gay-Lussac and Humboldt in discovering that the combining measure of hydrogen was 'about' twice that of oxygen.

From the numbers given by Lavoisier, which he thinks could not differ much from the true ones, Lavoisier proceeds to calculate the composition of water by weight. In this calculation he assumes that a cubic inch of oxygen weighs 0·47317 of a grain, and the same measure of hydrogen 0·037449.§ These numbers are not accurate: that for oxygen should be 0·3419, that for hydrogen 0·0213; so that the calculation errs considerably.‖ According to it, 2 ounces and 58·4 grains of hydrogen combine with 13 ounces 7 drachms and 13·6 grains of oxygen to form 16 ounces of water; whereas in reality 2 ounces by weight of hydrogen combine with 16 of oxygen to form 18 of water.

* *Watt Corr.* p. 178; or *Mém. de l'Acad. pour* 1781, pp. 473—4.

† The account of these repetitions was read to the Academy, "à la Rentrée publique de la Saint-Martin [November] 1783," but additions were made to it between that period and April, 1784, in which year it was printed, although the volume of the *Mémoires* which contains it, is entitled "pour 1781." *Watt Corr.* pp. 171, 151 and 152; or *Mém. de l'Acad. pour* 1781, pp. 468 and 270.

‡ *Watt Corr.* p. 179; or *Mém. de l'Acad. pour* 1781, p. 474.

§ *Watt Corr.* p. 179; or *Mém. de l'Acad. pour* 1781, p. 474.

‖ From another part of his paper we learn that he and Meusnier estimated hydrogen to be 12½ times lighter than air, which explains in part this result. (*Watt Corr.* p. 172; or *Mém. de l'Acad. pour* 1781, p. 468.)

From these weights, again, the relative volumes of the gases are calculated to be—

	Cubic Inches.
Oxygen	16919·07
Hydrogen	32321·29
	49240·36

whereas, as already mentioned, the number for hydrogen should be exactly double that for oxygen.

After announcing this result, Lavoisier proceeds to remark that "this single experiment* of the combustion of the two gases, and their conversion into water, weight for weight, does not permit us to doubt that this substance, hitherto regarded as an element, is a compound body; but one fact is not sufficient to certify so important a truth: it is necessary to multiply facts, and after having composed water artificially, to decompose it."† The remainder of his paper, accordingly, is occupied with a statement of the reasons which led him to anticipate that water would prove susceptible of decomposition; with a criticism of Priestley's experiments (already referred to) on the revivification of metallic calces by hydrogen, in which Lavoisier shows that water must have been produced, although Priestley did not observe it; with a brief account of his earlier attempts to decompose water by red-hot metals and other bodies, and his success with iron, zinc, and charcoal; and with some general observations on the power of plants to decompose water, and on the vinous fermentation as a process during which the same decomposition occurs.

This part of Lavoisier's paper does not require criticism. It is characterised by the greatest precision, perspicuity, and sagacity; but as none of Lavoisier's English contemporaries asserted any claim to a share in the first analysis of water, it is not necessary to say more.

On reviewing his synthetical researches into the composition of water, considered as original and independent inquiries, and apart from all questions of priority, they will be found inferior to the similar researches both of Cavendish and Monge. In the first place, however, it must be noticed that they are unexceptionable so far as the substances asserted to be the elements of water are concerned. The one body Lavoisier employed was 'l'air vital,' or oxygen. The other he named, when free, "*air inflammable aqueux*," and when combined, "*principe inflammable aqueux;*" for *hydrogen* was still as strange a word to him as it was to Cavendish, Watt, or Priestley. This aqueous inflammable air, he tells us, could be obtained by decomposing water by iron, or by dissolving iron or zinc in sulphuric or muriatic acid; but, unlike the English chemists, he did not infer that the gas was derived from the metal, but that, in all these cases, it came from the water (pure or acidulated), in contact with which the metal was placed. He declined to pronounce an opinion on the question, whether there are different kinds of inflammable air, or only one, liable to alteration, by mixture or combination with different substances which it can dissolve. It was sufficient for his purpose to inform the reader, that when he employed the term "inflammable air," he referred solely to that which could be obtained by the processes described above; in other words, to hydrogen.‡ Thus far his experiments were unexceptionable; but in four

* The phrase here is evidently not used in the sense of *solitary trial*, but in reference to a series of trials all of the same kind, which, taken together, form one experiment.

† *Watt Corr.* p. 180; or *Mém. de l'Acad. pour* 1781, p. 475.

‡ *Watt Corr.* p. 171; or *Mém. de l'Acad. pour* 1781, p. 468.

points, already so far referred to, they were less trustworthy. 1. The gases were not weighed before combustion, but measured, and their weight calculated from their volume. 2. The mode of measurement is not minutely described; but from the large quantities of gas operated on, the size of the gasholders, and the necessary complexity of the arrangement, including leather tubes whose capacity could not be exactly ascertained, it is impossible that delicate measurement can have been practised. The tubes further appear, from Lavoisier's statement, not to have been air-tight; and when the combustion of the gases was arrested, they must have been left full of unmeasured gas. 3. The water produced was not weighed in the vessel in which it condensed, as it was in the processes followed by Cavendish and Monge, but was transferred to a separate vessel, in which its weight was ascertained. The transference, however, cannot have been effected without loss. Even, therefore, if Lavoisier had employed dry gases, which, like Cavendish, he did not, and had been furnished with accurate determinations of their densities on which to base his calculations, which he was not; the essential faultiness of his method of procedure forbade the possibility of his demonstrating that, weight for weight, the gas burned and the water produced were equal. To what extent the weights differed is not ascertainable, for Lavoisier has not supplied us with the means of determining how great the departure from absolute identity between them was. 4. The analysis of the water was imperfect; for, besides the few tests tried in the only case where they are described, it is difficult to believe that nitric acid should not have appeared in Lavoisier's experiments as well as in Cavendish's, yet, as the latter pointedly remarked, none was detected.* The apparent purity of the water, however, made the interpretation of the results obtained all the more easy. It is impossible, therefore, to exalt Lavoisier's experiments above those of Cavendish, as some have done. They are liable to the same objection as those of Priestley, upon which Watt founded his opinion, namely, that they did not warrant the conclusion based upon them. And it is of no little importance to notice, that, in effect, Lavoisier acknowledged as much; for when he refers to Monge's experiments on the product of the combustion of hydrogen and oxygen, which were made contemporaneously with his own, he says of them: "He has operated *without loss*, so that his experiment is *much more conclusive* than mine, and leaves nothing to desire."† This amounts to a confession that, in his own researches, there *was* loss, and that his experiment was *not* conclusive.

Lavoisier's conclusions, however, whether he was entitled to them or not, were stated with a precision and clearness to which the announcements of Cavendish, Watt, and Monge cannot lay claim. They are contained in two brief, but most emphatic lines: "Water is not a simple

* The circumstances of the two experiments certainly differed; for in the one, a large volume of hydrogen and oxygen, mixed with a little nitrogen, was exploded *at once* by the electric spark, and in the other, a small jet of hydrogen and oxygen, mixed with a little nitrogen, burned comparatively slowly in an atmosphere of air (or, if the hydrogen in the jet were in excess, in an atmosphere of nitrogen). The difference in the circumstances, however, was not apparently so great as to account for the absence of acid in Lavoisier and Meusnier's trials, especially as the proportion in which they burned the gases, viz. 12 vols. of oxygen to 23 of hydrogen, gave more than a combining volume of the former, and secured the conditions essential to the production of nitric acid. In a later repetition of Cavendish's experiments, Lavoisier had no difficulty in observing the production of nitric acid. See Berthollet's letter to Blagden, quoted on page 343.

† *Watt Corr.* p. 178; or *Mém. de l'Acad. pour* 1781, p. 474.

substance, but is composed, weight for weight, of inflammable and vital air."* Lavoisier, therefore, certainly announced the true doctrine of the composition of water.

This seems the proper place for mentioning a fact which has not been referred to by any of the writers on the Water Controversy, and probably was not known to them; namely, that Lavoisier made a *second* repetition of Cavendish's experiments after the publication of the latter's paper. I have learned the circumstance from a letter addressed by Berthollet to Blagden in 1785, with the loan of which I have been favoured by the executor of the latter, R. H. Blagden Hale, Esq., of Cottles, Melksham, Wiltshire. What follows, is the portion of Berthollet's letter referring to the Cavendish experiments; the remainder is occupied with the account of the discovery of a comet in the constellation Andromeda, by M. Michain; with certain observations of Abbé Haüy on Crystallography; and with references to a paper by La Place; to certain chemical analyses by M. Pelletier; and to a Cometographie, by M. Pingré.

To Dr. Blagden, Gower street, Bedford Square, London.

Paris, 19 Mars, 1785.

Monsieur,—L'on s'est beaucoup occupé ici ces derniers tems de la belle découverte de Mr. Cavendish, sur la composition de l'eau. Mr. Lavoisier a tâché de porter sur cet objet toute l'exactitude dont il est susceptible. Ayant fait part de son projet à l'Académie, elle jugea qu'on ne devait rien négliger dans une expérience qui doit jeter du jour sur un si grand nombre de faits, et elle chargea la classe de chymie d'y assister et de lui en rendre compte.

Je ne vous decrirai pas les détails de cette expérience pour laquelle on n'a epargné ni dépenses ni soins; mais je vais vous en donner une idée: L'air déphlogistiqué a été retiré du précipité rouge et a passé trois fois à travers de l'alkali caustique. Le gas inflammable a été dégagé de l'eau même qu'on a fait couler dans deux tubes de fer qui contenaient des lames de fer contournées en spirales; avant de faire couler l'eau qui était distillé, on a fait le vide dans l'appareil. On a rempli d'air déphlogistiqué un grand récipient auquel venaient aboutir d'un coté un tube qui amenait l'air déphlogistiqué, et de l'autre un tube qui amenait le gas inflammable, et le gas et l'air étaient déterminés par des pressions égales à venir dans le récipient à mesure que la combustion se faisait.

Il s'est forme cinq onces et demi d'eau qui contenaient environ quarante grains d'acide nitreux: il n'y a eu que très peu de residu. La quantité d'air déphlogistiqué qui est entrée dans la composition de l'eau a été en poids à cette du gas inflammable à peu près comme 81 à 19 et cette de l'air déphlogistiqué qui s'est fixé dans le fer dans la décomposition de l'eau a été dans la même proportion avec le gas inflammable qui s'est dégagé.

Mr. Lavoisier veut repéter l'expérience en faisant brûler l'air déphlogistiqué dans le gas inflammable, et il y a apparence qu'alors on n'aura point d'acide nitreux, selon les belles observations de Mr. Cavendish. Mr. De Laplace a preuvé d'après tout ce qu'on sait déjà que l'acide nitreux était un composé de gas inflammable et d'une beaucoup plus grande quantité d'air déphlogistiqué qu'il n'y en a dans l'eau, et que le gas nitreux tient le milieu entre l'acide nitreux et l'eau; mais d'après ce

* *Watt Corr.* p. 178; or *Mém. de l'Acad. pour* 1781, pp. 473, 474.

que vous m'avez ecrit dans votre derniere lettre, il y a grande apparence que Mr. Cavendish laissera peu à desirer sur cet objet ; lorsque vous pourrez m'instruire des dernieres expériences que vous m'avez fait que m'annoncer, je vous en serai fort obligé.

Mr. Lavoisier a lu un mémoire dans lequel il a expliqué par les differentes affinités de l'air déphlogistiqué les précipitations mutuelles des métaux de leur dissolutions acides. Il a répeté l'expérience de Mr. Priestley sur la révivification du précipité rouge par le moyen du fer, et il n'a point retiré d'air fixe dans cette opération comme l'annonce Mr. Priestley.

Il attribue tout le gas inflammable qu'on retire des differentes dissolutions métalliques à la décomposition de l'eau; il croit que tous les métaux ont besoin d'être dans l'état de chaux pour être tenus en dissolution, et que pour être reduits en chaux ils prennent par l'intermede de l'acide, l'air déphlogistiqué d'une portion d'eau et que de là vient le dégagement du gas inflammable de cette eau. Il attribue cependant dans certains cas l'air déphlogistiqué qui s'unit au métal à l'acide lui-même; ainsi il croit que lorsqu'un métal forme de l'acide sulfureux avec l'acide vitriolique, cela dépend de ce qu'il s'empare d'une partie de l'air déphlogistiqué qui est dans l'acide vitriolique ; il explique de même le dégagement du gas nitreux dans les dissolutions par l'acide nitreux.

. . . .

Votre très humble et très obéissant serviteur,

[Signed] BERTHOLLET.

The main points in the letter, it will be seen, are, that hydrogen and oxygen were burned together; that water was obtained *containing nitric acid*, and leaving (apparently on evaporation) a slight residue ; or exactly the results which Cavendish obtained, and which he was surprised, especially in reference to the acid, that Lavoisier had not obtained. Even the slight residue was found which has been objected to in the criticisms of certain of Cavendish's results, as showing that he never procured pure water.

Berthollet declares that the water produced contained its constituents in the proportion of 81 parts, by weight, of oxygen, to 19 of hydrogen. The latter number is perhaps a mistake for 10, which would represent the quantity of hydrogen which can unite with 80 of oxygen, to produce 90 of water.

How those numbers were obtained, is not mentioned. It is manifest, however, from the appearance of nitric acid in the water, that the oxygen, instead of being in the proportion of about half a combining measure, must have been in excess. This was evidently Lavoisier's opinion, who proposed to burn oxygen in hydrogen, so as to keep the latter in constant excess, in the expectation of verifying Cavendish's observation that no nitric acid would be produced.

La Place's erroneous conclusion that nitric acid is a higher oxide of hydrogen than water, is a tacit compliment to Cavendish's sagacity in avoiding an inference which seemed almost forced upon the observer.

The exact period when this second repetition of Cavendish's experiment was made, is not stated, but it cannot have been very long before the date of the letter ; otherwise Berthollet, who regularly corresponded with Blagden, would have sent him earlier notice of it. It should thus seem that a considerable period elapsed before Cavendish's paper in the Philosophical

Transactions, 1784, was known in France, a point of some importance, as he has been accused of sending in great haste misdated copies of his paper to the Continent.

10. *Experiments and Conclusions of Monge concerning the result of the inflammation of Hydrogen and Oxygen in close vessels.*

Monge's experiments are detailed in a paper contained in the Mémoires de l'Académie des Sciences, for 1783 (printed in 1786) pp. 78 to 88. Mr. Muirhead has reprinted the paper along with an illustrative wood-cut in the "Watt Correspondence," pp. 205–218, and from his reprint I make my quotations.

Monge's researches are important, as they formed an original and independent inquiry which, had it conducted him to the same conclusions as it did Lavoisier, would have constituted him the discoverer of the composition of water in France, and have placed him on the same level of merit as its discoverer in England. His paper is entitled, "Mémoire sur le Résultat de l'inflammation du gas inflammable, et de l'air déphlogistiqué, dans des vaisseaux clos." The experiments recorded in it were made, he tells us, at Mézières in June and July, 1783, and repeated in October of the same year. "I did not then know," he adds, "that Mr. Cavendish had made them several months before in England, though on a smaller scale; nor that MM. Lavoisier and Laplace had made them about the same time at Paris, in an apparatus which did not admit of as much precision as the one which I employed."*

Monge's experiments differ from those of Priestley, Watt, Cavendish, and Lavoisier, in being limited to an inquiry into the nature of the product yielded by the combustion of inflammable air and oxygen, and in being prosecuted without any hypothesis as to the probable result of the combustion. In what exact sense he used the term inflammable gas, does not precisely appear, but he did not restrict it to a single substance, for for when referring to the temperature at which a mixture of oxygen and "*gaz inflammable*" takes fire, he observes that this "depends upon the nature of the inflammable gas,"† and he refers in illustration, to what occurs during the combustion of a candle, and of boiling oils.

He used, however, only one kind of inflammable gas in his experiments. This was obtained by dissolving clean iron filings in diluted sulphuric acid, so that it certainly was hydrogen. The oxygen he employed was obtained by heating red oxide of mercury; and he had recourse to many precautions to secure the purity of both gases from admixture with atmospheric

* *Watt Corr.* p. 206; or *Mém. de l'Acad. pour* 1783, p. 79. The originality of these researches is denied by Blagden, who says, "Mr. Monge's experiments (of which Mr. Lavoisier speaks as if made about the same time) were really not made until pretty long, I believe at least two months, later than Mr. Lavoisier's own [query, Mr. Cavendish's], and were undertaken on receiving information of them." (Letter to Crell, translated in *Watt Corr.* p. 73.) Blagden's denial, however, is quite irreconcilable with Lavoisier's declaration, that a few days after (quelques jours après) the 25th June, 1783, Monge sent an account of his experiments in a letter to M. Vandermonde, who read it to the Academy. (*Watt Corr.* p. 178; *Mém. de l'Acad. pour* 1781, p. 474.) As Monge's paper was not printed till 1786, the year in which Blagden's letter was published, it is probable that he had not read the statement quoted in the text when he wrote to Crell, but referred only to reports which had reached him of the experiments made in October, 1783, which he supposed to be Monge's earliest researches. The latter seems to have been quite willing to concede the priority of Cavendish's experiments, and the independence of Lavoisier's.

† *Watt Corr.* p. 218; or *Mém. de l'Acad. pour* 1783, p. 88.

air. Each was collected over water in a carefully graduated bell-jar, provided above with a tubulure and stopcock. To effect the combustion of the gases, a glass globe or balloon was employed, provided with an arrangement for passing the electric spark, as in Cavendish's apparatus. Monge's balloon, however, had three apertures, one communicating by a stopcock and tube with an air-pump, by means of which the balloon was exhausted, and two, communicating respectively by metallic tubes with the stopcocks of the bell-jars containing the hydrogen and oxygen. This arrangement was rendered necessary by the mode in which he experimented. He was most anxious to prevent any intermixture of the gases with atmospheric air, and to secure this he provided separate channels for the hydrogen and oxygen, which were shut off from communication with the outer air, till the experiment was concluded. He did not, moreover, mix the gases in their combining proportion, and transfer the mixture to the detonation-globe, as Cavendish did; but having ascertained their densities so that he could convert volumes into weights, he made a vacuum in the balloon, and transferred a certain portion of each gas into it, by opening the stopcock of the bell-jar which contained it. For reasons which he does not assign, he preferred to introduce at first into the balloon $\frac{1}{12}$th of its capacity of oxygen, and $\frac{11}{12}$ths of hydrogen. An electric spark was then passed, and an explosion determined. A twelfth part of its volume of oxygen, was, thereafter, introduced for the second time into the balloon, and a second explosion determined; and this process was repeated without renewing the hydrogen, till five or six explosions had occurred, when the supply of that gas was replenished, and the process proceeded as before. At the termination of the 137th explosion, the vacuum was renewed, with a view especially to empty the balloon of incombustible elastic fluid, which was supposed to be accumulating in it. This emptying of the balloon was repeated twice, before the conclusion of the experiment. The gas thus withdrawn was transferred to a receiver and preserved for subsequent examination, and the explosions were renewed till 370 had occurred, when Monge found that he had consumed 145 pints $\frac{91}{144}$ of hydrogen, and 74 pints $\frac{9}{16}$ of oxygen, numbers which I need not say, make a close approximation to accuracy. if considered as representing the combining volumes of hydrogen and oxygen ; but it will presently appear that a certain part of the gases escaped combustion. Those measures calculated from Monge's own determinations of the sp. gr. of hydrogen and oxygen, and corrected for a change in the height of the barometer, which had occurred during the experiment, corresponded to 3 ounces, 6 drachms, and 27·56 grains by weight.

The amount of water which the combustion of the gases had yielded, was ascertained by weighing the balloon, first with the liquid which it contained, and a second time after it had been emptied and dried. The difference gave the weight of the liquid, which amounted to 3 ounces, 2 drachms, and 45·1 grains. The gas withdrawn from the balloon by the air-pump was then weighed, and found to amount to 2 drachms 27·91 grains, which, added to the weight of water found, make up 3 ounces, 5 drachms, 1·01 grains, so that there was a difference between the weight of gases burned and of water found, of 1 drachm, 26·55 grains.*

* This is Monge's calculation, but will presently appear, that the gas withdrawn by the air-pump was not entirely hydrogen and oxygen; so that the difference between the weight of these gases consumed, and of water produced, was a little greater than is stated.

Monge remarks that this difference may have resulted, 1. From his having taken the mean height of the barometer during the experiment, as the basis of correction of volume for the whole quantity of gas expended, instead of correcting each quantity transferred to the balloon, according to the height of the barometer at the period when it was measured. 2. From his not having observed the changes in temperature in the bell-jars containing the gases, occasioned by their proximity to the balloon, which was heated by the explosions determined within it. 3. From the loss occasioned by evaporation during each renewal of the vacuum.

The air withdrawn by the air-pump from the balloon amounted to seven pints, and contained $\frac{1}{15}$th of its volume of carbonic acid. When this was removed by lime-water, the residue was fired by the electric spark, (which shows how needless Monge's repeated renewals of the vacuum in the balloon were), and was thereby diminished $\frac{1}{8}$th of its volume, from which it was inferred to contain a mixture of hydrogen and oxygen. The residue was incombustible in the air, but gave ruddy fumes with nitric oxide, and thereafter underwent contraction "like atmospheric air." From this result, Monge inferred that the residual gas contained $\frac{1}{4}$th of its volume of oxygen, without, however, showing that he was entitled to this conclusion.* The exact accuracy, however, of his statement on this point, is unimportant. From his whole analysis it appears, that the residual gas consisted of a mixture of carbonic acid, hydrogen, oxygen, and nitrogen. From its composition, Monge inferred, that it could not be regarded as a product of the combustion, but that it was a result of the impurities contained in the hydrogen and oxygen. These impurities, he thought, probably came, in part, from the air of the vessel in which the hydrogen was generated ; in part, from the water of the apparatus, which was agitated several times during the transference of the gases ; and in part from the water employed to dilute the sulphuric acid.

The liquid which collected in the balloon was perfectly transparent. It reddened blue turnsol paper "imperceptibly;" which Monge explains by adding, that the reddening effect was much less than in a previous experiment,† and was less than that occasioned by saliva. This acidity was not owing to carbonic acid, for the liquid had no action on lime-water. It rendered solutions of nitrate of silver and of mercury very slightly opalescent, from which we may infer the presence of a trace of some chloride; but Monge did not draw any conclusion from the faint action of these reagents. The liquid had no other peculiarity, except an empyreumatic odour, like that of distilled water. Monge, accordingly, inferred that the liquid was pure water containing a small quantity of sulphuric acid, which the hydrogen had carried over with it from the solution of iron that yielded it. This ingenious observer, however, it will

* The grounds on which it was founded are doubtful. To prevent mistake, I give Monge's own words. After stating that the residual gas was not combustible in the atmosphere, he adds, "Mais par son mélange avec l'air nitreux, il a rutilé, et s'est encore réduit comme l'air atmosphérique. Il contenoit donc encore à cette époque un quart de son volume d'air déphlogistiqué." (*Watt Corr.* p. 215; or *Mém. de l'Acad. pour* 1783, p. 86.) He seems to have inferred that the production of ruddy fumes and reduction of volume, proved the residual gas to be atmospheric air, and *therefore* to contain $\frac{1}{4}$th [$\frac{1}{5}$th] of oxygen.

† The reference to a previous experiment is important, and is explained by Monge's statement, that he repeated in October, 1783, trials which he had made in June and July of the same year. His first experiments were reported, soon after this performance, to the French Academy, as Lavoisier informs us in his memoir. (*Watt Corr.* p. 178; or *Mém. de l'Acad. pour* 1781, p. 474.)

be observed, did not examine the liquid with any test for sulphuric acid, but only supposed it to be present. That it was, may be doubted, considering the large volume of water with which the hydrogen was washed, and over which it stood; but whether or not sulphuric acid was present, it seems certain that the acidity of the water (which, in the earlier experiment, was manifesty well marked) was owing partly to the presence of nitric acid. The conditions of Monge's experiments were the same as those of Cavendish's; and it is certain from Monge's analysis of the gas extracted by the air-pump, that nitrogen was mixed with the hydrogen and oxygen detonated in the balloon. Nitric acid, therefore, *must* have been produced.*

The presence of this acid, then, we may reasonably infer, was overlooked by Monge, and the conclusion which he was at liberty to draw from his experiments was rendered thereby the simpler. He begins by observing, that a part of the water found was certainly contained in solution in the gases, but that the whole cannot be accounted for by a reference to this; for in that case, hydrogen and oxygen would each consist of nothing but water on the one hand, and the incondensible matter of fire and of light (la matière du feu, et de celle de la lumière) on the other. He infers, therefore, that when pure hydrogen and oxygen are detonated together, the only products are—pure water, the matter of heat, and the matter of light. Beyond this inference, however, he reaches only an alternative conclusion. "It remains to determine positively, whether—the two gases being solutions of different substances in the fluid of fire (le fluide du feu) considered as a common solvent—these substances, in consequence of the combustion, abandon the solvent, and combine to produce water, which will thus no longer be a simple substance; or whether the two gases being solutions of water in the different elastic fluids, these abandon the water which they dissolved in order to combine and form the fluid of fire and of light, which escapes through the walls of the vessels; in which case fire will be a compound substance."†

Monge thus would only say, that his experiments demonstrated that *either* water *or* fire was a compound substance; but he thought additional experiments of another kind were needed to determine which of the two entities was to be considered no longer a simple body. He points out that the supposition that water is a compound of (the bases of) hydrogen and oxygen, would explain the functions of water in vegetation, and account for many other phenomena, such as the moistening of cold surfaces by the flame of vegetable bodies; the condensation of water in the chimneys of stoves; and the violence of the detonation of gunpowder, which he thought must be referred solely to the vaporization of the

* In Monge's process matters were so arranged, that in certain of his trials there was a gradually diminishing *excess* of hydrogen over oxygen during the first five of six explosions which were as many as could be performed without replenishing the balloon with hydrogen. During those five explosions, no nitric acid could be produced, but at the sixth, the volumes of oxygen and hydrogen were equal, so that there was half an equivalent too much of the former, and this excess could not but form nitric acid with the nitrogen present.

† The original is as follows: "Il reste à savoir actuellement si les deux gaz étant des dissolutions de substances différentes dans le fluide du feu considéré comme dissolvant commun, ces substances, par l'inflammation, abandonnent le dissolvant et se combinent pour produire de l'eau qui ne seroit plus alors une substance simple; ou bien si les deux gaz étant les dissolutions de l'eau dans des fluides élastiques différentes, ces fluides quittent l'eau qu'ils dissolvoient pour se combiner et former le fluide du feu et de la lumière qui s'échappe à travers les parois des vaisseaux: et alors le feu seroit une matière composée." (*Watt Corr.* p. 216; or *Mém. de l'Acad. pour* 1783, p. 87.)

water produced by its combustion. But whatever weight these considerations had in inclining him to consider water a compound, "this hypothesis," as he himself calls it, presented a difficulty which he could not surmount. Inflammable gas and oxygen were well known to require only a simple elevation of temperature to determine their combustion; in other words, according to his view, to determine their separation respectively into so much fire, and so much of two substances (the basis of oxygen and inflammable gas) which united to produce water. But if fire were the solvent of these bases, and prevented their precipitation and combination, why should the introduction of fire into a mixture of the gases, *i. e.*, an increase of the solvent of their bases, determine a total separation of these from their solvent? According to Monge's hypothesis this must occur; but it seemed to him totally opposed to all that was observed in the analogous operations of chemistry. He concludes the paper accordingly with this remark, which is best given in his own words. "Il nous manque donc encore beaucoup de lumières sur cet objet, mais nous avons droit de les attendre, et du temps, et du concours des travaux des Physiciens."*

It is unnecessary to criticise Monge's conclusion further than to say, that it plainly excludes him from a claim to be considered a discoverer of the composition of water. It would be a fault, however, in any historian to pass him by unnoticed and uncommended, and in another section I shall endeavour to do his merits justice. At present I would only comment on his experiments, as compared with those of Cavendish; for, as Lavoisier acknowledged the superiority of Monge's processes to his own, it is unnecessary to contrast the methods of the two French philosophers.

Monge himself appears to have thought his method superior to that of Cavendish, inasmuch as the experiments of the latter were "plus en petit." Lavoisier thought that his countryman's experiment "ne laisse rien à désirer;" and recent critics of the Water Controversy have commended the French experiments as superior to those of Cavendish. Monge's experiments deserve great praise, and must be placed far above those of Lavoisier, not to speak of Priestley's inexplicable results. Whatever exceptions, in truth, a searching criticism may take against certain parts of Monge's process, no candid reader of his memoir will deny, that his researches made so close an approximation to accuracy, as fully to entitle him and every one else to infer, that water is a compound of hydrogen and oxygen. Nor can it be doubted, that though no experiments had been made in England, Monge's experiments would have conducted Lavoisier to this conclusion. Nevertheless, Monge's method of procedure was decidedly inferior to that of Cavendish. 1. He gained nothing by the greater scale on which he experimented, for he observed nothing that Cavendish did not observe, and he overlooked the production of nitric acid which Cavendish detected. He was a loser, indeed, by the magnitude of his operations; for it is a strange though common error to imagine that experiments cannot be performed on too large a scale; whereas, in *quantitative* determinations a limit is opposed to all endeavours greatly to enlarge the scale of operation by the increased difficulties which attend the accurate measurement of large weights, volumes, or manifestations of force. The scale of Monge's operations rendered it almost impossible, that all the measurements and weighings

* *Watt Corr.* p. 218; or *Mém. de l'Acad. pour* 1783, p. 88.

of gas and liquid, and all the requisite observations of the thermometer and barometer, should be quite accurately made; and they protracted the experiment over a period of time, which increased the difficulties attending its performance. By employing smaller quantities, he could have dispensed altogether, as Cavendish did, with any consultation of the two latter instruments, and could have weighed and measured with more precision.

2. The principle on which Monge proceeded was, to make the same experiment at once *qualitative* and *quantitative*, so that, for the sake of obtaining a large quantity of liquid for analysis, he rendered all his measurements less precise than they would have been, had the scale of his operations been smaller. Cavendish avoided this, by making two sets of experiments—one on a comparatively large scale to ascertain the nature or *quality* of the product of the combustion of hydrogen and oxygen; the other on a much smaller scale to determine the amount or *quantity* of this product.

3. Monge's apparatus was needlessly complex, and its complexity multiplied the chances of error. In Cavendish's arrangement, the gases were first mixed in their combining proportion, and thus a single bell-jar served to contain both. The empty globe was weighed before and after each passage of the electric spark, so that every explosion was a separate experiment, supplying an additional numerical datum. In Monge's experiments, on the other hand, the gases were mixed in the balloon in various proportions. There was but one (double) weighing, and the expenditure of 219 pints of gas by 370 explosions, supplied but a single quantitative result.

4. The analysis both of the gas withdrawn from the balloon, and of the liquid collected in it, was imperfect. I have already referred to the obscurity attending Monge's account of the conclusion he drew from the action of nitric oxide on the residual mixture of nitrogen and oxygen withdrawn from the balloon, and need not further allude to it.

Monge's analysis of the water, moreover, was also defective, not only because, like Lavoisier's examination, it included the application of fewer tests than Cavendish's analysis did; but because a positive impurity, namely an acid, was detected in the water, and nevertheless the inquiry was brought to a close without the nature of the acid being ascertained. Monge *assumed* it to be the sulphuric, without even applying the tests for that acid. How important an oversight this was, appears from the valuable results which attended Cavendish's exhaustive inquiry into the nature and source of the acid which occasionally showed itself in his experiments, as a product of the combustion of apparently pure hydrogen and oxygen.

I might make further objections, but I do not wish to be hypercritical. It was necessary for the defence of Cavendish to urge that beautiful in many respects as Monge's experiments were, they were decidedly inferior to those of Cavendish.

THE QUESTION OF PRIORITY.

11. *Who first discovered and taught that Water is a compound of Hydrogen and Oxygen? Date of Cavendish's experiments and conclusions.*

The question of priority between Cavendish, Watt, Lavoisier, and Monge, will not require much discussion, so far as the last is concerned, for he frankly concedes that Cavendish's experiments were of an earlier date than his own, and he did not draw such a conclusion from these as entitles him to be called *a* discoverer of the composition of water. The same will be said of Watt's claims, by those who agree in the opinion respecting the erroneous nature of his fundamental experiments and conclusions, which has been stated in Sections 6, 7, 8; and it might seem at first sight a needless undertaking to attempt to settle at what period erroneous conclusions were first drawn from inaccurate experiments. But Watt's claims cannot be dismissed in this summary way by those who disallow the validity of his conclusions, for they were accepted as well founded by his contemporaries for many years at least, and were regarded as identical with those of Cavendish and Lavoisier. Nor can it be doubted, that Watt's exposition of the compound nature of water materially conduced to the adoption of that view, especially in Great Britain. His claim to priority, accordingly, cannot be overlooked; and in discussing it, I shall in general assume for argument's sake, so far as it is practicable, that his views regarding the composition of water were unexceptionable, and concern myself chiefly with the date of his conclusions.

As for Lavoisier, he indirectly asserted in his own name a claim to priority over Cavendish, and cannot therefore be passed by. The question, moreover, between the English claimants, cannot be discussed without large reference to his proceedings; and whatever opinions may be formed regarding his originality or fair dealing, all must acknowledge that after the amplest assignment of merit to Cavendish or to Watt, or to both, there will remain a large share of praise for Lavoisier, to which no one else can lay any claim. I begin the discussion with the consideration of the evidence in favour of Cavendish's priority.

Dates of Cavendish's Experiments and Conclusions.

It will form the best preparation and apology for the length of this section, if I begin by at once drawing attention to what the friends of Cavendish have been too reluctant to acknowledge, namely, that the *date* of his *conclusions* is uncertain. I cannot but concur with Lord Brougham when he says, "that there is no evidence of any person having reduced the theory of composition [of water] to writing, in a shape which now remains, so early as Mr. Watt."* The *writing* here referred to goes back to April, 1783, whilst there is no holograph nor other document of Cavendish's containing an *explicit* declaration of his conclusions, of earlier date than the manuscript of his first "Experiments on Air," read to the

* Historical Note. *Watt Corr.* p. 252.

Royal Society in January, 1784. So far, therefore, as *direct* documentary evidence is concerned, Watt's claims over Cavendish can be antedated by more than eight months. The advocates of Cavendish are thus under the same necessity of appealing to *indirect* evidence to support his claim to priority, which the advocates of Watt are under, when seeking to establish the validity and accuracy of his conclusions. The indirect evidence, however, in the case of Cavendish is of a very weighty and important kind. It consists chiefly of three written documents: 1. Cavendish's Laboratory note-book, containing the record of all his experiments on air. 2. Two statements by Priestley in his paper "on the seeming conversion of Water into Air:" and 3. The letter from Blagden to Crell, already referred to. These documents and certain other records, which will be referred to in the course of the discussion, remove all doubt as to the date of Cavendish's *experiments*, and throw much light on the date of his *conclusions*. The advocates of Watt, indeed, at least in England, have conceded the originality and priority of Cavendish's experiments; (in this dissenting from Arago, with whom Dumas is understood to agree;) and have joined issue with the advocates of Cavendish, solely, or almost solely, as to the originality and priority of his conclusions. According to Arago, "Priestley records in detail, and as his own, experiments which prove that the water produced by the combustion of a mixture of hydrogen and oxygen has a weight exactly equal to that of the two gases which are burned. Cavendish, some time after, claims this result for himself, and insinuates that he had communicated it verbally to the chemist of Birmingham."* Sir David Brewster, on the other hand, says, "The friends of Mr. Watt in England . . . . have acknowledged the priority of his [Cavendish's] experiments to the full extent that it has been proved;" and again, "we have admitted the originality and the value of Mr. Cavendish's experiments."† Lord Jeffrey still more explicitly says, in reference to the years 1781 and 1782, "It is not, we think, to be denied, that Cavendish then performed those experiments, and observed those results from which either he himself at the time, or Watt at an after time, or he again after Watt drew the grand and momentous conclusion, that water was not a simple but a compound body."‡ Even, however, if the French Academicians were at one with Watt's English friends, in conceding the priority of Cavendish's experiments to those (whatever they were) upon which Watt founded his theory, it would be impossible to separate the question of the date of Cavendish's experiments from that of the date at which conclusions were drawn from them; the two therefore must be considered together. And this seems the proper place for referring more particularly than has hitherto been done, to Cavendish's manuscript note-book or journal.

Through the good offices of the Rev. W. V. Harcourt, and the courtesy of the Earl of Burlington, I have been favoured with a perusal of the original MSS.; and I can bear testimony to the perfect fidelity of the lithograph fac-simile published by Mr. Harcourt in the postscript to his address to the British Association in 1839.§ This fac-simile includes only a portion of the Note-Book, but all, however, that refers to the product of the combustion of hydrogen in air or oxygen. I have found nothing, more-

* *Eloge of James Watt.* Translation by Mr. Muirhead, *Watt Corr.* p. 230.

† *North British Review*, Feb. 1847, pp. 502, 503. This paper is referred to as Sir David Brewster's, in the *Edinr. Rev.* Jan. 1848, p. 84.

‡ *Edinr. Rev.* Jan. 1848, p. 72.

§ *Brit. Assoc. Rep.* 1839. Postscript to President's Address, pp. 22—69.

over, in the omitted passages which could in any way tell against Cavendish, so that those who have access only to the lithograph fac-simile may rest satisfied that nothing has been kept back which would materially affect the determination of the question under discussion. On the other hand a complete publication of the Note-Book (which, however, its size forbids) would, incidentally at least, strengthen the case of Cavendish's advocates, by showing the continuity and consistency of his whole researches from 1778 to 1785. In referring to this Note-Book, I shall quote from Mr. Harcourt's lithograph copy, when it contains the passages which I wish to refer to, that the reader may have the means of verifying the quotations. The unlithographed portion of the MSS. will be quoted from the original (the property of Lord Burlington), which is paged and indexed by Cavendish himself. The following description by Mr. Harcourt will explain the present condition of the Laboratory Notes :—"They fill a volume of unsewn and single, but paged and indexed, octavo sheets, in his own hand, bearing dates from February, 1778, to May, 1785, in the following proportions; in 1778, thirty-three pages; in 1780, thirty-six; in 1781, seventy-five; in 1782, forty-five; in 1783, fifty-three; in 1784, forty-four; in 1785, thirty-three. I found them in a packet entitled 'Experiments on Air.'"*

In judging of the contents of these Notes, it is of the utmost importance that the reader should keep in recollection that Cavendish's observations on the synthesis of the elements of water did not constitute an isolated inquiry, but formed an integral portion of a continuous series of experiments on air extending from 1778 to 1785. This has not been observed, or at least has not been admitted by the advocates of Watt. "If," says Sir David Brewster, "he [Cavendish] had discovered the composition of water in July, 1781, is it credible that he would have kept it secret till January, 1784, and that he would then have brought before the public so great a discovery under the title of 'Experiments on Air?'"† It was in entire consistence, however, with the whole train of his researches, and with the contents of the paper published in 1784, in which he discussed much more than the composition of water, that he should have entitled his communication "Experiments on Air." He gave the same title to his paper on the synthetical production of nitric acid, and for the same reason, namely, because the production from their elements of this acid and of water, was ascertained in the course of an inquiry into the products of the oxidation of bodies in air. In consequence, however, of this fact being overlooked, Cavendish's experiments on the production of water are generally referred to, as if they had originated solely in the observations of Warltire and Priestley on the Detonation of Inflammable Air, with a view to test the ponderability of heat, of which they are alleged to have been an avowed repetition. This they certainly were, but they were at the same time something more, as we have already seen in part, in section 5. It was stated there, and also in the abstract of Cavendish's "Experiments on Air" (1st series, see pp. 141 and 142) that his researches contemplated the ascertainment of the changes which various combustibles, not inflammable air only, produced upon common air when burned in it till they had deprived it of the power of supporting combustion, or in the language of the day had phlogisticated it. Cavendish's own words in his introduction to the first series of "Experiments on Air," are—"The following experiments were made *principally* with a

* *Brit. Assoc. Rep.* 1839, p. 32.
† *North British Rev.* Feb. 1847, p. 494.

view to find out the cause of the diminution which common air is well known to suffer *by all the various ways* in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." The evidence of this has been given already, so that we are now concerned only with the date of the researches referred to. They were ultimately embodied, to the extent they were made public, in the paper on a New Eudiometer, as well as in both the series of "Experiments on Air;" but many were never published.

From the lithographed earlier portion of the Note-Book, it appears that in 1778 Cavendish investigated the action of nitric oxide on air, and on oxygen, and ascertained, as Lavoisier had already done, that nitric acid was produced, but not carbonic or sulphuric acid. In this year also he made many trials as to the best way of employing nitric oxide in the eudiometer, and executed several approximative analyses of air.*

No experiments are recorded in the Note-Book under date 1779, but in 1780 they were resumed, and a great part of those observations was made, which are recorded in the paper on the new eudiometer. The action also of liver of sulphur on air and on oxygen, and the effect of various so-called phlogisticating bodies, such as burning sulphur, spirits of wine, and luna cornea on air, were ascertained, and trials were made as to the *vis inertiæ* of nitrogen;† and the nature of the gas produced by heating together nitre and charcoal. The whole of these experiments, without exception, had for their object the determination of the properties of atmospheric air, and were chiefly intended to bring to light the products of its phlogistication, and demonstrate the essential nature of that process.

The most important result of these inquiries was the determination, with a near approach to accuracy, of the relative amount of oxygen and nitrogen in common air, without a knowledge of which it was plainly impossible to experiment to purpose on the effect of combustibles on the atmosphere. I have given previously the result of Cavendish's analysis of common air as calculated from his standard for oxygen, but a direct announcement of it occurs in his Note-Book. He mentions that common air diminished by liver of sulphur lost $\frac{1}{4 \cdot 92}$ of its bulk, and states what its test appeared to be by his own and Ingenhouszt's eudiometer. He then adds, "In all probability both methods make the air appear better than it really is; we will suppose therefore that the quantity of pure air in common air is $\frac{10}{48}$."‡

Having thus, then, ascertained that the reduction of volume effected on air by its phlogistication (*i.e.* deoxidation) was about one-fifth, and having ascertained also that neither carbonic, sulphuric, nor nitric acid was the invariable product of the phlogistication of air, he proceeded to the experiments with hydrogen, the date of which chiefly concerns us.

It will be remembered that in a passage added to his paper between the period of its being read to the Royal Society and printed, Cavendish stated that all the experiments recorded in the earlier part of his Memoir, on the explosion of Inflammable Air with Common and Dephlogisticated Airs, except those which relate to the cause of the acid found in the

* *Unlith. MSS.* pp. 1—12, also 24—31. Mr. Harcourt has drawn attention to these researches in the postscript of his address to the Brit. Assoc. (*Report*, 1839, p. 34.)

† The curious experiments in question may be regarded as the earliest recorded observation on gaseous effusion, the laws of which have been so beautifully investigated by Prof. Graham.

‡ *Unlith. MSS.* p. 109.

water, were made in the summer of the year 1781, and were mentioned by him to Dr. Priestley.* It is of the utmost importance, therefore, to ascertain what those experiments were.

With the extreme caution and accuracy which characterised his researches, Cavendish began by ascertaining "whether there was any penetration of parts on mixing common and inflammable air by means of the eudiometer."† In other words, he was anxious to learn whether the mere mixture of inflammable air with common air would lead to the deoxidation of the latter, without the application of flame, or the transmission of the electric spark. He found, on mixing the gases, that the diminution in bulk did not exceed $\frac{3}{2250}$ of the whole; and manifestly concluded that the diminution was not real, but only apparent. He further tried whether a mixture of inflammable and common air remained uniformly mingled, or separated after standing some hours into a lighter and heavier portion, and he came to the conclusion that the gas from the lower part of the vessel in which the mixture was made appeared to contain very little, if at all, more common air than that from the top.‡

Having thus ascertained that inflammable and common air might be mixed without acting upon each other, and might be preserved in a state of nearly uniform mixture for many hours, Cavendish proceeded to repeat the experiments of Warltire and Priestley on the explosion of inflammable air with common air and oxygen. These experiments, it will be remembered, were performed by Warltire with a view to settle the question of the ponderability of heat, and by Priestley, as "a random experiment" to amuse his friends. Cavendish's first recorded observation on the subject was performed on the 3rd of July (1781), and is entitled "Explosion of inflammable air by electricity in glass globe, to examine Mr. Warltire's experiment."§ Six different explosions, in which the proportion of hydrogen and common air was varied, were made, and the results are tabulated.|| These experiments were performed on July 3rd, 4th, 5th, and 6th. The specific gravities of the residual gas¶ were tried on July 17th (p. 118); and on August 4th and 7th, comparative experiments were made with hydrogen from zinc and iron (pp. 120—122). The first reference to the production of water in the *globe* experiments occurs under date August 4th :— "The globe was dry before firing, but was immediately covered with dew on firing." But the appearance of water must have been observed before; for the experiments recorded in the published paper of 1784, as made by burning together hydrogen and *air*, were made in July; and on a Sunday in that month, 135 grains of water were obtained by their combustion, and the liquid ascertained by analysis to be pure.** A result of the same

* *Phil. Trans.* 1784, p. 134.

† *Unlith. MSS.* p. 113.

‡ *Unlith. MSS.* p. 114. This experiment was a remarkable anticipation of the similar researches made at a later period, by Hope, Dalton, and Graham, on the homogeneousness of gaseous mixtures, and the diffusion and effusion of gases. I need not say that Cavendish's observations were quite in keeping with those of Prof. Graham, for he drew off the gas somewhat slowly by a syphon; and we know that, in these circumstances, hydrogen diffuses out of a vessel in larger volume than a heavier gas, such as air, does.

§ *Lith. MSS.* p. 115.

|| *Lith. MSS.* p. 119. Substantially the same table is given in the *Experiments on Air. Phil. Trans.* 1784, p. 127.

¶ Nitrogen, pure, or mixed with a little hydrogen or oxygen, which remained uncombined after the explosion.

** *Lith. MSS.* p. 127.

kind, obtained long after, follows this in the Note-Book, bearing date November 18th, 1782.*

A considerable portion of August was spent in making observations with an instrument for measuring the force of the explosions which occurred, when different proportions of hydrogen and air or oxygen were burned together. The apparent object of these experiments was, as Mr. Harcourt suggests, to furnish "a test of the identity and purity of the gases, as well as of their combining proportions."†

During June and July, many preliminary experiments were made with oxygen;‡ but the first explosion of oxygen and hydrogen of which a record is given, occurs under date September (1781), when thirty grains of *acid* water are said to have been produced, which yielded nitre when saturated with alkali.§ A similar experiment is recorded on September 28th, as having been made with washed oxygen, mingled with a little less than twice its volume of hydrogen, when fifty grains of *acid* water were produced. The last experiment of this year was made on October 20th, by exploding oxygen from red lead with a little less than twice its volume of hydrogen, and the resulting water was found to be acid.

"Such," as Mr. Harcourt observes, "were the experiments made in 1781 concerning the reconversion of air into water, by decomposing it in conjunction with inflammable air, which Priestley and Cavendish mention as having been communicated to the former, and repeated, in consequence, by him in April, 1783."‖

I have given these dates somewhat fully, and the reader can easily follow them in Mr. Harcourt's lithograph of the MSS. in the British Association Report, 1839. It appears from them that all the experiments which Cavendish ultimately adduced, as justifying the conclusion that water is a compound of oxygen and hydrogen, were made by him in 1781, as he asserts in the first passage which he added to his paper of 1784. He had also observed in this year, as he affirms in that memoir, the production of nitric acid when apparently pure hydrogen and oxygen are detonated together; but he had not, as yet, discovered the *cause* of this acid appearing, as the terms of the interpolated passage also imply: so that his published paper of 1784 exactly tallies with the records of the Note-Book, so far, at least, as we have yet compared them.

The inquiry seems now to have been laid aside for about a year, during which Cavendish made a number of additional experiments on the points essential to the construction of an accurate eudiometer. These required many laborious observations on the properties of oxygen, nitrogen, and nitric oxide, besides various tedious manipulations with different forms of eudiometer, and a multitude of analyses of different specimens of air. They are recorded in various parts of the Note-Book from p. 154 to p. 199, and were, no doubt, preparatory to the publication of his account of the new eudiometer, which was laid before the Royal Society in January, 1783.¶

* *Lith. MSS.* p. 128.

† Postscript to Address to Brit. Assoc. *Report*, 1839, p. 36. It appears to have been the indications of this instrument which led Cavendish to his well-founded conclusion, that the cause of the absence of nitric acid, when hydrogen and air, instead of hydrogen and oxygen, are burned together, is the reduction of temperature below the combustion-point of nitrogen, occasioned by the excess of it present.

‡ *Unlith. MSS.* pp. 54—107.

§ *Lith. MSS.* pp. 108 and 146.

‖ Postscript to Address to Brit. Assoc. *Report*, 1839, p. 37.

¶ It will be shown further on, that Cavendish's experiments on air, involved the

In October, 1782, Cavendish resumed his inquiry into the cause of the production of nitric acid when oxygen and hydrogen are burned together; and his experiments with the oxygen obtained from plants exposed to sunlight* were made in October, November, and December of this year.† In the course of these experiments with oxygen from plants, the proportion of the gas was varied, and excess of oxygen was found to increase the production of acid. The early part of January, 1783, was occupied with similar experiments on mixtures, in various proportions, of oxygen from red precipitate and inflammable air, and led to the same result.‡ On the same day, apparently, January 12th, or at some period between that and the 18th, nitrogen was purposely mixed with a measure of hydrogen, and a little more than a combining measure of oxygen, and the resulting water was found to be evidently acid. These experiments were repeated on the 18th and 27th of the month. Similar experiments are also recorded out of their order, as made on January 3rd, 4th, and 24th.§ Trials are also recorded, as made with air from Turbith mineral, at p. 240, but no date is given. The previous sheet is dated July, 1783; but the Note-Book does not follow an invariable chronological order, for pages purposely left blank are occasionally occupied with experiments made at intervals of even two years from each other. Other researches, which were reported in the paper of 1784, are mentioned under various dates during 1783: thus, in March, we have the record of the "Examination whether air yields any nitrous acid, when phlogisticated by lime-liver of sulphur."‖ This is followed by an account of the investigation "Whether red precipitate contains any nitrous acid;"¶ and that by the description of the process for preparing oxygen from Turbith mineral,** and a record of the results obtained when oxygen thus prepared was detonated with hydrogen.†† A considerable space is then occupied with the account of experiments on the gas evolved when a mixture of nitre and charcoal is heated, and also on the gas yielded by the distillation of charcoal.‡‡

The conclusions to which those experiments conducted Cavendish, are referred to in his published paper of 1784, in his illustration of his views concerning nitrogen being a compound of nitrous acid and phlogiston.§§ These records are followed by the account of the observations on the action of sunlight on tinctures of green leaves, and on dephlogisticated spirit of nitre (colourless nitric acid) to which he specially refers in illustration of his views of the growth of vegetables.‖‖ They were made in December, 1783.¶¶ Pages 270-277 contain notes chiefly on the deflagra-

prosecution of a triple inquiry, viz. 1st, the analysis of air, including the demonstration of its constancy in composition; 2nd, the discovery of the general product of combustion in air and oxygen, which led directly to the discovery of the composition of water; and 3rd, the detection of the source of the nitric acid, which occasionally accompanied the water resulting from the combustion of hydrogen. These three main inquiries branched off into endless minor ones, and necessitated new investigations into the properties of all the substances employed. The interval we are considering was, to a great extent, spent in such an investigation into the properties of oxygen and nitrogen, as materially conduced to the explanation of the production of nitric acid.

* *Phil. Trans.* 1784, p. 131.

† *Lith. MSS.* p. 200.

‡ *Lith. MSS.* p. 210.

§ *Lith. MSS.* pp. 216, 217, & 218.

‖ *Unlith. MSS.* pp. 232—236; *Phil. Trans.* 1784, p. 141.

¶ *Unlith. MSS.* p. 237; *Phil. Trans.* 1784, p. 142.

** *Phil. Trans.* 1784, p. 132.

†† *Unlith. MSS.* pp. 240—242; *Phil. Trans.* 1784, p. 133.

‡‡ *Unlith. MSS.* pp. 244—265.

§§ *Phil. Trans.* 1784, p. 135.

‖‖ *Phil. Trans.* 1784, p. 147.

¶¶ *Unlith. MSS.* p. 266.

tion of nitre with tin and iron filings.* The succeeding pages, from 278 to 281, record observations on the quantity of fixed air discernible by lime-water. Page 282 refers to the evolution of inflammable air from zinc in caustic alkali. Pages 283-285 detail further experiments on the quantity of fixed air perceptible by lime-water. Pages 286-291 are on the distillation of red precipitate with iron-filings.† Pages 292-295 describe a process by which red precipitate was prepared, and the next four pages, 296-299, refer to experiments made on the combustion of sulphur and phosphorus, on March 6th, 1784. Pages 300 to 301 recount experiments on the preparation of dephlogisticated spirits of nitre,‡ made in April and November, 1783. This closes the register of experiments referring to the first series of experiments on air. Page 302 describes a process referred to in the second series of these experiments (those, namely, on the production of nitric acid by the combination of nitrogen and oxygen); and at 306 the record begins of the experiments made on the convertibility of common air into nitric acid by sending electric sparks through it, which are described in the paper published in 1785.§ The remaining dates all belong to 1784 or 1785.

Such were the experiments recorded by Cavendish up to the period when he read his paper to the Royal Society. I have reported them somewhat fully, comparing them with the index furnished by himself, as it is a point of importance in reference to the delay which attended the publication of his experiments of 1781, to show in what way he spent the interval between that period and January 15th, 1784. Dates are not attached to all the pages of his Note-Book, so that the exact period when each experiment was performed cannot be ascertained. As late, however, as January 4th, 1784, an experiment is reported on the deflagration of tin with nitre, which probably formed part of the researches into the rationale of the evolution of oxygen from melted nitre, referred to in the published paper. The latest certain date is December, 1783, when the experiments on the action of sunlight on vegetable tinctures and colourless nitric acid were made. The day of the month is not given in reference to the tinctures, but the acid is stated to have been "exposed for two or three days to a weak sunshine, about December 25th, 1783."‖ It thus appears that Cavendish did not *complete* the researches which he published in 1784, till at least Christmas,¶ 1783; and as his paper was read on the 15th of the succeeding January, and was therefore probably transmitted to the Secretary of the Royal Society some days before, and as from its length it must have occupied some considerable time in drawing up, Cavendish cannot be accused of having been slow in communicating his results to the public.

Such, then, are the contents of the MS. journal up to 1784. It will presently be referred to more particularly, when the question is under consideration; Was it a journal only of experiments, or of experiments and conclusions?

* These experiments are not *specially* referred to in the published paper, but it cannot be doubted that they form part of the series which led Cavendish to the conclusions that he published concerning the cause of the evolution of oxygen from melted nitre.

† These experiments were made partly with a view to test the truth of Priestley's declaration, that iron yields inflammable air when heated, the accuracy of which statement Cavendish doubted (*Phil. Trans.* 1784, p. 137); partly with a view to furnish the rationale of the evolution of oxygen from red precipitate, to which he makes so many references. (*Ibid.* pp. 142—144.)

‡ *Phil. Trans.* 1784, note, p. 148.

§ *Experiments on Air* (2nd series), *Phil. Trans.* 1785, p. 372.

‖ *Unlith. MSS.* p. 267.

II. The second document referred to, as throwing light upon the question before us, is Dr. Priestley's "Experiments relating to phlogiston, and the seeming conversion of water into air," read to the Royal Society, June 26th, 1783. Two statements occur in the latter section of it which treats of the conversion of water into air, in which Cavendish's researches are referred to. The first of these has been quoted already in full, so that a part only of it need be given here. "Still hearing," says Priestley, "of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the reconversion of air into water, by decomposing it in conjunction with inflammable air."* Then follows the detailed account of the repetition made with inflammable air from charcoal, and oxygen from melted nitre; (see ante, p. 286) which need not be quoted, except the following sentence,—"the result was such as to afford a strong presumption that the air was reconverted into water, and therefore, that the origin of it had been water."† At the close of his paper Priestley again refers to the subject, whilst describing an experiment of his own, which he says "cannot be explained so well on any other hypothesis [than that of the convertibility of water into air], any more than Mr. Cavendish's experiment on finding water on the decomposition of air."‡

These, unquestionably, are the passages to which Cavendish refers, when he states that in consequence of what he told Dr. Priestley, the latter "made some experiments of the same kind as he relates in a paper printed in the preceding volume of the Transactions."§ The acknowledgment that Cavendish's experiments preceded and suggested Priestley's, was thus made by the latter, before it could be claimed by the former. It may further be added, that in the abstract of Priestley's paper, drawn up by Mr. Maty, (Sec. R. S.,) and contained in the journal book of the Royal Society, it is stated that "these arguments received no small confirmation from an experiment of Mr. Cavendish, tending to prove the reconversion of air into water; in which pure dephlogisticated air and inflammable air were decomposed by an electric explosion, and yielded a deposit of water equal in weight to the decomposed air."||

Watt also added a note to his paper before it was printed, in which he says, "I believe that Mr. Cavendish was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired."¶ This statement is not accurate, as the friends of Watt and of Cavendish equally (though for different reasons) acknowledge, but its accuracy is immaterial. I refer to it only as showing that Watt believed that Cavendish had preceded Priestley in the performance of certain experiments with inflammable air and oxygen.

It thus appears from the testimony of Priestley and Watt; from the journal book of the Royal Society; from the public declaration of Cavendish at the period when he published his researches; and from the private record of his experiments at the period when they were performed, that before January, 1783, he made those observations on the production of water from the combustion of hydrogen and oxygen, which he adduced in his paper of 1784, as showing that water is a compound of these gases. It cannot be doubted, therefore, that the account given in Arago's Eloge of Watt, which represents Priestley's experiments as original, and as having preceded Cavendish's, is inaccurate.

* *Phil. Trans.* 1783, p. 426. † *Op. cit.* p. 427.
‡ *Ibid. ibid.* p. 433. § *Phil. Trans.* 1784, p. 134.
|| Quoted by Mr. Harcourt, *Brit. Assoc. Rep.* 1839, p. 44.
¶ *Phil. Trans.* 1784, note, p. 332.

It remains then only to inquire whether the experiments of the French chemists were of prior date to Cavendish's. But on this question there is no room for doubt. Monge* (ante, 353) frankly conceded that the English experiments were of earlier date than his own, for which he only claimed, as he was fully entitled to do, independence and originality. Lavoisier also acknowledged that Blagden had informed him of Cavendish's experiments before he performed his own (ante, 353).† But, according to his informant, he concealed the amount of information given him. The following is Blagden's letter in reply to Lavoisier, addressed to Dr. Crell, and published in his "Chemische Annalen" for 1786. I quote it at length, as translated by Mr. Muirhead,‡ because, apart from the light which it throws upon the date of Cavendish's experiments, it is regarded by the advocates of Mr. Watt as perhaps the document of most importance to them in disproving the priority of Cavendish's *conclusions* to those of Watt.

Translation of a Letter from Dr. Blagden, Sec. R. S. L, to Dr. Lorenz Crell. *Not dated.*

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

"In the spring ('Frühjahr') of 1783, Mr. Cavendish communicated to me and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us, that out of them he must draw the conclusion, that dephlogisticated air was nothing else than water deprived of its phlogiston; and *vice versâ* that water was dephlogisticated air united with phlogiston. About the same time ('um dieselbe Zeit') the news was brought to London, that Mr. Watt of Birmingham had been induced by some observations to form ('fassen') a similar opinion. Soon after this ('bald darauf') I went to Paris, and in the company of Mr. Lavoisier, and of some other members of the Royal Academy of Sciences, I gave some account of these new experiments and of the opinions founded upon them. They replied, that they had already heard something of these experiments, and particularly, that Dr. Priestley had repeated them. They did not doubt, that in such manner a considerable quantity of water might be obtained; but they felt convinced that it did not come near to the weight of the two species of air employed; on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in and united with the airs, and deposited in their combustion. This opinion was held by Mr. Lavoisier, as well as by the rest of the gentlemen who conferred on the subject; but as the experiment itself appeared to them very remarkable in all points of view, they unanimously requested Mr. Lavoisier, who possessed all the necessary preparations ('Vorrichtungen'), to repeat the experiment on a somewhat larger scale, as early as possible. This desire he complied with on the 24th June, 1783 (as he relates in the latest volume of the *Paris Memoirs*). From Mr. Lavoisier's own account of his experiment, it sufficiently appears, that at that period he had not yet formed the opinion that water was composed of dephlogisticated and inflammable airs; for he expected that a sort of acid would be produced by their union. In general, Mr. Lavoisier cannot be convicted of having advanced any thing contrary to truth; but it can still less be denied, that he concealed a part of the truth. For he should have acknowledged that I had, some days before, apprised him of Mr. Cavendish's experiments, instead of which, the expression "il nous apprit," gives rise to the idea that I had not informed him earlier than that very day. In like manner, Mr. Lavoisier has passed over a very remarkable circumstance, namely, that the experiment was made in consequence of what I had informed him of. He should likewise have

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* *Watt Corr.* (*Mém.* par M. Monge), p. 206; or *Mém. de l'Acad. pour* 1783, note, p. 79.

† *Watt Corr.* (*Mém.* par M. Lavoisier), p. 176; or *Mém. de l'Acad. pour* 1781, p. 472.

‡ *Watt Corr.* pp. 71—74.

stated in his publication, not only that Mr. Cavendish had obtained " une quantité d'eau très sensible," but that the water was equal to the weight of the two airs added together. Moreover, he should have added, that I had made him acquainted with Messrs. Cavendish and Watt's conclusions; namely, that water, and not an acid, or any other substance ('Wesen'), arose from the combustion of the inflammable and dephlogisticated airs. But *those* conclusions opened the way to Mr. Lavoisier's present theory, which perfectly agrees with that of Mr. Cavendish; only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston. Mr. Monge's experiments (of which Mr. Lavoisier speaks as if made about the same time) were really not made until pretty long, I believe at least two months, later than Mr. Lavoisier's own, and were undertaken on receiving information of them.

"The course of all this history will clearly convince you, that Mr. Lavoisier (instead of being led to the discovery by following up the experiments which he and Mr. Bucquet had commenced in 1777) was induced to institute again such experiments solely by the account he received from me, and of our English experiments; and that he really discovered nothing but what had before been pointed out to him to have been previously made out and demonstrated in England."

Cavendish also stated, in one of the additions made to his paper, that a friend of his (no doubt Dr. Blagden) "gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time, so far was M. Lavoisier from thinking any such opinion warranted, that till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."*

To neither of these representations did Lavoisier make any reply, and his account is at variance with that given by his colleague, La Place, in a private letter to De Luc, which has been brought to light by the publication of the Watt Correspondence. The letter was written on June 28th, 1783. In it La Place says, "M. Lavoisier and I have repeated recently before Mr. Blagden and several other persons, the experiment of Mr. Cavendish upon the conversion into water of dephlogisticated and inflammable airs, by their combustion; with this difference, that we have burned them without the assistance of the electric spark, by bringing together two currents, the one of pure air, the other of inflammable air. We have obtained in this way more than 2½ drachms of pure water, or which, at least, had no character of acidity, and was insipid to the taste; but we do not yet know if this quantity of water represents the weight of the airs consumed. It is an experiment to be recommenced with all possible attention, and which appears to me of the greatest importance."†

In the face of Blagden's, Cavendish's, and in effect La Place's, denial of the accuracy of Lavoisier's statements, it is impossible to accept his version of matters, even to the extent of acknowledging his experiments to have been original and independent, like those of Monge; and as the earliest date, moreover, to which his experiments go back, is 24th June, 1783, he cannot possibly contest priority with Cavendish, whose experiments had been repeated by Priestley in the spring of that year, before Lavoisier had heard of them.

Whatever, then, is doubtful in the Water Controversy, this at least is certain, that Cavendish was the first who observed that when given weights of hydrogen and oxygen are burned together in certain proportions, they are replaced by the same weight of pure water. The whole dispute, so far as priority is concerned, thus turns upon the date of Cavendish's conclusions, and if they can be shown to have been deduced

* *Phil. Trans.* 1784, p. 134. † *Watt Corr.* p. 41.

from his experiments at the period when they were made, the controversy is at an end. It will suffice, moreover, if they can be shown to have been arrived at, at any period before the time when Priestley repeated Cavendish's experiments, and gave the account of his repetition to Watt, on which the latter founded his conclusions. Priestley's paper, it will be remembered, on which Watt's letter was a commentary, was accompanied by a letter from the former, dated April 21, (1783),* so that to establish priority for Cavendish, his conclusions must be shown to have been drawn at least before that date; and as Priestley's experiments must have been made some time before that period, which marks the date of their completion, and the reduction of his conclusions to the written form in which they now appear, we must go back somewhat earlier in seeking for the precise time when Cavendish's conclusions were drawn. From a statement in the Watt Correspondence (p. 17) it appears that we must go back at least a month; for Watt was acquainted with Priestley's experiments as early as the 26th of March, 1783. Wedgwood was made acquainted with them on the 23rd of the month, (ante, p. 94.) Beyond these allusions there exist no means of limiting the date more precisely; but it is probable that Priestley repeated Cavendish's observations in the beginning of this month; for all the earlier dates of the Watt Correspondence refer to experiments on the conversion of water into air. Without, therefore, affecting greater precision, I shall for the present assume that the question for consideration is: Did Cavendish come to the conclusion that water is a compound of oxygen and hydrogen, before Priestley repeated his experiments in March, 1783? The advocates of Watt affirm that he did not, and they adduce in support of their view the following arguments.

1. They affirm that a passage in Cavendish's paper of 1784, referring to Watt, contains what is equivalent to a confession of obligation on the part of the former to the latter, which amounts to a concession of priority. If this allegation be true, the controversy is closed; for if Cavendish confessed himself indebted to Watt for his conclusions, all further proof of obligation is superfluous. Watt's letter of April 26, 1783, was not publicly read to the Royal Society till the 22nd of April, 1784, that is, several months after the reading of Cavendish's paper on the 15th of January of the same year.† No reference whatever was made to Watt in the latter essay, when it was read; but before it was printed, and after Watt's views were made public, Cavendish added the following passage: "As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston *deprived of part of their latent heat*, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. Watt says is true," &c.‡

The italics are not in the original; the remainder of the interpolation is occupied solely with a vindication of the phraseology which Cavendish employed in reference to heat, and has been given already in the abstract of his paper (ante, p. 156). This is the passage relied upon, as containing Cavendish's acknowledgment of Watt's originality, and, by implication, priority. Sir David Brewster says of it,—"That he [Cavendish] acted ungenerously to Watt, his best friends must admit,—for he has admitted it himself. The omission, in his 'Experiments on Air,' of all notice of

* *Phil. Trans.* 1783, p. 398. † *Phil. Trans.* 1784, p. 330.
‡ *Phil. Trans.* 1784, p. 140.

Mr. Watt and his theory, was unworthy of a philosopher; and he stands self-condemned, because he corrected the omission before he *printed* his paper."* Sir David's judgment proceeds upon the supposition that Cavendish was indebted to Watt's letter for the views which he published on the nature of water. But even if he had been (which he was not), it was impossible that he could have referred to Watt at an earlier period than he did, for the latter had debarred this by withdrawing his letter to Priestley, which was not publicly communicated to the Royal Society till Cavendish's paper had been read. Up to this period it was a *private* letter, the public reading of which its author spontaneously desired might be delayed to an indefinite period. No one, therefore, had a right, even if aware of its contents, to make public reference to it, nor was it possible to discover by any public or authoritative document, what portion of it Watt wished to disavow. Had Cavendish, for example, referred to Watt as believing that water by distillation in earthen retorts could be converted into atmospheric air, the latter might justly have denounced him for quoting a private letter of April, 1783, as showing his opinions in January, 1784. No one, in truth, can blame Cavendish, because he did not refer to the contents of a letter which by its author's express wish was not made public.†

Had Cavendish, nevertheless, been personally acquainted with Watt, and conscious, moreover, that he was indebted to him for his conclusions, he would have acted dishonestly, not to say ungenerously, had he not privately communicated with him before publishing his paper. But these illustrious men were as yet complete strangers to each other, and no one will affirm that if Cavendish's conclusions were the fruit of his own unaided researches, he was under the slightest obligation to acknowledge that another had drawn similar conclusions from a repetition of his experiments; and that the reference to Watt quoted above, admitted no obligation to him, will be apparent, I think, to every impartial reader. The passage, in truth, which should be consulted in full, contains a denial rather than an acknowledgment of obligation to Watt; inasmuch as it is is mainly occupied with the expression of a refusal to adopt his views. Watt, as was natural to one who had made the laws of heat a subject of deep study, attached great importance to the part which caloric played in transmuting inflammable air and oxygen into water. He gave great prominence, accordingly, to the discussion of this in his paper; whilst Cavendish, who was a disbeliever in the materiality of heat, made no reference to its evolution as essential to the production of water from its elements, and referred to Watt's paper simply to explain why he did not.‡

2. It is contended by the advocates of Watt, that Cavendish's Note-Book contains no record of any conclusion having been drawn by him from his experiments on the synthesis of hydrogen and oxygen, and that therefore he cannot be held to have come to a conclusion regarding the nature of water before Watt's letter was written.

* *North Brit. Rev.* Feb. 1847, p. 505.

† *Phil. Trans.* 1784, p. 330.

‡ It seems well to notice here, that Cavendish's refusal to adopt Watt's language in reference to heat, was not an opinion expressed then for the first time, as if with the purpose of accounting for an omission in his own paper or lessening the merit of his rival's views. In 1783, Cavendish, in his commentary on Mr. Hutchin's observations on the freezing of mercury, had stated at length, that he dissented from Black's views on the latency of heat, and preferred those of Newton, according to which heat was a state or condition of matter, and not a substantial entity.

When Mr. Harcourt published his lithographed fac-simile of part of the Note-Book, he observed regarding it, "Few rough day-books of experiments would tell their own tale with such certainty and distinctness as these; in few, could the consecutive course of reasoning be traced thus clearly from the experiments themselves: there cannot remain a doubt on the mind of any one who reads them, that in January, 1783, Cavendish had not only discovered the certain fact that oxygen and hydrogen in definite proportions form water, but likewise the strong probability that oxygen and nitrogen form nitric acid, two months before Priestley began to experiment, and Watt to speculate, on the notice which Cavendish had given the former of the composition of water, and four months before Lavoisier received from Blagden a similar notice."* To this statement Lord Brougham replies, "I must add, having read the full publication with fac-similes, Mr. Harcourt has now clearly proved one thing, and it is really of some importance. He has made it appear that in all Mr. Cavendish's diaries and notes of his experiments, not an intimation occurs of the composition of water having been inferred by him from those experiments earlier than Mr. Watt's paper of Spring, 1783."†

On the same subject Sir David Brewster remarks, "The publication of the lithographs of Mr. Cavendish's experiments upon air, which commenced in July, 1781, are regarded by his advocates as establishing his claim to the discovery of the composition of water."‡ He then states his dissent from this view, observing, "It is indeed beyond all belief that he [Cavendish] could have drawn any conclusions, or formed any theory, seeing that Mr. Hudson, to whom the Duke of Devonshire had entrusted the whole of his papers, has declared that he does not find in these journals of the experiments, anything more than the simple statement of the facts without any casual mention of theoretical opinions."§ On Mr. Hudson's statement Mr. Muirhead comments as follows: "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."‖ Lord Jeffrey is much more guarded in the expression of his opinion on the value of the Note-Book, than the writers just quoted, but he also is "disposed to think that there is more in the absolute omission of any notice of this conclusion in the full contemporary journal of the experiments, than Mr. Harcourt is willing to allow."¶

The advocates of Watt have not overstated matters when they affirm that Cavendish's Note-Book does not contain any announcement of his views concerning the nature of water. Mr. Harcourt, indeed, never asserted that it did, but he, as well as Dr. Peacock and Dr. Whewell, have perhaps not sufficiently guarded their estimate of the Note-Book as conclusive of the question of priority between Cavendish and his rivals. A just criterion by which to test what kind and amount of information the Note-Book can give, may be found in the answer to the question: Would the diary of experiments *alone* enable us to ascertain what

* *Report Brit. Assoc.* 1839, p. 38.

† *Lives of Men of Letters, &c. Life of Watt,* p. 401.

‡ *North Brit. Rev.* Feb. 1847, p. 493.

§ Ibid. p. 495.

‖ *Watt. Corr.* p. xxxvi.

¶ *Edinb. Rev.* Jan. 1848, p. 125.

Cavendish's views concerning water were? To this I think there can be but one answer: It would not. It is only by a comparison of the manuscript journal with the published paper, that we can make the former serviceable in illustrating Cavendish's views; but the absence of records of conclusions is no proof that conclusions were not drawn. If the friends of Cavendish, indeed, have built too much upon the contents of his Note-Book, the friends of Watt have as unwisely undervalued it. The use they make of it, is to say, that had Cavendish drawn conclusions regarding the nature of water from his experiments, they would have appeared in his Note-Book, but as it contains no conclusions, it is incredible that he can have drawn any. Yet Watt's advocates themselves acknowledge that Cavendish *had drawn* conclusions from his experiments (whether borrowed from Watt or not, does not at present concern us) before he wrote his paper of January 15th, 1784. The Note-Book, however, extends to 1785, yet there are no conclusions respecting water in those portions of it belonging to 1784 and 1785, any more than there are in those referring to the period between 1781 and 1783. Lord Brougham, as we have seen, says that there is no intimation of Cavendish having drawn his conclusion earlier than Mr. Watt's paper of Spring, 1783, but in truth there is no intimation in the journal of his having drawn *any* conclusion at *any* time. Mr. Muirhead observes that Cavendish in his Note-Book never uses any expressions which could imply a 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. From this it might be supposed that Cavendish had implied some approval of Warltire and Priestley's views, and that he had suspiciously omitted a reference to the combination of the two gases: whereas in reality the Note-Book contains no expression of opinion of any kind, concerning the origin of the water. In short, the value of the MS. Note-Book as evidence for or against Cavendish's priority, must be determined by first ascertaining what the purport of the book was. If it can be shown that it was intended to receive the record of conclusions as well as of experiments, then the absence of the former in the case of water, cannot but be regarded as fatal to the view that Cavendish had interpreted his experiments at the period when he registered them. But if the opposite shall appear to have been the character of the journal, then its silence as to conclusions will be a characteristic merit, not a suspicious defect. The application of a twofold criterion will enable us to determine what in reality was the character of Cavendish's journal.

Firstly. Does it, *after the period* when he certainly had formed his conclusions concerning water, contain an account of these?

Secondly. Does it contain the conclusions which he certainly drew from the experiments on *other* subjects which are recorded in it?

To both these questions the answer must be in the negative. This has been stated in reference to the first already, but admits of fuller demonstration than has yet been given. The journal is not a bound volume filled in continuously like a diary, so that unless conclusions were registered along with the experiments which led to them, they must be separated from them by references to other matters; and if formed later in time must appear under a different date. The volume consists of a succession of unsewn and single sheets, each of which is marked with the four consecutive numbers, denoting the place of its pages in the series. Many of the pages are only partially occupied with writing, and several have nothing on them but a number. The same page, also, not unfrequently contains records belonging to different years, as the reader may see by consulting Mr. Harcourt's lithographed fac-simile of the Note-Book. Had Cavendish accordingly designed to enrol his conclusions in his journal, he could easily have done so in the blank spaces which still remain unfilled. Yet whilst the paper of 1784 is a running commentary on the experiments detailed in the Note-Book, none of the conclusions contained in the former concerning water appear in the journal, although its index was not completed till at least late in the year 1785, and therefore long after Cavendish had published his views concerning the composition of water.

Had Cavendish, moreover, entertained any deceitful project of antedating his conclusions, he could most easily have done this, for any of the sheets could have been replaced by a new one with fictitious dates, without disturbing the remaining leaves of the journal. And without taking even this trouble, the insertion of a few lines on one of the blank pages would have been sufficient to record his views of the nature of water. Thus page 127 of the MS. (see lithograph fac-simile) contains experiments made in 1781; on the combustion of hydrogen and air, and the appearance of water as a product of it, and half of page 128 contains a similar experiment made in 1782; whilst the rest of the page and the whole of page 129 are empty; so that if the conclusion at which Cavendish had certainly arrived in June, 1783, when Blagden reported it to the French chemists, had been engrossed on either of the last-named pages, it would have borne the date of November 18, 1782. The omission, however, of any such conclusion unequivocally proves that when Cavendish indexed his journal in 1785, he deliberately left nothing on its pages, but the simple record of his experiments with hydrogen and oxygen.

Could it nevertheless be shown, that the various experiments not directly referring to water, were recorded in connection with the conclusions ultimately published as deduced from them, then it could not but appear singular that a solitary exception had been made in the case of observations on the synthesis of the elements of water. It is necessary, therefore, to apply the second criterion proposed above. To do this it is only necessary to compare the two series of Experiments on Air, as well as the paper on the New Eudiometer,* with the MS. journal. Such a comparison will show, that scarcely any of the conclusions contained in the paper are to be found in the Note-Book. Some conclusions there certainly are, but very few, and these not the great generalizations afterwards published, but only the interpretations of single phenomena. Cavendish's journal, in truth, was neither a day-book containing only the bare statement of the experiments which he performed, nor the mere register of a completed inquiry, and the conclusions to which it had led. It was such a journal as every working chemist, I may venture to say, is accustomed to keep; not the mere jotting down of weights and measures, and and other numerical quantities; nor the simple record, in the fewest words, of the phenomena observed during the progress of an inquiry, and still less a detail of all the motives which led to a particular experiment being tried, or of all the conclusions which were drawn from it, or of all the changes of opinion which occurred during a lengthened and difficult investigation. The peculiar character of such a journal, inter-

* The portion of the Note-Book lithographed by Mr. Harcourt refers almost entirely to the first series of "Experiments upon Air." The portions omitted contain the experiments upon which the conclusions contained in the two other papers mentioned above, were founded.

mediate between a day-book,* and a completed memoir or essay, is the full account it renders of phenomena which are described by one fresh from witnessing them, and by the multiplication of numerical and other observations, which in the end are only partially made public, but which never appear excessive to an experimenter, at the period of his researches, when his object is to fortify *himself* in his convictions. It is quite consistent with such being the prevailing nature of a journal, that it should occasionally and sparingly refer to motives and conclusions. This is a point on which only those who have themselves engaged in scientific researches, or are familiar with the habits of those who prosecute them, can fully judge, and I leave the matter to their decision. I will add, however, that I believe that every working laboratory would be found, at any period when it might be visited, presenting the phenomenon of an observer freely explaining to his friends and pupils the motives which led him to engage in a particular research, and the conclusions to which he had come, whilst scarcely any and often no reference to either motive or conclusion would be found in his note-books, although his views would finally re-appear more or less modified in the record of the completed inquiry. Let us test Cavendish's journal in this way.

The three greatest generalizations to which his experiments on air conducted him, were (1) that the atmosphere is constant in composition; (2) that water consists of dephlogisticated air and phlogiston; (3) that nitric acid is the product of the union of phlogisticated air (nitrogen) and oxygen. The Note-Book, however, is not more prolific of conclusions in reference to the first and third of these discoveries than to the second. No one, for example, would discover from it what Cavendish's motive was in trying the majority of the eudiometrical experiments recorded in its pages, nor what conclusions they were found to justify. Here and there we get glimpses of the aim of the journalist, and sometimes we are told both his motives and conclusions, as at page 109, which is entitled "Comparison of the experiments on the diminution of dephlogisticated air with *Scheele's opinion.*" But such statements are the exception: thus we find no description of his new eudiometer, but only references to its employment, and we should never discover from these, that Cavendish had come to the conclusion that the atmosphere has the same composition day by day, in town and country, when containing odorous matter and when free from it. Various experiments also are recorded of which we can now only surmise the object, as neither the motive which led to their performance, nor the conclusion which they were thought to justify, are chronicled in the Note-Book or published paper. Such, for example, are the trials on the "*vis inertiæ* of phlogisticated air," (MSS. pp. 80 to 82,) and those made to determine whether there is any penetration of parts on mixing common and inflammable air (MSS. p. 113), and those referring to the persistence of common and inflammable air in a state of uniform mixture (Ibid. ibid). The *immediate* conclusions drawn from those experiments are stated, but they are recorded as isolated observations, and although it is scarcely possible to doubt concerning the motives which prompted their trial, or the use which was made of them as parts

* In proof of the truth of this, it may be mentioned that among Cavendish's papers remain the original jottings or "*minutes*," as he calls them, of some of his observations; from these, probably, the Note-Book was drawn up. The following allusion to these *first* notes, occurs in his Journal in reference to the height of the thermometer in an experiment on the expansion of common air. "The number is set down 85 *in the minutes*, but must certainly be a mistake for 80." (*Unlith. MSS.* p. 369.)

of a continuous systematic inquiry, no professed use was made of them in the published essays, nor is their signification expounded in the Note-Book. Similar remarks apply to the notes on which the paper of 1784 was founded: thus at page 115 we have the heading, "Explosion of Inflammable Air by electricity in glass globe to examine Mr. Warltire's experiment." The result of this repetition we know from Cavendish's paper to have been such as to induce him to deny the truth of Warltire's declaration, that a loss of weight attends the detonation of mixtures of inflammable air with common air or oxygen in shut vessels; yet we should never be certain of this from the Note-Book, for we have it stated on four different occasions, that the globe seemed to lose in weight, whilst in others it did not. Again; we know from the published paper that oxygen from red lead, from turbith mineral, and from plants, was used by Cavendish, with a view to discover whether the nitric acid which appeared in certain of his explosions, was derived from basic nitrate of mercury contained in the red precipitate from which his first specimens of oxygen were prepared; and that he attached special value to the oxygen from plants as most certain to be free from acid. Nothing of all this, however, appears in the Note-Book. The experiments merely are given as if they had been isolated inquiries, and the conclusion to which they led, concerning the uniform production of nitric acid in certain circumstances, does not appear. Again; we know that Cavendish varied the proportions of hydrogen and oxygen which he detonated together, and that he came to the conclusion that in ordinary circumstances nitric acid appears when the oxygen is in excess; and further, that he believed the source of the nitric acid to be nitrogen present as an impurity, and that he put this idea to the proof by adding a little nitrogen to a mixture of hydrogen with excess of oxygen, and then exploding it, when his anticipation was verified. We know also, that he inferred from these observations that nitrogen is a compound of phlogiston and nitrous acid, and that he considered this view to be corroborated by the results which he obtained when he heated charcoal and nitre together. Yet although all those experiments are fully recorded in the Note-Book, the views which they were undertaken to test, and were thought to justify, are not stated, and no one probably would have surmised the purport of some of the trials, such as those with charcoal and nitre, or have felt certain what conclusion *any* of them justified.

Many more examples illustrating the same thing might be given, but these may suffice. If points on which Cavendish dwells so fully in his published paper, as the source of the nitric acid which appeared during his repetition of Warltire's experiment, and the nature of nitrogen, are not expounded even briefly in his journal, there is nothing at all singular in no account being given of the conclusions which were drawn from those detonations in which *no* nitric acid appeared.

There is nothing, then, suspicious in the circumstance that Cavendish has not stated his views concerning the nature of water in his Laboratory Journal. Nor can any weight be attached to the declaration, that the enormous importance of such a discovery as that of the composition of water, might be expected to induce him who made it to record it however briefly, even in his roughest note-book; for if it can be shown that his journal was not intended to contain accounts of discoveries however important, we can find no fault with its omitting all reference to the theory of the composition of water. It was dealt with as its author's other theories were. Three of these, which he deemed of the highest importance, were, 1. That the universal product of the phlogistication of air is not carbonic acid. 2. That the universal product of phlogistication is water. 3. That nitrogen is a compound of phlogiston and nitric acid. Yet the journal may be searched from end to end, without any statement being found of any one of these conclusions. Nor need we feel surprised at this. Cavendish was like a merchant with his day-book, journal, and ledger: the minutes corresponded to the first; the note-book to the second; and the published paper to the third. We might as well blame the merchant for not recording in his journal that he had made a profitable speculation, although this was most clearly indicated in his balance sheet, as blame Cavendish for not recording in his note-book those generalizations which he purposely left to be announced in his published paper. If any one, moreover, is disposed to add, that as a very fortunate merchant is likely to talk or hint of his success long before he balances his accounts, so Cavendish might be expected to give some utterance to his satisfaction at making a great discovery long before he gave a detailed exposition of it in a formal essay, I will say in reply, that Cavendish did refer to his discovery of the nature of water before he published it, and this to more than one person. The discussion of this, however, belongs to another section of our argument, where it will be considered.

Cavendish's Journal, then, is not necessarily a document of no importance to our present inquiry, because it does not contain the generalizations which he ultimately based upon it. What, then, is its value? It may be compared, I think, to a map or chart laid down by a traveller, who has been engaged in exploring a new and unsurveyed territory. It lies before us, unaccompanied by the designer's journal, which would explain to us what object he had in his journey; why he directed his steps in one direction rather than another; what false movements he made, and what important goals he succeeded in reaching. But no one who acknowledges (and all competent judges do acknowledge) that the designer of the map was a man of rare experience and sagacity; possessed of all needful accomplishments; and before the date of this map, universally regarded as distinguished by unusual modesty and unblemished integrity; will believe it possible that this private chart, which has only accidentally come before the public eye, can be the blind record of an aimless journey. And even when we cannot discover why he turned to the east rather than to the west, or to the north rather than to the south, we will not doubt that some good reason justified the change of direction. Should, moreover, a record of the traveller's progress afterwards appear, which recounts the steps of his journey from point to point, as those are laid down in the map, it will be difficult to resist the impression, that the dates inscribed upon the chart as marking his daily progress, denote with more or less precision the *periods at which* he made the discoveries, which he connects with these dates in the completed record of his journey. And this, in fact, is the conclusion to which all the critics of the Water Controversy have come, in reference to the map taken as a whole. In other words, when Cavendish is found stating in his published paper a particular conclusion, such as, that sunlight "enables one body to absorb phlogiston from another," and cites, in illustration of this, the effect of sunshine in bleaching vegetable tinctures, in rendering nitric acid yellow, and in reducing salts of gold; whilst experiments of different dates registering the observation of these phenomena, in the course of researches made purposely to develop them, are recorded in his journal; few, if any, will affect to deny, that his belief in the dephlogisticating power of sunlight, dated at least from the period when he *completed* his experiments. The number of separate researches, however, chronicled in the journal, is very large; and the immense majority of them have no conclusions connected with them in that record, whilst the clearest interpretation is put upon each of them (which is referred to at all) in the published paper. In these circumstances no impartial critic can avoid the conclusion, that a definite interpretation was, sooner or later, put upon each research, at or near about the period when the latest experiment of each series of observations was performed. If any other view, indeed, were entertained, it would lead to the incredible supposition, that an immense number of most significant researches, prosecuted by one of the greatest natural philosophers, taught him, at the period when they were made, nothing, or almost nothing, and remained a sealed book to himself for years, till all at once, or within a very short period, the whole of them suddenly acquired a meaning, and assumed a shape, which have made them a model for later inquiries. All the critics of the Water Controversy would reject a supposition so extravagant as this. Yet if they do so, they must concede that the *onus probandi* lies upon them if they deny that the probabilities are all in favour of Cavendish, when he in effect declares that he was as quick in furnishing an interpretation of his experiments on the production of water from its elements, as in expounding the significance of the other researches recorded in his journal. The sagacious interpreter of ninety-nine phenomena may be safely believed, when he declares that he had interpreted a hundredth phenomenon, much more significant than many of those the meaning of which he is acknowledged to have seen. I apprehend, therefore, that by all those who do not set out with the hypothesis, that Cavendish was a dishonourable man, (and with such I enter at present into no argument,) the circumstance that his experiments on the production both of pure water and of acid water from the detonation of hydrogen and oxygen, were completed in January 1783, will be regarded as strongly favouring the belief, that his conclusions as ultimately published are at least of as early a date. More than this I am not anxious to contend for here; but if this can be established, it is idle to affirm that his journal does not testify in his favour, or that it testifies against him. And in truth, although the majority of the advocates of Watt have professedly rejected the evidence of Cavendish's journal, they have solicitously repelled the conclusions which his advocates have drawn from its contents.

In reality, therefore, whatever may be said, it is felt upon both sides, that Cavendish's Note-Book is a very important document. His opponents accordingly, sensible of this, have struggled hard to represent the laboratory journal as testifying only to certain phenomena having been witnessed, whilst his advocates insist on adducing it in evidence of conclusions also having been drawn. This question, which may be best disposed of here, has arisen in the following way. Mr. Harcourt has contended in the postscript to his address, that "the experiments which Cavendish made in the summer of 1781, not only necessarily involved the *notion*, but substantially established the *fact* of the composition of water."* On this statement, Sir David Brewster remarks, "The advocates of Cavendish, thus driven to the wall, take refuge in the allegation that the experiments of 1781 involve the inference. Were this the case, the history of science would require to be re-written. The ex-

* *Brit. Assoc. Rep.* 1839, p. 23.

perimenter would thus enter the niche of the philosopher, and the highest efforts of intellectual power would cease to be appreciated."* To the same effect Mr. Muirhead remarks, "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 [the production of water from its elements] 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."† Lord Jeffrey, with judicial impartiality, takes a middle view of the validity of Mr. Harcourt's argument. "We do not deny," says he, "that there is at first sight something plausible and taking in this view of the matter, especially when addressed to a generation which has always been familiar with the conclusion, and with the universal assent of mankind in the sufficiency of the evidence referred to. Yet it requires but a moderate acquaintance with the actual history of the progress, even of the most obvious truths, and of the tenacity and vitality of prejudices and errors, to make us cease to wonder at the incredulity with which what is at last felt to be demonstration is often at first received, or at the distrust with which even the authors of great discoveries have often regarded their own achievements."‡ On this contested point I would remark that there is justice in the views alike of Mr. Harcourt and of his impugners. Two separate propositions, in truth, have been confounded in the discussion; the one that Cavendish's experiments would have led every one to Cavendish's conclusion; the other, that they led Cavendish to his conclusion. If Mr. Harcourt intended to maintain the first of these propositions, which I apprehend he did not, the objections of Watt's advocates are well founded. It would be idle to affirm that an experiment is self-interpretative, even to him who appreciates its phenomenal significance. There is no phenomenon, in truth, of which philosophy has exhausted, or we may safely say will exhaust, the full meaning. How many lessons a drop of water would teach him who should observe *all* the properties it possesses, no wise man would venture to guess, but the wisest would acknowledge that an omniscient observer of the whole of its characters would learn from it much that it has not taught us, perhaps more than it has yet taught all the generations of men. This remark applies to every phenomenon without exception, with which the physical sciences deal. The converse, no doubt, is in a sense equally true. There is no phenomenon which does not to some extent interpret itself. All men philosophise on all about them, and learn something from everything they see; so that after all the question must ever be, what and how much has each learned from the things he has witnessed; and if personal testimony be wanting on this point, we can only hazard surmises. Had we nothing, therefore, but Cavendish's Journal, from which to argue, we could only say that his experiments must have taught him something, but what did not certainly appear; although even then we should feel justified in asserting, that he was more likely than most men to have been largely instructed by them. It is quite certain, moreover, that others besides Cavendish witnessed in as great perfection as he did the phenomena which are supposed by his advocates to have instructed him, and nevertheless, drew no precise conclusion or drew a false one from them. Sir David Brewster and

* *North Brit. Rev.* Feb. 1847, p. 495. † *Watt Corr.* p. xciv.
‡ *Edinr. Rev.* Jan. 1848, p. 128.

Mr. Muirhead have referred to Priestley, Warltire, Macquer, and Sigaud de Lafond as having been in this predicament. But these cases are not in point, for none of those observers witnessed phenomena which would have justified the conclusion which Cavendish's experiments did. Monge's observations, however, were nearly identical with those of Cavendish, as we have already seen, yet Monge could not decide which of two conclusions to deduce from them. This fact is of itself sufficient to show that Cavendish's experiments were not self-interpretative in the sense of compelling the conclusion which he deduced from them. On the other hand the implicit endeavour of the advocates of Watt to show that he displayed an extraordinary sagacity in putting the right interpretation upon Cavendish's experiments as repeated by Priestley, must be qualified by a reference to the fact, that Cavendish's conclusion quickly carried conviction to the minds of Lavoisier, La Place, and Meusnier, as well as to many other philosophers both in England and abroad, not excepting even Priestley, who first tested their accuracy, as we shall presently see. In short, whilst it must be unreservedly denied that Cavendish's experiments could not fail to lead any one to the conclusions all now connect with them, it must with equal plainness be contended that they were eminently significant and suggestive.

It is quite a tenable proposition, accordingly, that Cavendish's experiments taught Cavendish something; and that they taught what he affirms they did teach him. If we credit, in truth, the account which he gives of his motive for exploding hydrogen and oxygen together, we shall find a very slight additional exercise of faith sufficient to make us believe, that his researches led him to the discovery which he professed to have reached by means of them. His own account of matters in his published paper, (which the records of his journal confirm,) is, that whilst engaged in an inquiry into the products of combustion and oxidation, and anxious to discover what occasioned the diminution in volume which air underwent when deoxidized, he received the account of Warltire and Priestley's experiments, in which a dewy deposit was observed to follow the explosion of inflammable air with common air or oxygen.

He refers to these experiments as twofold, the first made with a copper vessel, and supposed to show that heat is ponderable; the second tried with a glass globe, and bringing to light the deposition of dew. The first experiment he dismisses in a few words as one which did not succeed with him; of the second he says, that "as it seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely." It thus appears that the great object of his inquiry was to discover where that portion of the air went, which lost its elasticity, and seemed to disappear when bodies were oxidized in confined portions of it. That "the air thus lost or condensed" was not annihilated, but had passed into some new physical state, or into a condition of combination in which all its ponderable matter was still present, was manifestly with Cavendish a fixed conviction from the first; so that the special question he aimed at answering was, what was the particular substance or substances in which the lost air existed in a state of condensation. That the lost air was changed into carbonic acid in all cases was the prevailing opinion when he commenced his researches. He showed that this was a mistake in so far at least as many phlogisticating (oxidable) bodies were concerned. He proved further, that the condensed air had not passed into the state of either sulphuric or nitric acid, and he was on the look-out for some other body distinct from those acids, in which the lost air might be shown to exist, when Priestley and Warltire's observation of the appearance of moisture when inflammable air and common air were burned together, so arrested his attention that he resolved to repeat it with the utmost care. No one, I think, can question that it was the appearance of dew, where the prevailing theory of the day taught that carbonic acid should appear, and where it was possible, as Cavendish evidently believed, that sulphuric or nitric acid might have appeared, which induced him to repeat the observation with so much care. Priestley and Warltire had in effect reported two things; the one that so much air disappeared; the other that so much water made its appearance; and it was the relation of the one phenomenon to the other, which Cavendish thought it so well worth while to examine more closely. In truth his statement seems most plainly to imply, that before he made any experiments on the subject, he suspected that the water which appeared in Warltire's experiments, was the substance into which the lost air had passed, and in which it would be found. On this view his experiments have one and all the greatest significance, and are most happily contrived, so as to discover what connexion subsisted between the disappearance of gas, and the appearance of water. Unless, indeed, the advocates of Watt will contend that Cavendish's experiments were aimless trials, prompted by no motive, and pregnant with no conclusion, they will find it very difficult to explain why he made such observations as he did, unless the object he had in view was the one I have stated. They have sought to show that it was the problem of the ponderability of heat, which mainly interested him; but this view is totally untenable, because—

1. Cavendish was a disbeliever in the materiality of heat, and therefore, we may be certain, in its ponderability.

2. He explicitly affirms that the apparent demonstration which Warltire had furnished of the subjection of heat to gravity had not for him, even if confirmed, more than a secondary interest; the appearance of dew in glass vessels, not the loss of weight in copper ones, having chiefly fixed his attention.

3. Far less elaborate experiments than those he tried would have been sufficient to determine whether Warltire's view as to heat being heavy, was true or not. Warltire did nothing more himself than fill a globe with a mixture of common and inflammable air, weigh the globe, send an electric spark through its contents, and then weigh it again; and Cavendish did not need to do more in repeating the supposed *experimentum crucis.* He tells us, however, in his published paper, "that in *all* the experiments the inside of the glass globe became dewy as observed by Mr. Warltire, but not the least sooty matter could be perceived. Care was taken in *all* of them to find how much the air was diminished by the explosion, and to observe its test." From his journal, moreover, it appears that he tried even more than this; for though he did not publish the fact in his paper, he determined the specific gravity of the residual, or uncondensed gas, after each explosion. (Lithograph MSS. pp. 116—118.) The three points thus determined, viz. 1. the diminution in volume which the electric spark occasioned in mixtures in various proportions of inflammable and common air; 2. the test of the uncondensed or residual gas, *i. e.* the amount of oxygen (if any) remaining in it; 3. the density of the unburned gas,—were not required, and their determination was a most needless waste of labour, if the ponderability of heat was the only problem which the experiments were intended to solve. The

fact that such determinations were made, is of itself proof sufficient that Cavendish had some other object in view; whilst, on the other hand, they were exactly such experiments as an observer would try whose object was to ascertain what connexion subsisted between the appearance of water and the disappearance of air. In proof of this, it may be noticed that Cavendish was in advance of the great majority of his contemporaries in a knowledge of the composition of atmospheric air. He knew that it was constant in composition; that it contained between $\frac{1}{4.8}$th and $\frac{1}{5}$th of its volume of oxygen. He was certain, therefore, that every specimen of common air, when completely phlogisticated, (deoxidised) would undergo a diminution in volume of nearly $\frac{1}{5}$th. He first, therefore, observed the reduction in bulk which a particular mixture of hydrogen and air underwent when the electric spark was transmitted through it. This gave him the amount of *hydrogen* and *air* which had *together* disappeared as gases, or lost their elasticity. He then tested the residual gas in his nitric oxide eudiometer, and thereby discovered how much *oxygen* had lost its elasticity during the combustion. This result he further checked by determining the specific gravity of the residual gas. Proceeding in this way, and mingling the hydrogen and air in various proportions, he quickly found one ratio as the mean of six trials, in which the volumes were such that the hydrogen exactly, or nearly exactly, phlogisticated or deoxidised the air, so that the latter was diminished in volume $\frac{1}{5}$th, and the residual air was found free from oxygen by the test of nitric oxide, and answered to the characters, and had the specific gravity of nitrogen. A table exhibiting these results, except that no reference is made in it to the density of the residual gas, is given in the published paper;* and a similar table will be found in the Journal (lithographed MSS. p. 119). From this it appears that, in July or August, 1781, he had ascertained that hydrogen, like other oxidable bodies, diminishes air by $\frac{1}{5}$th of its volume, and that the quantity of hydrogen required to effect this is (nearly) double the volume of the oxygen which it withdraws.†

Such are the only experiments recorded by Cavendish in his Journal, or referred to in his published paper in demonstration of the quantities of gas which disappeared during the combustion of hydrogen and air. They taught him that a given measure of hydrogen will always remove a measure half as great of oxygen from the air in which it is allowed to burn; and that, therefore, if we wish to deoxidise a measure of atmospheric air completely, we must burn it along with $\frac{2}{5}$ths of its volume of hydrogen. He had thus settled one-half of the question in order to solve which he undertook the repetition of Warltire's experiments. He knew *how much* air disappeared (namely, $\frac{1}{5}$th) when it was phlogisticated by hydrogen, and *what part* of the air disappeared, viz. its dephlogisticated part, or oxygen; and at every explosion he saw this disappearance of oxygen followed by the appearance of moisture or dew. The nature of this dew had then to be examined; and accordingly the account of the globe experiments in his Journal is followed by the record of those combustions of hydrogen and air in a glass cylinder, which he tells us in his paper were performed with a view to obtain a sufficient quantity of the dew for analysis. In this experiment, we find him applying the information which the globe detonations had given him, for he causes the

* *Phil. Trans.* 1784, p. 127.

† Cavendish did not represent the combining measure of hydrogen as *exactly* double that of oxygen, which it is, but he came very near this ratio.

gases to meet in the proportion of one measure of hydrogen to $2\frac{1}{2}$ times that quantity of air; and the liquid which condensed he refers to twice by the name of water, and describes its analysis as related in the published paper.* This is the only experiment referred to in his "Experiments on Air," as demonstrating the production of pure water from the combustion of hydrogen and atmospheric air.†

Such, then, were the experiments made in 1781 and 1782, from which Cavendish inferred in his published paper of 1784, "that when inflammable and common air are exploded in a proper proportion almost all the inflammable air, and near $\frac{1}{5}$th of the common air, lose their elasticity and are condensed into dew," and "that this dew is plain water, and consequently that almost all the inflammable air, and about $\frac{1}{5}$th of the common air are turned into pure water."‡

When all that has been stated is considered, it cannot be regarded as an extravagant proposition that Cavendish's experiments had led him to the conclusion just quoted, long before he published it. There may be

* *Phil. Trans.* 1784, p. 129.

† In the page of the journal where this experiment is recorded (*Lith. MSS.* p. 128), a second experiment of the same kind is referred to, of later date, viz. Nov. 18th, 1782, to which, as well as to the original trial, Sir David Brewster refers (*North Brit. Rev.* Feb. 1847, p. 494) as inconsistent with the belief entertained by Mr. Harcourt, that Cavendish could, at the earlier date, have "established the composition of water," because "the equality of the water and the gases was not and could not be measured, and the water was not absolutely pure," and also because "if he [Cavendish] had concluded from his experiment in 1781, that water was not a simple substance, but composed of two gases, why was he repeating and recording such experiments as this sixteen months after?" "The deduction," continues Sir David, "is unavoidable, and we hold it to be *proved by this fact alone*, that he had drawn no such conclusions." Sir David, however, has overlooked that Cavendish settled the question of equality of weights between the gases and the water solely by his globe experiments, and that those made with the cylinder were undertaken only with a view to procure a large quantity of water for analysis. No attempt, accordingly, was made to render the cylinder trials quantitative. In the second experiment, Cavendish distinctly mentions that he did not know what quantity of gas he burned; and in the first, the quantities are stated quite generally. No data are given, as of the height of the thermometer and barometer, by means of which the volumes expended could be converted into weights; and it is further explicitly declared, that some of the water produced was lost. The weight of the water procured in either case is given in round numbers, only to show that it was examined on a sufficiently large scale to admit of any impurities in it being detected. In Cavendish's quantitative trials, volumes were not converted into weights, but (to take the simpler case) a given weight (viz. a globeful) of hydrogen and oxygen was changed by the electric spark into the same weight of water. The water certainly was not *absolutely* pure, but it made so near an approach to purity, that Cavendish, after testing it very carefully, said of it, as it was obtained in the first cylinder experiment, that "it seemed pure water." It has been shown already (ante, p. 339), that none of his rivals obtained purer water than he did, or analysed it so carefully.

As for the repetition of the experiment in 1782 showing that no conclusion had been drawn from the trial of 1781, it is sufficient to notice that, from its date, it was certainly made contemporaneously with the experiments on the influence of variations in the relative proportion of hydrogen and oxygen on the production of nitric acid, and evidently with a view to satisfy Cavendish, *after he had discovered that nitric acid was occasionally produced*, that this did not appear when hydrogen and common air were burned together. This is manifest from the explicit way in which he states that the water obtained in the second experiment "was not at all *acid*, nor gave the least red colour to paper tinged with red flowers." (*Lith. MSS.* p. 128.) No such special reference is made to the absence of acid from the water obtained in the corresponding experiment of 1781 (*Lith. MSS.* p. 127), at which period Cavendish had not discovered that nitric acid was developed in any circumstances when hydrogen and oxygen are exploded together.

‡ *Phil. Trans.* 1784, p. 129.

said to be two extreme hypotheses on the subject, the one that imputed to Mr. Harcourt, that the experiments involved the conclusion, and that therefore their dates are the same; the other, that of the advocates of Watt, that Cavendish put no interpretation upon his experiments till he read Watt's letter of 1783, which first taught him their significance. That neither of these views can be accepted without qualification, all impartial critics will, I think, acknowledge. But if we are to choose between the two, we shall find the probabilities immensely in favour of the former. In judging of this, it must be remembered that we are at present proceeding solely on the evidence furnished by the MS. journal, and the published paper as compared together, and that for brevity's sake we have selected the experiments with hydrogen and air, omitting those with hydrogen and oxygen. I have chosen those trials because the inquiry was much more complicated than when oxygen was substituted for air, and the chances of error were therefore much greater. Moreover, Watt's letter refers solely to experiments with inflammable air and oxygen, and could not therefore have furnished Cavendish with the *direct* interpretation of the phenomena which appeared when atmospheric air was employed. His experiments with air, in truth, stand alone, nor was any repetition of them made before he published his paper. They were so nicely contrived, that the mean of six trials enabled him to ascertain to what extent hydrogen diminishes air when exploded along with it. They were managed in such a way that the same trials proved that no loss of ponderable matter occurred during the combustion; and they supplied the demonstration that the nitrogen of the air remained unchanged, whilst all its oxygen disappeared. One additional experiment showed that the dew which resulted from the combustion was pure water. We are gravely asked to believe, nevertheless, that he who conducted this beautiful inquiry had been taught by it nothing concerning the origin of the water. Yet if he were not seeking to connect the disappearance of oxygen and hydrogen with the appearance of water, what, I would ask, was he in search of? He was confessedly examining the products of combustion with a view to explain why air was diminished in volume when it supported combustion. The particular combustible which he was employing was hydrogen, and when it was burned in certain proportions with air in a shut globe, it diminished the volume of the air, and nothing was left in the globe but nitrogen and water, whilst the quantity of nitrogen was exactly that which the volume of air taken at the beginning of the experiment contained. The nitrogen of the air thus stood aside, as a portion of it which remained unchanged, and the only substance standing in the place of a product of combustion, was a weight of water which *of necessity* equalled that of the oxygen and hydrogen which had lost their elasticity. All this Cavendish knew; all this he had discovered for himself, relying upon data which were almost entirely of his own furnishing; and having once made such experiments he never repeated them, though two years elapsed before he made them fully public, and on them alone he rested his published conclusions, so that he manifestly regarded them as needing no repetition or extension in order to justify his views. Notwithstanding all this, it is declared to be incredible, that he who sought for the product of the combustion of hydrogen and found water, believed that water was the product of that combustion. I unhesitatingly affirm, on the other hand, that nothing but the strongest evidence to the contrary can invest with improbability the belief that at the period of their performance Cavendish interpreted his experiments as demonstrating that hydrogen and oxygen were *turned* or *converted* into pure water. To this extent then, at least, I adopt the statement that his experiments bore for him their conclusion in their face. Those who deny this must not stop short with denying it, but must show what conclusions he drew from his experiments if not those he recorded.

The MSS. journal then is good evidence, on the lowest estimate of its value. Its dates are important as earlier in time (so far as the experiments on the production of water are concerned) than the conclusions considered as dating from the spring of 1783; and we may justly put upon them the higher value that they mark, within more or less narrow limits, the probable periods when conclusions were drawn from the experiments the dates of which are given.

It may most justly, however, be urged that if Cavendish's experiments on the combustion of hydrogen and air were of such a nature as to interpret themselves, it is very unlikely that the secret which they revealed in 1781, was never betrayed by Cavendish till 1783. I have already acknowledged the probability on other grounds of such a betrayal, and in truth it is admitted on all hands, as I have heretofore shown, that Cavendish gave *some* account of his researches to Priestley and to Blagden before he published them. These statements were referred to only in illustration of the priority of Cavendish's experiments. They are held however by the impugners of his claims, to furnish the best means of disproving the priority of his conclusions to those of Watt. I shall consider them, therefore, as coming before us in the shape of objections urged by the advocates of the latter.

III. Watt's friends lay great stress upon the fact, that Cavendish communicated to Priestley only his experiments and not his conclusions. On this point Lord Brougham observes, "Sir Charles Blagden inserted in the same paper [of 15th January, 1784], with Mr. Cavendish's consent, a statement that the experiment had fiist been made by Mr. Cavendish in summer, 1781, and mentioned to Dr. Priestley, though it is not said when, nor is it said that any conclusion was mentioned to Dr. Priestley, nor is it said at what time Mr. Cavendish first drew that conclusion. *A most material omission.*"* To the same effect Mr. Muirhead remarks, "Although in July, 1784, when the Philosophical Transactions for that year were printed, he [Cavendish] 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."† Sir David Brewster takes the same view; "Cavendish," he observes, "assures us that he communicated his experiments of 1781 to Dr. Priestley, but does not mention when, and we know that he did not communicate any conclusions to that distinguished chemist."‡

It is thus affirmed of Cavendish: 1st. That he communicated to Priestley his experiments, but not the conclusions which he drew from

* *Historical Note, Watt Corr.* p. 247.

† *Watt Corr.* p. xxxvii. In another part of his introductory remarks, Mr. Muirhead says that Priestley "alludes to *one experiment* of Mr. Cavendish as being known to him." (p. xxxviii.) This Gallican rendering of *an* into *one* is quite unwarranted, for Priestley, although he recurred to the subject frequently, and made special reference to Cavendish's experiments after Watt's paper was published, found no fault with Cavendish's declaration, that "*all* the foregoing experiments were mentioned by me to Dr. Priestley."

‡ *North Brit. Rev.* Feb. 1847, p. 495.

them. 2nd. That he does not say *when* he made this communication. 3rd. That he does not say *when* he first drew the conclusion for himself. From all which it is inferred that there is no evidence of Cavendish having preceded Watt in his view of the composition of water. I shall consider these propositions in turn.

I. As to the first, it could have been wished that the impugners of Cavendish's claims had told us what they understood by "*an experiment*," and in what terms they supposed Cavendish to have recounted "all the foregoing experiments." They cannot surely profess to believe that Cavendish would only tell his brother philosopher that he took certain pieces of apparatus and certain chemical substances, placed them together, and saw certain phenomena? Yet they seem to wish us to understand as much, so that the revelations of Cavendish would be (to take one example) of the following kind: "6140 of inf. air, or $\frac{1}{4.024}$ of whole contents was put into the bott.; it did not lose at all on firing, nor on standing one-half hour. 8615 of water run in on opening, and 230 more on shaking, in all 8345, or.....its test was .339; its spe. gra. was $\frac{1}{50}$ less than com. air, 9900 being tried and inc. weight $\frac{1}{4}$ gra."*

I have taken the preceding quotation at random from Cavendish's journal, which contains, as we have seen, the simple record of his experiments. Is it credible that this, or any of the other brief statements, intelligible only to him who recorded them, are examples of the kind of information which Cavendish gave Priestley? No one, I am persuaded, who has listened to an observer's account of his experiments, will entertain the belief that Cavendish would describe his researches to another chemist, merely as he has recorded them in his journal. The friends of Watt have, wisely enough, avoided giving their definition of the word "experiment," as referred to by them. It is a fundamental rule, however, of all interpretation, that we are not to put our meaning upon an author's words, but are to seek to discover in what sense he employed them. What we have first to ask, therefore, is not how narrow a limitation may be put upon the word "experiments," but what meaning did Cavendish intend and expect it to convey.

That he used it in a much wider sense than as merely the description of the apparatus which he employed, the manipulations which he practised, and the phenomena which he saw, is manifest from the place in his paper where he introduces the reference to his revelations to Dr. Priestley. He has recounted all his experiments on the production of both pure water and acid water from the combustion of hydrogen with air and with oxygen, and then he declares that all of them, "except those which relate to the *cause* of the acid found in the water," were mentioned to Dr. Priestley, who was induced in consequence to repeat some of them. If we look back accordingly to the preceding pages, we shall be able to throw some light upon the extent of information which was given. Before doing so, however, I would notice that there was a peculiar significance in Priestley rather than any one else being made acquainted with Cavendish's observations. It was Priestley and Warltire's experiments that he had been repeating, and he was using an apparatus and materials similar to those which they employed. There were no parties, therefore, to whom the result of a repetition of their trials could be so interesting as to them. At the same time, if the repetition had merely confirmed their results, there would be little occasion for much detail in recounting the repetition. We know, however, that Cavendish could not confirm Warltire's results, and that he

* *Lith. MSS.* p. 117.

went far beyond Priestley, who in turn repeated Cavendish's experiments as new to him, and called them, though in a sense extensions of his own trials, by Cavendish's name. It was thus only, or chiefly, where his procedure or his results were *different* from theirs, that it could be an object of interest to either party to recount Cavendish's experiments. Thus much then premised, we may be certain that, limiting himself almost entirely to the points of difference, he would inform Priestley that he had not been able to confirm Warltire's alleged observation, that the globe lost weight, but that he had confirmed the truth of Priestley's statement in reference to the appearance of dew after each explosion. He would then, doubtless, proceed to recount "all the foregoing experiments," which Priestley approved of Cavendish's declaring he had communicated to him. He would therefore more or less fully explain the results of his varying the proportions of air and hydrogen, the maximum amount of reduction which he found could be effected upon air by exploding it with hydrogen, and the test or quality of the residual gas; on all which points Priestley himself had made no researches. He would then proceed to inform him, in some such words as he uses in his published paper, that "the better to examine the dew," he burned comparatively large quantities of hydrogen and air, and in this way obtained a portion of liquid which he analysed and found to be pure water. This would be followed by a description of the experiments which he made "to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air," in the proportion of a little less than two volumes of the latter to one of the former, and of his conclusion that almost all of it [the mixture of gases] lost its elasticity, whilst the liquid obtained was "water united to a small quantity of nitrous acid." Less than this cannot well have formed the subject of Cavendish's communication, so that Priestley must be held to have been informed by Cavendish of the following facts:—1st. That the moisture or dew which Warltire and Priestley had seen deposited on the sides of the glass globes in which their explosions were conducted, was pure water. 2nd. That when common air and hydrogen were exploded in the proportion of a thousand volumes of the former to four hundred and twenty-three of the latter, the whole of the hydrogen and $\frac{1}{5}$th of the common air disappeared, or lost their elasticity, and were replaced by a certain quantity of water; whilst no oxygen was found in the unburnt, or residual gas. 3rd. That when oxygen and hydrogen were exploded in the proper proportion, almost all of the mixture disappeared, or lost its elasticity, and was replaced by a certain quantity of acid water. 4th. That the disappearance of the gases, and the appearance of water, were not attended by any loss of weight, as Warltire had stated; the globe weighing the same before and after the explosion.

Such, I think, is a fair rendering of the kind and amount of information which Cavendish gave to Priestley. I have interpreted the term experiments as signifying "trials and their results," so far as phenomena were observed, but not so far as conclusions were drawn. Yet every one, I think, must acknowledge it to be very unlikely that so much information as that stated above can have been given, and no more. Cavendish, who was not what Priestley styled himself, a trier of "random experiments," is not likely to have reported observations to another, till they had conducted himself to some conclusion. No one, indeed, was more slow in communicating his results till they had taught him some distinct lesson, nor is he likely to have volunteered an account of them to another, unless he had some truth to enforce upon him by means of their evidence. The fact, also, mentioned by Cavendish, that he did not communicate to Priestley the observations on the cause of nitric acid appearing in the water, whilst he kept back from him none of the other results of his experiments with hydrogen, strongly favours the idea that he withheld an account of the series of researches which was unfinished, whilst he was ready and willing to reveal the other, which was completed. Moreover, the communication made to Priestley was in all probability an oral, not a written one; and the latter, we cannot doubt, would not be a silent and irresponsive listener, but would in his own lively way question Cavendish on the subject of his communication. In such a conversation, however, between two very sagacious men, on a subject in which they were both greatly interested, it could not but be, that some explanation would be given by Cavendish of the objects which he had in view, in experimenting in a different way from Priestley, and of the extent to which these objects were attained by the change of procedure. In other words, some statement would be made regarding the conclusions to which they led. Whilst, therefore, it is impossible, and would be idle to attempt to define with absolute accuracy the nature and extent of the information which Cavendish gave to his brother philosopher concerning all "the foregoing experiments," it can still less be conceded to the advocates of Watt that Cavendish himself intended us to understand that he had no conclusions to communicate, or that he did not choose to communicate those which he had drawn. Cavendish, in a word, must not be adduced as a witness against himself, to the effect that he revealed no conclusions to Priestley. Thus much premised, I now turn to Priestley's own account of what Cavendish told him.

Priestley's statement is meagre and incidental, but nevertheless of much importance. According to Lord Brougham, its author "says nothing of Mr. Cavendish's theory, though he mentions his experiment;"* and this ground is generally taken by the other friends of Watt. The whole passage from Priestley's paper has already been quoted in demonstration of the priority of Cavendish's experiments. (Ante, p. 284.) A reference, accordingly, to a part of the first sentence will be sufficient here. "Still hearing of many objections to the *conversion of water into air*, I now gave particular attention to an experiment of Mr. Cavendish's, concerning the *reconversion of air into water*, by decomposing it in conjunction with inflammable air."† This passage, historically considered, is a very remarkable one. It contains the first *direct* reference which has reached us of the possibility of converting a mixture of two elastic fluids, viz., pure air (or oxygen) and inflammable air into water. It refers, moreover, not to a phenomenon (for no one ever *saw* gases *change* into water), but to a conclusion, viz., that in virtue of certain appearances which attend the "decomposition" of air (oxygen) in conjunction with inflammable air, it might or should be inferred, that the gases in question undergo conversion into water. This conclusion or inference, moreover, stands connected with "Mr. Cavendish's experiment;" and was either drawn by him, or by Priestley, before that repetition was made, which supplied the basis of Watt's conclusions. It is necessary, accordingly, to inquire somewhat minutely into Priestley's reference, with a view specially to determine whether Lord Brougham and those who agree with him, are right in affirming that no indications of a theory appear in it.

In the first place, then, it may be noticed, that Priestley's statement

* *Historical Note, Watt Corr.* p. 246.

† *Phil. Trans.* 1783, p. 426.

contains an account, (1.) of an experiment, and likewise (2.) of the purpose for which it was tried, or the conclusion which was drawn from it. 1. The experiment consisted in decomposing air (respirable air or oxygen) in conjunction with inflammable air. 2. It was an experiment "concerning the *re*conversion of air into water."

To clear away a slight preliminary difficulty, it may be observed, that Priestley speaks of the *re*conversion of air into water; not of the conversion simply, for a reason which the previous part of his paper explains. He was engaged in experiments on the conversion of *water* into *air* at the time when he learned or recalled the account of Cavendish's trials; and regarding the latter as complementary or correlative to his own, he uses the duplicative particle *re* to indicate this. It may be cut off, accordingly, and then Cavendish's research will be named, "An Experiment *concerning* the Conversion of Air into Water." Priestley's words thus imply, that the experiment in question was one of two things. 1. An experiment or research made to ascertain *if* air *is* convertible into water; or 2. an experiment, which for whatever purpose undertaken, led to the conclusion *that* air *is* convertible into water.

Cavendish's researches, in truth, come under both definitions, if his own account of matters is accepted; but into this it is not necessary to enter here, as it will be enough if I can show that they come under the second definition. Let it be observed, then, that whilst Priestley does not give any details respecting the nature of Cavendish's experiment, he plainly intends us to learn these, from the description which he proceeds to give of his repetition of it; so that the general steps of their procedure were the same, and what was observed by Priestley must be regarded as having been previously observed by Cavendish. Thus much, indeed, is conceded by the advocates of Watt, at least by Lord Jeffrey, who says of Priestley's experiments, "the whole series was professedly entered on as a mere repetition of those of Cavendish."*

Priestley reported himself to have observed, that when inflammable air (of a certain kind) and oxygen were exploded together, a weight of water was found, as nearly as he could judge, equal to the weight of the gases burned; and the conclusion which he drew from it he thus states:—"The result was such as to afford a strong presumption, that the air was reconverted into water, and therefore that the origin of it had been water."† In other words, he drew from his repetition, more or less decidedly, the conclusion, that inflammable air and oxygen are convertible into water; and that water originates in, or is produced from, or out of these gases. This conclusion was by no means entertained by Priestley with the unhesitating confidence which marked its adoption by Cavendish and Watt, and was by and bye abandoned by him, exactly when the evidence in favour of it became strongest; but when he wrote the passage on which I am commenting, he thought that there was at least "a strong presumption" of its truth. The meaning, then, of the word "*concerning*" as applied to the experiment detailed, will now be apparent. It was an experiment "concerning the conversion of air into water," in the sense of

* *Edinr. Rev.* Jan. 1848, p. 94. It has been shewn already, however, that Priestley's repetition was not an exact one, and Cavendish pointed out that Priestley (1) had not employed hydrogen as he did, but inflammable air from charcoal; (2) that he probably used a greater proportion of this gas; and (3) that he did not observe the production of nitric acid. These points of difference, however, notwithstanding their intrinsic importance, are immaterial to our present inquiry.

† *Phil. Trans.* 1783, p. 427.

an experiment which rendered it highly probable that air is convertible into water, and that water has its origin in the gases which can be converted into it. This experiment, moreover, was Cavendish's; not Priestley's. Before the latter proceeded to repeat it, he recorded it as "Mr. Cavendish's experiment concerning the reconversion of air into water," so that the conclusion which it justified in Priestley's hands, it had already justified in Cavendish's. In short, before Priestley had made that repetition of experiments which furnished Watt with the ground of his conclusions, Cavendish had already furnished Priestley with their interpretation. The evidence seems to me conclusive; but I wish carefully to mark the limits of the interpretation thus furnished. I do not seek to affirm that Cavendish gave Priestley a lengthened exposition of his views on the composition of water. All that I wish to claim is, that when Cavendish reported his experiments of 1781 to Priestley, he accompanied the report with thus much of conclusion, that they proved *that inflammable air and oxygen could be converted entirely into pure water, and that water originates in these gases.*

Against this view of matters the advocates of Watt strongly protest, affirming that Priestley was at the greatest pains to disavow having drawn any conclusion from his repetition of Cavendish's experiments; and that, on the other hand, he was careful to explain that he was entirely indebted to Watt for the conclusion which he connected with them. Lord Jeffrey refers at greatest length to this view of the case, and concludes his protest by asking, "Is it possible to doubt that the expression . . . . . as to the experiments 'affording a strong presumption' of the truth of this theory, was never intended to imply that Priestley had himself originated that theory, or even been struck with the force of that presumption; but only that, after it had been suggested by his friend [Watt], he could not but acknowledge that there were some probabilities in its favour?"*

I do not refer to his lordship's arguments in full, because, in truth, it seems to be a sufficient reply to them to point out that they prove too much. They, in effect, represent Watt as having interpreted experiments before they were made, and Priestley as having tried experiments with positively no object, unless to ascertain what Watt would say of them when they were reported to him. Watt's friends are not very consistent in maintaining this, for they require us to acknowledge that he was unacquainted with the contents of Priestley's paper read June 26th, 1783; yet as Priestley sent that paper to the Royal Society on April 21st, he cannot have been cognizant of the contents of Watt's letter of 26th April (the germ of his "Thoughts on the constituent parts of water,") of the same year; so that it does not seem very reasonable to affirm that Priestley, who had not yet received this letter, can have been fully furnished with an account of Watt's hypothesis. From internal evidence, however, it is manifest that additions were made to Priestley's paper after the date (April 21st) prefixed to it in the Transactions for 1783.† Before the

* *Edinr. Rev.* 1848, p. 92.

† This is evident from the fact, that the earliest account which Watt received from Priestley of the delusive character of his experiments on the power of porous retorts to convert water into air, was in a letter from London, of date April 29th, that is, eight days after Priestley had sent his paper to Sir Joseph Banks, whilst the paper itself contains an account of the experiments reported in this letter to Watt. The paper must therefore have been altered before at least it was printed, and, in all probability, before it was read in June. The title also supplies evidence of alteration, for it contains

additions were made, (before indeed the 29th of April) Priestley was familiar with the contents of Watt's letter, but this only renders his total omission of Watt's name in connexion with the experiments on the conversion of inflammable air and oxygen into water, the more at variance with the supposition that he intended to acknowledge obligation to Watt for the "strong presumption" which his repetition of Cavendish's experiments led him to entertain.

It is no part of my argument, whilst reasoning thus, to represent Priestley as ignorant or negligent of Watt's conclusions from the experiments in question. But it is a most material part of my argument to insist upon the passage under consideration, as not referring to conclusions drawn by Watt, but to conclusions drawn by Cavendish. The friends of the former can assign no reason why his name should have been omitted by Priestley, when recounting his repetition of Cavendish's experiments. It could not be from reluctance to allude to Watt, for he refers to him frequently throughout the paper, and the omission of any allusion to him when referring to his own "strong presumption," is irreconcilable with the notion that he wished to be understood as indebted to Watt for the conclusion he connected with his own experiments.

Lord Jeffrey, indeed, attaches importance to the circumstance that Priestley disclaims all pretensions to theorise upon the facts he communicates in the letter to Sir Joseph Banks which accompanied his paper. "The principal *facts*," says he, "are I think sufficiently ascertained; though I do not presume to give any opinion with respect to the *theory* of them." This, however, was only one of those general disclaimers in which Priestley was fond of indulging, when his habit of hasty generalisation had involved him in more than ordinary confusion. Never was this disclaimer more out of place than on the present occasion, for his *facts* were many of them most insufficiently ascertained, and were afterwards disproved or contradicted by himself; and his paper was full of theories. Singular, also, though it may appear to those who are not familiar with the inconsistencies of statement which abound in Priestley's writings, he acknowledged in another place that this paper did contain conclusions, *i.e.* theories. In 1785, he published experiments and observations relating to air and water. In the account of these he says, "In the following experiments I also had a particular view to *a conclusion* which I had drawn from those experiments, of which an account is given in my last communications to the Royal Society; viz. that inflammable air is pure phlogiston in the form of air, at least with the element of heat." This reference shows that the communication he refers to, is the one adduced by Lord Jeffrey as containing the disavowal of any purpose of theorising. Yet Priestley himself furnishes this comment on it, "I shall have occasion to notice my own mistakes with respect to *conclusions*, though all the facts were strictly as I have represented them."*

the words, "*seeming* conversion of water into air;" whereas, when the paper was written, the conversion was believed to be real. Illustrations of the same fact will be found at p. 414 (*Phil. Trans.* 1783), where reference is made to experiments "which *at first seemed to favour* the idea of a conversion of water into air;" and the three concluding pages of the memoir must, from their contents (pp. 430—434), have been added after the experiments which brought to light the fallacious nature of the conversion had been made. The fact, we shall afterwards find, has a secondary interest, as demonstrating the liberty which was conceded to members or correspondents of the Royal Society, to make additions to their papers after they had been sent to its officials, and this without alteration of the original date.

* *Phil. Trans.* 1785, p. 280. Lord Jeffrey quotes another passage to the same

It is amusing, indeed, to hear the great defender of phlogiston, who was faithful even to death in his allegiance to one of the most visionary, and, in the end, clumsy and complicated false theories which the world has ever seen, gravely declaring himself indifferent to theory. The truth of the matter is, Priestley was an inveterate theorizer, only he was constantly changing his notions, and to a great extent concealed his love of theorizing from himself, by representing the theories he held credible as *facts*, and marking as theoretical only those which he discredited or had abandoned. It is accordingly not at all improbable that he ranked the conversion of gases into water at the time when he published his paper, not as a theory, but as a sufficiently ascertained fact. It may also be added that the indifference to theories for which he claimed credit, was in a certain sense true. It was true in the sense that he loved to change his theories; dealing largely in them, but never letting them grow old on his hands. He had not, certainly, a profound faith in the justice of Cavendish's conclusions, but he had as little in the justice of Watt's. For a short time he believed them, but he speedily abandoned his belief, and the chief reason, I apprehend, why the tacit appeal of Cavendish and Watt to Priestley, to act as umpire between them, was never responded to, is the fact that before the appeal was made, Priestley had become convinced that both of his friends were in error in their conclusions, and he naturally thought it a matter of very small importance to settle which of them had first committed a blunder. The critics of the Water Controversy, accordingly, have in vain sought to claim Priestley as witnessing solely in favour of one or other of the English rivals. Of this we shall immediately have an example. He witnessed clearly in favour of neither, and had come to no settled conviction himself, till years after the original controversy was over, when he reached a faith concerning the nature of water which no one probably but himself held, and of which he had to complain that no one would gratify him by offering to refute it. It is necessary, accordingly, in referring to Priestley's "facts and theories," always to look to their dates, and in application of this principle, I would here urge that, in April, 1783, he had such faith in Cavendish's observations, as to publish a repetition of them, and to announce the "strong presumption" as to the composition and origin of water which they led him to entertain.

Lord Jeffrey, indeed, seeks to show that "when reverting two years after to those speculations of 1783, he [Priestley] takes care in a paper read to the Royal Society in 1785, to give Watt the sole credit of the theory of which we are now speaking; and accurately to distinguish between his own *experiments* and the *conclusions* deduced from them by his friend—in exact conformity with that partition of credit or of labour which Watt had publicly adopted at the time. 'Mr. Watt,' he says, '*then concluded from some experiments of which I gave an account*, and also from some observations of his own, that water consists of dephlogisticated and inflammable air; in which Mr. Cavendish and M. Lavoisier concur *with him*.'"* This passage, however, even if we take it merely as it stands, only shows what in another place I have adduced it to prove, viz. what were the experiments of Priestley from which Watt drew his conclusion, and what that conclusion was. It does not declare

effect, from the body of the paper, but not more precise than the one to which I have referred. (*Edinr. Rev.* 1848, p. 91.) It is answered, therefore, in the reply to the preceding quotation.

* *Edinr. Rev.* 1848, p. 92.

that Priestley drew no conclusion from his repetition of Cavendish's experiments before he reported it to Watt, or that Cavendish drew none from the original trials before he reported them to Priestley; so that the question whether or not such conclusions were drawn, is left quite undetermined by this reference. The quotation, moreover, given above is a partial one, and in justice to all parties the entire paragraph should be consulted. It is as follows; the passage which is about to be quoted immediately preceding that already given. "In the experiments of which I [Priestley] shall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. Cavendish, Mr. Watt, and M. Lavoisier. Mr. Cavendish was of opinion that when *air* is decomposed, *water* only is produced, and Mr. Watt concluded," &c. &c.*

From the entire quotation, then, it appears that Priestley was far from dividing the merit attaching to the discovery of the composition of water between Watt and himself. On the other hand, he *commences* with a reference to Cavendish, and informs us what his views on the subject were. Priestley's meaning will be obscure to those who are not familiar with Cavendish's belief that water was an invariable product of oxidation. I have pointed this out at great length in a preceding section of the discussion (ante, p. 324), for it has not received sufficient attention from the critics on either side of the Water Controversy. I need only, therefore, say here, that Priestley's statement is to be interpreted as signifying that Cavendish held that when respirable air (common air or oxygen) is decomposed, *i. e.* is phlogisticated or deoxidised by combustible or oxidable bodies, water alone is produced. Cavendish, as I have already shown, supposed all oxidable or phlogisticating substances to contain hydrogen and to yield water when burned, whilst their other (hypothetical) constituents separated, so that nothing but water was called into being, or as Priestley phrases it "*produced;*" thus nitrogen, *ex hypothesi*, a compound of nitric acid and hydrogen, produced water and set free the nitric acid; hydrogen, a compound, *ex hypothesi*, of water and anhydrous hydrogen, when oxidised *produced* one quantity of water, and set free another which had pre-existed in it. Into a minute analysis, however, of the exact meaning of each word it is not necessary to enter. It is enough if the passage plainly declares that Cavendish held opinions of his own concerning the production of water by the action of combustible bodies on air. He who laid it down as a general proposition that *every* combustible when it burns in air produces water, must *a fortiori* have held this view concerning the particular combustible hydrogen; for it was the phlogisticating body on which he had made the greatest number of experiments, and the only one which he professed to have seen actually produce water. Priestley, then, had no purpose of declaring, and never did declare, that the doctrine of the convertibility of air into water, was originally taught him by Watt. To assert that he did acknowledge, or should have acknowledged this, proves nothing because it proves too much, and involves the holders of the opinion in irreconcilable contradictions. According to them Priestley's repetition of Cavendish's experiments taught him nothing, till Watt interpreted them for him. This assuredly is an untenable view, for Watt knew nothing of Cavendish's original trials, nor of Priestley's repetition of them till it had been made, so that he had no share in inducing Priestley to make the repetition. Yet certainly Priestley had some very strong motive for trying Caven-

* *Phil. Trans.* 1785, p. 279.

dish's experiment, and for taking so much pains in preparing the gases quite pure and dry. Thus much might be urged on any view of matters, but it acquires peculiar force when we remember that Priestley had, earlier than Cavendish, exploded inflammable air and oxygen in a glass globe; had seen dew or moisture after the gases were fired; and had weighed the globe before and after the explosion. It was, in truth, Priestley's experiments including Warltire's, which Cavendish had been repeating, and if this repetition had brought to light no result which they had not observed, there could have been no occasion for Priestley repeating a repetition of his own experiments, or for his connecting any one's name with them, but his own and Warltire's.

There were thus no parties less concerned to interest themselves in what Cavendish had done, if his trials were in no respect novel, than Warltire and Priestley, so that it is more necessary to seek for a motive for either of them trying Cavendish's modification of their experiments, than it would be if the latter had been repeated by any one else. To Warltire they could give no satisfaction, for they showed that his proposed *experimentum crucis* on the ponderability of heat was not crucial; and that his notion that the water which appeared, was simply deposited from one or both of the gases which were exploded together, was unfounded. Priestley, on the other hand, had, with more than ordinary discretion, forborne from theorising upon them, so that he had no prejudice to overcome on seeing an unexpected conclusion deduced from them; but on the other hand, he had as little interest in paying attention to the repetition of what he has styled "a random experiment," and has dismissed with the incidental notice that inflammable air was not likely to supplant gunpowder in warfare. Unless, therefore, his experiments as repeated by Cavendish had appeared to him in a new and important light, no reason can be assigned why he should have returned to them in the way he did. Nevertheless, we find that he went over them a second time with great attention; that he took great, although misdirected, pains in preparing the requisite gases; that he thought the results so important that he reported them to the Royal Society; communicated them to Watt; and often referred to them years after their performance; whilst notwithstanding all this, he never referred to them as his own, but spoke of them, considered as one research, as "Mr. Cavendish's experiment concerning the reconversion of air into water." All this is inexplicable on the version of matters favoured by Watt's friends. They might avoid acknowledging that Priestley must have had some motive for following in the steps of Cavendish, or acknowledge only some trivial reason why he did; but they cannot escape the obligation under which all critics of the Water Controversy lie, to account for his styling the modified repetition of his own and Warltire's researches, "Mr. Cavendish's experiment." Watt's name must stand altogether aside, for no one can show, or affects to show, that he had any share in inducing Priestley to try again Cavendish's experiments. Even, therefore, if it could be proved that this second trial acquired all its significance from the interpretation which Watt put upon the phenomena which it brought to light, this would only explain why Priestley recorded and published his repetition, not why he made it.

I set therefore aside as untenable the proposition that Watt supplied the motive for Priestley's repetition of the "Cavendish experiment." To assert this is to assert an anachronism. The friends of Watt have justly enough relieved him from any participation in the blunders which Priestley committed; and have been at no little pains to claim for him a total ignorance of what Cavendish had done. They should honestly abide the consequences of the position they have assumed, for they cannot with consistency date Watt's claims earlier than the *close* of Priestley's repetition of "Mr. Cavendish's experiment."

I return accordingly to the point from which I departed to make this long digression. My object was to show, that Priestley reveals that a conclusion, as well as an experiment, was made known to him by Cavendish. The latter tells us that it was "*in consequence*" of certain announcements which he made to Priestley, that the latter made certain trials, "as he relates" in his paper of 1783. On turning to that paper we find that Priestley has already and spontaneously acknowledged this, and that the account he gives is to the effect, that being very solicitous to demonstrate that water could be converted into air, and beset with objections to the probability of its occurrence, he bethought himself of "Cavendish's experiment concerning the reconversion of air into water," and "paid particular attention to it." He justly thought that if by means of his experiment he could establish the convertibility of gas into liquid, he would increase the probability of his own converse proposition that liquid was convertible into gas. His account, also, of the mode in which he experimented is in entire conformity with his professed object, however injudicious his modifications of Cavendish's process were. He spent great pains in preparing the gases in a state of perfect dryness; he did his best to collect the water produced during their combustion; and he would have been glad to have possessed a more delicate balance to have assured himself that the weight of water produced was equal to that of the gases consumed. These observations were manifestly made in the light of a foregone conclusion. The sole object in trying them was the expectation that they would prove that the gases underwent conversion into water, and this expectation was entertained because they had already proved this in the hands of another. That other was Cavendish. He had reported his experiments to Priestley, not as uninterpreted observations, but as connected with a doctrine which was new to Priestley, viz. the convertibility or conversion of air into water.

Two points only call for further notice. The word "air" does not signify atmospheric air, but gas, viz. the gas or gases used in the experiments. This is manifest from Priestley and Maty both employing "air," when stating the results of trials in which atmospheric air was not made use of, but only oxygen and inflammable air. It had more special reference, however, to the oxygen; the inflammable air being most generally referred to, not as an elastic fluid, but as phlogiston It was intended also, no doubt, to include atmospheric air so far as its dephlogisticated part or oxygen was concerned, but Priestley had no occasion to enter into any precise definition of this, as he referred to Cavendish's experiment only to prove that respirable air might be converted by phlogisticating it, into water; and oxygen and atmospheric air were by himself and his contemporaries frequently referred to as only different degrees of purity of the same body, *viz.* respirable air (ante, p. 216).

The other point is of more importance. What is the exact signification ot the term *concerning*, as connected with the words, conversion of air into water? Some additional light is thrown on this point by the language used by Mr. Maty in his official abstract of Priestley's paper, which has been quoted already in full. The Secretary refers to Cavendish's experiment as one "tending to prove the reconversion of air into water." Cavendish's trials were thus brought before the Royal Society a second time as calculated to prove such a conversion. We have thus imputed to Cavendish, by both Priestley and Maty, opinions as to the convertibility of certain gases into water. More than this we cannot with certainty infer from the mere words. But when we take them in connexion with the fact that Cavendish's experiments appeared to Priestley so demonstrative of conversion actually occurring, that he made a careful repetition of them to enable him to assert that it did occur;—we shall not hesitate to conclude that the word *concerning* is intended to signify that the experiments in question were regarded by Cavendish as proving, more or less unequivocally, the conversion which they were said to concern. I must guard myself, however, against any appearance of overstating either Cavendish's or Priestley's conclusions. Priestley assuredly had but a hesitating faith in the result to which he professed to have been led. He would not declare that his repetition irresistibly compelled the conclusion which he timidly drew; he would not say more than that his results afforded "a strong presumption" to that effect; but he thought it *at the time* strong enough to induce him to make use of it as an argument in favour of the convertibility of water into air. It is quite certain also, that his faith in Cavendish's results had been greatly shaken by his detection, before the publication of his paper, of the fallacy of his own observations on the convertibility of water into atmospheric air. He cared little, probably, for the Cavendish experiment, except as assisting in the demonstration of his own views; and when they were found by himself to be hopelessly untenable, he lost his interest in Cavendish's trials, and probably doubted their truth also. Yet it is curious to notice how tenaciously he clings to them even after the abandonment of his own speculations. In the close of his paper, after recounting an experiment which militated against his notions, he consoles himself with the reflection that, though it does not prove the conversion of water into air, still another experiment "cannot be explained so well on any other hypothesis, any more than Mr. Cavendish's experiment on finding water on the decomposition of air;"* so that whilst he would not assert that it supplied demonstration of the transmutability of air into water, and therefore of water into air, he nevertheless thought that this was the conclusion naturally deducible from it. He afterwards abandoned this belief, and in his later papers dissented from Watt, Cavendish, and Lavoisier, in reference to the nature of water. But he did not do so because he denied, as some have stated, the justice of their reasoning, but because he had not faith in their data. He had the best of all reasons for losing faith in his own experiments, for, as we have seen already, they cannot have yielded the results he declared they gave; but as renegades generally go to the furthest extreme from their original belief, he not only denied his own trials, but refused to credit that Cavendish and Lavoisier had obtained, or that any one else could obtain, from hydrogen and oxygen their weight of pure water. This change in faith, however, cannot affect the reality of his former belief, and it was held consistently enough. He believed the inference just to the extent he believed the premises true. No man's belief goes or can go beyond this.

As for Cavendish, if we had nothing but the words employed by Priestley to guide us to a knowledge of what his conclusions were, we should be quite uncertain of their precise nature. But when we take Priestley's words in connexion with Cavendish's journal, where experiments, exactly fitted to demonstrate the convertibility of gases into water,

* *Phil. Trans.* 1783, p. 433.

are recorded; and when we find him in his published paper stating, "that almost the whole of the inflammable and dephlogisticated air is *converted* into water; and in another place, "that almost all the inflammable air, and about $\frac{1}{5}$th of the common air, are *turned* into pure water;" and when we further find him stating, as the widest generalisation of his entire series of experiments on air, that water is produced during oxidation, thus overestimating, instead of undervaluing (as he is accused of doing) the importance of the appearance of water during combustion:—when all this is considered, we shall not, I think, find much difficulty in believing that Cavendish employed the word *conversion*, when he communicated with Priestley, in the sense in which he employed it in his published paper. In other words, Cavendish told Priestley, that if the latter would experiment with hydrogen and oxygen in the way he had done, he would find that they could be entirely converted into their joint weight of water. But if Cavendish could say this, he could also say that he had discovered the composition of water. As much is acknowledged by Lord Jeffrey, with his characteristic impartiality. He is referring to the absence of any conclusions from Cavendish's journal, on which he remarks, "If he had even stated in the detail of it [one of his experiments] that the airs were *converted*, or *changed*, or *turned* into water, it would probably have been enough to have secured to him the credit of the discovery, as well as to have given the scientific world the benefit of it in the event of his death, before he could prevail on his modesty to claim it in public."* Such a declaration he did not make in his journal for reasons which we have already seen; but if he made it to Priestley, its force is equally valid.† I will only add that *some* interpretation must be put upon the words used by Priestley in reference to Cavendish, by even the most grudging critics of the latter. They must dispose in some way of this remarkable fact, that before the Water Controversy had or could have arisen, before Priestley had made the experiments (whatever they were) from which Watt drew his conclusions, Cavendish is found entertaining views on the composition of water. The word *conversion* could refer only to an hypothesis or a theory, to an anticipation or a conclusion. Thus much all must concede, but with it they must also acknowledge that Cavendish did not begin for the first time to interpret his experiments, after he learned the contents of Watt's letter of 1783. The proposition, therefore, that Cavendish communicated to Priestley only experiments, and not conclusions, must be set aside; and discussion can only turn on the question, what were the conclusions which he certainly revealed?

II. The second statement of Lord Brougham, and those who agree with him, is, that Cavendish does not say *when* he made his communication to Priestley. This is quite true so far as minute dates are concerned; and

* *Edinr. Rev.* 1848, p. 125.

† The journal (*Unlith. MSS.*) is not absolutely silent on this point. Its index contains the following reference to experiments made between January 3rd and February 7th, 1783. "P. 216. Whether any fixed air is produced by exploding com. and inf. air, and examinat. water *produced* by exploding com. and inf. air in glass globe." The index is not dated, and we cannot be certain when it was made. The last portions of it cannot have been added till 1785, but, from the appearance of the ink, and the difference in the handwriting, I feel certain that it was filled up from time to time, at intervals, during several years. It is impossible, however, to fix the date of the quotation, but the words "*water produced*" are remarkable as occurring in a reference to experiments made in January, 1783, and imply that the generation of water from gases was referred to by Cavendish as observed by him at that time.

in all probability Cavendish could not have recalled, when writing in 1784, the precise period to a day or a week, at which, more than a year before, he reported his experiments to Priestley. We are apt to forget that he could not anticipate the rigid criticism of all his statements, to which more than half a century afterwards the Water Controversy was to give occasion. I make this remark, because conclusions unfavourable to his accuracy or fair dealing have been drawn, from the absence on his part of minute statements concerning dates, which have acquired all their importance from long subsequent events; whereas an anxious particularity about such matters, which at the time could not have appeared to their recorder of great importance, would rather have savoured of concealed design, than have been consistent with the unsuspicious brevity of an honest and independent observer.*

In truth, however, Cavendish has marked with sufficient precision the period when he made his communication to Priestley. The experiments reported were made, he tells us, in the summer of the year 1781; and Priestley, he adds, relates a repetition of them in the *Phil. Trans.* for 1783. The date of Priestley's paper is April 21st of that year, so that the communication must have been made some time between the summer of 1781 and the spring of 1783. By a reference to Cavendish's journal, we find that the last experiment which he can have communicated to Priestley was made on September 28, 1781, whilst the majority were made in July of that year.†

From the *Watt Correspondence,* moreover, we learn that Priestley was experimenting on the conversion of water into air as early as 8th December, 1782;‡ and the first reference to the production of water from the explosion of a mixture of oxygen and inflammable air, occurs under date 26th March, 1783. So that Cavendish's communication must have been made to Priestley some time before this date, and probably not earlier than October, 1781. Such minute references, however, are quite unnecessary. The only point of any importance is quite certain, viz. that whatever Cavendish told Priestley, he told him not later than the spring of 1783, before the latter made the repetition, and furnished the data from which Watt drew his conclusion.

III. The third proposition of Lord Brougham and the other advocates of Watt is, that Cavendish did not state *at what time he first drew his conclusion* concerning the nature of water, which his Lordship regards as a most material omission. This point, however, has been sufficiently considered in the discussion of his first proposition, where it has been shown, that Cavendish revealed conclusions as well as experiments to Priestley when he made the communication, the date of which has just been determined.

On this point, nevertheless, I would remark, that I have no purpose of asserting that Cavendish's views on the composition of water, as published in his paper of 1784, were fully arrived at, when he made this

* I am very reluctant to say anything to the prejudice of Lavoisier, but it is impossible not to be struck with the needless particularity with which he records dates, in an account which all critics seem now ready to acknowledge is a *post factum* and unjustifiable endeavour to establish his priority as the discoverer of the Composition of Water. *Watt Corr. Mémoire par M. Lavoisier*, pp. 173—178; or *Mém. de l'Acad.* pp. 472—474.

† *Lith. MSS.* pp. 115, 127 & 147, and *Brit. Assoc. Report for* 1839, p. 36.

‡ *Watt Corr.* p. 3.

communication to Priestley. On the other hand, I believe that they altered and expanded from 1781 onwards to 1784, as he became better and better acquainted with the composition of atmospheric air, and the nature and properties of oxygen, hydrogen, and nitrogen. Two distinct epochs at least can be marked in the progress of his views.

1. The discovery that all the oxygen present in air can be converted by combustion with twice its volume of hydrogen, into their joint weight of water. 2. The discovery that when the same gases were taken pure (or apparently pure), they yielded nitric acid as well as water, which threw a difficulty in the way of the conclusion drawn from the experiments made with atmospheric air. The removal of this difficulty led to protracted researches on the nature of nitrogen, and to an extension of Cavendish's views concerning the presence of hydrogen in oxidable bodies, so that he would not expound his opinions on the nature of water, after he discovered the origin of the nitric acid, in the same way as he did when unaware of its source. And as the *precise* period when he made his communication to Priestley is not known, it would be idle to attempt minute precision as to how much he told, or could have told the latter. Thus much, however, as already urged, I contend for, viz. that Cavendish told Priestley that hydrogen and oxygen could be transmuted into pure water, and that, therefore, water consisted of these, *whatever they were.*

Could it be shown, nevertheless, that Cavendish was led by the appearance of nitric acid to the conclusion which Priestley, as well as La Place, afterwards erroneously drew, that the true product of the combustion of hydrogen in air, or oxygen, is not pure water, but water *and* (*or*) nitric acid, we might suppose that his interpretation of the acid experiments shook his faith in the conclusion drawn from his previous observations on the production of pure water. But we know from the most unexceptionable authority, for it is one of the few conclusions recorded in his journal, that from the first he regarded the nitric acid as derived from foreign matter. A record to this effect occurs in his Note-Book, under date September, 1781;* and he never ceased experimenting till he had shown that this view was well founded. He made his communication to Priestley, however, before he had ascertained the cause of the acid, as he tells us himself, so that it may be considered certain that he confined his statements to the researches which he made in 1781. To no one, probably, would he then have announced his views in the form of a proposition, such as, "Water consists of dephlogisticated air united to phlogiston," as he does in his published paper; for he evidently did not settle the terms in which he should announce his views, till he had come to a conclusion concerning the nature of nitrogen, and the source of the nitric acid, both of which points he disposes of in his published paper, *before* entering on the exposition of his theory of the composition of water. He was the only one among all the observers concerned in the Water Controversy, who encountered the perplexing phenomenon of the production of nitric acid. It might have turned out that this body resulted from the union of substances present in the oxygen and hydrogen as constituent ingredients. These might separate from the gases, and form nitric acid, at the high temperature which attended their combustion when pure, although they were unable to undergo the same change at the lower temperature which characterised the combustion of hydrogen and air. In this case, the ingredients of water would have been proved to be compound substances,

* *Lith. MSS* p. 147.

which, by mutual combination produced the more complex compound, water. The terms, accordingly, in which the composition of water should be stated, would have required to be altered, although the fact of its consisting of hydrogen and oxygen remained unaltered. An example will illustrate the justice of this remark. The older chemists were well aware that when muriatic acid and ammonia meet, they unite and produce sal ammoniac, and could confidently affirm that the constituents of sal ammoniac are muriatic acid and ammonia. Their successors prosecuted the inquiry further, and discovered that muriatic acid consisted of chlorine and hydrogen, and ammonia of nitrogen and hydrogen, so that it could then be stated that the ingredients of sal ammoniac are chlorine, hydrogen, and nitrogen, and yet the salt might still with perfect justice be represented as consisting of muriatic acid and ammonia. So also Cavendish, from the moment that he satisfied himself that when hydrogen and atmospheric air were exploded together, the nitrogen remained unaltered in quality and quantity, whilst the whole of the hydrogen and oxygen went into the water, could affirm, once for all, that water consisted of hydrogen and oxygen, although he might leave unsettled the terms in which he should announce this, till he had more thoroughly investigated the properties of hydrogen and oxygen. And this is what he actually did; for whilst to the last he affirmed that hydrogen was one of the ingredients of water, he held it probable that the former was in its turn composed of ingredients, viz. the inflammable air from the metals, and a little water.

IV. I come now to the last, but, as it is considered by those who disallow his claims, the most formidable objection to Cavendish's priority. This is a passage in the letter which Blagden addressed to Crell in 1786. The letter has been quoted in full already, in illustration of the priority of Cavendish's experiments to those of the French chemists. We are now to consider it as throwing light on the date of his conclusions. It is only valid in so far as the disproval of Lavoisier's claim to be an independent, and the first, discoverer of the composition of water, is concerned. The friends of Watt, however, take a different view of its value, and attach great importance to certain omissions which they think they can detect in it, and which seem to them incompatible with the claims set up for Cavendish over Watt. It is thus what the letter does not contain, rather than what is to be found in it, which is deemed of most importance. The following passages, selected from the entire letter, as the text of Mr. Muirhead's commentary on it, will also serve to introduce mine:—"I [Blagden] can certainly give you the best account of the little dispute about the first discoverer of the artificial generation of water, as I was the principal instrument through which the first news of the discovery that had been already made, was communicated to M. Lavoisier. The following is a short statement of the history. In the spring of 1783, Mr. Cavendish communicated to me, and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us that out of them he must draw the conclusion that dephlogisticated air was nothing else than water deprived of its phlogiston, and, *vice versâ*, that water was dephlogisticated air united with phlogiston. About the same time 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 conclusions opened the way to M. Lavoiser'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."* On this passage Arago remarks:—"That expression, '*about the same time*,' cannot be, to use Blagden's own words, '*the whole truth*.' '*About the same time*,' *proves nothing;* questions as to priority may depend on weeks, on days, on hours, on minutes. To be precisely accurate, as he had promised to be, it was indispensable that he should say whether the verbal communication made by Cavendish to several members of the Royal Society, preceded or followed the arrival in London of the news of Watt's labours. Can it be supposed that Blagden would not have explained so very important a circumstance, if he could have brought forward an authentic date favourable to his friend?"† To the same effect, Lord Jeffrey says:—"When he [Blagden] admitted that the news of Watt's conclusion had come to London about the very same time with the first revelation of Cavendish's, he must have seen that he had already recognised his right at least to *divide and share* the honour of the discovery with Cavendish; and that it wholly depended on the fact of their *relative* priority, which of them was entitled to by far the largest share. That he, the client and partial friend, to say the least, of Cavendish, should have been willing to let their shares *appear equal*, will be conclusive proof, with most people, that he very well knew that a more exact apportionment would have been anything but favourable to his patron. But it is not the less certain that such an apportionment was due—to truth, to science, and to the parties themselves,—and also that Blagden had the means of making that apportionment; and we fear we must add, that he studiously *evaded* making it! He must have known perfectly whether he had first heard of this conclusion from the one or the other; and, if he had first heard it from *Cavendish*, is it possible to doubt that he would have said so? After mentioning the actual communication by that gentleman, it is almost impossible that he should not (in that case) have introduced his notice of Watt's by saying, 'Soon after this,' or if it came *very* soon, 'Almost immediately after.' Even if nothing depended on the priority, this was the natural and almost inevitable way of connecting the two notices, if they had really reached him in that order."‡ Similar views are expressed by Mr. Muirhead, who discusses Blagden's letter at great length;§ and Sir David Brewster expresses a very unfavourable opinion in reference to its contents. Blagden's letter accordingly demands a somewhat careful consideration.

It is an essential part of the chief argument urged in the preceding quotations, to represent Blagden's purpose in writing his letter to have been to enter into the question of priority, not only as between the French and English rivals, but likewise as between Cavendish and Watt. This representation, however, is not, I believe, well founded; and it is important to show that it is not, and that Blagden restricted himself to the defence of Cavendish against Lavoisier, and purposely avoided any minute reference to the priority of the former to Watt. I agree with the friends of the latter in thinking that Blagden *designedly*

* *Watt Corr.* pp. lxvii & lxviii.

† *Eloge of James Watt, Corr.* pp. 231, 232.

‡ *Edinr. Rev.* 1848, p. 119.

§ *Watt Corr.* pp. lxvii—lxxii.

abstained from any attempt to mark accurately the date of Cavendish's communication to him; but I differ entirely from them as to the motives which led to this. Blagden's motives, in truth, in writing to Crell, have been greatly misunderstood and misrepresented. He has been accused of officiously interfering to do Watt a wrong, and of obtrusively making himself a party in a dispute with which he had no occasion to meddle. These charges are not deserved, and would never have been made had the circumstances which led to his writing been fully known and appreciated. Several facts, however, throwing much light on his correspondence with Crell, have not been referred to by the critics on either side of the Water Controversy, whose notice they appear to have completely escaped. I am indebted for a knowledge of them to three papers which I found among the Cavendish MSS., entrusted to my care by Lord Burlington.

The first of these is the fragment of a letter in Blagden's handwriting, but bearing no address or signature. It was certainly, however, addressed to Cavendish, as appears by the reference in the postscript (hereafter given) "to your paper;" and I identified the writing as Blagden's by comparing it with letters bearing his signature, remaining among the Cavendish papers, and with the fac-simile of his handwriting given by Mr. Weld.* It exactly agrees also with the handwriting of Blagden's paper on the "Cooling of Water below its freezing Point," with the original MS. of which I have been favoured by R. H. Blagden Hale, Esq. The following is the entire document:—

"In a number of Crell's Annals, which I happened not to have looked over before (May, 1784), I found the following passage: 'Mr. Cavendish in London has imitated (repeated)† the experiments of M. Lavoisier, to produce water from dephlogisticated and inflammable air by combustion. He has laid before the Royal Society the result of his experiments, which confirm that change of the airs, or the new generation of water. His memoir has met with great approbation, and even the assent of such a well-informed chemist as Mr. Kirwan. Nothing appears by which it is possible to judge from whom Mr. Crell received this information.'‡

"*Thursday Morning, March* 10.

"'It is right to mention that, in the next number of the Annals (for June), there is a letter from Mr. Kirwan, mentioning your paper in proper terms, without any notice of M. Lavoisier's name or pretensions.'"§

I have quoted the entire statement as I have found it amongst the Cavendish papers, because it proves that Blagden and Cavendish had encountered Crell's reference to the latter's researches long before Blagden published his letter of 1786. It was probably one of his duties, as Cavendish's assistant, to translate for the latter passages from the foreign journals referring to chemistry. Various such translations remain among the Cavendish MSS., especially of papers from the German, in which language Cavendish does not appear to have been a proficient. I gather as much at least, from a passage in a letter to him from Blagden, dated

* *Hist. of Royal Society*, vol. ii. p. 175.

† In the original the word "*repeated*" is written above "*imitated*," evidently as a synonyme. The German is "*Nachgemacht*."

‡ I have compared this translation, and the two others, which will presently be given, with their German originals, and have found them accurate. I could not obtain personal access to the *Chemische Annalen* for 1784 and 1785. Mr. Weld, however, kindly furnished me with transcripts of the passages in question from the copies in the Library of the Royal Society.

§ *Chemische Annalen* Von Dr. Lorenz Crell, May, 1784.

"Dover, September 23, 1787," in which he says, "I hope you got Mr. Heydinger to read Crell's letter; there was something about your subscription for his journal, which he allows to have been all duly paid, and an account of the freezing of mercury by natural cold in Russia, perfectly conformable to Mr. Hutchins's experiments. Be so good as to open and read, *or get read*, any letters that *you think may contain news.*"* This, however, is a point of secondary importance.

The account of matters by Crell, contained in the quotation given above, is as inaccurate as it well could be, and Blagden might well wonder who could have supplied the information. It represents Cavendish as having followed Lavoisier, whereas the latter himself acknowledged that he had followed Cavendish. It further refers to Kirwan as having approved of Cavendish's paper, whereas he was the only chemist who at the time publicly expressed dissent from his views. It was not, improbably, a desire to correct this mistake that led Kirwan himself to write to Crell, as Blagden mentions he did in the postscript to his letter. The following is that part of Kirwan's communication which refers to Cavendish, as I find it translated by Blagden, no doubt for the use of the former. The words within square brackets are not in the original MS.

" '*Extract of a letter from Mr. Kirwan in London to Professor Crell.* (*Chem. Annals, No. VI. p.* 523, *June,* 1784.) Mr. Cavendish has laid before the Royal Society a series of experiments, by which he shows that water is generated by the combustion of dephlogisticated with inflammable air. And in effect [in fact, *in der that*] it is very probable that in this case dephlogisticated air is converted into water by its combination with phlogiston [dem Brennbaren] as (since) [da] according to the experiment, the residuum contains no fixed air, and consists only of a little phlogisticated air, together with the water. Mr. Cavendish, however, did not stop at the proof of this important and unexpected phenomenon, but went a step further, and endeavoured to prove, that in all cases in which respirable air is phlogisticated, water is always generated, and never fixed air;† for which purpose he laboured to invalidate the proofs which I had given of the generation of fixed air. I answered his objections in a fortnight, confirming the proofs I had already given, and adding new ones. He opposed new arguments to this last paper of mine, to which I again replied, and thus the affair now rests.' "

This communication of Kirwan's probably awakened Crell's attention to the erroneous nature of the account he had given of Cavendish's experiments. At all events when he received a copy of Cavendish's paper he published an abstract of it, along with an apology for the inaccuracy of his previous account of its contents. A translation of part of this forms the third paper illustrating the correspondence with Crell, which I have found among the Cavendish MSS. It is marked—"Translation from Mr. L. Crell's Chemical Annals, 1785, part 4, p. 324. 'Experiments on Air, and the Water therefrom, by Mr. Cavendish in London.' "‡

* The letter is signed "C. Blagden," and addressed to "The Honble. Henry Cavendish, Bedford Sqre. London."

† This reference adds another proof to the many given already, that Cavendish was understood by Kirwan, and allowed himself to be represented, as regarding water as the invariable product of the phlogistication or deoxidation of air. He made no objection to such a version of his views being given to the continental philosophers, any more than to the Royal Society.

‡ The German is, "Versuche über die Luft und das daraus erfolgende Wasser; vom Herr Cavendish in London."

Crell has affixed a note to this title, which is the only part of the document which concerns us. It is translated in the MS. before me, which is not in Blagden's writing, as follows :—"'This extract contains the substance of a paper presented to the Royal Society in London, by Henry Cavendish, Esq., and which has not only been inserted in the Philosophical Transactions, but has also been published separate, under the title of (*Experiments on Air, London, by J. Nichols,* 1784, 4° *Maj. p.* 37). Soon after Sir Joseph Banks, Bart. (President of the Royal Society,) was so obliging as to send me a copy, for the purpose of mentioning it in these Chemical Annals. This becomes a twofold duty upon me, because I have committed the same error as most of my compatriots and other men of letters, by ascribing to Mr. Lavoisier the discovery of the water resulting from the different kinds of inflamed air.* (See Chem. Annals, 1785, Part i. p. 48.) Justice alone therefore demands of me to return to Mr. Cavendish (whom I take this opportunity to assure of my most sincere esteem), the well-earned honour of *the first discovery* of this so very important and remarkable phenomenon (which appears clearly from this paper), and at the same time to correct some other circumstances in mine above-mentioned publication.'"

I have printed these translations as I have found them in the Cavendish MSS. because they are substantially correct, and they show the exact amount of knowledge which Blagden and Cavendish possessed, concerning the different references to the latter's paper which were made by Crell. These points are of special importance as throwing light upon Blagden's letter of 1786. From the letters quoted above it appears that—

1. It was not Blagden, but Crell, who first referred to Cavendish's experiments.

2. Crell erroneously represented Lavoisier as having preceded Cavendish, and assigned to the former the honour of the first discovery of the composition of water.

3. Neither in Crell's two statements, nor in Kirwan's letter, did any allusion to Watt occur, but only Cavendish and Lavoisier were referred to as connected with the disputed discovery.

Whilst Crell was thus unintentionally misleading his readers, Lavoisier's completed researches into the nature of water, which were printed in 1784, reached England, and Blagden became acquainted with the representation which Lavoisier had given of the extent to which he had made him acquainted with Cavendish's researches. This representation amounted to an impeachment of Bladgen's veracity, and to a claim of independent, if not of first discovery, in respect of Cavendish. Blagden accordingly wrote to Crell, denying the accuracy of Lavoisier's statements, and asserting in the most decided terms that Cavendish's experiments and conclusions preceded Lavoisier's.

When all that has been stated is considered, the limited object of Blagden's letter will, I think, be apparent, and he will be freed from any charge of officiously intermeddling by sending it. There was a peculiar propriety in Crell's Annals, rather than any other foreign journal, being selected as the channel for communication with the public, because that journal had already published three different papers on the disputed discovery, and had commenced by preferring Lavoisier's claims, which he was now publicly asserting in his own name.

* The sense has here been a little mistaken. The German is, "die Entdeckung des Wassers aus den angezündeten Luftarten," and should be translated, "the discovery of the water from the burned airs," *i.e.* from the combustion of certain gases.

There was a positive necessity also for Blagden being the writer of a reply to Lavoisier, for none but Blagden certainly knew what account he had given to the French chemists of Cavendish's researches, and the latter, had he undertaken his own defence, could only have said, as he did in his paper, that his friend Dr. Blagden had reported to him that he had told Lavoisier so and so.

On the other hand, there was no occasion for discussing in Crell's journal the claims of Watt, for it had never so much as mentioned his name, and further it had, by ultimately assigning the merit of the first discovery of the composition of water to Cavendish, rendered it unnecessary to defend him in its pages against Watt. The dispute between him and Cavendish had been carried on in England in private, so that its circumstances were probably very little known on the continent, unless in Paris, where La Place, Lavoisier, Meusnier, and Monge were ready to divide the honour of the disputed discovery amongst them, without ascribing any merit to either of the English claimants. An English journal, accordingly, was the only fitting place for a discussion of the rival claims of Cavendish and Watt.

Again, whether in an English or a foreign journal, there could have been no propriety in Blagden adjudicating on the merits of the two English chemists. He could say nothing for Cavendish, which the latter could not much better say for himself. And if he would not, in his own name, enter into public controversy with Watt before the Royal Society, which was the only body qualified to deal with the question, we may be certain that he would not permit his assistant to vindicate him at second hand in a foreign journal. For these reasons I contend that the object of Blagden's letter was to vindicate his own veracity, and Cavendish's originality, against Lavoisier's implied denial of both. A sense of justice led him, indeed, to mention Watt as having discovered for himself what Cavendish first pointed out to all. But he left altogether unconsidered the question of priority of discovery as between them.* Thus much then premised, I would notice that if the view of the purport of Blagden's letter which I have taken, be the just one, we must consider him as having refused for certain reasons to enter into any balancing of merits between Cavendish and Watt; and we must decline therefore to regard him as Watt's advocates do, viz. as a witness who said all that he could say in exaltation of Cavendish's claims, and must therefore be considered as having had nothing to say on those points on which he said nothing. As to the motives which induced Blagden to be so sparing in his reference to the dates which marked the period when Cavendish drew his conclusions, we can only surmise what they were, and may be quite wrong in our surmises. We have seen what the hypothesis of the advocates of Watt is, and I have acknowledged its truth to the extent of admitting that he deliberately left the dates undefined; but that he did so because he was conscious that Watt really preceded Cavendish in drawing his conclusion, I altogether discredit. Other and much more satisfactory reasons can be assigned for Blagden's reserve. Before referring to them, however, I would notice that when he informs us that in the spring of 1783 Cavendish communicated to him certain results, and that about the same time news was brought to London that Watt had

* I have contended in another place, that Watt's conclusion was very different in signification from Cavendish's, but, at the same time, have pointed out that they passed for identical with their contemporaries.

drawn a similar conclusion, he must be considered as intending us to understand that he declined entering upon the question of priority, although he tacitly claims it for Cavendish, by recounting his conclusions first in order, and referring to Watt's in a more secondary way, as a *similar* opinion. It was plainly his belief that Cavendish preceded Watt, and he wished that, I think, to be understood; but he entered into no justification of this view. It may be impossible for us to determine why he contented himself with the mere declaration of his opinion on the question of priority, without any attempt to establish it by proof, but we should at least acquit him of the harsh charges which have been brought against him, that he was guilty of equivocation, evasion, or suppression of the truth to serve Cavendish, whose assistant he was. For had principal and assistant conspired together to misrepresent matters to the advantage of the former, which they have been implicitly accused of doing, even to the extent of the direct falsification of dates, nothing could have been more opportune or less susceptible of detection than the fabrication of a date in April, 1783, which should give Cavendish decided priority over Watt, whose claims date from 26th April of that year. Perjury such as this would have completely escaped detection, unless Blagden chose to make confession of his guilt; but this Cavendish, according to the favourite hypothesis of the advocates of Watt, had effectually guarded against, by the liberal gifts and promises of money which he had made to Blagden. Blagden, then, did not choose in this case to tell a falsehood to serve his patron, and if he sinned at all against Watt, the sin was one of omission. His defence, however, can be urged on far higher grounds, for as I have sought to show elsewhere, the reproaches which have been cast on his honour and fair dealing, are altogether unfounded. It is highly probable that he could not establish Cavendish's priority to Watt by reference to a contemporaneous record of earlier date than 26th April, 1783. The advocates of Watt argue, as if Blagden could not but possess a memorandum of the very day on which Cavendish had unfolded his conclusions to him, so that when the latter's priority was called in question, his assistant should have been able to produce an authentic document of past date, demonstrating when the communication was made to him. It is forgotten, however, in reasoning thus, that Cavendish's priority was not called in question till the public reading of Watt's paper, April 29, 1784, *i. e.* a year later than the period at which Cavendish announced his conclusions to Blagden. Unless, therefore, they could have divined the occurrence of the Water Controversy, and had provided themselves with documents suitable for the vindication of Cavendish's priority, they were not likely to be prepared with such evidence. It is not to be imagined that Cavendish called together a circle of his friends on a particular day, and formally announced to this select audience that he had formed a conclusion concerning the composition of water, so that several parties might be expected to be ready with their note-books to bear testimony to the very hour at which the disclosure was made. From the evidence of his MS. journal, we know that his researches into the source and conditions of the appearance of nitric acid were not completed till January, 1783,* so that he was not likely to communicate his conclusions freely, till after that date, and yet if disposed to communicate them at all, he might be expected to do so in the spring months of that year, after the reading of

* *Lith. MSS.* p. 211, and *Brit. Assoc. Report*, 1839, p. 37.

his paper on the New Eudiometer in January, enabled him to prosecute with undivided attention the other inquiries embraced in his "Experiments on Air." But in all probability he did not summon a convocation of his friends on a certain day, but revealed to them one by one as he had occasion to meet them, the theories to which his experiments had conducted him. And to Blagden especially, with whom he was in constant intercourse, his communication was less likely to have been formal than to any one else. The principal and his assistant would discuss the matter together whilst proceeding with the additional researches in which both were occupied, and the results of all his observations would in this way be conveyed by Cavendish to Blagden, at various intervals during a period of weeks or even of months. When, accordingly, Watt referred a year afterwards to his letter of April 26, 1783, as showing the date of his conclusions, neither Cavendish nor Blagden might be able to produce any document of earlier date, to demonstrate the latter's priority; since in ignorance that any controversy would arise, they had taken no pains to prevent its occurrence; and Blagden might find himself altogether unable to affirm on oath, that one of the many conclusions made known to him by Cavendish. viz. that concerning the composition of water, had certainly been made to him before April 26, 1783. It is thus possible that the silence of both Cavendish and Blagden on this point amounts to an acknowledgment that they could not produce documentary evidence fixing the precise date; and even that they could not swear upon oath that communications had passed between them on the point in dispute, before the date of Watt's letter. But their silence can by no means be interpreted as an admission that they acknowledged Watt's letter to have been known to them before such communication was made. Cavendish probably found to his regret that he had no means of authenticating his originality against Watt by *extrinsic* evidence except by reference to his communication to Priestley, which for reasons already stated I believe he regarded as establishing his priority; and beyond this, accordingly, he contented himself with claiming the conclusions which he published as his own, and with acknowledging no obligation to Watt. But to claim originality was to claim priority, for Cavendish was certainly familiar with Watt's letter before he published his paper, as Blagden's allusion to Watt in his letter to Crell is in itself sufficient to prove, and nevertheless when he added a passage to his paper of 1784, commenting on the letter, he did so only to express dissent from certain of its views without expressing or implying any obligation to its author.

Whether these surmises as to the motives which induced Blagden to avoid particularity are well founded or not, I leave the reader to determine. It is of more importance to insist upon two points which are not hypothetical: 1. No charge of plagiarism was publicly brought by Watt against Cavendish. We know from Watt's correspondence that he expressed in private strong suspicions regarding the fair dealing of his rival: we know also, however, from the same source, that the only ground of these suspicions was the real or supposed identity of Cavendish's conclusions and his own, and the fact of his letter to Priestley having been made known to his rival. But Watt had no knowledge as to the period at which Cavendish had drawn his conclusions, so that, for anything he could show to the contrary, Cavendish might have preceded him even by years. In his published paper, accordingly Watt brought no charge against his rival, and the only reference he made to him occurs in a note, added shortly before the publication of his letter in the shape in which it now appears.* This note, moreover, is occupied with an ascription of credit, not blame, to Cavendish, and Watt limited himself to what he could prove, viz. that he entertained certain views regarding the composition of water in April, 1783. There was thus no public charge preferred against Cavendish, nor is it in any degree likely that he was made acquainted with the suspicions privately entertained against him by his English rival. Cavendish contented himself accordingly with adding to his paper the first interpolated passage in which he refers to what he had told Priestley, and what Blagden had told Lavoisier concerning his researches. The reference to Priestley showed that Cavendish preceded him, and therefore, as I have already contended, Watt who followed Priestley. Against Lavoisier, on the other hand, he brought a direct charge, in reply to the latter's implied denial of his originality.

2. If Cavendish thought it unnecessary to make a formal avowal of priority to Watt in 1784, there were additional reasons why he should not, either in his own name or in Blagden's, assert it in 1786. We learn from Watt's son that his father, "after becoming in 1785 a Fellow of the Royal Society, formed the personal acquaintance of Mr. Cavendish, and lived upon good terms with him."† It would have been fatal to this friendly acquaintance to have reopened the controversy in Crell's journal in 1786, and it was eminently in keeping with Cavendish's notorious indifference to fame, that he should have forbidden Blagden to stir the question of priority between him and Watt; and it was not less in keeping with his undeviating love of truth that he should have sanctioned, as perhaps he dictated, the reference to Watt as an independent observer. It was very different with Lavoisier; he had impeached, by implication at least, the veracity of Blagden, and through him that of Cavendish, and the latter, though too modest and unambitious to be very solicitous about his intellectual reputation, could not afford to lose his good name, and took ample measures accordingly to defend himself against the representations of Lavoisier. The silence, therefore, of Cavendish and Blagden in reference to Watt's claims, implied no concession of them, and is explicable on natural and satisfactory grounds.

It cannot after all, however, but be regretted that Watt did not publicly prefer a charge against Cavendish, which would have justified and rendered necessary an equally public reply. As it is, charge and reply are equally wanting; the former, it must not be forgotten, as much as the latter. The friends of Watt constantly argue as if a charge had been made but never refuted, and carry the reader's sympathies with them, especially if he forgets, as he is very likely to do, that the Watt Correspondence which he reads in print as a public accusation, was in Cavendish's time a sealed book to nearly all the world, and certainly to him. If we adopt the opposite view and take for granted that he honestly discovered the truths which he taught as his own, and that he supplied the materials from which Watt drew his conclusion after they passed through Priestley's hands, who had in part interpreted them, we shall not find anything strange in Cavendish's simple avowal of his own originality and priority. The deep consciousness of this would keep him from anxiously demonstrating that he had reaped as well as sown the harvest which others claimed, and the absence of any public denial of this deprived him of the opportunity of vindicating his capacity and integrity. He was apparently content that it should be so, and Blagden, probably in conformity with his wishes, stated that he and Watt had announced their

* *Phil. Trans.* 1784, p. 332.

† *Watt Corr.* p. iv.

conclusions about the same time, whilst he was careful to claim for Cavendish originality, and by giving him precedence, also priority.

The section just completed, has extended to a great length. It will be observed, however, that it is the most important division of the Water Controversy so far as Cavendish is concerned, inasmuch as it is occupied with his defence against the charges of unacknowledged obligations, and of posteriority to Watt, which have been so largely preferred against him. Throughout this section I have acted chiefly on the defensive; I now recapitulate very briefly the chief conclusions contended for, in the form of direct arguments in favour of Cavendish.

In evidence then of Cavendish's priority to Watt, the following indirect but important proofs may be announced.

1st. It is matter of positive certainty that Cavendish was the first who converted a given weight of hydrogen and oxygen into the same weight of water; and that he did this both with hydrogen and the oxygen of air, and with hydrogen and pure oxygen.

(1.) The experiments of Priestley from which Watt drew his conclusion, were, besides being very inaccurate, confessedly a repetition of Cavendish's, and therefore later in date.

(2.) The experiments of Lavoisier were also, and confessedly, a repetition of Cavendish's : and

(3.) Monge's experiments, which were original, are acknowledged by himself to have been of later date than the English researches.

2nd. Cavendish's reference to Watt in his paper of 1784, contains no acknowledgment of obligation, or concession of priority to the latter. Cavendish never formally asserts his priority to his English rival because it was never formally called in question. He was the first to publish his views, and at the period of publication there was no rival in the field. When Watt afterwards indirectly sought to establish priority over him, he made no alteration in his original demands, but contenting himself with acknowledging Watt as, on his own showing, an independent observer, he maintained for himself all that he had previously claimed.

3rd. Cavendish's manuscript journal is mainly a record of facts, with a statement occasionally of particular conclusions from single experiments or isolated researches, but it does not embody any of the great generalizations to which those observations, considered as a continuous series, conducted him. Its silence, therefore, in reference to conclusions, supplies no argument against such having been drawn; whilst a comparison of it with the published paper of 1784, shows that its recorded facts were generalized by their observer, and employed by him to establish various theories announced in that paper. The probabilities, therefore, are all in favour of the inference that the experiments recorded on the synthesis of hydrogen and oxygen formed no exception to the others detailed, but that like them they were more or less fully interpreted at, or soon after, the period when they were made. The dates, therefore, of the journal may justly be considered as marking more or less precisely the periods when the various theories published in the paper of 1784 were formed; and thus January, 1783, may be considered as the latest date, denoting the time when Cavendish formed his conclusion concerning the composition of water; whilst the probabilities are exceedingly great that he was ready to announce that hydrogen and oxygen are the elements of water, in 1781, after completing his experiments on the combustion of hydrogen and common air.

4th. Cavendish and Priestley both bear witness to an account having been given by the former to the latter of his experiments of 1781; and from the tenor of both statements, it appears manifest that Cavendish taught Priestley a process by which the supposed element water could be produced out of hydrogen and oxygen, by means of the electric spark or the application of flame, which determined their conversion into water. It is further certain that the communication was made to Priestley, before he supplied Watt with an account of the experiments from which he drew his conclusions.

5th. The letter which Blagden wrote to Crell in 1786, was intended to defend his own veracity against Lavoisier's impeachment of it, and to claim for Cavendish priority to the French chemist, who was likewise accused of plagiarism from his English rival. But it formed no part of Blagden's intention to discuss the question of priority between Cavendish and Watt, so that his silence on this point, which the English rivals were quite competent to settle for themselves, supplies no argument against the priority of Cavendish, and this priority, moreover, Blagden asserts, although he enters into no justification of his assertion.

It thus appears that the priority of Cavendish's experiments can be established beyond the possibility of cavil, and that the priority of his conclusions is in the highest degree probable, if not absolutely certain.

12. *Date of Priestley's Experiments, and of Watt's Conclusions from them concerning the Composition of Water.*

The questions with which this section is occupied, have to a great extent been anticipated in the preceding one; but there are some points raised by the friends of Watt which call for additional consideration. According to Mr. James Watt, his father's theory of the composition of water, was formed long before Priestley instituted his repetition of Cavendish's experiments. On this subject he remarks, "It may with certainty be concluded from Mr. Watt's private and unpublished letters, of which the copies, taken by his copying machine, then recently invented, are preserved, that his theory of the composition of water was already formed in December, 1782, and probably much earlier. Dr. Priestley in his paper of 21st April, 1783, p. 416, states that Mr. Watt, prior to his (the Doctor's) experiments, had entertained the idea of the possibility of the conversion of water or steam into permanent air. And Mr. Watt himself, in his paper, Phil. Trans. p. 335, asserts that for many years he had entertained the opinion that air was a modification of water, and he enters at some length into the facts and reasoning upon which that deduction was founded."*

The "private and unpublished" letters here referred to, are those which, after the note was written, were printed in the Watt Correspondence, and a comparison of it with Watt's published paper will show that his son's view is quite untenable. Mr. Muirhead indeed, though he reprints this note, gives a different account of matters, and so do Lords Brougham and Jeffrey. The first of those gentlemen details the progress of Watt's views, in the summary placed at the end of his introductory remarks, in the following terms.

* Note by Mr. James Watt to *Lord Brougham's Historical Note, Watt Corr.* p. 248.

"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."*

The italics in the last sentence are my own, and mark the fact of most importance to our present inquiry, viz. that Watt was unaware of Priestley's observations on the synthesis of inflammable air and oxygen, till March, 1783, and as his conclusion, that water consists of these bodies, was deduced from the experiments referred to, it cannot be of earlier date than the period of their performance. It is true that Watt had speculated on the convertibility of water into air in 1782, and long before; but his hypothesis, as the first extract from Mr. Muirhead's summary shows, and as I have pointed out at length in a previous section, (ante, p. 329) contemplated only a conversion of water-vapour into gas, without deciding anything as to the chemical qualities or composition of that gas, except that a preference seems to have been given to the idea that it would be identical with atmospheric air.† Upon this point, however, it is unnecessary to dwell. Lord Brougham justly observes "that Mr. Watt formed his theory during the few months or weeks immediately preceding April, 1783, seems probable."‡ Lord Jeffrey limits the date still more precisely. He contends, as has been stated already, that Watt drew his conclusion from unrecorded experiments made by Priestley with hydrogen and oxygen, but he considers the letter to Mr. Hamilton, of 26th March, 1783, as marking a period *even earlier* than that at which Watt drew his conclusion from the experiments in question. After quoting the letter which has just been adduced from Mr. Muirhead's summary, he remarks concerning it, "Here we have all the essentials concentrated, and brought to bear upon each other in one view as if expressly to demand, at once, and supply a solution. Yet *that solution is not given!* and we must therefore hold, had not yet been clearly perceived. It had presented itself, no doubt, and was already fermenting in the powerful and capacious mind which had so clearly conceived, and so lucidly defined the problem. But the fermentation was not completed, nor the term of incubation expired. Even the penetrating and intrepid spirit of Watt was baffled and perplexed for a season, and required time for consideration and circumspection before coming to a decision. The decision, to be sure, did come as we know *within three short weeks* after this date, and perhaps a good deal sooner."§

* *Watt Corr.* p. cxxiv—v.

† Watt, it will be remembered, at first accepted Priestley's porous retort experiments, where water was apparently converted into atmospheric air, as the realization of his hypothesis.

‡ *Historical Note*, by Lord Brougham, *Watt Corr.* p. 257.

§ *Edinr. Rev.* Jan. 1848, p. 131. The italics are not in the original.

On this point, then, the majority of the critics on both sides of the Water Controversy may be regarded as unanimous, and Watt's theory of the composition of water as a compound of oxygen and inflammable air or phlogiston, may be considered as dating from 27th March, 1783, or a few days later.

The particular date of Priestley's experiments cannot be ascertained, nor is it a point of any importance; by a comparison, however, of Priestley's paper of 1783 with the Watt Correspondence, we can surmise pretty accurately when they were made. The first letter of Priestley to Watt is dated 8th December, 1782, and probably marks the period, when the former commenced his researches into the conversion of water into air. It contains the account of an experiment on this subject, and the remainder of the Correspondence, in so far as it discusses the nature of water, is occupied with references to similar trials up to March, 1783; but no allusion occurs, previous to this date, to the conversion of air (gas or gases) into water, and the first account of an experiment on this point is contained in the letter to Mr. Hamilton of 26th March, already referred to. It is preceded in the Watt Correspondence, by a note from Priestley to Watt, dated March, but the day is not mentioned, in which one of the latest and most inaccurate of the former's experiments on the distillation of water into atmospheric air is recorded. It is highly probable, accordingly, that Priestley repeated Cavendish's experiments early in March, 1783, (ante, p. 94) for as he communicated his results almost as soon as he obtained them, to Watt, and as he, in his turn, speedily communicated them to his friends, we should have fouud some earlier allusion to them in the Watt Correspondence, if they had been made before the period in question.

The claims, however, of the various rivals in the Water Controversy are not affected by the conclusions to which we may come concerning the very day, or week, or even month, in which Priestley's experiments were made, or Watt's interpretation was connected with them. All must acknowledge that Cavendish's experiments preceded Priestley's repetition of them, and that Priestley's repetition preceded Watt's conclusions from it.

13. *Date of Lavoisier's Conclusions concerning the Composition of Water.*

There is not, and cannot be, any question of priority between Lavoisier and the English chemists, if it be conceded that the latter, or at least that Cavendish, substantially discovered the composition of water, although he held peculiar views concerning the nature of hydrogen; so that in truth it is not imperative on a critic of the Water Controversy to determine when the French chemist came to his conclusions. But it will render the discussion more complete if this is considered, and it may be dismissed in a few words. Lavoisier's own claim goes back only to the 25th of June, 1783,* and even this date is not ratified by his colleague La Place, who wrote to De Luc on the 28th of the month, stating that he and Lavoisier were not at that time satisfied that the quantity of water produced in their experiments by the combustion of hydrogen and oxygen, represented the weight of the gases consumed.† It is certain, however, that on the 25th of June, Lavoisier declared himself satisfied that water

* *Watt Corr.* (*Mém. par* M. Lavoisier) p. 178; or *Mém. de l'Acad. pour* 1781, p. 473.
† *Watt Corr.* pp. 41, 42.

is composed of hydrogen and oxygen, and we may accept this date as at least the earliest which he could affix to his conclusions. It places him later by some two months than Cavendish and Watt, both of whom we have seen had, in April, 1783, arrived at the conclusions which they afterwards published. Even, therefore, if Lavoisier's claim to be an independent discoverer of the composition of water were tenable, which it is not, he could assert no claim to be its first discoverer.

QUESTION OF PLAGIARISM.

14. *Alleged Plagiarism of Cavendish.*

In the preceding sections I have avoided as much as possible all reference to the accusation preferred against Cavendish, of having borrowed, without acknowledgment, his theory of the composition of water from Watt; and likewise that preferred against Lavoisier of having borrowed his conclusions both from Cavendish and Watt. I have sought indeed to show, that Cavendish certainly formed his theory for himself, so that he could be under no necessity of robbing Watt or any one else, and this view I strongly maintain; but in the present section of the argument, I shall, as far as possible, set this pre-judgment aside, and consider the charges brought against Cavendish by Watt and his supporters, as if the question had come for the first time before us. As for Lavoisier, it has been impossible to avoid treating him as a detected plagiarist, but it will be necessary to recur to the accusations preferred against him, were it only to inquire what can be said in his favour.

I begin with Cavendish. The charge against him arose in the following way. I take the account from Mr. Muirhead's introductory remarks, as it is quite explicit, though brief :—

"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 2nd of June, 1785. In one of Mr. De Luc's letters, dated 1st March, 1784, he mentions that he had heard some particulars of the paper which Mr. Cavendish had read, but nothing concerning *the conclusions* stated in it as to the composition of water, appears to have been then reported to him. The imperfect 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.*'"* In a second letter, written four days later,

* *Watt Corr.* pp. lviii & lix.

De Luc adds, '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 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."*

De Luc was an upright, honourable, and accomplished man, and an attached friend of Watt. His motives are beyond suspicion, but he is obnoxious to the grave charge of having made a very partial and one-sided inquiry into the question he so summarily decided, and of having leaped to the conclusion that Cavendish had acted unfairly to Watt, upon a mere perusal of the paper of the former, and without making the slightest investigation into the history of his researches.

The grounds of the charge he preferred were twofold. 1st. The certainty that Cavendish had read Watt's letter to Priestley before he drew up his paper. 2nd. The suspicious identity of the language in Cavendish's paper and in Watt's letter. An acquaintance, however, with Watt's letter, was quite compatible with Cavendish having formed his theory before he saw the epistle, and the identity of language, even if it had been absolute, did not of necessity imply borrowing, if the conclusions to be expressed in the terms of a very restricted nomenclature, were, in form at least, identical. It was plainly incumbent on De Luc to have inquired into both these points, before he proceeded to fill Watt's mind with suspicions against Cavendish. He made no such inquiry, however; nor was there a scientific man in London in a worse condition for judging of Cavendish's opinions, than De Luc. He was reader to queen Charlotte, and from Mr. James Watt we learn that, "following the motions of the court, [he] was not always in London, and seldom attended the meetings of the Royal Society."† He tells us himself that he seldom went to it;‡ and Mr. Muirhead informs us "that Mr. De Luc having gone to Paris in December, 1783, and there passed the month of January, 1784, returned to England in February."§ He was thus frequently absent from London, seldom at the Royal Society, and not even in England, when Cavendish's paper was read. His acquaintance with English, moreover, though considerable, was limited, as I have shown already (ante, p. 79). De Luc was, therefore, in most unfavourable circumstances for knowing what researches Cavendish had been prosecuting, and no one was less entitled to bring a summary charge of plagiarism against him. In truth, De Luc

* *Watt Corr.* pp. lx & lxi. De Luc's letters, from which the preceding extracts are translated by Mr. Muirhead, are printed in the *Watt Correspondence* in their original French, pp. 42-50.

† Note by Mr. James Watt to Lord Brougham's *Historical Note, Watt Corr.* p. 219.

‡ *Watt Corr.* p. 43.

§ *Watt Corr.* Introd. Remarks, p. lviii.

betrays his ignorance of Cavendish's proceedings, by the explanation which he suggests as to the origin of his paper. His notion was, that when Watt's letter was privately communicated to different members of the Royal Society, it did not excite attention, "but that some vague idea of it may have remained in the mind of Mr. Cavendish, which afterwards germinated and produced this memoir."* According to this view, Cavendish's experiments must have been *post factum* trials, made to justify a foregone conclusion; whereas, we know that his experiments on the production of water from its elements, were made before January, 1783, and that Priestley repeated some of them in the spring of that year, before Watt's letter was written, which is supposed to have led to the original trials. There is thus no one from whom the accusation against Cavendish could well have come with less weight than from De Luc; and although I cast not the slightest imputation on his motives, I will say that his zealous friendship for Watt lessened his qualifications for being an impartial critic of Cavendish's paper. Let us see, however, what the force of his accusation is.

That Cavendish was acquainted with the contents of Watt's letter is certain; so that if the fact of his having read it, or heard it read, is sufficient to prove him a plagiarist, his defence must be abandoned. It is also certain, however, that he made no concealment of his acquaintance with its contents, although he acknowledged no obligation to its author. This appears from Blagden's letter to Crell, of 1786, in which he refers to Watt's conclusions as known to him and, by implication, to Cavendish, in the spring of 1783; and still more distinctly from a letter from Kirwan to Watt, in which the following passage occurs:—"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 [June]. This he authorised me to tell you."† Cavendish, in truth, if not a plagiarist, had no occasion either to conceal or to make known his acquaintance with Watt's letter. There is not the slightest reason for supposing that he requested a perusal of the letter. On the other hand, Priestley appears to have communicated its contents to whomsoever he thought proper, and to Cavendish directly or indirectly among others. Watt's supporters, indeed, speak of the letter as having been in the custody of Sir Joseph Banks, and Watt himself furnished Blagden with a note to be added to his paper,‡ certifying, as he tells De Luc, that the letter was "left in the possession of the president until it was read."§ Blagden, however, altered this statement, substituting for it that "the letter was reserved," without mentioning the president's name; || and he certainly was justified in making the alteration. Watt's letter may have

* *Watt Corr.* p. 46.

† *Watt Corr.* p. 39. The passage quoted in the text is very ambiguous. A grammarian would find it difficult to decide whether it was Cavendish, Blagden, or Lavoisier, who authorised Kirwan to make the communication to Watt. There can be little doubt, however, that Blagden is the party referred to. It is uncertain, also (as indeed, Mr. Muirhead and Lord Jeffrey acknowledge), whether the words "*your theory*" signify Watt's theory, or "one identical with yours," that is, Cavendish's. I am by no means certain, for my own part, that the latter is not the meaning of the words. The question, however, is not of importance to our present purpose. The letter is quite explicit as to Blagden having heard Watt's letter read, and from him, if in no other way, Cavendish would become acquainted with its contents.

‡ *Watt Corr.* p. 63.

§ *Ibid. ibid.* p. 67.

|| *Phil. Trans.* 1784, p. 330.

been, in a certain sense, in the custody of the president, although seeing that it was addressed to Priestley, and not to Sir Joseph Banks, and had been desired by its author not to be publicly read, it is difficult to be certain what control the president had over it. It is beyond question, however, that it was not in his possession, or in that of any other office-bearer of the Royal Society, during a part, at least, of the interval between its being sent and its being read. On the 4th of April it was in the hands of De Luc, to whom we find Watt writing, desiring him to make an alteration on it;* and on the 15th of the same month, Sir Joseph Banks writes to Watt, "On the receipt of your favor, I wrote immediately to M. De Luc, requesting him to deliver to me your letter to Dr. Priestley;"† so that Sir Joseph's custody of the letter was only nominal, and it is impossible to discover who took charge of it between April 1783, and April 1784; but at all events it was read to members of the Royal Society, or circulated amongst them, with the consent of its writer, its receiver, and its nominal custodier, so that, in whatever way Cavendish became acquainted with its contents, he was quite at liberty, and was probably invited, to study them. Nor did the fact of his being allowed or requested to peruse the letter, carry with it an obligation to make public reference to its contents. The only point, therefore, of any importance is, had Cavendish formed his theory before he was acquainted with Watt's letter? I have already, however, contended that he owed nothing to it, and that he was, therefore, under no obligation to refer to it, even if he had thought himself at liberty to do so. De Luc's mere assumption, accordingly, that Cavendish derived his theory from Watt's letter, requires no further consideration.

The second ground on which De Luc bases his accusation is, the alleged suspicious identity of the language in Cavendish's paper and Watt's letter. It was impossible, however, that two chemists coming to the same conclusion‡ concerning the composition of water, could differ much in their mode of stating that conclusion; and the remark applies with especial force to chemists of the Phlogiston School, whose nomenclature was so limited, that it left them little choice of expression in expounding the narrow doctrine which was their guiding idea. Nevertheless, it must be acknowledged, that language is so elastic and expansible a medium of thought, that two independent observers of the same truth are not likely to exhibit absolute identity in their mode of stating it. That there is nothing, however, suspicious in the similarity of Cavendish's language to Watt's, will appear, I think, from the following comparison. Watt states his conclusion thus:—"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," &c., &c.§ Cavendish's conclusion is as follows:—"I think we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or in other words, that water consists of dephlogisticated

* *Watt Corr.* pp. 49, 50.

† *Watt Corr.* p. 53.

‡ I use the word "same" here, in the qualified sense already referred to. Watt's conclusion was not identical with Cavendish's, for they used the term phlogiston in different senses; but their employment of the same term makes the wording of their conclusions identical.

§ *Phil. Trans.* 1784, p. 333.

air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston; since, according to this supposition, these two substances united form pure water."* In comparing these statements, it must be observed that no one of the phrases used by Watt was peculiar to him or devised by him. The term "phlogiston," on the other hand, in the sense of inflammable air, was introduced by Cavendish in 1766 as a name for hydrogen,† and as synonymous with inflammable air in a less restricted signification, was as familiar to him as it was to Watt, as a doctrine of Priestley's and Kirwan's. Even, therefore, if Cavendish had learned to interpret his experiments from Watt, he did not require to borrow from him the terms in which he should state his conclusion; and it was impossible, on the other hand, holding the opinions that he did, that there could have been any very great difference in the wording of his theory from that of Watt.‡ Nevertheless, their terms are not identical, as a comparison of the passages quoted will show; and whilst Watt attaches great importance to the function of latent or elementary heat, as concerned in the decomposition and recomposition of water, Cavendish attaches no importance to this, but gives great prominence to the doctrine that phlogiston may contain water as an essential constituent. Nor is this all. In three different parts of his paper Cavendish gives the conclusion from his experiments on the production of water from its elements, in terms totally distinct from any employed in Watt's letter or paper. In the summary of his experiments with hydrogen and air Cavendish says, "Almost all the inflammable air, and about $\frac{1}{5}$th part of the common air, lose their elasticity, and *are condensed into the dew* which lines the glass." And again, "Almost all the inflammable air, and about $\frac{1}{5}$th of the common air, *are turned into pure water*." In the summary also of the experiments with hydrogen and oxygen, he says, "Almost the whole of the inflammable and dephlogisticated air is *converted into pure water*."

These passages contain the interpretation of experiments made, and in all probability as we have seen, interpreted before Watt's letter was written; and they present not only as good, but in reality a better, because a less hypothetical statement of Cavendish's conclusions than the passage quoted from his paper. The substitution of the word "phlogiston" for "inflammable air," and the notion that the former might contain water, which originated in speculations concerning the

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

† He does not claim the name as of his devising, nor had he ever sought to *demonstrate* that inflammable air was phlogiston, although he inferred that hydrogen was this entity. Priestley and Kirwan professed to have established this, and he refers to it accordingly as their view, but the notion that phlogiston and the inflammable air of the metals are identical, had been familiar to him for some seventeen years.

‡ The reader can test the justice of this remark, by trying to what extent he can vary the enunciation of the doctrine that water is a compound of phlogiston and dephlogisticated air, and that the latter is water from which its phlogiston has been withdrawn. One synonyme may be substituted for another, as, Watt says "Water is composed of", Cavendish, "Water consists of"; but beyond this there is extremely little room for change of expression. Cavendish, for example, calls dephlogisticated air "nothing but dephlogisticated water." Watt does not use this phrase, but employs the exactly equivalent one, that dephlogisticated air is "water deprived of phlogiston." A proposition so simple as that published by Cavendish and Watt, in the terms of a common nomenclature, admitted of no material difference in its wording. An ingenious advocate, in truth, might make out as plausible a case against Cavendish on the score of the suspicious *variation* of his terms from those of Watt, as De Luc does, on the score of their suspicious identity.

nature of hydrogen, added nothing to the clearness of the original view. It would have been better, in truth, if Cavendish had avoided the term "phlogiston," and had contented himself with stating, that hydrogen and oxygen can be condensed, or converted, or turned into pure water. This, however, is immaterial to our present inquiry; it is enough that the passages quoted prove, that Cavendish has stated his conclusion three times over in his own terms; and that the words of his fourth statement were as much his as Watt's, nay, were peculiarly his, so far as the identification of phlogiston with inflammable air is concerned. De Luc's assertion, therefore, that Cavendish used Watt's words was totally unjustified, and was made without any inquiry into Cavendish's independent researches and opinions.

It may be added, that we have the testimony of De Luc himself (as the quotations from his letters show), that it was inconsistent with the known character of Cavendish and Blagden, that the one should have been guilty of plagiarism, and the other a party to it. It further appears, that when De Luc requested a perusal of Cavendish's paper, its author at once gave his sanction to its being sent to him by Mr. Planta, Sec. R. S.; and communicated his willingness to allow De Luc to read the manuscript in a courteous note to him through Blagden, although, as De Luc himself observes, Blagden was well aware of the friendship which subsisted between him and Watt.*

Such were the baseless grounds upon which De Luc hastened to prepossess Watt with the notion that Cavendish had stolen his theory from him; and such was the origin of the Water Controversy, at least as it concerns the English rivals.

The accusation of plagiarism which has just been considered, was privately preferred against Cavendish before his paper was printed, or Watt's memoir was publicly read. Cavendish, however, we have seen, made some additions to his paper before it was published; and Watt's communication, which was originally laid before the Royal Society in the form of two letters—the one to Priestley, the other to De Luc, also underwent alterations, and received additions before it was printed. In the course of these modifications of the rival memoirs, certain statements were made, and certain errors in date committed, which, since the revival of the Water Controversy, have been made the ground of very grave accusations against Cavendish and Blagden, who are suspected of unfair dealing towards Watt. In the discussions to which these charges have given rise, Watt's published paper has generally been considered as unexceptionable, so far at least as *his* alterations are concerned; whilst Cavendish's memoir has been made the subject of much condemnatory criticism, on account of the additions made to it between the period of its being read and printed. I shall presently show, however, that Watt's additions are obnoxious to the very same charges as Cavendish's; and I shall subject both memoirs accordingly to a criticism of the same kind. The discussion will comprehend two points: the one, the interpolations; the other, the erroneous dates in Cavendish's and Watt's papers.

* *Watt Corr.* p. 45.

15. *Interpolations in Cavendish's and Watt's Papers of date* 1784.

The word *interpolation*, as employed by the advocates of Watt, to mark the alterations made on the text of Cavendish's paper, cannot perhaps be objected to on etymological grounds. Yet, I think, I do not wrong his impugners when I say, that they employ the word in its secondary sense, as an improper alteration, or falsification, such as would destroy the validity of a legal document, and throw doubts upon the authenticity and authority of a sacred codex. It might have been well, accordingly, if some less equivocal word had been used, such, for example, as 'insertion,' 'alteration,' or 'addition,' as the case might be. I am not anxious, however, to dispute about words, and I shall continue to employ the questionable term in the sense simply of an insertion or addition, although I hope to be able to show that the so-called interpolations in Cavendish's and Watt's papers are very innocent and harmless things. First, then, of Cavendish.

The alterations made on Cavendish's paper of 1784, between the reading and the printing, are three in number; two of them being insertions or interpolations in the body of the memoir, and one of them an addition at its close. They have been pointed out already in the abstract of the "Experiments of Air," and are marked by square brackets in Mr. Muirhead's reprint of the paper.* Nothing appears in the printed paper to distinguish these passages from the rest of the text. They have been discovered by a reference to the MS. in the Royal Society's archives; and we are indebted to Lord Brougham for bringing them to light. The supplementary addition or postscript refers solely to Lavoisier's views on the nature of water, which did not reach this country in their completed shape till after Cavendish's paper had been read, so that he could not refer to them previously. This addition is in Cavendish's own hand-writing, but it has given rise to no controversy between his advocates and those of Watt, so that it need not further be referred to.

The two interpolations were doubtless made, in consequence of Watt's publication of his letter to Priestley of 1783, and his implicit claim of priority over Cavendish. The first interpolation contains the passage so frequently referred to, in which Cavendish announces that he had communicated certain of his experiments to Priestley, and that Blagden had made similar communications to Lavoisier. The second interpolation contains the reference to Watt's paper, and the reasons which influenced Cavendish in making no allusion to latent or elementary heat, and has been already discussed. Both insertions are in Blagden's handwriting.† Such are the interpolations so much referred to, consisting, it will be observed, of two entire paragraphs inserted into the text, which, so far as I am aware, was not altered. It is difficult to see what fault can be found with them, provided it was permissible to introduce interpolations at all—a point to which I shall presently refer. Nor in truth, do the

* *Phil. Trans.* 1784, pp. 134, 140, 150. Mr. Muirhead's reprint, *Watt Corr.* pp. 129, 135, 147.

† We are indebted to Lord Brougham for pointing out this; and Mr. Weld has done those interested in the matter the further service of printing a facsimile of part of the first interpolation, along with facsimiles of Cavendish's and Blagden's handwriting, so that all may determine for themselves by which of these parties the interpolation was written. (*Hist. of the Royal Society,* vol. ii. p. 174.) The writing is certainly Blagden's. It has some similarity to Cavendish's, but is nevertheless easily distinguished from it. The point, however, is one of very small, if of any, importance.

supporters of Watt openly complain of the contents of these inserted passages, although naturally enough they regret them, seeing that the first contains the most cogent proof we possess of Cavendish's priority to Watt. Nevertheless, they refer to the interpolations as suspicious, if not unfair, and attach great importance to the fact of their being in Blagden's handwriting. Mr. Muirhead, for example, carefully incloses them between brackets, and marks them as interpolations "by Dr. Blagden, after the paper had been read." Sir David Brewster, also, specially notices that the two additions are in the handwriting of Dr. Blagden;* and afterwards reproaches him for having "inserted his interpolations in Cavendish's memoir;"† adding, "it is the testimony, therefore, of Dr. Blagden alone, that has disturbed the current of scientific history. It is his testimony, not appealed to by Cavendish, but gratuitously offered by himself, that contains the allegation that Cavendish mentioned to him and others his conclusions."‡ It is vain, however, to attempt to separate Blagden from Cavendish, or to build anything on a matter in reality so trivial as the handwriting of the interpolations. I think that I do not wrong the supporters of Watt when I say, that a reluctance to impeach Cavendish has led them to shift the supposed blame of making these additions from him to Blagden. The blame, however, if there be any, cannot so be shifted. The attempt to do so, if successful to the extent of implicating Blagden, only gives us two culprits instead of one, and increases Cavendish's guilt, inasmuch as it convicts him of cowardice as well as dishonesty, and represents him as trying to hide his malpractices by bribing his dependent to become his cat's-paw. Sir David Brewster offers no proof that Blagden's interference was unsolicited and gratuitous; and none can be given. We have no means of *positively* deciding why the interpolated passages occur in Blagden's handwriting; but of this we are quite certain, that Cavendish adopted and homologated them in full, for he permitted them to appear as integral parts of a paper which went forth to the world as written "by the Honourable Henry Cavendish, F.R.S." Much more, however, may be said in defence of Blagden. The first part of the first interpolation, which refers to the communication to Priestley, can with no probability be supposed to have been suggested by him. It refers to statements made by Cavendish to Priestley, concerning which none could bear direct witnesses but themselves; so that it is infinitely more probable that Cavendish dictated to Blagden what he should write, than that Blagden proposed to Cavendish to make such a statement. There is nothing, moreover, in the least degree suspicious in Blagden's pen having been employed in engrossing the interpolations. It was part of his duty, as Cavendish's assistant, to act as his secretary and amanuensis; to translate papers for him, and to conduct his correspondence. The note De Luc received, the extracts from Crell's journal already given, and various papers among the Cavendish MSS. in the possession of Lord Burlington, fully establish this. It was probably, therefore, as part of his customary duty, not as a gratuitous interference, that Blagden copied out the so-called interpolations for the press. It is needless, however, to insist on this. What if Blagden did suggest, nay, urge the interpolations? What harm was there in this, provided they contained only truthful declarations? The friends of Watt laud De Luc for his zealous interference in behalf of Watt, although it was altogether gratuitous and unsolicited; and why is Blagden to be blamed

* *North Brit. Rev.* 1847, p. 492.

† *Ibid. ibid.* p. 504.

‡ *Ibid. ibid.* p. 505.

if he interfered to defend his friend's reputation and honour? Hard accusations have been brought against Blagden, and an eye to his current salary and expected annuity is said to have been the main motive to his zeal in Cavendish's cause. But even if this invidious charge were true, it matters not if, in the interpolations, Blagden wrote only the truth. I put therefore aside, as perfectly irrelevant, the question—whether Blagden was the author of the interpolations, or only the clerk who penned them? Cavendish alone is responsible for their contents.

Thus much settled, the far more important question comes before us: Were interpolations of any kind, still more of such a kind as Cavendish inserted in his paper, permissible? The impugners of his claims do not assert, in so many words, that they were not, but they constantly argue as if they were unjustifiable, and represent them as improperly sanctioned by the office-bearers of the Royal Society, and especially by Blagden, who became one of its secretaries on May 5th, 1784.* It is quite certain, however, that it was the practice of the Royal Society, in 1784, to permit authors to alter their papers after they were read. The chief judge of the propriety of such alterations appears to have been one of the secretaries, who was practically the editor of the Society's Transactions. Into a minute proof of this practice having been permitted, it is not necessary that I should enter. The complaint of Watt's supporters is, that liberties were conceded to Cavendish, which would not have been allowed had any one but his own assistant been the secretary. If, however, I can show that equal and still greater liberties were granted to Watt, the accusation will lose all its force. This I shall presently seek to demonstrate by a reference to Watt's paper; meanwhile requesting the reader to take for granted that interpolations were permitted to be made within certain limits, by all the contributors to the Philosophical Transactions, I proceed to inquire whether the character of the interpolations which Cavendish introduced, rendered them unfair or inadmissible. Before doing so, however, it seems desirable to notice, that the Royal Society has been severely censured for permitting its MSS. to be altered, on any plea, by their authors before publication. The censure, however, is undeserved, and the proof of this lies in the fact, that some of our best conducted scientific societies still permit similar alterations to be made. And, in truth, it is no compliment to our freedom from envy and jealousy, that much more rigid rules now regulate the majority of our scientific bodies than were customary in the preceding century. The present system secures many advantages, which were forfeited by the looser practice of a former period, but it sacrifices some of the benefits which flowed from the ancient rule.†

The former practice, then, of the Royal Society, is not to be sum-

* *Weld's Hist. of R. Society*, vol. ii. p. 561.

† I should not wish to be understood as disapproving, in the slightest, of the rigid rules which are now enforced on the contributors to the *Transactions* of scientific societies. The number of contributors is now so immense, that different observers are often engaged in exactly similar inquiries, and collisions between rival claimants to discoveries are much more liable to occur than they were fifty years ago. Every regulation, accordingly, which can lessen the probability of these occurring, is desirable; for though controversies, when they do arise, are as difficult to settle now-a-days as they ever were, many are prevented from occurring by the voluntary subjection of the body of scientific inquirers to certain fixed rules, which enable the reality and the date of any discovery to be sharply defined and readily certified. Such rules, however, were much less necessary in the days of Cavendish and Watt than they are now, and no one complained of their non-existence.

marily condemned. The only question, however, of real interest, is—Were the statutes then in vogue administered impartially? That no unfair liberty was granted, to Cavendish at least, will appear, I think, from the following considerations. If the present rules of the Royal Society had prevailed in his day, he would have been required to place his interpolations in the shape of notes or postscripts to the original paper, and to date them. In that case, they would probably have been marked, as written some time in April or May, 1784. They would have suffered nothing, however, by such an arrangement; on the other hand, the second would have been more distinct than it is at present, for, as it stands, it contains an anachronism, inasmuch as it forms part of a paper dated January, and yet refers to another (by Watt) not made public till the succeeding April. Otherwise, the interpolations gain nothing by their insertion in the body of the paper, except that they are read continuously with that part of the text which they are specially intended to supplement.*

The first interpolation contains a declaration that certain statements were made to Priestley and Lavoisier, and a brief criticism of their researches. No one will affirm that Cavendish did Watt, or any one else, a wrong, or was guilty of any unfairness in offering the criticism; and as for the alleged declarations, either they were or they were not made; and if they were, as Cavendish, Priestley, Blagden, and in part Lavoisier, testify was the case, then their announcement contravened no principle of justice, and implied no unfairness.

The second interpolation is a criticism of Watt's views on elementary heat, and a disavowal of participation in them, which could not possibly have been made till after these views were published; and the concluding addition, which was a true postscript, though not marked as such, contained a similar criticism of Lavoisier's Memoirs on Water, which did not reach this country till some time after Cavendish's paper had been read. Cavendish's interpolations were thus of a perfectly legitimate and admissible character, and although the Royal Society had never permitted another of its members to interpolate his papers, it would have been blameless for sanctioning the additions which Cavendish made. I might well, then, spare any further justification of Cavendish on this point, but other evidences of his fair dealing can easily be furnished, and I add them, because Blagden, acting under his sanction, has been so pertinaciously censured for writing these innocent interpolations.

It may be noticed, then, 1st. That had Cavendish had a fraudulent intention, of passing off any of the additions made to his paper as having formed from the first, part of the text, nothing would have been more easy for him than to have re-written a sheet or two of his MS., so as to incorporate the addition with it. The accommodating practice of the Society rendered this quite feasible, for we have seen that Planta, the secretary, entrusted Cavendish's MS. to De Luc, and we may be certain that he would have entrusted it to its author.

2nd. Before the paper was printed, Cavendish's supposed accomplice was secretary, and could have managed the fraud without the chance of detection, so far as the appearance of the MS. was concerned; yet we find that Cavendish was not at the trouble of writing out the interpolations himself, although neither of them occupies a page in print.

* This, however, is a gain. The present practice often separates, by many pages, an innocent addition, which would tell with much more force if inserted in the text.

3rd. We learn from Mr. Weld, that the first and most obnoxious interpolation "appears in a supplementary form, on a smaller sheet of paper and of a different quality."* Dr. Davy has most justly referred to this fact as irreconcileable with the notion that there was any intention of palming off the interpolation as part of the original text.†

4th. The anachronism in the second interpolation, is incompatible with the notion that there was a purpose of antedating it. Cavendish could easily have disavowed faith in the existence of latent or elementary heat, without mentioning Watt's name, but his reference to him, and to his "paper lately read before this society," enabled every one who chose to consult the date of Watt's paper, which was published in the same double volume of *Transactions*, to observe that the interpolation was of later date than the body of the paper. I do not, however, mean to assert that Cavendish was consciously guilty of an anachronism. Like other men, he wrote chiefly for his contemporaries, to whom the dates of his paper and of Watt's were familiar, and he did not foresee how paradoxical his statement would appear to a later generation. His is not the only paper in the *Phil. Trans.* containing anachronisms. We shall presently find that Watt's paper is not free from them; and I have already referred to their occurrence in a paper by Priestley. (Ante, p. 384.)

I now proceed to Watt's paper. The interpolations in it have hitherto passed almost, if not altogether, unregarded, and its supposed freedom from these has been triumphantly contrasted with their alleged improper existence in Cavendish's memoir. Mr. Muirhead, for example, formally notices, that the imperfect reference to Cavendish's experiments contained in Watt's paper, "was not in the original draft, nor in the press copy of the letter as sent to Mr. De Luc, but was afterwards added in pencil;"‡ from which his readers cannot but infer, that the rest of the paper (no part of which is inclosed in brackets in the reprint) was all written before it was read on April 29th, 1784. Nay more, the paper, which is in the form of a letter to De Luc, is dated Nov. 26th, 1783, except certain portions marked with double commas, which are extracts from the original letter to Priestley of April 26th, 1783, so that the latest date of any part of the memoir, as it now stands, is Nov. 26th; yet it appears, that two most important portions of it were not added till after it was read, and others not till considerably after the nominal date of November, which the entire essay bears. Had Mr. Muirhead applied impartially, his principle of including interpolations between brackets, the reprint of Watt's paper would have exhibited as many as that of Cavendish does. I do not regret that he has not done so, for the interpolations in Watt's paper are of as innocent and permissible a character as those in Cavendish's essay, and it was very needless, as we have seen, to parade those in the latter. Since, however, this was done to the work of the one author, it should have been done to that of the other, so that no reader might be misled by the partial application of an invidious rule; and the duty was the more incumbent on Mr. Muirhead, that the Watt Correspondence, which he edited, supplies the means of indicating the additions or interpolations in Watt's paper. I proceed to notice what they are.

This, indeed, might be done without breaking through the rules in fashion, if the interpolations were enclosed between brackets, and marked by foot-notes assigning the dates.

* *Weld's Hist. of Royal Society*, vol. ii. p. 173.

† *Edinr. Phil. Journal*, 1849, p. 45.

‡ *Watt Corr.* p. 80.

Neither the letter to Priestley, nor that to De Luc, appears to have had any heading, and had the first letter, which treated as much of the conversion of water into atmospheric air, as of the conversion of inflammable air and oxygen into water, been provided with a title, it must have been considerably different from the one it now bears. This was not furnished till May, 1784, on the 25th of which we find Blagden, who was then Secretary to the Royal Society, and had charge of the printing of the *Transactions*, proposing to Watt that his letters to Priestley and De Luc should be incorporated, and adding, "Be so good as send me what you think the properest title to be inserted before these papers in the Transactions."* Watt replies on the 27th, "I am really at a loss what title to give the paper, but propose the following, 'Thoughts (conjectures) on the constituent parts of Water and of Dephlogisticated Air; with an account of some experiments on that subject."† This late addition of the title, is a point of as much importance as the date of any of the additions made to Cavendish's paper; for, as I have shown already, in discussing Watt's conclusions, Lord Jeffrey, apparently unaware of the period at which the title was added, has referred to it, as showing the scope of Watt's opinions in 1783: "Watt's letter," says his Lordship, "professed *only* to embody his own 'Thoughts on the constituent parts of Water,'"‡ whereas we know that it was as much occupied with thoughts on the conversion of water into atmospheric air. From the same letter of May 27th, we learn that the first note to Watt's paper§ was furnished at the same time as the title. This note is also of importance, as it contains the implicit claim to priority over Cavendish, which has been so often referred to. Here, then, are two additions, insertions, or interpolations, as unwarrantable as those in Cavendish's paper, and as much calculated to forward the interests of their author as those of Cavendish were to serve his. That those interpolations were sanctioned, nay invited (as the title at least was), by Blagden, constitutes the very gist of my argument; for here we have the alleged enemy of Watt, and the unfair favourer of Cavendish, suggesting to the former the best way of incorporating his two letters, whilst, at the same time, he tells him that "it is absolutely at your option to decide upon whichever of those methods you shall prefer;"‖ and invites him to furnish a title according to his own discretion. The addition of the note was suggested by Watt; and Blagden at once acceded to it, except that he altered a statement which it contained, that the letter to Priestley had been in the custody of the President, in which, as I have already shown, it had not been during the entire period referred to by Watt. These facts are of themselves sufficient to show that, in 1784, it was customary to permit members to alter their papers after they were read, and that Watt was granted this liberty as amply as Cavendish was. Nor is this all. On 4th April, 1784, Watt writes to De Luc, begging him to alter a phrase in the former's first letter,¶ and, on the 10th, De Luc replies, "I have corrected the phrase in the letter to Priestley."** So that Watt was permitted to alter, in 1784, a letter which bore date (including, of course, the alteration), 1783, and which was nominally in the custody of Sir Joseph Banks, but actually in possession of Watt's friend, De Luc. Further, on April 17th, 1784, Watt writes to De Luc, in reference to the letter addressed to him,

* *Watt Corr.* p. 62.
† *Watt Corr.* p. 64.
‡ *Edinr. Rev.* 1848, p. 99.
§ *Phil. Trans.* 1784, p. 330.
‖ *Watt Corr.* p. 62.
¶ *Watt Corr.* p. 50.
** *Ibid. ibid.* p. 51.

"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."* And, in another part of the same epistle, he writes, "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."† On the same day, Watt wrote to Sir Joseph Banks, informing him of the alterations, "lest it should be said by any body that the letter was fabricated at a later date than it bears."‡ In spite of this information, however, Sir Joseph did not require the additions to be dated, and they now form integral parts of a paper marked Nov. 26th, 1783, although they were not furnished till April 17th, 1784. The proceedings, then, of the office-bearers of the Royal Society were consistent and impartial, so far as the permission of alterations in Cavendish's and Watt's papers was concerned. Watt was liberally dealt with, and had every request granted, except one, viz. that the dates of the experiments mentioned in his letter to De Luc should be inserted upon the margin of his paper.§ He left it to Blagden to judge of the propriety of this, and Blagden omitted them. Watt would have gained nothing by the mention of them, so far as the theory of the composition of water was concerned, for the date of the first is 7th May, 1783, that is later than the letter to Priestley, from which Watt was recorded as claiming priority.

I need not repeat that I have not the slightest intention of imputing to Watt any sinister purpose in introducing those interpolations. I have referred to them only to show how idle the objections of his friends to Cavendish's alterations are. They have unconsciously laid hands on a double-edged weapon, which smites their client as sorely as it does his rival. The interpolations of Watt are, in truth, as numerous, as important, and as objectionable as those of Cavendish, or, if the reader pleases, they are as justifiable and as innocent. Justice, however, requires that, when a second edition of the Watt Correspondence appears, the interpolations in Watt's paper should be marked as distinctly as those in Cavendish's.

16. *Erroneous Dates in Cavendish's and Watt's Papers of* 1784.

Two most unfortunate errors in date—the one in Cavendish's paper, and the other in Watt's—were overlooked during their passage through the press. For the last of these Blagden was responsible, and probably also for the first; though this is not quite certain. Both were speedily detected, and formally corrected; the one by Cavendish, and the other by Blagden. Notwithstanding this, the less generous impugners of Cavendish's claims have extravagantly magnified the importance of these mistakes, and have, not obscurely, denounced them as evidences of most culpable carelessness, if not wilful fraud, on the part of Blagden, who allowed them to remain, if he did not contrive them, to serve the interests of his patron Cavendish. Only those who interpret every doubtful circumstance in the history of the discovery of the composition of water by the uncharitable and utterly untenable hypothesis that Cavendish and Blagden conspired to rob Watt of the honour of making it, could have found proofs of false dealing in these typographical errors. They are

* *Watt Corr.* p. 54.
† *Ibid. ibid.* p. 55.
‡ *Ibid. ibid.* p. 56.
§ *Ibid. ibid.* p. 64.

thus referred to by Watt himself, in a letter to De Luc of 27th June, 1786: "It seems odd, but in the detached memoirs of Mr. Cavendish and myself, on the composition of water, they should both be wrong dated,—Mr. Cavendish's dated 'Read January 1783,' when it was read January 1784, and my letter to Dr. Priestley dated April 1784, when it was written April 1783."* On this passage Mr. Muirhead furnishes the following comment: "This refers to the copies of Mr. Cavendish's memoir for private circulation, which were circulated by him *before* the publication of the seventy-fourth volume of the Transactions for 1784, having on their title-page this date: 'Read at the Royal Society, January 15th, 1783.' The date at the head of the paper itself is rightly given in the Philosophical Transactions, but omitted in those copies. It is not the letter to Dr. Priestley, but that to Mr. De Luc, which is misdated in the Philosophical Transactions; being there dated '26th November, 1784,' when the real date was 1783."†

From this account it will be seen that Cavendish's paper bears the proper date—namely, 15th January, 1784—in the *Phil. Trans.*, but that the separate copies were marked 1783. Watt's paper, on the other hand, was wrongly dated alike in the *Transactions*, and in the detached copies, in both of which it is headed "Read April 29th, 1784," and bears in addition the date "November 26th, 1784." Mr. James Watt, referring to the latter date, candidly acknowledges that it "is evidently an error of the press;"‡ and he simply remarks, "that another extraordinary error of the press was committed in the numerous separate cópies of his paper circulated by Mr. Cavendish."§ With these statements no fault can be found. The errors in date were singularly unfortunate, and naturally enough appeared to Watt very extraordinary; but his recent supporters have gone far beyond him and his son in denouncing these errors. Arago considers them as most formidable. "To complete *the imbroglio*," says he, "the foremen, the compositors, and printers of the *Philosophical Transactions* also took part in it. Some dates in them [Cavendish's and Watt's papers] were typographically wrong. In the detached copies of his paper which Cavendish distributed to various learned men, I observe a mistake of one whole year. By a sad fatality—for it is a real misfortune to give rise unintentionally to annoying and unmerited suspicions—not one of those numerous errors‖ of the press was favourable to Watt! God forbid that I should, by these remarks, intend to cast any imputations on the literary probity of those illustrious philosophers whose names I have mentioned; they only prove that, on the subject of discoveries, the strictest justice is all that can be expected from a rival, or a competitor, however high his reputation may already be."¶ Sir David Brewster takes as unfavourable a view of the wrong dates. "Mr. Watt's paper," says he, "with that of Cavendish, was printed under the sole superintendence of Dr. Blagden, who had been appointed Secretary to the Royal Society on the 5th May; and in a controversy like this, where charges of various kinds have been reciprocated by the hostile parties, it deserves to be seriously noted that Mr. Watt's paper is printed with the *erroneous date of* 1784, *in place of* 1783, and that the separate copies of Mr. Cavendish's paper have the *erroneous date of* 1783 *in place of* 1784. The obvious effect of these two errors was to give a priority to the labours

* *Watt Corr.* p. 70. † *Ibid. ibid.* p. 70. ‡ *Ibid. ibid.* p. viii.
§ *Ibid. ibid.*
‖ The errors referred to here as numerous, were only two.
¶ *Eloge of James Watt.* Mr. Muirhead's translation, *Watt Corr.* p. 232.

of Cavendish over those of Watt; and when we consider that the separate copies of papers are chiefly circulated abroad before the publication of the *Transactions*, and would not fail to produce their impression in quarters where no correction of the error could be made, we must reprobate the *negligence* of a functionary—if that be a right name for the deed—who, in the very first act of his official duty, made so great a mistake in favour of his friend and patron. We shall have occasion again to glance at this double contingency, but, in the meantime, we cannot but express our conviction, that in a court of justice it would shake the testimony of the witness who permitted it, and damage the cause of the party whom it was intended to benefit."* In another place also Sir David repeats his condemnation of Blagden's conduct, declaring that, "in the performance of his principal duty, viz. in superintending the printing of the *Philosophical Transactions*, the new secretary commits, or allows to be committed, two gross errors of date, both of which are favourable to his patron, and unfavourable to Mr. Watt."† Mr. Muirhead, who always takes the most unfavourable view of Cavendish's and Blagden's proceedings, affirms that "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."‡

It is refreshing to turn from these uncharitable surmises, to the generous judgment which the most successful of all the defenders of Watt, Lord Jeffrey, passes upon both Cavendish and Blagden. "The higher elements," says his lordship, "of our nature are not so discordantly blended within us, as that the love of honourable fame should lead to the disregard of truth and honesty. But Cavendish was almost as remarkable for his indifference to fame, as for the high principle and honour that belonged to his station and his character; and it would have been strange indeed, if, for the sake of adding one more to his many intellectual triumphs, he had stooped, by a deliberate falsehood, to the very lowest depths of moral degradation. Nor have we any reason to think that his friend Blagden, who had not even the temptation of a rival claim to mislead him, would have stooped to such a baseness." § There are few impartial persons who will find in the erroneous dates anything at variance with Lord Jeffrey's estimate of the character of Cavendish and his assistant. By those who, with myself, believe that Cavendish owed nothing to Watt, the notion that he could have been a party to the falsification of dates will be at once discarded as utterly incredible; and as for Blagden, he must have been a much less able and accomplished person than he is universally acknowledged to have been, if he could have expected to serve his patron by so clumsy and transparent a device as the alteration of a date in the unauthoritative copies of his paper. In reality, however, there is no proof that Blagden was responsible for the wrong date. It formed no part of his official duty as secretary to superintend the printing of the detached copies of Cavendish's paper. It lay between the author and the printer to arrange concerning these; and although it is probable that Blagden, in his capacity of assistant, took charge of them, Cavendish was as responsible for the contents of the papers as he was.

* *North Brit. Rev.* Feb. 1847, p. 493.
† *Ibid. Ibid.* p. 504.
‡ *Watt Corr.* p. lxvi.
§ *Edinr. Rev.* 1848, p. 88.

How the error arose, it is perhaps impossible now to discover; but probably an alteration took place in the paging of the *Transactions,* as is customary at the present day, before or after the separate copies were printed, and during the alteration of the types one figure was substituted for another. I can say nothing positive on this point, however, as I have not seen any of the detached papers, or any special account of them. It is not so very rare a thing, nevertheless, for an error to creep into a revise, that we need wonder very much at it. That it was accidental, we may confidently affirm, for the following reasons: 1. The wrong date neither did, nor could alter in any respect the relative priority of Cavendish, Watt, and Lavoisier. If a passage referring solely to the first had been antedated, whilst the remainder of the paper retained the later date, there might be some ground for suspicion of fraud. But how does the case stand? Cavendish's paper, as it appears in the *Transactions,* contains two references to priority; the one is the account of the revelations made to Priestley concerning the researches of 1781, of which a repetition is declared to be recorded by the latter "in the preceding volume of the *Transactions.*" The other reference to priority states that an account of Cavendish's theory was given to Lavoisier "last summer." A third passage notices certain opinions of Watt announced by him "in a paper lately read before this Society." These references also occur in the detached copies, so that the relative priority of the three claimants is represented in the same way in both issues of the paper, with only this exception, that they are dated backwards from 1784 to 1783 in the detached copies; so that if Cavendish antedated his own researches by a whole year, he also antedated by the same period the revelation to Lavoisier, and the reading of Watt's paper. Everything, in fact, was shifted back twelve months, but this left the relative claims of all parties exactly as they were before the shift took place. It cannot be imagined that Cavendish or Blagden was so stupid as to expect to gain any advantage by such a useless device as this.

2. Cavendish's contemporaries, whom the separate copies are supposed to have been specially intended to deceive, were furnished with direct means of detecting the erroneous date by the references to "the preceding volume of the *Transactions,*" and to the "last summer," both of which allusions applied to 1783, and were therefore incompatible with the paper containing them bearing the date January, 1783. And in truth, there is no reason to imagine that Watt's reputation suffered from the error in date. It is true that Cuvier, in one essay, gives the date of the reading of Cavendish's paper as January, 1783, and the friends of Watt make much of the mistake.* But even here Cuvier has not contrasted Cavendish with Watt, and is not asserting anything to the disadvantage of the latter on the authority of the false date; and in his éloge of Cavendish† he gives the date accurately, "le 14 de Janvier, 1784." And we may set off, against what little wrong was done to Watt by Cuvier in his first statement, published "at the distance of four-and-twenty years from the circulation of the erroneous date," when it could do no mischief, the corresponding wrong to Cavendish occasioned by Lord Jeffrey's reference to the title of Watt's "Thoughts" as dating from 1783, and consider the two philosophers as quits.

3. A further proof of the accidental nature of the error in date, will be furnished to all impartial parties by the consideration, that the utmost Blagden or Cavendish, or both could hope to achieve by it, if it were

* *Rapport Historique*, p. 57. Quoted by Mr. Muirhead, *Watt Corr.* p. lxv.

† *Eloges Historiques*, tome ii. p. 87.

wilful, was the misleading of those to whom the separate copies were sent, for a few weeks or months, till the *Phil. Trans.* were published. And the only parties whom it could have been of any moment to deceive in this way, viz. the friends of Watt and those of Lavoisier, could not possibly have been misled by it, for the former had had access through De Luc to Cavendish's MS., and the latter knew from Blagden that Cavendish had read no paper on the composition of water to the Royal Society in 1783. Whom, then, could the wrong date be intended to lead astray? The answer of all unprejudiced persons will be, I think, No one. A detached copy of a memoir published in the transactions of a society, is of authority only in so far as it is identical with the text of the *Transactions,* of which it professes to be an isolated portion, so that where there is any difference between the two, as there is here in a particular date, the *Transactions* only are authoritative.

4. But all this reasoning might have been spared. Cavendish detected the error, and immediately furnished a correction of it in the form of a letter to the editor of the *Journal de Physique,* which I quote in full.

LETTER OF CAVENDISH TO MONGEZ.

A Londres, ce 22 *Fevrier,* 1785.

En lisant, Monsieur, la traduction de mon mémoire sur l'air, publié dans le *Journal de Physique,* je fus frappé de le voir datté de Janvier, '83, comme si la lecture en eut été faite *alors,* devant la Société Royale. J'eus recours aux exemplaires détachés imprimés pour l'usage de mes amis sur l'un desquels apparemment avoit été faite votre traduction; je trouvai à mon grand étonnement que l'imprimeur avoit fait cette même faute dans toutes les copies, malgré que l'original publié dans les *Transactions Philosophiques* avoit été datté, comme il devoit l'être, de Janvier, '84. Je vous serai très obligé, Monsieur, de vouloir bien faire mention de cette méprise dans le cahier prochain de votre Journal.

Je suis mortifié d'être dans le cas d'ajouter qu'il s'en faut de beaucoup que la traduction soit exacte; on a manqué le sens en plusieurs endroits. J'ai l'honneur d'être, avec des sentiments distingués,

Monsieur,

Votre très humble et très obeiss[t]. serviteur.

A Monsieur T. A. Mongez, le Jeune, &c. &c. &c.
Au Bureau du Journal de Physique à Paris.*

* *Brit. Assoc. Rep.* 1839, pp. 65, 66. I have alluded to this letter in the personal narrative, as if it had certainly been published by Mongez, in conformity with the impression conveyed by Mr. Harcourt's references to the letter (*Brit. Assoc. Report,* 1839, pp. 41 and 65.) Whilst, however, this sheet is passing through the press, I have discovered that Mongez did not publish Cavendish's letter in his Journal. There is not indeed *legal* proof that the letter was sent to Mongez. A draft of it remains among the Cavendish MSS., from which Mr. Harcourt has printed it. That the letter, however, was actually despatched to Mongez, and was received by him, admits of every confirmation short of absolute proof. Cavendish's first series of "Experiments on Air" was published with the wrong date (1783) in the *Journal de Physique* for December, 1784, p. 417, and January, 1785, p. 38. The second series of "Experiments on Air" was published in the part of the same journal for August 1785, p. 107. To this page a note is attached by the *Redacteurs* of the *Journal de Physique,* in which they acknowledge that they have received from Cavendish a more accurate translation into French of his first "Experiments on Air" than the one they had published. For reasons assigned they decline to insert this, and with Editorial reluctance to confess error, they do not point out the mistake in date, to which Cavendish had drawn their attention in the same letter (*vide supra*), in which he had complained of their inaccurate translation. A fatality seems to have attended the dates of Cavendish's papers in 1784, for Kirwan's Remarks on his "Experiments on Air," and Cavendish's reply to Kirwan, are misdated in the *Journal de Physique,* t. xxvi. pp. 414–425.

On this letter Mr. Muirhead remarks "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."* This ungenerous comment scarcely requires notice. In how many quarters Cavendish corrected the error, we do not know; but it savours of the ludicrous to suggest, that it was incumbent on him to send letters round Europe, announcing that a solitary typographical error existed in the private issue of his paper. At the present day, when journals are so much more numerous than they were in the days of Cavendish, a writer who detected an error in the detached copies of a paper from the Transactions of a society, would be considered to have done enough, if he wrote to a single influential journal pointing out the mistake; Cavendish, however, did more. The letter to Mongez, if it had been published, would have informed the French Philosophers of the error. Crell was furnished with information which secured the German chemists from mistake, and in England no correction was needed.

The erroneous date in Watt's published paper admits, if possible still more fully than that in Cavendish's, of being proved to have been undesigned and accidental. Blagden, certainly, is responsible for it, as Watt declined to have the proof sheets sent to him to Birmingham, and expressed his confidence that Blagden would properly incorporate the letters to Priestley and De Luc, and make the other alterations which were desired.† Blagden, accordingly, would deserve the severest reprehension, if it were in any degree probable that he had betrayed the trust Watt placed in him. He must, moreover, have been a very bold falsifier, if he could have altered a date, which was to come under the notice of Watt (who had commissioned fifty separate copies of his paper) soon after the error was committed, and probably before the volume of the *Transactions* in which it appeared was published. The erroneous date, however, bore error upon its very face. It might have been overlooked by one who consulted the paper solely with a view to study its contents, but could not have been passed over by one who referred to it in order to settle the chronology of Watt's writings. The paper it will be remembered, bears two dates, and these as they appear in the *Phil. Trans.* are incompatible with each other, and thereby betray that one, at least, is erroneous. The title is as follows:—

"*Thoughts on the Constituent Parts of Water and of Dephlogisticated Air; with an Account of some Experiments on that subject. In a Letter from Mr. James Watt, Engineer, to Mr. De Luc, F.R.S. Read April* 29, 1784.

Birmingham, November 26, 1784.

Dear Sir,

In compliance, &c. &c."

Any one who consulted this heading, with a view to fix the period when the contents of the paper were written, could not fail to observe that the dates were irreconcilable, for they represent a letter which was publicly read in April, 1784, as not having been written till the succeeding November, that is six months after it was read. A reader who observed this, would be at a loss to determine which of the dates was wrong; that of the private writing of the paper, or that of its public reading. But if

* *Watt Corr.* p. lxiv.

† *Ibid. ibid.* p. 68.

he turned to the errata at the close of the volume, (which none of the defenders of Watt or impugners of Blagden, seem to have thought of doing), he would find the following correction, "Vol. LXXIV. part II., [page] 329 [line] 7, *for* 1784, *read* 1783;" from which he would at once learn, that the letter was written in November, 1783, and read in April, 1784. Upon this point I might spare any further comment. An error which betrays itself, and which was corrected by him who originally overlooked it in the same volume in which it occurs, will not be regarded by any candid inquirer as an artful blunder to serve a sinister purpose.

It is easy, however, to push the defence of Blagden further. Watt, it will be remembered, sent a note to him to be added to his paper, referring to the letter to Priestley, but left a blank which Blagden should fill up. The passage in Watt's letter was as follows, "The letter [Priestley's] therefore remained in the custody of the President until ——— ; when at the author's request it was read before the society."* The first clause of this sentence Blagden altered to "the letter, therefore, was reserved *until the 22nd of April last.*"† Now the note containing this passage forms an integral part of a document dated (as corrected in the erratum) November, 1783, so that the "April last," counting from this, would be April, 1783, and thus the passage represents the letter to Priestley, as read to the Royal Society a year before the period when it actually was communicated to it, and many months before Cavendish's paper was read.

There can be no doubt that Blagden intended the "April last," to date from the period of the reading, not from that of the writing of the letter to De Luc. The date of the reading was April 29th, 1784, and the letter to Priestley was read on the 22nd of the same month and year. There is nothing, however, in the note to point this out. But for the Watt Correspondence, every reader would imagine Watt's paper to have been written in November, 1783, so that the April preceding it was of necessity the April of the same year. I have pointed this out to show the exaggeration of the statements already quoted, in which the advocates of Watt declare, that of all the typographical errors that could have been made, those only were suffered, or committed, which gave Cavendish an advantage over Watt; whereas Blagden thus appears to have filled in a blank in Watt's note, in such a manner as to represent the public reading of the first version of his paper, as having occurred many months before the reading of Cavendish's. This may suffice as a reply to the declaration, that none of Blagden's errors were favourable to Watt.

The worst, then, that can be charged against Blagden, is negligence in the discharge of his duties as Secretary to the Royal Society, nor am I required to defend him against this accusation; yet I will say a word in defence of one who has been so unjustly and severely blamed, because he overlooked typographical errors. It should be remembered by those who wish to do him justice, that he was new to the duties of the secretaryship when the mistakes we have been considering occurred, and that he offered to send the proofs of his paper to Watt, which the latter declined. It should further be noticed, that Watt's paper was not a continuous single document which could at once be put in the hands of the printers, but consisted of at least three papers; the first, Watt's letter to Priestley; the second, his letter to De Luc; the third, his letter to Blagden, containing the title and the note claiming priority. There were thus three documents which had

* *Watt Corr.* p. 64.

† *Phil. Trans.* 1784, p. 330.

in whole or in part to be dovetailed together, so as to constitute a continuous memoir, and the risk of error was much greater than if the whole had been written out by Watt, and despatched to Blagden ready for the press. It is probable, moreover, that Blagden put into the printers' hands the letter to De Luc with the proper date affixed to it, as he received it from the latter, and that they were guilty of the blunder about which so much has been said; but whether or not this was the case, it does not demand a great stretch of charity towards Blagden to suppose that the error, which he afterwards corrected, was an oversight. Watt himself, was so confused with the many alterations which he had made on his paper, that he blundered still more grossly than Blagden, in reference to the error in date, which he stated occurred in the letter to Priestley, instead of in the letter to De Luc.*

And as if to show how charitable authors should be in judging typographical errors, and what need there is for caution in imputing motives to those who have suffered misprints, Mr. Muirhead, who judges Blagden so harshly, has imitated him in introducing an error into his edition of Watt's paper. I have already referred to Mr. Muirhead's care as an editor, in the publication of the Watt Correspondence, and the error I have to notice I fell upon accidentally, for I have not busied myself seeking for mistakes. It occurs in the note already referred to, as altered by Blagden, containing the explanation of the reason for withdrawing the letter to Priestley. In the *Phil. Trans.* it runs thus: "The author, having heard of Dr. Priestley's new experiments, begged that the *reading* might be delayed."† Whereas according to Mr. Muirhead's reprint, Watt "begged that the *meeting* might be delayed."‡ What would Mr. Muirhead say, if some of the extreme supporters of Cavendish were to insinuate that there was something very suspicious in this innocent misprint, and that he had a sinister purpose of serving his client by representing him as delaying only the meeting, so quickly did he expect to be able to obviate the difficulties which Priestley's new experiments threw in the way of his theory? Mr. Muirhead would justly be indignant at a charge so unfounded as this, yet it is as well founded as that preferred against Blagden. If Mr. Muirhead, guided by a standard of editorial accuracy much more precise than any author set before him in Blagden's days, with abundant leisure to execute his task, and printed papers to copy from, could not, with all his solicitude to avoid error, escape making a blunder, we may well be lenient towards the new Secretary of 1784, who, in the days of careless editing, had to construct a continuous paper out of three unconnected documents; and we may further assign to him a merit which we cannot impute to Mr. Muirhead, viz. that he at least detected his error, and pointed it out in an erratum. There are few literary or scientific men who have not had the mortification of discovering, when it was too late, some overlooked error in their printed productions; and I know not who among them can venture to cast the first stone at the erring Blagden. I, at least, will request that my typographical errors may be leniently judged.

17. *Alleged Plagiarism of Lavoisier.*

Four French philosophers, La Place, Lavoisier, Meusnier, and Monge, are connected with the discovery of the composition of water, but only Lavoisier has been excepted against by his English rivals. Monge, as

* *Watt Corr.* p. 70. † *Phil. Trans.* 1784, p. 330.
‡ *Watt Corr.* p. 78.

we have already seen, disclaimed priority to Cavendish, and did not even establish a right to be considered an independent discoverer of the true nature of water, so that I shall say nothing concerning him in this section; his integrity having been conspicuous and never called in question. La Place also, is without reproach, so that he will be passed over without notice; although in another section, where I shall have the more pleasant duty of contrasting the merits of the rivals in the Water Controversy, he will receive honourable mention. Meusnier, likewise, appears to have been simply an experimental assistant or colleague of Lavoisier, and need not be specially referred to, so that the last named chemist alone will come before us as accused of plagiarism. I should have been glad to have been spared the consideration of this charge, encouraging as I do the hope that among the unpublished papers of Lavoisier, which Dumas is understood to be editing, will appear something fitted to lessen the force of the accusations which have so long remained unanswered and unanswerable. But it would be affectation to conceal that at present, so far as published documents are concerned, Lavoisier stands forth as a detected plagiarist. His distinguished countrymen, Arago and Dumas, have in the meanwhile abandoned his defence; for Arago styles him in his éloge of Watt "a pretender" to the discovery of the composition of water,* and Dumas "adopts completely and in all its parts, the history which M. Arago has written of the discovery of the composition of water."† Thus much, however, is said in the Eloge in vindication of Lavoisier, that with the exception of Watt, all those whose names figure in Arago's narrative were to a greater or less degree guilty of falsehood, at least to the extent of concealment of truth.‡ I am slow, however, to credit that Arago and Dumas can sanction so hateful a doctrine, as that disregard of truth by one man can in the slightest degree justify its disregard by another; but if they do, I need only say that the multiplication of Lavoisier's fellow-transgressors will not lessen the guilt of his transgression. Though it were true indeed, which it is not, that Lavoisier's rivals as well as himself, were plagiarists, he could not, on the lowest ground, plead justification on that score. None of them borrowed anything from him, so that he could not say that he only committed reprisals; nor did he know anything concerning their alleged obligations to each other when he claimed the disputed discovery, so that he could not even plead that he had only followed a bad example. He must stand therefore alone, and be judged as to his fair dealing, without any reference to the short-comings or delinquencies of others; and I fear that he stands self-condemned. It appears from the Watt Correspondence§ that he found in private a zealous defender in De Luc, who tried hard to convince Watt that though Cavendish had shamefully wronged him, Lavoisier was altogether innocent, and owed nothing to the account which had been given him of the English experiments. Watt, however, was deaf to all De Luc's protestations. "You see," says he, in a letter to the latter, "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."‖ De Luc laboured hard in reply to shake this opinion;¶ but Watt returned him for answer, "I must still

* Mr. Muirhead's Translation, *Watt Corr.* p. 228.

† *Comptes Rendus de l'Acad. des Sciences,* 20 Janvier, 1840, p. III.

‡ *Eloge of Watt.* Mr. Muirhead's Translation, *Watt Corr.* p. 230.

§ P. 41. ‖ *Watt Corr.* p. 40. ¶ *Ibid. ibid.* p. 41.

differ from you in regard to Mr. Lavoisier's knowledge of my theory before he even made his experiments."*

Watt was justified in his suspicions of Lavoisier. Reference has already frequently been made to the visit which Blagden paid to Paris in June, 1783, and to the account which he gave to Lavoisier of Cavendish's experiments, and the conclusions drawn from them. Lavoisier, as we have also seen, acknowledged that a communication had been made to him by Blagden, but represented it as much more limited than the latter asserted it was. The problem, therefore, of Lavoisier's innocence turns chiefly upon the question, Is his account of matters, or Blagden's, the more credible? The difference, it will be remembered, lay chiefly in this, that Lavoisier represented Blagden as having only told him that water could be obtained by the combustion of inflammable air in close vessels (ante, p. 336); whilst Blagden asserted that he had further informed Lavoisier, that "the water was equal to the weight of the two airs added together;" that he had likewise made known to him the conclusion drawn both by Cavendish and Watt as to the composition of water; and that both Lavoisier and his friends were incredulous as to the equality of weights, and as to the validity of the conclusion. Sir David Brewster is the only party, so far as I am aware, who has called in question the accuracy of Blagden's statement. He seeks to convict him of a failure of memory in 1786, when he impeached Lavoisier's veracity in his letter to Crell, as to the statements which he had made to the French chemists in 1783. "The assertion of Lavoisier," says Sir David, "that Blagden mentioned to him *only* the experiments of Cavendish, and the fact that, in the account given of the French experiment of the Academy of Sciences on the 25th June, Lavoisier states that the conclusion as to the compound nature of water was drawn by La Place and himself, may be fairly held as a proof that Dr. Blagden had forgotten in 1786 the extent of the communication which he made to the French chemists in 1783, and may have made a second mistake also in his statement that Cavendish communicated to him, and his particular friends in the Royal Society, in the spring of 1783, the conclusions which he drew from his experiments;"† and again: "Are we not entitled to suppose that in his [Blagden's] mind, the year 1783 took the place of 1784, and that the communication of his conclusions, alleged to have been made by Mr. Cavendish in 1783, were actually made in the beginning of 1784, just before his paper was read to the Royal Society, and that he did not communicate these conclusions to the Academicians in 1783, because they had not then been communicated to himself. This seems to be the only supposition upon which we can reconcile the declarations of Lavoisier and La Place with the declarations of Dr. Blagden: and it relieves both parties from the mutual recrimination of their friends that neither of them had told the whole truth."‡

Sir David's objections would be very serious if they could be maintained, but they are so completely answered by Lord Jeffrey, that I need only quote his observations on the subject. "As it has been surmised, in palliation of the disingenuousness which it appears to impute to most eminent and meritorious individuals, that Blagden, when thus writing at the distance of two years, may have misrecollected the extent of the information he had given verbally to the Parisians so long before, it is fortunate that we can now show, from documents recently brought to

* *Watt Corr.* p. 42.

† *North Brit. Rev.* Feb. 1847, p. 487.

‡ *Ibid. ibid.* p. 488.

light, that he had openly given the same account *immediately* on his return to this country; and desired it to be communicated on his authority, to those whom it most concerned. In a letter, accordingly, addressed to Watt by Kirwan, in December, 1783, immediately after the former had first heard of the French experiments, and expressed his suspicions of their origin, he says, "*Lavoisier certainly learned your theory* (not experiments merely, but 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 Lavoisier*, last July (mistake for June). *This he authorised me to tell you.*"* And Cavendish himself, in his paper read in January, 1784, put openly on the record of the Royal Society, that "during the last summer a friend of mine gave an account of these experiments to M. Lavoisier, *as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of its phlogiston;* but at that time M. Lavoisier was so far from thinking any such opinion warranted, that *till he was prevailed upon* to repeat the experiment, he had difficulty in believing that nearly the whole of the two airs could be converted into water."

"These we think," continues Lord Jeffrey, "were public enough challenges to the advocates for the French discovery; and we are yet to learn that any champion ever appeared to take them up."†

It appears that none did. Yet it is incredible that the French chemists can have been ignorant of the charges preferred against Lavoisier. Cavendish's paper was translated in Crell's Chemische Annalen, and was thus made accessible to readers of German. It appeared also in Mongez' *Journal de Physique*, which was published in Paris; and it is not at all improbable that De Luc, who corresponded with La Place about Lavoisier's synthetical experiments on water, gave the former some hint as to Watt's estimate of the originality and fair dealing of Lavoisier. Blagden's letter to Crell also, was very unhesitating and minute in its statements, and implicated, to a certain extent, other members of the French Academy besides Lavoisier. One of these was La Place, and among others were in all probability MM. Le Roi and De Vandermonde, who, Lavoisier states, were witnesses along with several other Academicians‡ of his repetition of Cavendish's experiments. Berthollet also took great interest in the experiments made in England and in France on the production of water, and corresponded with Blagden on the subject. A letter which passed between them is given elsewhere. At all events some half-a-dozen members of the Academy heard the account which Blagden gave Lavoisier "of these new experiments and of the opinions founded upon them;" and Blagden taxes them all with having been sceptical, both as to the experiments and the conclusions drawn from them. "They did not doubt," says Blagden, in his letter to Crell, "that in such manner a considerable quantity of water might be obtained; but they felt convinced that it did not come near to the weight of the two species of air employed; on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in and united with the airs, and deposited in their combustion. *This opinion was held by Mr. Lavoisier, as well as by the rest of the gentlemen who conferred on the subject;* but as the experiment itself appeared to them very remarkable in all points of view, *they unanimously requested*

* *Watt Corr.* p. 39.

† *Edinr. Rev.* Jan. 1848, pp. 70, 71.

‡ *Watt Corr.* p. 176.

Mr. Lavoisier, who possessed all the necessary preparations, to repeat the experiment on a somewhat larger scale, as early as possible. This desire he complied with on the 24th June, 1783, (as he relates in the latest volume of the Paris memoirs)." *

It must be considered in the highest degree improbable that Blagden's letter should have escaped the attention of every one of the Academicians referred to in it; yet if they were acquainted with it, we may be certain that they would have called Blagden to account, had he imputed to them opinions which they did not hold. Against Lavoisier himself, moreover, Blagden reiterated with great pertinacity a formidable list of charges, which I abridge from the letter to Crell. He affirmed—1st. That at the period of his communication to him, Lavoisier had not formed the opinion that water was composed of hydrogen and oxygen, but believed that an acid would result from their union. 2nd. That he was informed of Cavendish's experiments *some days* before he made his own; not on the very day that he tried them. 3rd. That his experiments were made *in consequence* of what Blagden had told him. 4th. That he was not told that Mr. Cavendish had obtained "une quantité d'eau très sensible," but that the water was equal to the weight of the two airs added together. 5th. That he was made acquainted with Messrs. Cavendish and Watt's conclusions. 6th. That he was not led to the alleged discovery, as he represented he was, by following up his own experiments commenced in 1777, but was induced to make the experiments which he reported to the Academy, solely by the account which he received from Blagden of the English experiments. And the indictment is wound up by the unequivocal declaration that Lavoisier discovered nothing but what had before been pointed out to him, to have been previously made out and demonstrated in England.†

That such an impeachment of Lavoisier's originality, veracity, and good faith, would have been allowed to pass unnoticed, if it could have been answered, is incredible. It may be suggested, that it did not attract the attention of those whom it concerned at the period of its publication; but this is exceedingly improbable. Lavoisier survived its appearance some eight years, and many of his fellow-academicians, La Place among the rest, long outlived him, so that it must be considered extremely unlikely that the contents of Crell's letter to Blagden escaped the attention of the party accused, and of all his friends and contemporaries. The force of this argument is increased by the consideration that Blagden, who often visited Paris, and was in constant correspondence with the philosophers there, is very likely to have put his letter in the way of those whom it concerned, or to have drawn attention to it. The first reference to it in the Water Controversy was made by Arago, so that, sooner or later it came to be known in France. It is not necessary, however, to insist upon Lavoisier's silence as implying inability to answer any or all of Blagden's statements. We have no right to affirm this, and I do not wish even to assert that Blagden was justified in all he said, although I feel assured that he thought he was. I have pointed out in another place (ante, p. 345) that he was wrong in affirming that Monge's experiments were certainly of later date than Lavoisier's, and were confessedly repetitions of them. It must, however, be considered upon any view very inexplicable, that no notice should have been taken in France of Blagden's letter. The only point, nevertheless, of very great import-

* Mr. Muirhead's Translation, *Watt Corr.* p. 72.

† Mr. Muirhead's Translation, *Watt Corr.* pp. 72—74.

ance is, were Blagden's accusations substantially true; and that they were, will, I think, appear from the following considerations. Lavoisier acknowledges that *some* account of Cavendish's experiments was given him by Blagden, and La Place bears independent testimony to the fact. It is quite certain, moreover, that all that Blagden professes to have told the French chemists, he was in a condition to tell them, and no reason can be assigned why he should have concealed anything when he spontaneously made a communication concerning the experiments and conclusions of Cavendish and Watt. On the other hand, everything favours the belief that having once entered upon the subject, he would claim for one or other, or both of the English chemists, all that he could claim for them.

It is quite certain, however, that before June, 1783, Cavendish had not only ascertained that when inflammable air and oxygen are burned together in proper proportions, they yield their own weight of water, but that Priestley had also publicly declared that he had confirmed this result.

It is also certain that both Cavendish and Watt, whether independently or not, had inferred from the experiments of the former, and their repetition by Priestley, that water was composed of inflammable air and oxygen; and that Blagden knew that they had drawn these conclusions. He is fully entitled to credit therefore, when he declares that he announced all this to Lavoisier and his colleagues. But if he did, Lavoisier cannot be acquitted of disingenuousness and plagiarism. Again; the only party, except Blagden and Lavoisier, who has given any account of the conference between them is La Place. And his account does not tally with Lavoisier's, for he tells De Luc that he and Lavoisier had been occupied in repeating before Blagden and several other persons, "Mr. Cavendish's experiment" on the conversion of inflammable and dephlogisticated air into water by their combustion.* Such an experiment was necessarily quantitative, and La Place, accordingly, refers, as if it were a matter of course, to the question of equality of weights between the burned gases and the produced water; and although he writes three days after Lavoisier had announced to the Academy a conclusion which was not justifiable unless the weights were equal, he declares that neither he nor Lavoisier yet knew whether they were. His statement, accordingly, so far as it goes, is in accordance with Blagden's, and at variance with Lavoisier's account of matters. Further; Lavoisier acknowledges that his original expectation was, that inflammable air in burning would yield sulphuric or sulphurous acid;† and he tried, on two occasions, to demonstrate the production of an acid, whilst he does not profess to have had the slightest expectation that water would be generated. Blagden's declaration, therefore, that he and his colleagues were incredulous as to the production of water, is highly credible, and quite in keeping with Lavoisier's own statements. It seems probable also, although it is impossible to be certain on the point, that before Blagden communicated with him, he had arranged an apparatus resembling that for the oxyhydrogen blowpipe, with a view to try on a large scale what was the product of the oxidation of hydrogen; and although Blagden had never visited Paris, Lavoisier would certainly have sooner or later employed this apparatus, and might have made the discovery which he missed. As it was, however, before he had put it in action, he was

* *Watt Corr.* p. 41.

† *Mémoire par* Lavoisier, *Watt Corr.* p. 173; or *Mém. de l'Acad. pour* 1781, p. 473.

informed that the problem which interested him was already solved. He seems to have thought that his pre-arrangements justified him in proceeding as if he had not been anticipated, and entitled him to publish his conclusion as if it were new to the world. A conscious rediscovery, however, is one of the strangest contradictions in terms. Finally; Blagden was on very friendly terms with the scientific men in Paris, which he often visited. When he appealed, therefore, to Lavoisier's fellow-academicians, he appealed to those with whom he would soon be confronted, and by whom he would be called to account for any misrepresentation he should give of their views. He was, moreover, a cautious and somewhat formal person, who had no interest in offending the French philosophers : yet he risked their good-will by the version which he gave of his conference with Lavoisier. Nor was it at all necessary for Cavendish's vindication, that he should have entered into so minute an account of his interviews with the French philosophers as he did. It may be added, as pointed out already in the personal narrative, that Blagden did not forfeit the good-will of the French philosophers by his letter to Crell. The terms of his will sufficiently demonstrate this. When all these things are considered, the credibility of Blagden's uncontradicted accusation of Lavoisier must be considered as greatly enhanced. It seems impossible, therefore, to acquit Lavoisier of the charge preferred against him alike by Cavendish, Blagden, and Watt, which, in the cutting words of the last, amounted to this, that after Lavoisier had had the theory of the composition of water explained to him, "he invented it himself."*

GENERAL SUMMARY.

18. *Relative Merits of Cavendish, Watt, and Lavoisier.*

I have stated frequently throughout the long discussion which is now brought to a close, that to each of the three great claimants of the discovery of the composition of water, a large though unequal share of merit must be assigned. I proceed now, accordingly, to state in what proportion each appears to me entitled to honour. Merit, however, is not a thing which in any case admits of precise definition or accurate estimation, as the question of the reality, the nature, or the date of a discovery does. Those who dissent from the conclusions advocated in the preceding sections of this discussion, will of course apportion the merit due to Cavendish, Watt, and Lavoisier, otherwise than I do; and against this I have nothing to say. Some of those, also, who entirely or substantially accept as valid the conclusions already urged, may nevertheless differ widely from me, as to the way in which the honours of the disputed discovery should be divided amongst the three great claimants. I do not regret the probability of this, but rather welcome it, as more consistent with an intelligent acquaintance with the facts of the case, than a uniform estimate of the relative merits of the rivals would be. The questions already considered are the only ones on which it is worth while to take a side strongly. It would be a mere waste of time to discuss the question

* *Watt Corr.* p. 61.

of merit with those who rank themselves on the opposite side from me, for there can be no agreement between us. It would be as unprofitable to seek to compel all who are on the same side as myself, to use the same balance and weights as I do, in apportioning the merit due to the discoverer of the composition of water, between Cavendish, Watt, and Lavoisier. It will be enough, therefore, if I briefly state the grounds on which I urge the following conclusions. First, then, of Cavendish. He only of the three is entitled to be called THE DISCOVERER OF THE COMPOSITION OF WATER, if that title is to be given undivided to any one of them. He first consciously converted hydrogen and oxygen into water, and first announced the possibility and reality of such a conversion. Whatever else is doubtful in the Water Controversy, this at least is certain, that Cavendish was the first who formed water out of its elements. He alone is entitled to the undivided merit of having first observed that (nearly) two measures of hydrogen and one of oxygen may be burned into their own weight of water; and he supplied the data to which, whether conscious of the fact or not, Watt and Lavoisier were indebted for the foundations of their conclusions. Even, therefore, if we should concede to the supporters of Watt all that they can claim for him, viz. that he first drew the just inference from experiments on the synthesis of hydrogen and oxygen, we should still be entitled to claim for Cavendish one-half of the merit of the discovery; inasmuch as he supplied the data, whilst Watt supplied the conclusion. Such a liberal division of merit, however, is by no means satisfactory to some of the advocates of Watt, who, in their extreme and confident partisanship, would exclude Cavendish from any share of merit whatever. They have sought, accordingly, as Watt can only claim the conclusion, to impute to certain of Cavendish's predecessors the observation of the *production* of water from its elements. The claimants against Cavendish are Macquer, Warltire, and Priestley. Their pretensions, which are urged by others, not by themselves, may be dismissed in a word. All that Macquer did, was to observe that inflammable air, when burning, deposited a liquid, to appearance water. On the source of this fluid he offered no opinion, and he only assumed, he did not ascertain, that the liquid was water. Arago indeed states, that Macquer analysed it; "qui après verification se trouva être de l'eau pure."* No authority, however, for this statement is given; and to the best of my knowledge, Macquer's results were published only in his "Dictionnaire de Chymie." From this work, Mr. Muirhead has translated Macquer's account of what and he and M. Sigaud de Lafond saw, when they held a saucer over burning inflammable gas, "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."† Nicholson, in his dictionary, comments thus on the statement of Macquer:—"He does not say whether any test was applied to ascertain this purity."‡ The French chemists, in short, only guessed that the liquid was pure water, and possibly guessed wrongly; for it may have contained a trace of sulphuric acid transferred from the materials employed to yield the gas, which appears to have been hydrogen. It is strange that Mr. Muirhead, after quoting from Macquer the passage which I have borrowed from him, should thrice in

* *Annuaire du Bureau des Longitudes*, for 1839, p. 349. In Mr. Muirhead's Translation of the Eloge of Watt, the quotation is rendered, "a liquid like water, and which *on analysis* proved to be pure water." (*Watt Corr.* p. 224.)

† *Dictionnaire de Chymie*, tome ii. p. 314. Quoted from *Watt Corr.* p. xxviii.

‡ Article, Water, vol. ii. p. 1018.

effect state "that the French chemists *ascertained*" the liquid "to be pure water."*

Warltire's observations on the appearance of water are thus reported by Priestley:—"I must add, that the moment he [Warltire] 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 [Priestley] it was a mere random experiment, made to entertain a few philosophical friends."†

Priestley and Warltire, it thus appears, assumed without examination that the liquid was water. The former drew no conclusion as to its origin; and the latter did not regard it as produced, but as simply deposited from the common air in which it had pre-existed as ready formed vapour.

From this account it will be seen, that although Warltire and Priestley's experiments confessedly suggested Cavendish's researches, it was left to him to demonstrate that the liquid was pure water, and that it was equal in weight to the gases burned. Till both those facts were ascertained, no one was entitled to infer, or did infer, that water was *produced* during the combustion of hydrogen, and Cavendish was the first to ascertain them. This cannot, in truth, be denied; and the impugners of his claims have thought it well to lessen if possible his merit, by declaring that the water he procured in his experiments was not quite free from foreign matter, and was not sufficiently analysed. Sir David Brewster affirms that in one of the experiments the water "was not *absolutely* pure;" and dwells upon the fact, that in another the liquid yielded on evaporation a trace of sediment.‡ Mr. Muirhead states that Cavendish applied "*some* tests to the liquor condensed;"§ and Lord Jeffrey declares that Cavendish's processes were conducted with far less care and accuracy than those afterwards instituted by Lavoisier, Monge, Berzelius, or Dumas.‖ These objections are easily disposed of. We may concede to Sir David Brewster that the water was not *absolutely* pure, but in this respect his rivals had no advantage over him. Absolutely pure water, indeed, is a thing which very few persons, even chemists, have seen. We may also concede to Lord Jeffrey, that Berzelius and Dumas might have improved on Cavendish's process; but not that Lavoisier and Monge did, for their processes were still more improvable than Cavendish's; and Berzelius and Dumas would have been the first to protest against experiments of 1781 and 1782 being tried by the more exacting rules, which the united labours of a host of chemists, spread over more than half a century, have rendered imperative, by the improvements which they have introduced. As for Mr. Muirhead, I do not know on what principle of justice, he three times over attributes to Macquer the observation that hydrogen yields, when burned, pure water, although he only made a guess, and did not try a single test; and yet denies that Cavendish's careful analysis entitled him to call the liquid he analysed pure water. It must be a substance much more difficult to recognise than people generally imagine, if the "some tests" which Cavendish applied, did not entitle him to affirm what it was.

* *Watt Corr.* pp. xxviii, lxxxix, and cxxiii.
† *Expts. & Obs. on Air*, 1781, p. 398.
‡ *North Brit. Review*, Feb. 1847, p. 494.
§ *Watt Corr.* p. xxxv.
‖ *Edinr. Rev.* Jan. 1848, p. 134.

The friends of Watt are using a very dangerous argument in reasoning thus; for if Cavendish is to be condemned because he only tried "*some* tests," what is to be said of Priestley who tried *none*, and has left on record no proof that either he or Watt was entitled to infer that pure water had been produced? In reality, as I have shown already, Cavendish's analysis was more complete than Lavoisier's, and more accurate than Monge's or Priestley's; and the fact that they overlooked the occasional production of nitric acid is the best proof that Cavendish more rigidly investigated the product of the combustion of hydrogen and oxygen, than any or all of them did. It is mere hypercriticism, then, to cavil at Cavendish's experiments; for if they were not such as to entitle him to his conclusion, still less were those of Priestley, Lavoisier, and Monge; and the result we must arrive at is, that not one of the rivals discovered the composition of water, and that it has not yet been discovered. I shall assume it, therefore, as a settled point, that Cavendish was the first who converted a given weight of hydrogen and oxygen into the same weight of pure water.

Monge's original and independent observations were misinterpreted, so that they do not demand consideration here; but the other conclusions, viz. those of Priestley, Watt, and Lavoisier, go back to Cavendish's researches, on which at first or second hand they were based. His observations thus stand first in *chronological* order; and first also in what I may call *organic* sequence. They were the root out of which his own conclusions immediately sprang, and those of his rivals mediately branched.* He is thus *doubly* entitled to at least half the merit attaching to the discovery of the composition of water, inasmuch as he furnished the data on which it was based by all its claimants.

I claim, however, for Cavendish the remaining half of the merit due also. And this on two distinct grounds already discussed at length. The first, that though direct proof is wanting, indirect evidence of the most cogent kind can be adduced to show that he had formed a just theory of the composition of water, before Priestley had repeated his experiments, or supplied Watt with the data from which he drew his conclusions. The proofs of this are, as we have seen,

1st. That Cavendish, a highly honourable, upright, and modest man, claimed the conclusion as his own; acknowledged no obligation to Watt; and accused Lavoisier of plagiarism.

2nd. That the dates of his journal strongly favour the belief, that he had formed a just theory of the composition of water as early as January, 1783.

3rd. And, most important of all, that he announced his experiments and their results to Priestley, in a statement to the effect, that hydrogen and oxygen can be converted into water, which has its origin in these gases.

This announcement contains a just theory of the composition of water, although it does not make use of the word *phlogiston*, which Cavendish employed in the full exposition of his theory. The substitution, however, of that word for hydrogen (inflammable air from the metals) was not occasioned by any alteration in opinion as to the convertibility of hydrogen and oxygen into water, but arose solely out of speculations on

* Lord Jeffrey concedes this so far as Watt is concerned, and it is still more evident in reference to Priestley and Lavoisier.

the nature of hydrogen. Cavendish's theory, indeed, was altered for the worse by the change in terms, and rendered needlessly hypothetical. So soon, however, as he affirmed that water could be generated out of two gases, in which it had its origin, he had demonstrated his discovery of the nature of that liquid, and announced the true, though not the complete theory of its composition. Cavendish, therefore, in so far as he told Priestley that hydrogen and oxygen can be entirely converted into water, of which they are the factors or constituents, was prior to him, to Watt, to Lavoisier, and Monge, and was the first to interpret his own experiments.

Cavendish has a second claim of a lower, but, nevertheless, important kind, to be called the discoverer of the composition of water; for, even if it were certain that Watt drew a conclusion from Priestley's repetition of Cavendish's experiments, before Cavendish drew one from the original trials, we have seen that Priestley did not employ hydrogen, and that Watt did not affirm that water consisted of hydrogen and oxygen, but spoke of the combustible element of water as inflammable air, without special reference to hydrogen, and apparently with special reference to the inflammable gas from charcoal; so that whatever Cavendish borrowed from Watt (if he borrowed anything), he did not, and could not, borrow from him the doctrine that water consists of hydrogen and oxygen; and even if it were conceded that Cavendish's conclusion was not arrived at till after he had read Watt's letter, it would still be true that his theory, though later in date than Watt's, was the first true theory of the composition of water. It has been urged against this view, that Cavendish accepted Watt's theory as identical with his own, and that it is, therefore, idle for any one else to attach importance to differences he may detect between the two theories. The argument is specious, but not the less hollow. Cavendish did not accept Watt's conclusion as of equal value with his own, without a protest in reference to an apparent difference between them. The wording of their theories closely corresponded, and so far their conclusions appeared identical, but the term phlogiston, or inflammable air, stood *specially* for hydrogen in the one theory, and for charcoal gas (if for any single elastic fluid) in the other, and to this fact Cavendish drew attention. He did not affirm that charcoal gas would *not* serve as well as hydrogen for the production of water, but he would not make himself responsible for the declaration that it would; he only professed to be certain that the inflammable air from metals (hydrogen) was the combustible constituent of water. Watt and Priestley together were sponsors for the inflammable air from charcoal being a true *hydrogen,* or water producer, and on their authority he accepted Watt's conclusion as equivalent to his own. He would have acted unjustly if, without repeating Priestley's experiments, he had done otherwise. Nevertheless, his approval was only conditional; for he speaks of Priestley's experiments as "of the same kind," not as absolutely identical, and he draws attention to three points of difference, viz. that Priestley obtained no nitric acid; that he "used a different kind of inflammable air, namely, that from charcoal," and that he perhaps used a greater proportion of it.* And, at a later period, we know from his MS. Journal, that he justified his caution by discovering that the charcoal-gas and hydrogen are not identical. Cavendish's conditional

* *Phil. Trans.* 1784, p. 135.

approval, then, of Watt's conclusion, does not make him responsible for the errors it contained; still less does it affect the fact, that Cavendish's own theory was the earliest true theory of the composition of water.

As for Watt, the merit which can be assigned to him cannot, according to the view taken in the preceding pages, be great, at least in one respect. If his conclusions, as I have contended, were drawn from experiments in which hydrogen was not used, and if his theory did not affirm that hydrogen was one of the elements of water, then he was at no time *a* discoverer, much less *the* discoverer, of the composition of water. His theory, however, at the period of its announcement, passed for as good as Cavendish's and Lavoisier's, and had as much effect for a time as their juster conclusions, in inducing belief in the compound nature of water. On this account, even the extreme advocates of Cavendish's merits should, if impartial, assign Watt a place of honour in connexion with the discovery of the true nature of water; and, if they do not, the historian of the progress of science must be his eulogist. His theory, though inaccurate, unquestionably made a close approximation to the true theory, and was very clearly and perspicuously stated. In particular, he urged with great fulness and emphasis, in his private letters, that water is not an element or simple substance, that it is composed of ingredients, and that he knew what they were. His quaint recipe "to make water" shows how fully he realised, that it might be compounded or manufactured out of substances which he specified. There can be no doubt that Watt's theory increased the faith of Cavendish and Lavoisier in their own views, and won the approval of the great majority of their scientific contemporaries.

It did service thus, at a certain epoch in the progress of discovery, and has a place in the history of science, whether it pleases us that it should have such a place or not. When we consider, indeed, that Watt expounded very fully that water is a compound, and came so near its true composition as to assert that one ingredient of it is oxygen, and the other combustible gas, or a combustible gas, we must acknowledge that, had his been the earliest interpretation of the earliest experiments on the subject, his merit would be very great. But when we call to mind that the experiments which he interpreted were shaped to his hand by Cavendish, and presented to his consideration in their simplest and most demonstrative form, with their validity as establishing the convertibility of a combustible gas and oxygen into water, already announced by Cavendish and confirmed by Priestley, we can assign him only a secondary and subordinate place as a discoverer of the composition of water. He was but a follower in the path of Cavendish. Had the latter never experimented, or had he never reported his results to Priestley, there is no reason to suppose that Watt would have conjectured, even remotely, that water is a compound of oxygen and inflammable air. He was not on the track of such a discovery. His speculations on the convertibility of steam into a permanent gas by the change of all its latent into sensible heat, did not point in that, but in exactly the opposite direction. He was following Priestley in all his devious wanderings, and going astray along with him, into the belief that water was transmutable into atmospheric air, when Priestley's repetition of Cavendish's experiments (in which all that was true and significant was Cavendish's) arrested him in his mistaken course, and enabled him to approximate to the true theory of the composition of water. He taught nothing, however, to Cavendish, who, in due time, when his inquiry was completed, made it public to all.

Watt must rank below Cavendish and Lavoisier, though how far below them I will not attempt to decide. To the consideration of the French philosopher's merits I now turn.

Lavoisier has not the slightest claim to be considered the discoverer of the true nature of water; nor did he demonstrate its synthetical production more fully than Cavendish did. He added absolutely nothing to the results which the latter had obtained, and his method of procedure was less accurate and ingenious, and differed for the worse, in so far as the experiments were tried on too large a scale.

Berzelius, in apportioning the merit of the discovery under discussion between its three great claimants, assigns to Lavoisier the first announcement that water is a compound, and that when produced by the combustion of hydrogen and oxygen, it "weighs exactly as much as the combined gases weigh together."* In a previous section, however, it has been shown so fully that he was anticipated in both those announcements by Cavendish, and in the first by Watt, and that he was informed of their conclusions by Blagden before he formed his own theory, that I need not recur to the subject. What was especially his, was the precise definition of the combustible element of water, as a peculiar gas having the physical properties of the other gases, and the ordinary attributes of matter, so that the halves of water were physically similar, though chemically unlike. This seemed to Cavendish, who commented on it, in the close of his own paper, a slight alteration and a very doubtful improvement on his own view. To us it appears in a very different light. Lavoisier's merit was assuredly very great. Cavendish encumbered his theory by needless and unwarranted speculations on the nature of phlogiston. His declaration that it consisted of water and hydrogen involved the awkward and embarrassing doctrine that water was an element of water. He further contended, that every substance when oxidized yielded water, and that the addition of oxygen to any body was equivalent to the removal of water from it. His view was thus, to a great extent, only a modification and extension of the doctrines of Stahl and the other theorists of the Phlogiston School. Cavendish, like them, believed that every combustible and oxidable body contained phlogiston, but, unlike them, he identified it with hydrogen. The main point, however, of difference between their views was, that the older chemists were content to affirm, as their most general proposition, that burning bodies evolved or emitted phlogiston, which, by its addition to atmospheric air, converted it into phlogisticated air;† whereas Cavendish contended that the phlogiston or hydrogen always united with the oxygen of the air, and produced water. Such a view, however unfounded, implied no confusion or hesitation as to the nature of water. An erroneous opinion was entertained as to the nature of hydrogen, and likewise as to the extent of its distribution among combustible bodies, but no error was thereby introduced into the definition of water, as a compound of hydrogen and oxygen. The clearness, indeed, of Cavendish's conception of this, is shown by the very excess of his generalisation, for he showed the importance which he attached to his discovery, by representing it as fitted to explain every combustion and every oxidation; and with con-

* *Lehrbuch*, 1843, p. 371.

† This term was restricted by Cavendish and his contemporaries to nitrogen, but by the older chemists it was applied to atmospheric air so altered as to extinguish flame, whether this resulted from the loss of oxygen or the addition of carbonic acid (or other irrespirable gas); or the simultaneous loss of the one and gain of the other.

sistent and rigorous fidelity to his theory, he contended that wherever hydrogen and oxygen met and combined, water must be produced. It is quite clear, however, that although Cavendish announced the true nature of water with so great explicitness that it was not left for any later expositor to deprive him of the honour of being the discoverer that it is a compound, nevertheless, the erroneous views he connected with it could not but have led both himself and his contemporaries far astray; and, had they not been immediately neutralised by the expositions of Lavoisier, they would have delayed indefinitely the overthrow of the mystical and delusive doctrine of phlogiston.

Similar remarks apply to Watt's theory apart from its errors. He, like Cavendish, believed that phlogiston contained water: and he further held, which Cavendish did not, that nitrogen and carbonic acid, as well as water, were, what we should now call oxides of phlogiston, which entity he identified with inflammable air as procured from various sources, not with hydrogen. Had Watt's theory, accordingly, been published first and alone, it would have diverted chemistry out of its legitimate course, and delayed its rational progress still more than Cavendish's erroneous doctrines would have done. Phlogiston and oxygen, according to Watt's view, might be converted into carbonic acid or nitrogen, or atmospheric air, as well as into water, so that substances consisting of very different ingredients were held to consist of but slightly varying proportions of the same elements. This view, apart from its not defining phlogiston as hydrogen, could not but have set the chemistry of the gases on the wrong track for at least many years. It was the publication of Lavoisier's memoirs, contemporaneously, or nearly so, with Cavendish's and Watt's papers, which prevented their erroneous speculations from doing any mischief. And however strong may be our feeling and expression of indignation against Lavoisier's plagiarisms, we must guard against allowing our disapprobation of his concealment of obligation to his English rivals, to make us unjust towards him, in reference to what was all his own. He has, in truth, paid a heavy penalty for his disingenuousness; and for some seventy years has been held up to the scorn of the world as a detected plagiarist. Yet never, perhaps, was the policy of honesty more apparent than in the case of Lavoisier, for he might safely have conceded to Cavendish and Watt all that was theirs, and yet have left to himself what would have secured, and does secure to him, the highest honour. Had he done so, the English chemists would have been foremost in praising him, and his name would now be revered by us in a way it is not likely soon to be. The wrongs, however, which he did to others cannot justify us in wronging him; and I feel peculiar pleasure in insisting upon his merits in connexion with the discovery of the true nature of water. He assuredly did chemistry an unspeakable service by abolishing the vague and mischievous phlogiston, which proved an *ignis fatuus* even to men like Cavendish and Watt, and substituting for it, as a name for the combustible element of water, the simple term, aqueous inflammable air, and afterwards hydrogen. He thus made the one element of water as manifest and tangible a reality as the other, and deprived it of all those mysterious and imaginary attributes which had so long invested phlogiston with a phantom existence. The believers in that entity, unwisely for themselves, had identified it with inflammable air, or located it in that body. From that moment it had to forswear any physical difference from other forms of gaseous matter. It needed Lavoisier, however, to perceive and to demonstrate this, and to urge it by multiplied experiments and arguments. And it was most fortunate for us that his views were published before those of the English chemists had time to spread, so that the discovery of the nature of water, which seemed in their hands destined to prolong the existence of phlogiston, was converted by Lavoisier into an instrument by which he for ever effected its overthrow. It would be difficult, accordingly, to over-estimate Lavoisier's merits in this respect. At all events, for my own part, looking back, almost with regret, on the many hours I have spent in wading through the wearisome and often unprofitable writings of the Phlogiston School, I will be foremost in acknowledging obligation to Lavoisier for having put so decisive and final a stop to the speculations of its disciples. Modern chemists cannot be too thankful that they have not grown up under the false system which led to so much waste of time and capacity by their predecessors, and all their gratitude is due to Lavoisier.

Further, he was the first who consciously analysed water into its elements. He appears to have been indebted—as he tells us himself—to La Place* for the suggestion that the hydrogen which is obtained by the solution of metals in diluted acids results from the decomposition of water,† so that, to some extent, he must divide the honour with Lavoisier. Into this question, however, I do not enter further than to remark, that it adds another honour to the many which render La Place illustrious. The processes by which water was first decomposed, appear all to have been of Lavoisier's own devising, although he had the assistance of Meusnier in performing the necessary experiments; so that he is justly entitled to be called the first analyst of water. And we may affirm with great confidence, that the faith of mankind in the modern theory of the nature of water, was increased twofold by the discovery of the French chemist that water can not only be compounded out of hydrogen and oxygen, but can also be decomposed into these gases. Analysis—*i.e.* the resolution of any composite or compound whole into its constituent parts—is a method of investigation more familiar to us as a means of ascertaining truth, and more easily and more frequently employed by the great majority of thinkers in all their investigations, than synthesis,—*i.e.* the production or construction of a compound whole out of its components. Inductive reasoning, accordingly, is much more practised by all but the highest intellects than the deductive method of research. I will venture to affirm, indeed, that no teacher of chemistry at the present day who wished to demonstrate to his pupils the compound nature of water, would select Cavendish's synthetical process as the means of proving it, but would exhibit in preference the methods by which it may be analysed. The universal practice of our lecturers, and of the authors of our text-books, sufficiently proves this. Lavoisier's method is not the best analytical one, for it exhibits only one of the elements of water in a state of isolation; so that we should now prefer Mr. Grove's application of a high temperature, which sets both elements of water free; or Nicholson and Carlisle's application of the voltaic current, the action of which is very rapid, and may be exhibited on a scale of great magnitude, which Cavendish's experiment, even if equally demonstrative, cannot be. Lavoisier's process, however, was the only, and therefore the best analytical method of the time, and was as new to mankind, and as unexpected as Cavendish's. It could not but increase the belief of those who had already accepted as

* *Mém. par* Lavoisier. *Watt Corr.* p. 181; or *Mém. de l'Acad. pour* 1781, p. 476.

† *Leçons sur la Philosophie Chimique,* par M. Dumas, p. 158.

true the latter's conclusions; and it probably carried conviction to the minds of many who, like Monge, hesitated as to the significance of the synthetical experiments. No solicitude, accordingly, for Cavendish's reputation can justify us in refusing Lavoisier a very high place in the history of the discovery we are considering. When we remember that he gave the simplest and most accurate exposition of the phenomena attending the synthesis of hydrogen and oxygen; that he first effected and most lucidly expounded the analysis of water; and that, by the use which he made of the fact that water is a specific compound in his interpretation of other phenomena, he did more than any of his contemporaries to impress mankind with a sense of the importance of the discovery which had been made, we cannot hesitate to place him side by side with Cavendish, with whom he would probably, from the first, have been ranked in honourable brotherhood, but for his own lack of generosity to his English rival.

Some other points, however, require to be noticed, before we can settle completely the relative merits of Cavendish, Watt, and Lavoisier. The friends of Watt are fond of affirming that Cavendish did not appreciate the importance of his discovery, as the very title of his paper, "Experiments on Air," shows. The very opposite, however, is the truth; for it was Cavendish's fault, as we have seen, to over-estimate the value of his discovery, which he affirmed supplied the rationale of every combustion and oxidation, besides explaining the most important phenomenon of vegetation and the action of sunlight in effecting chemical changes. The title, too, was quite appropriate, for the main object of the paper, from first to last, was to record an experimental inquiry into all the phenomena which attended the phlogistication, or deoxidation of atmospheric air by various phlogisticating or oxidable bodies besides hydrogen. His paper entered largely, moreover, into the consideration of the nature of nitrogen and oxygen as constituents of the atmosphere, and furnished a new process by which their relative amount, or its quantitative composition, could be determined. Its title, accordingly, was most befitting, and we might as well contend that its employment a second time, as the heading of the next paper Cavendish published,* implied an indifference to the greatness of the discovery of the composition of nitric acid, which it recorded, as affirm that its earlier adoption as the title for the paper of 1784, is at variance with the idea that Cavendish appreciated his discovery of the composition of water. Such reasoning, if valid, would tell still more against Watt than against Cavendish, for the former put no title originally to his paper, so that its readers were left to discover for themselves what the importance of its contents was, and Cavendish and Lavoisier had drawn the attention of all to the importance of the new views concerning water, before Watt did the same by his title.

Much reference has also been made in depreciation of Cavendish's merit, to the delay which attended the publication in full of his views. In reality, however, it is much more easy to account for Cavendish's delay than for Watt's. The latter withheld from publication a declaration that water consisted of inflammable air and oxygen, because a totally different proposition, viz. that water can be converted into atmospheric air, proved untenable. His friends refer triumphantly to the Watt Correspondence as demonstrating that he uniformly maintained in private, the theory which nevertheless he would not publish. This fact, however, only ren-

* *Phil. Trans.* 1785, p. 372.

ders his public silence the more inexplicable; and in spite of his alleged unhesitating confidence in the truth of his theory, he told Sir Joseph Banks that one of his reasons for withdrawing his letter was, his "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.*" This passage proves how deliberate Watt's delay was, and what amount of confidence he had in his theory when he withdrew it. He was convinced, I believe, all throughout, that his conclusion was just, *provided the data on which it was founded were correct*, but after Priestley had so grievously misled him, as to the conversion of water into air, he very naturally began to doubt whether his ingenious, but inaccurate friend might not have equally misled him as to the conversion of air or gases into water, and he hesitated to publish his theory till he should be certain that Priestley's experiments were accurate. How well founded this suspicion of Priestley's accuracy was, I need not say, and how much it weighed with Watt in causing his delay, is shown by the fact that when he drew up the new version of his views, he was careful to fortify Priestley's observations by a minute reference to those of Lavoisier, which De Luc had reported to him.† We may confidently affirm, accordingly, that Watt's faith in the convertibility of inflammable air and oxygen into water, was shaken by Priestley's mistake so much that he would not assert its certainty, but waited for many months, seeking for the means of establishing its truth. It is idle, therefore, for the advocates of Watt to reproach Cavendish with his delay.

From this needful digression, I return to Cavendish. His merit is not lessened in any respect; on the other hand, it is positively increased by the delay which attended the publication of his first series of "Experiments on Air." Let it be noticed in the first place, that it is not the fact that Cavendish kept back a completed inquiry, and thereby gave grounds for the suspicion that he distrusted his researches, or did not appreciate them. I have shown that his MS. journal records experiments referred to in the paper of 1784, which were made so late as Christmas, 1783, whilst the paper was laid before the Royal Society on the 15th of the succeeding January, so that no delay attended the publication of the inquiry *after it was completed*. It is quite true that the experiments on the production of pure water from the combustion of hydrogen and atmospheric air, were made as far back as 1781, and might have been reported earlier than 1784. A very sufficient reason, however, can be given why Cavendish did not immediately publish these researches, viz. the occasional appearance of nitric acid, when oxygen was substituted for atmospheric air. Whilst this unexpected and perplexing phenomenon remained unexplained, it would have been premature to have published a declaration, that hydrogen and oxygen yield by their combustion pure water. Had Lavoisier, who expected the oxide of hydrogen to be an acid, encountered nitric acid (in his earlier researches),‡ in the circumstances in which Cavendish found it, (viz. where the oxygen was slightly in excess of the hydrogen, whilst a little nitrogen was present as an im-

* *Watt Corr.* p. 52. † *Phil. Trans.* 1784, p. 332.

‡ The letter from Berthollet to Blagden, quoted in another place, shows that Lavoisier, in his later trials, confirmed Cavendish's observation of the production of nitric acid, and that La Place was led completely astray as to the nature of the acid, by the circumstances of its origin.

purity in the gases) we may be certain that this phenomenon would have appeared to him more important than even the production of pure water, and that he would have thoroughly investigated the source of this acid, before he made public his interpretation of the phenomena which he had witnessed. Watt also wrote to Priestley, "I maintain my hypothesis until it shall be shown 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.*"* He tells De Luc also that "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."† From these references, it is manifest that Watt would have attached as much importance as Cavendish did, to the appearance of nitric acid, and would have altered, or perhaps abandoned, his hypothesis, had he been aware of the production of acid, during the combustion of apparently pure oxygen and hydrogen. Cavendish's rivals, therefore, may be regarded as both testifying to the wisdom of his delay, till he should have discovered the origin of the nitric acid. He did not, however, complete his investigation into this subject, till January, 1783, and he announced his results to Priestley, not later than the succeeding March, and to the French chemists in the succeeding June. He stands, therefore, altogether acquitted of suspicious or blameable delay. His whole paper he did not publish till a year later, but the records of his journal completely demonstrate that this apparent procrastination did not result from any loss of confidence in the accuracy of his conclusions concerning the composition of water, or any change of opinion on this subject, but was occasioned solely by the prosecution of collateral inquiries, which he deemed essential to the completeness of his memoir. One chief cause of delay was the publication of his account of the new eudiometer, which formed an essential part of his elaborate inquiry into the phenomena which attend the phlogistication of air. It was essential that this paper should be published before the record of his views concerning water, for the interpretation which he gave of the production of this liquid when hydrogen and atmospheric air were burned together, would have been unintelligible to his readers, had he not previously announced his discovery of the constancy in composition of air, and stated what was the relative amount of oxygen and nitrogen contained in it.

Another cause of delay was the publication, in 1783, of his commentary on Mr. Hutchins's experiments on the freezing point of mercury. These experiments were made in 1781 and 1782, at Hudson's Bay, but the record of them did not reach this country till late in the latter year, and it was not read to the Royal Society till 1783. As Cavendish, however, had furnished Mr. Hutchins with the apparatus he employed, and with directions how to use it, it devolved upon the former to furnish a commentary which should accompany Mr. Hutchins's paper. An abstract of this commentary is given in the analysis of the papers on heat, from which it will appear that the experiments which it called for, and the study of Mr. Hutchins's numerous results, must have occupied a considerable period.

Again: it was impossible to adopt the conclusion that water is a compound of hydrogen and oxygen, and not be led to speculate in a way which had not been done before, concerning the nature of these gases. Cavendish, accordingly, and Watt also, made many experiments to

* *Watt Corr.* p. 27.

† *Ibid. ibid.* p. 30.

determine the constitution of the elements of water, and discussed at great length the theory of the different processes by which oxygen can be prepared. Cavendish, further, was directly led by his discovery that nitrogen and oxygen can be burned together into nitric acid, to investigate minutely the nature of the former gas as well as of this acid. In addition: Cavendish saw at once that the doctrine of the compound nature of water, could not but throw much light on the phenomena of vegetation, and he explains at length his view of the function of water in maintaining the growth of plants, and discusses the deoxidising power of sunlight, which he refers to its power of causing the separation of hydrogen from the chemical substances it decomposes. In this way he accounts for the evolution of oxygen by the green parts of living plants, when exposed to the sun.

It was natural—it was, I may almost say, inevitable—that such collateral speculations should have been entertained by a believer in the compound nature of water. Similar views, more or less fully expounded, occur in the papers of Watt, Lavoisier, and Monge, and to have omitted them would both have lessened the value of their separate papers, and have left other writers to claim the credit of making these applications of the new doctrine. Add to all this, that Cavendish was exceedingly cautious, never published in haste, but on the other hand, left behind him many finished inquiries which he had not given to the world, and nothing remarkable, or blameable, or fitted in any way to lessen his merit, will be found in his so-called procrastination.*

If ever, indeed, delay was justified by its fruits, it was in Cavendish's case. Had he hastened to publish, he might have won a single laurel wreath, but as it was he gained a triple crown. He claimed in the end, not only the discovery of the composition of water, but that also of the constant quantitative composition of the atmosphere, and he had in his hands the rapidly expanding germ of the discovery of the nature of nitric acid.

Once more. Cavendish's merit as discoverer of the composition of water, is much greater than that of Watt and Lavoisier, even if it were conceded that they also had made the disputed discovery. The French chemist, before he made an experiment on the subject, had been informed that at least three persons, viz. Cavendish, Watt, and Blagden, were satisfied that water was composed of inflammable air and oxygen. The path, accordingly, along which he should travel, and the objects which he should encounter, were pointed out to him, so that he could not fail to observe all the essential phenomena, and could foresee the inference which he should draw, in the light of the foregone triple conclusion which had been made known to him. It was easy, therefore, for Lavoisier to appear before the Academy, with a single insufficient experiment, and profess to found his theory of the composition of water on it alone.

Watt, likewise, had much simpler data supplied to him by Priestley,

* It was in truth with Cavendish the exception, not the rule, to publish an inquiry. The large number of papers, several in a state fit for publication exactly as they stand, which remain among the Cavendish MSS., show both that he was in the practice of delaying indefinitely the publication of finished researches, and that he has left us in entire ignorance of his reasons for so doing. Any endeavour, therefore, like that of Lord Brougham, to say when Cavendish *should* have published a paper, seems to me a hopeless attempt; still more, any endeavour to make delay in publishing an argument in favour of want of belief in what was delayed.

than Cavendish's experiments furnished to himself. The problem the first had to consider was: "given inflammable air and oxygen, changing into their combined weight of pure water, what conclusion is warranted?" Cavendish had the much more difficult enigma to solve: "given those gases changing sometimes into water, sometimes into water and nitric acid, what is the just inference?" Again: Priestley and Watt considered only what happened when inflammable air and oxygen were burned together, but Cavendish's inquiry embraced also the action of burning hydrogen and air on each other; and as a necessary step towards this, he spent months of labour on the analysis of air, before he could proceed to deduce from his researches into its composition the proportion in which hydrogen and oxygen should be burned together, so as to ensure the combination of the entire volume of each gas taken.

Further: Cavendish had no predecessor in his researches, and was embarrassed by the assertion that heat was ponderable. Priestley, on the other hand, had Cavendish's experiments before him as an example and guide; and Watt was assisted towards his conclusion by being familiarised with the doctrine of the convertibility of gases into water, which Priestley undertook to test, and professed to have verified. Priestley thus comes between Cavendish and Watt, and transferred from the former to the latter, not only his experiments, but at least an outline also of his conclusions.

Finally: I know of no explanation of Priestley's extraordinary charcoal-gas experiments so plausible as this, viz. that aware that Cavendish had obtained pure water equal in weight to the gases burned, he took for granted that the water he himself procured, was also pure, and set down as due to his imperfect balance the deficiency in weight which arose from the charcoal-gas being employed instead of hydrogen; so that Priestley's experiments appeared valid, only because they were supplemented by those of Cavendish, on which they rested. It was to the latter, therefore, and not to Priestley, that Watt was really indebted for the foundation of his claim to be one of his rivals.

When thus it appears that Cavendish was first in the field, and that he furnished his rivals with the grounds of their conclusions; whilst the one also was fully informed of his theory through Blagden, and the other received an account of it through Priestley; when we further consider that the problems he had to investigate were more numerous and more difficult than those they undertook to solve; and when, lastly, we learn that besides first observing all that they claimed to have observed concerning the combination of hydrogen and pure oxygen, he also investigated the action of hydrogen on atmospheric air, which led him to a new method of analysing air; and also the action of hydrogen on artificial mixtures of nitrogen and oxygen, which led to the discovery of the true nature of nitric acid,—we shall not hesitate to affirm that so far as the discovery of the composition of water by synthesis is concerned, Cavendish must rank above Lavoisier, and far above Watt, however liberally their merit be estimated.

CAVENDISH AS A NATURAL PHILOSOPHER.

PAPERS ON HEAT.

CAVENDISH devoted much attention to the subject of Heat, and appears to have discovered for himself the great laws, which in the language of the material hypothesis of heat, determine the relation of bodies to latent and specific caloric. Among his unpublished papers remains an extensive series of experiments on the heat of liquidity and gaseity, and on specific heat, besides an inquiry into the change of temperature which accompanies chemical combination when that is attended with the evolution of a gas. The original record of these researches occupies 120 small octavo pages, which are not dated up to the 89th. It bears date February 5th, 1765, and as the experiments registered on the preceding pages must have occupied many weeks, it is certain that Cavendish's investigation into the chemical relations of heat, must have commenced in 1764, and occupied a considerable portion of that year. He was thus, probably, contemporaneous with Black in many of his discoveries on heat, though not in his earliest, which go back to 1758. From the way, however, in which Black made his observations public, not through the press, but in his University Lectures at Glasgow and Edinburgh, it is difficult to determine the exact date of many of his discoveries; and still more difficult to ascertain to what extent those who were not privileged to hear his prelections, and were not in direct communication with his pupils, were familiar with his views.

How far accordingly Cavendish was conversant with Black's speculations cannot be precisely determined. Cavendish refers to some of them as known to him, but of others he seems to have been quite ignorant; and it is incredible that he should have made the elaborate researches he records, had he been aware of all that now appears in the posthumous works of Black on heat. The inquiry seems to have been undertaken solely for his own instruction, for he published no part of it till nineteen years after most of the experiments were completed, and then only a trifling fragment of it appeared incidentally in a paper which will be presently referred to, on the freezing of Mercury, read to the Royal Society in 1783.* Perhaps a reluctance to enter into even the appearance of rivalry with Black, prevented him from publishing researches which might be thought to trespass upon ground which the latter had marked off for himself, and preoccupied. In truth, however, with Cavendish publication was the exception, not the rule, and he has left so many completed researches unpublished, that no special hypothesis is needed to account for those upon heat having remained in manuscript.

* Among the Cavendish MSS. are both the original notes on heat, and a treatise drawn up from them, which, as Mr. Harcourt has pointed out, and, as the personal references show, "was written for the use of some individual, it does not appear whom." (*Brit. Assoc. Rep.* 1839, p. 45.) Cavendish's views were thus reduced to a shape in which they might at once have been printed, had he cared to publish them.

Mr. Harcourt has given an interesting sketch of the contents of the Cavendish MSS. on heat, to which I would refer those who are curious to know what our philosopher observed for himself on this subject.* A brief notice, however, of his observations will make his printed papers on heat more intelligible, and will justify what was said of Cavendish as an investigator of the laws of heat in the personal memoir. I shall quote entirely from what Mr. Harcourt calls "the paper of results and deductions." It occupies 50 pages quarto, and has evidently been drawn up from the notes, already referred to, which extend over 120 pages octavo.

The first experiments commented on, were similar in principle to those made by Dr. Brooke Taylor and others, to determine whether or not the mercurial thermometer is an accurate and uniform measurer of temperature; and consisted in mixing a given weight of cold water with a given weight of hot water, and *vice versa*, with a view to ascertain whether the temperature of the mixture is the arithmetical mean of the temperatures of the hot and cold water. The experiments were made with all the precision and accuracy which might be expected from the known character of the observer; and the immediate results obtained were carefully corrected, so as to eliminate the loss of heat occasioned by the cooling of the vessel in which the mixture was made, and by the abstraction of that which was spent in raising the temperature of its walls, and of the stirrer used in effecting the complete mixture of the two liquids. The results were so accurate that, to take one example, the temperature of the mixture came out 149·2°, whilst "the heat of the mixture by computation should be 149·1°, therefore the heat of the mixture is $\frac{1}{10}$th of a degree greater than it should be by computation." (MS. p. 9.)† By these trials Cavendish justified the truth of the proposition which determined him to make them, namely, "that on mixing hot and cold water the quantity of heat in the liquors taken together should be the same after the mixing as before, or that the hot water should communicate as much heat to the cold water, as it lost itself."‡ Thus much settled, Cavendish proceeded to try the effect of mixing unlike liquids at different temperatures with each other. He introduces these experiments with the following significant notice: "One would naturally imagine that if cold mercury or any other substance is added to hot water, the heat of the mixture would be the same as if an equal quantity of water of the same degree of heat had been added, or in other words that all bodies heat and cool each other when mixed together equally in proportion to their weights. The following experiments, however, will show that this is very far from being the case." (MS. p. 12.)

It is plain from this statement that Cavendish was not aware of the experiments which Black and his pupils had tried, or about that period were trying; and the fruit of his researches shows that had he published his results when he obtained them, he would have deprived Irvine, Crawford, and Wilcke of much, if not all the praise they have gained, and would have anticipated or rivalled Black in some of his most important observations.

* *Brit. Assoc. Rep.* 1839, pp. 45—50.

† This quotation, and those which follow, are from the Quarto Manuscript referred to in the text, which, along with Cavendish's other unpublished papers, Lord Burlington has kindly placed at my disposal.

‡ The whole passage will be found in *Brit. Assoc. Rep.* 1839, p. 48. It occurs on page 8 of the MS., but is directed to be substituted for p. 1, which is deleted.

His first experiments were made with quicksilver and water, at different temperatures, and the general result he arrived at was, that "hot water is cooled near as much by the addition of 1 part of cold water as by that of 30 parts of mercury of the same heat." (MS. p. 14.) These numbers refer to parts by weight, as all those in Cavendish's similar experiments do, and his determination of the specific heat of water as 30 times greater than that of mercury, as inferred from a comparison of equal weights, is exactly in accordance with later observations. The results thus obtained by mixing cold mercury with hot water, were checked by others, where the mercury was the hotter of the two liquids. These trials led to the conclusion, that "water cools hot mercury as much as 30·42 its weight of mercury can do, or the effect of water in cooling hot mercury is 30·42 times greater than that of mercury." (MS. p. 18.) This result is confirmed in various ways, and the statement of it is followed by the account of experiments with other liquids. When spirits of wine and mercury at different temperatures were mixed, it was found, that "by a mean of all the experiments the effect of spirits of wine in cooling hot mercury or spirits seems to be 22·7 times greater than that of mercury, and consequently 1·326 times less than that of water." (MS. p. 23.)

From those experiments and others which I have not mentioned, another and important conclusion was deduced, which is thus stated by Cavendish: "It should seem, therefore, to be a constant rule that when the effects of any two bodies in cooling one substance are found to bear a certain proportion to each other, that their effects in heating or cooling any other substance will bear the same proportion to each other." (MS. p. 26.) After a brief commentary on this law of reciprocal proportior, the following generalisation of all the researches detailed is given. "The true explanation of these phenomena seems to be, that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees by the thermometer, than it does to raise other bodies the same number of degrees." (MS. p. 27.)

This clear and complete definition of the fundamental law of specific heat, is followed by the account of experiments of the same kind as those previously described, made with hot mercury and solutions of sea-salt and pearl ashes, as well as diluted oil of vitriol; and that is succeeded by a table of similar trials made upon solids. In these observations water was taken at an average temperature of about 150°, whilst iron filings, lead shot, tin in fragments, sand, white glass, white marble, brimstone, Newcastle coal, and charcoal, at an average temperature of 50°, were respectively added to the hot water.

An endeavour was made to determine the specific heat of air by blowing it from a smith's bellows, through the worm of a still surrounded by hot water. This method was identical in principle with that followed at a later period by Delaroche, Berard, and others, except that in Cavendish's trials the air was colder than the water, whilst in later researches the gas has been raised in temperature, and made to heat, not to cool, the water. "The quantity of air blown through the worm was $216\frac{1}{2}$ oz., and was heated 63° in passing through; therefore it requires as great a quantity of heat to raise $216\frac{1}{2}$ oz. of air, 63 degrees by the thermometer, as to raise 2573 oz. of water, 0°·96, and therefore the effect of air in heating and cooling other bodies is 5·5 times less than that of water. By another experiment made in the same manner, its effect seemed 9·2 times less than that of water, but the quantity which the water was cooled by blowing the air through the worm, was so small in both these experiments, that one can give but a very imperfect guess at how much its effect is." (MS. p. 32.) Many years had to pass before more accurate results were obtained in reference to the specific heat of any of the gases, including air.

The passage last quoted, is the concluding one of Cavendish's observations on specific heat. Page 33 of the quarto MS. is headed Part 2nd, and is devoted to latent heat.* This subject is introduced by the following statement: "As far as I can perceive, it seems a constant rule in nature, that all bodies in changing from a solid state to a fluid state, or from a non-elastic state to the state of an elastic fluid, generate cold, and by the contrary change they generate heat."—MS. p. 33. Cavendish then proceeds to explain that Dr. Cullen has sufficiently proved that most, if not all, liquids generate cold, when evaporated at a less heat than that which is sufficient to make them boil;† whilst "there is also a circumstance daily before our eyes, which shows that water generates cold," when heated till it passes into ebullition. The circumstance to which Cavendish refers, is the same which fixed Black's attention, and led him to experiment on what he termed the conversion of sensible into latent heat during evaporation; namely, "that water as soon as it begins to boil, continues exactly at the same heat, till the whole is boiled away, which takes up a very considerable time. No reason, however, can be assigned why the fire should not continually communicate as much, or nearly as much heat to it after it begins to boil, as it did when it wanted not many degrees of boiling; and yet during all this time it does not grow at all hotter. This I think shews that there is as much heat lost, or, in other words, as much cold generated by the evaporation, as there is heat communicated to it by the fire. . . . . If no cold was produced by the evaporation, the water should either grow hotter and hotter the longer it boiled, or else it should be entirely converted into steam immediately after it arrived at the boiling point."—MS. p. 34. From this consideration the conclusion is generalised, that all volatile solids and liquids must generate cold when distilled, for they all require a long time after they reach their point of evaporation or ebullition, before they are completely volatilised.

These views pointed to a method by which it might be ascertained what "was the quantity of cold produced," or, as Black would have said, what was the quantity of heat rendered latent during the ebullition of water. To determine this, it was only necessary to ascertain at what rate a given weight of water rose in temperature when exposed to a (hypothetically) uniform source of heat, from a given thermometric degree lower than 212°, till it began to boil; and thereafter, to estimate the addition of heat to the boiling water, as proceeding at the same rate as it did before the liquid passed into ebullition. The source of heat which Cavendish employed, was a compound spirit lamp, so arranged that he could kindle seven or fewer wicks at pleasure. It was placed below a

* Cavendish does not use the term 'latent heat,' but prefers, as will be seen in the sequel, to speak of the 'generation' of heat and cold. His views on this point will be considered more fully in the notice of his paper on the freezing of mercury, where he states them at length.

† Cullen's paper first appeared in the *Edinr. Physical and Literary Essays*, vol. ii. p. 159. This was published in 1770, and determines the date of Cavendish's observations, discussed in the text, as later than that year. It was afterwards reprinted along with Black's "Experiments upon Magnesia Alba, &c.," with the title, "An Essay on the cold produced by evaporating fluids, and of some other means of producing cold."

tin vessel containing a weighed quantity of water, and enclosed in a metallic case to prevent loss of heat. A thermometer in the liquid marked its temperature at the beginning of the experiment; and the time which the water took to rise a given number of degrees after the lamp was lighted, was carefully watched, up to the point of ebullition. The liquid was allowed to boil some twenty minutes, and the vessel being then weighed, the loss in weight gave the amount of water vaporised. The result in the first recorded trial, with four wicks of the lamp lighted, was, that "there is as much heat lost by converting any quantity of water into steam, as is sufficient to raise that quantity of water 982°, or in other words, there are 982° of cold generated by converting water into steam." —(MS. p. 39).

Dr. Black had observed, as Cavendish was informed, "that in distilling water, the water in the worm tub is heated thereby much more than it would be by mixing with it a quantity of boiling water equal to that which passes through the worm. Upon this principle I made some experiments to determine how much heat is generated by converting water from the state of an elastic to that of a non-elastic fluid."—(MS. p. 40). "The heat produced by the condensation of the vapours of boiling water, by a mean of several experiments tried in the foregoing manner was about 920°, so that it seems likely that there is just as much heat produced by the condensation of steam into water, as there is cold by the changing of water into steam."—(MS. p. 41.)*

The account of these trials is followed by that of an entirely original series of experiments undertaken with a view "to find whether any cold was generated by the emission of fixed air in dissolving alkaline substances in acids. The way," continues Cavendish, "I tried it, was by finding how much more heat was produced by saturating soap-leys, spirits of sal ammoniac made with lime, and lime slaked with water (all which substances contain no fixed air), with spirit of salt, than by saturating the same substances saturated with fixed air, that is, a solution of pearl ashes, the mild spirits of sal ammoniac and whiting mixed with water, with the same acid. By a comparison of the experiments, it seemed that the cold generated by the emission of the fixed air, was sufficient to heat a quantity of water equal in weight to the fixed air emitted, about 1000 or 1700 degrees."—(MS. p. 42.)

Cavendish now passes from the heat of gaseity to that of liquidity, and records a series of "experiments to show that bodies, in changing from a solid state to a fluid state, produce cold; and in changing from a fluid to a solid state, produce heat."—(MS. p. 43.)

These are introduced by a statement that the propositions in question may be proved by a reference to the long time required to thaw ice, or to freeze water, and also by the phenomena which attend the congelation of this liquid, when cooled some degrees below 32° without freezing, and then agitated. In these circumstances it suddenly begins to freeze, and its temperature immediately rises to the freezing point. A direct trial

* From the Octavo Notes, which formed the basis of the Quarto MS., we learn some of the numbers from which the mean given in the text was calculated. At page 68 (8vo), occurs the entry, "*Therefore heat gen.* by condensation of vapours = 923°." At page 70 there is a similar entry, "= 941°"; and at page 71 another, "= 942°." Despretz made the latent heat of water-vapour 955°.8; Dulong, 977°.4; and one of the latest and most accurate observers, Brix of Berlin, makes it 972°. (*Graham's Chemistry*, p. 57.) Cavendish, as already mentioned, obtained 982° as the result of his observation on the rate at which boiling water acquired heat.

is then described, in which "the quantity of cold produced by the changing of snow into water" was determined, "by dissolving a given quantity of snow in warm water. The cold produced seemed to be about 170 degrees. There seemed no difference between the cold produced by snow, and by the same quantity of ice."—(MS. 44.)*

Reference is then made to the cold produced by mixing salt and snow. "There can be no doubt," Cavendish observes, "but this increase of cold is owing to the melting of the snow," (MS. p. 45.); and then he alludes very briefly to some trials made to determine the quantity of cold produced by mixing snow with different liquids, which did not lead to any very precise result. He was more successful with other liquids. "I find also," says he, "that cold is generated by the melting, and heat by the hardening of spermaceti. The cold produced by the melting of spermaceti is sufficient to cool a quantity of water equal to it in weight, about 70 degrees, and nearly the same degree of heat is produced by the hardening of spermaceti." (MS. p. 45.)

The last series of recorded experiments has the most direct bearing on his published papers on heat. They were made with the more fusible metals, taken singly and in combination. "Some tin and lead were melted separately in a crucible, and a thermometer put into them and suffered to remain there till they were cold. The thermometer cooled pretty fast till the metal began to harden round the edges of the pot; it then remained perfectly stationary till it was all congealed, which took up a considerable time. It then began to sink again. On heating the metal with the thermometer in it, as soon as the metal began to melt round the sides, the thermometer became stationary as near as I could tell, at the same point that it did in cooling, and remained so till it was entirely melted. On putting a thermometer into melted bismuth, the phenomena were the same, except that the thermometer did not become stationary till a good deal of the metal was hardened, unless I took care to keep the thermometer constantly stirring about. It then remained stationary till it was almost all hardened.

"I do not know what this difference between bismuth and the two other metals [lead and tin] should be owing to, except to its not transmitting heat so fast as them. I forbear to use the word conducting, as I know you have an aversion to the word, but perhaps you will say the word I use, is as bad as that I forbear." (MS. pp. 46, 47.)†

Similar observations were made with mixtures in various proportions, of lead, tin, and bismuth. The mixtures were found to "begin to abate of their

* In the Octavo Notes, different experiments made by melting ice and snow with hot water are recorded. In these the cold generated, in other words, the latent heat of ice (or snow) is 154°, 151°, 142°, and again 154°. (*MS. 8vo.* pp. 86—88.) In his published views on heat, Cavendish preferred the mean of those numbers to 170°, which he gives in the Quarto Manuscript, apparently from a single experiment recorded in the notes as yielding 171°.

† The party thus addressed is not known, and cannot be guessed at. Cavendish admitted very few to his intimacy; but even among those few, it would be impossible to assign a satisfactory reason for selecting one rather than another, as the probable object of his confidence, and even deference, on the point referred to in the text. It is quite possible that the paper, though intended specially for some one, was never sent to him. It is difficult to account for its existence among Cavendish's papers, if it had been despatched to some friend. A copy, however, may have been made, and the original retained, and perhaps this is rendered probable by the incomplete condition in which the Quarto Manuscript remains, so far as its conclusion is concerned. This is referred to more particularly a little further on.

fluidity in a heat considerably greater than that in which they grow hard, whereas in the simple metals, I could not perceive any difference between the heat in which they ceased to be perfectly fluid and that in which they hardened. . . . . . .

"As soon as the metal began to abate of its fluidity the thermometer began to sink, extremely slow in comparison of what it did before, and continued to do so till it was taken out, so that I think there can be very little (MS. p. 48,) doubt but what these metallic substances generate heat in hardening, as well as the simple metals." (MS. p. 49.) Cavendish then mentions his belief that the reason why the thermometer never became stationary in the melted alloys, was the separation of their constituent metals from each other, when solidification commenced. He demonstrated the truth of this view by showing that when an alloy of lead and tin was allowed to cool slowly, and cut into two pieces, the lower half had a higher specific gravity, and evidently contained more lead than the upper. (MS. p. 49.) This concludes the quarto MS. which ends abruptly thus: "To understand this you must read the following proposition." (MS. p. 50.)*

Such is a short account of Cavendish's unpublished researches on heat. That he was unacquainted with Black's contemporaneous researches, except to a small extent, is evident from the tenor of his remarks. On this subject, Mr. Harcourt, after stating the number which Cavendish assigned to the latent heat of steam, offers the following commentary. "At what date Black and Watt arrived at a similar result, I know not. Nor do I know the precise year in which Black first taught the doctrine of specific heat. Dr. Thomson says, 'that the specific caloric of bodies is different, was first pointed out by Dr. Black in his lectures at Glasgow between 1760 and 1765. Dr. Irvine afterwards investigated the subject between 1765 and 1770 (*Black's Lectures,* I. 504), and Dr. Crawford published a great number of experiments on it, in his treatise on heat (1779), but Professor Wilcke, who published the first set of experiments on the subject (*Stockholm Transactions,* 1781), introduced the term, specific caloric.' 'I have been informed,' he adds, 'by the late Professor Robison, that Wilcke's information was got from a Swedish gentleman who attended Dr. Black's lectures, about 1770.' It appears probable, from what I have stated, that this unpublished series of experiments by Cavendish is the first made upon this subject. After these and immediately preceding those of the date Feb. 1765, is Cavendish's determination of the number of degrees of 'cold gen. by thawing ice or snow,' which he found on an average to be 150°. In the account which Black gives in his lectures, of his determination of the quantity of heat absorbed in the melting of ice, he says, 'These two experiments, and the reasoning which accompanies them were read by me in the Philosophical Club or Society of Professors in Glasgow in the year 1762.' "† This passage has given offence to some of the advocates of Watt; for it is one of the unfortunate secondary results of the Water Controversy, that in the anxiety on the one side to exalt, and on the other to depreciate Cavendish's merits, he has been contrasted and

* In the parcel which contains the Quarto Manuscript, are three fragments on heat, two of which Mr. Harcourt has printed. (*Brit. Assoc. Rep.* 1839, p. 47.) They are only repetitions, however, with slight variations of part of the contents of the Quarto Manuscript, and I have not thought it necessary to reprint them. The third fragment is entitled, "A compleat proof that the quantity of heat in different bodies, at a given temperature, is not in proportion to their specific heats." From the employment of the term *specific heat,* which Cavendish never employs in his other MSS., I think this fragment must be of much later date than the Octavo Notes or Quarto Manuscript.

† *Brit. Assoc. Rep.* 1839, p. 46.

compared with other natural philosophers, but especially with Black, for whom Lord Brougham has claimed discoveries which the opponents of Watt attribute to Cavendish, whilst Mr. Harcourt, in the passage just quoted, assigns Cavendish some share in a discovery which others regard as due only to Black. In defence of the latter, Mr. Muirhead quotes a passage from an unpublished letter of Black to Watt, of 15th May, 1780, which remains among the papers of the latter. "I began," says Dr. Black, "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 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."* After adducing this passage, Mr. Muirhead adds in a note:—"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."† Mr. Harcourt, however, neither states, nor insinuates that Cavendish had anticipated Black in recognising the existence of latent heat in fluids. He has drawn attention, three times over, in the context of the passage quoted, to Cavendish's acquaintance with Black's observation "that in distilling water and other liquors, the water in the worm-tub is heated thereby much more than it would be by mixing it with a quantity of boiling water equal to that which passes through the worm."‡ What Mr. Harcourt suggests is, that Cavendish *may have been* the first to determine, not the presence, but the *amount* of latent heat in steam, and further that he may have been earlier than Black, or his pupils, in prosecuting experiments on the *specific heat* of bodies. The letter quoted by Mr. Muirhead refers only to the general doctrine of latent heat, and is silent on the subject of specific heat, so that it brings to light nothing incompatible with Mr. Harcourt's cautious and temperate suggestions. A peculiar difficulty, indeed, attends the determination of all the dates of Black's discoveries, for his posthumous lectures, edited by Professor Robison, are frequently in error as to dates (doubtless accidentally), and this to the disadvantage of Black himself. It is often, accordingly, impossible to determine within several years, when a particular series of researches was made, so that we cannot do justice either to Black or to his rivals. From a statement in Professor Robison's preface to Black's Lectures, it appears that "it was late before he [Black] had made such experiments as satisfied him in respect to the precise quantity of the heat latent in steam, not till the summer of 1764," (page XLII.); so that Cavendish's determination of that point cannot be considered as earlier than his. It would appear, however, to have been contemporaneous, nor is it in any degree probable that Cavendish was aware of Black's observations, for he so frankly refers to him in various of his papers, both published and unpublished, that we may be certain that he would have alluded to his determination of the latent heat of steam, had he been acquainted with it. That he does not, will be sufficient to convince all candid judges of his ignorance, and if Black's *ipse dixit* is to be accepted as settling the nature and date of his observations, (which is Mr. Muirhead's argument) then Cavendish's *ipse dixit* should also be accepted as deciding the nature and date of his. It may be added, as already incidentally mentioned, that it is im-

* *Watt Corr.* Introd. p. xxiii.

† *Watt Corr.* p. xxiv.

‡ *Brit. Assoc. Rep.* 1839, p. 48.

possible to imagine that Cavendish should have taken the trouble he did, to make observations which he would not and did not publish, could he have obtained the information which they taught him, accredited by one whom he respected so much as he did Black. Further, it is certain that, except the latter's students in the University of Glasgow, very few previous to 1766 had the means of learning what discoveries Black had made; and how imperfect a medium of publication university lectures are, we see proved at the present day, when news of all kinds travel so much more rapidly than they did eighty years ago, by the difficulty which professors, who confine the publication of their views to their unprinted lectures, have had in vindicating their priority against rivals at no great distance from them, to whom these prelections were unknown. This remark applies with peculiar force to Black's earlier lectures, when he was little known out of Scotland as an authority on heat, and it is specially pertinent in reference to one who led so retired a life as Cavendish did. We shall presently find, also, that Cavendish ultimately published some of his observations on latent heat, with an incidental object in view, and that though pointedly referring to Black, whom he had plainly no thought of rivalling, yet he spoke of the researches as original on his own part.* So far as regards specific heat, there exists no room for doubt, that had Cavendish published, early in 1765, his views on it, he would now be reputed one of its independent discoverers.

I have made these remarks chiefly because it might otherwise seem as if Cavendish actually wronged Black; not with any purpose of putting him forward as a rival of that great chemist. Cavendish voluntarily suppressed all, or nearly all publication of his experiments on heat; and it would be idle now to put forward claims for him as a discoverer in that department of science. But it is only justice to him, and it is no injustice to Black, to refer to the beautiful and independent researches of the former, as evidences of his ingenuity, capacity, perseverance, and accuracy, when seeking to form an estimate of his intellectual character. But for the statements of Mr. Muirhead, I should have referred to his unpublished experiments on heat in no other light than in this.

Thus much premised, I now proceed to the consideration of Cavendish's published papers on heat, and first, of that published in 1776, entitled "An Account of the Meteorological Instruments used at the Royal Society's House."†

This paper is occupied with an account of the thermometers, barometer, rain-guage, hygrometer, variation-compass and dipping-needle, then in use at the Royal Society's apartments. As all those instruments have long been replaced by others constructed with the improvements which later discoveries have suggested, I have thought it needless to make a formal analysis of the entire paper.‡ The only part of it which concerns us in this section, is that treating of the Society's thermometers; and this only in so far as it refers to the best method of constructing

* *Phil. Trans.* 1783, p. 312.

† Read to the Royal Society, March 14th, 1776. *Phil. Trans.* vol. lxvi. p. 375.

‡ It may also be added, that the Royal Society has, since 1843, ceased to prosecute systematic meteorological observations. They were commenced in 1773, and carried on without interruption till 1843, when they were discontinued, in consequence of the Government, on the recommendation of the President and Council of the Royal Society, having established at the Royal Observatory at Greenwich, under the superintendence of the Astronomer Royal, a magnetical and meteorological observatory, where observations are made on an extended scale, which are regularly published. *Phil. Trans. for* 1844, and *Weld's Hist. of Royal Society*, vol. ii. p. 76.

such instruments. To this subject Cavendish devoted much attention, and he was the means of introducing methods of accurate construction, which have since been universally followed. The accuracy of all Cavendish's observations on heat was no doubt in great part owing to the care which he evinced in selecting or constructing the thermometers he employed. Before his time, even so-called standard instruments, as they were made in England, were frequently in error two or more degrees. This he showed to be mainly owing to the inaccurate way in which the degree of boiling water was ascertained. Two points in particular were neglected even by the best artists. To the first he refers in the following terms:—"It has been too common a custom, both in making experiments with thermometers and in adjusting their fixed points, to pay no regard to the heat of that part of the quicksilver which is contained in the tube, though this is a circumstance which ought by no means to be disregarded; for a thermometer dipped into a liquor of the heat of boiling water, will stand at least 2° higher, if it is immersed to such a depth that the quicksilver in the tube is heated to the same degree as that in the ball, than if it is immersed no lower than the freezing point, and the rest of the tube is not much warmer than the air. The only accurate method is to take care that all parts of the quicksilver should be heated equally. For this reason, in trying the heat of liquors much hotter or colder than the air, the thermometer ought if possible to be immersed almost as far as to the top of the column of quicksilver in the tube."* As the entire immersion, however, of the thermometer into boiling liquids would often have been extremely inconvenient, and indeed impossible, a table is subjoined by means of which the difference between the indicated and the real temperature may be ascertained and allowed for; the table being constructed on the datum, "that quicksilver expands 11,500th part of its bulk by each degree of heat."

The other circumstance "which ought to be attended to, in adjusting the boiling point of a thermometer, is, that the ball should not be immersed deep in the water; for, if it is, the fluid which surrounds it will be compressed by considerably more than the weight of the atmosphere, and will therefore acquire a sensibly greater heat than it would otherwise do."† An apparatus is then described, by means of which both the sources of inaccuracy pointed out may be avoided. It consists essentially of a tall metallic boiler filled to a small depth with water, and having two apertures in its lid; one for the insertion of the thermometer, which is made steam-tight by corks or tow after the tube has been introduced; and a second and wider perforation, elongated into a canal, and acting as a chimney, to carry off the steam, which is allowed to escape by a small aperture. With this apparatus matters could easily be arranged, so that the bulb should be but a slight depth below the surface of the boiling water, whilst the greater part of the stem of the thermometer was surrounded by hot steam, so that bulb and stem were at the same temperature. In such an arrangement, the thermometer is found to stand not sensibly higher when the water boils vehemently, than when it boils gently; and the method admits of great accuracy in the fixing of the boiling point of water.

These directions were afterwards generally sanctioned by the Royal Society,‡ and are still in practice in the construction of standard thermometers; but there is as much reason now as there was in the days of

* *Phil. Trans.* 1776, p. 376.

† *Ibid. ibid.* p. 379.

‡ *Ibid. ibid.* 1777, p. 816.

Cavendish, to complain of the want of uniformity between professedly accurate instruments.

The remaining papers of Cavendish on heat will enable us to discover what use he made of his acquaintance with a method for accurately constructing thermometers; and how he applied his theory of the nature of heat, and his observations on its generation and destruction, to the explanation of remarkable natural phenomena. The first paper I have to notice is entitled, "Observations on Mr. Hutchins's Experiments for determining the degree of Cold at which Quicksilver freezes."* To understand this paper, two communications of Mr. Hutchins's to which it refers, and the preliminary observations which led to his trials, must be considered. This may be done most conveniently by adducing a few details from a very elaborate paper which follows Cavendish's in the 73rd vol. of the *Phil. Trans.*, entitled "History of the Congelation of Quicksilver, by Charles Blagden, M.D., F.R.S., Physician to the Army."

From the time of the alchemists downwards, chemists had been in the habit of regarding quicksilver as a quite peculiar substance which differed from all the other metals, in being liquid at ordinary temperatures, and appeared possessed of an essential principle of fluidity. These views were first overturned by Professor Braun of Petersburgh, who, after his removal to that city, prosecuted an inquiry which had already been commenced in Germany, on the action of freezing mixtures, and the lowest temperature which they could secure. Availing himself of the cold which characterises a Russian winter, Professor Braun selected a day (14th December, o.s., 1759) when the thermometer in the open air stood so low as $-34°$ F., and made a freezing mixture with nitric acid and snow, into which he plunged his thermometer. He was astonished to perceive it rapidly sink to $-69°$, which was lower by almost thirty degrees than it had fallen in any preceding experiment then on record. It did not stop however here; for, by using fresh supplies of the freezing mixture, the mercury descended to $-100°$, $-244°$, and $-352°$. At this stage in the inquiry, Braun removed the thermometer, and was astonished to find the mercury quite fixed and immovable. He repeated the experiment some days later, and as soon as the quicksilver became immovable, broke the bulb of his thermometer, and "obtained a solid, shining, metallic mass, which extended under the strokes of a pestle; in hardness rather inferior to lead, and yielding a dull dead sound like that metal."† The conclusions which these observations justified, viz. that mercury can be solidified, and that it is a true metal which melts with a very small degree of heat, were clearly perceived by their first observer. He failed, however, to see the significance of another phenomenon which he observed, viz. that mercury contracts and becomes denser when congealed, so that the solid metal sinks in the liquid. Making no allowance for this, Braun supposed the descent of the mercury in the stem of the thermometer to be solely due to the contraction occasioned in the metal whilst *still liquid* by its reduction in temperature; and, in consequence, he set down the freezing point of mercury as some hundred degrees below Fahrenheit's zero. He was unable, however, to determine it exactly, "owing to the various impediments that occurred from adhesion of the quicksilver in the thermometrical tube, hollows left in the bulb as it froze, portions of the mercury remaining uncongealed, and many other causes."‡

* Read to the Royal Society, May 1st, 1783. *Phil. Trans.* 1783, p. 303.

† *Phil. Trans.* 1783, p. 332.

‡ *Ibid. ibid.* p. 335.

Braun's experiments excited universal interest throughout Europe, and, in compliance with his suggestion or request, that his observations might be repeated at Hudson's Bay, the Royal Society procured the co-operation of Mr. Hutchins, who tried the necessary experiments at Albany Fort, Hudson's Bay, in January and February, 1775. He found no difficulty in freezing mercury, by placing it in a mixture of nitric acid and snow, when the temperature of the air was — 37° F. The mercury descended to — 430°, and, when the tube was broken, appeared in greater part frozen, and admitted of being hammered.* This was the fullest confirmation which Braun's experiments had received, but Hutchins found it impossible to determine, even within wide limits, the freezing point of mercury. In 1781, however, he repeated his experiments with a different apparatus; and this brings us to Cavendish's paper. The result of Braun and Hutchins's observations had been to show, that it was vain to attempt to determine the freezing point of mercury by enclosing it in a graduated thermometer tube, seeing that it was impossible to determine how much of the diminution in volume of the mercury as it cooled was owing to contraction before, how much to contraction after, congelation. Cavendish, accordingly, suggested a very simple apparatus, which enabled the one phenomenon to be observed without interference from the other. No attempt was made to freeze the mercury in the bulb of the thermometer, which was employed solely to indicate the temperature. It "was enclosed in a glass cylinder swelled at bottom into a ball, which, when used, was filled with quicksilver, so that the bulb of the thermometer was entirely surrounded with it."† The object of this arrangement was to freeze the quicksilver in the outer cylinder, without freezing that in the thermometer, and to observe the point at which the latter stood when the greater part, *but not the whole*, of the mercury surrounding it was frozen. The principle of this method was the fact, very familiar in our own day, but clearly understood by few at the period when Cavendish wrote, viz. that when a liquid is undergoing congelation, its temperature remains stationary, after it begins to freeze, till it is entirely frozen. This fact, of which Black was the great expounder, Cavendish, as we have seen, had examined minutely for himself, some nineteen years before the paper we are considering was written, and he expounds it in this paper as a thing still requiring exposition, in the following terms: "If a glass of water, with a thermometer in it, is exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze, till it is entirely congealed, and will then begin to sink again. In like manner, if a thermometer is dipped into melted tin or lead, it will remain perfectly stationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it will again begin to descend; and there was no reason to doubt that the same thing would obtain in quicksilver.

"From what has been just said it was concluded, that if this apparatus was put into a freezing mixture of a sufficient coldness, the thermometer would immediately sink till the quicksilver in the cylinder began to freeze, and would then continue stationary, supposing the mixture still to keep cold enough, till it was entirely congealed. This stationary height of the thermometer is the point at which mercury freezes, though, in order to make the experiment convincing, it was necessary to continue

* *Phil. Trans.* 1776, p. 176. † *Ibid. ibid.* 1783, p. 303.

the process till so much of the quicksilver in the cylinder was frozen as to put the fact out of doubt."*

The apparatus already described was sent by Cavendish to Mr. Hutchins, who employed it at Albany Fort, in Hudson's Bay, in 1781. It completely answered the expectations of its deviser, as appears from Mr. Hutchins's paper, entitled "Experiments for ascertaining the point of Mercurial Congelation, read [to the Royal Society] April 10th, 1783."† He had every assistance in prosecuting his observations, for, curiously enough, Dr. Black, without being aware of the process which Cavendish had proposed in 1776, suggested an exactly similar mode of procedure. His letter containing this is dated 1779, and seems to have reached Mr. Hutchins before Cavendish's communication did.‡ It thus appears, that Hutchins had the advantage of Cavendish and Black's suggestions, and that the apparatus he employed was furnished by the former, and was in conformity with the instructions of both the philosophers.

The results of Hutchins's observations are stated succinctly in the following passage from Blagden's history of the congelation of quicksilver. "They have not only confirmed the preceding observations relative to the solid state into which quicksilver can be brought by cold, its metalline splendour and polish when smooth, its roughness and crystallisation where the surface was unconfined, its malleability, softness, and dull sound when struck; but have also clearly demonstrated, that its point of congelation is no lower than $-40°$, or rather $-39°$, of Fahrenheit's scale; that it will bear, however, to be cooled a few degrees below that point, to which it jumps up again on beginning to congeal; and that its rapid descent in a thermometer through many hundreds of degrees, when it has once past the above mentioned limits, proceeds merely from its great contraction in the act of freezing."§ The results thus referred to, were obtained, both by the artificial congelation of mercury, and also, and very satisfactorily, by its natural freezing. On 26th January, 1782, the temperature at Albany Fort was so low, that mercury standing in the open air was frozen through a considerable part of its mass. "As this," observes Mr. Hutchins, "was a certain method to find the point of congelation, I introduced the mercurial thermometer and the spirit thermometer into the fluid part, after breaking off the top of the phial, and they rose directly and became stationary, the former at 40° or $40\frac{1}{2}°$, the latter at $29\frac{3}{4}°$, both below the cypher."‖

I now return to Cavendish's comments on these observations. A large part of the former is occupied with the account of an examination of the accuracy of Mr. Hutchins's thermometers, which were sent home, and carefully examined as to their graduation by Cavendish. The most accurate thermometer showed the freezing point of mercury to be $-40°$, and "as it appeared," observes Cavendish, "from the examination of this thermometer after it came home, that $-40°$ thereon answers to $-38\frac{2}{3}°$, on a thermometer adjusted in the manner recommended by the Committee of the Royal Society, it follows, that all the experiments agree in shewing that the true point at which quicksilver freezes is $38\frac{2}{3}°$,

* *Phil. Trans.* 1783, p. 305.

† This paper will be found at the end of the 73rd vol. of the *Phil. Trans.* after the index, where it appears out of its proper place, in consequence of the MS. having been mislaid. The paging is the same as that of Cavendish's paper in the same volume, but is distinguished from it by an asterisk at each figure.

‡ *Phil. Trans.* 1783, p. *305.

§ *Ibid. ibid.* p. 345.

‖ *Ibid. ibid.* p. *368.

or, in whole numbers, 39° below nothing."* From later observations it appears, that the corrected temperature is about half a degree too high, and that we may call the freezing point of mercury 39°·5.

The most interesting portions, however, of Cavendish's paper, are those which contain his speculations on the relation of heat to fluidity. The first of these refers to some remarkable appearances observed by Mr. Hutchins in his experiments. One of these was, that the thermometer, when plunged into very cold mercury, was occasionally observed to sink three or four degrees below $-40°$, the reputed freezing point of the metal, and then suddenly to rise again to the point from which it fell, and remain there stationary for many minutes. This phenomenon, Cavendish infers to depend upon mercury admitting, like water, of being cooled in certain circumstances, below its freezing point, without freezing, whilst, if agitated, as by the insertion of a thermometer, it immediately rises to the point of congelation, and remains stationary there whilst the liquid is freezing. "The cause of the rise of the thermometer," he observes, "when the water begins to freeze, is the circumstance, now pretty well known to philosophers, that all, or almost all, bodies, by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; and that cold is produced by the contrary process."†

In continuation of these remarks, Cavendish observes, a little farther on, "that if it was not for this generation of heat by the act of freezing, whenever a vessel of water exposed to the cold was arrived at the freezing point, and began to freeze, the whole would instantly be turned into solid ice."‡ He pursues this idea somewhat further, and adds, "In like manner, it is the cold generated by the melting of ice which is the cause of the long time required to thaw ice or snow. It is this also which is the cause of the cold produced by freezing mixtures; for no cold is produced by mixing snow with any substance, unless part of the snow is dissolved."§ He then refers to the unpublished experiments already noticed, which led him to the conclusion that the latent heat of ice or snow, is 150°. In explaining this, as in all his papers on heat, he uses the terms, *production, or generation of heat and cold,* in reference to which he adds the following important note: "I am informed that Dr. Black explains the above mentioned phenomena in the same manner; only instead of using the expression, heat is generated or produced, he says, latent heat is evolved or set free. But as this expression relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their containing more or less of a substance called the matter of heat, and as I think Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is generated. Mr. Wilke, also, in the *Transactions* of the Stockholm Academy of Sciences, explains the phenomena in the same way, and makes use of an hypothesis nearly similar to that of Dr. Black. Dr. Black, as I have been informed, makes the cold produced by the thawing of snow 140°; Mr. Wilke, 130°."‖ This passage, it will be observed, is closely accordant in its general bearing with the second interpolation in the "Experiments on Air," (First Series, 1784), in which Cavendish declines to adopt Watt's phrase of elementary heat.

* *Phil. Trans.* 1783, p. 321. † *Ibid. ibid.* p. 311.
‡ *Ibid. ibid.* p. 312. § *Op. et loc. cit.*
‖ *Ibid. ibid.* Foot note, pp. 312, 313.

The reference to the generation of cold by the liquefaction of snow, is followed by an allusion to the unpublished experiments already considered, on the solidification of melted tin and lead, and the conclusion is drawn "that those metals, as well as water and quicksilver, may bear being cooled a little below the freezing or hardening point (for the hardening of melted metals and freezing of water seems exactly the same process) without beginning to lose their fluidity."*

The succeeding part of the paper contains a minute and careful criticism of all Mr. Hutchins's experiments, which does not call for special reference, as the result at which he arrived has long been universally credited by men of science.

Another point, however, required examination. The mercury, as we have seen, appeared to sink four or five hundred degrees below zero, although in reality the temperature was little below —40°. Cavendish has added a sequel to his paper referring to this. It is entitled, "On the contraction of quicksilver in freezing," and the general purport of it may be gathered from the first sentence, which is as follows. "All these experiments prove that quicksilver contracts or diminishes in bulk by freezing, and that the very low degrees to which the thermometers have been made to sink, is owing to this contraction, and not to the cold having been in any degree equal to that shewn by the thermometer."† The extent to which mercury contracts, Cavendish inferred to be "not much less than its expansion by 500° or 510° of heat, that is, almost one twenty-third of its whole bulk." He thought it likely, however, that congealed mercury might vary in density like other metals solidified from a state of fusion, especially when frozen in the stem of a thermometer, and he had no means of ascertaining the amount of this variation.

The concluding part of this paper discusses the cold of the freezing mixtures employed to congeal mercury. This subject Cavendish pursued in other papers, where his views are displayed more fully. His chief object is to shew that the cold produced by a mixture of nitric acid with snow, which was the freezing mixture made use of, is owing to the melting of the snow; but that in all probability there is a certain degree of cold in which the nitric acid, so far from dissolving snow, will yield part of its own water, and suffer that to freeze, as is the case with solutions of common salt. And as the generation of cold, or reduction of temperature can only be determined by the liquefaction of the snow, the cooling must be arrested, if the opposite process, that, viz. of the freezing of water, is going on. It should thus happen that the warmer, within certain limits, the materials are, the greater will be the additional cold produced, and such was the case in Mr. Hutchins's observations, and also in experiments made by Cavendish himself.

He then notices that, paradoxical though it may appear, nitric acid, somewhat diluted, produces a greater cold when mixed with snow, than stronger acid does. He points out very lucidly the cause of this anomaly, viz. that there is a point, up to which nitric acid generates heat by combining with water, and that it is not till this degree of dilution has been attained, that cold is produced. In other words, as we should say at the present day, strong nitric acid combines with water in several proportions to form definite hydrates, and during this chemical combination, heat is evolved; but when these hydrates are added to snow, such combinations are not formed, but a simple solution of the snow occurs,

* *Phil. Trans.* 1783, p. 313. † *Ibid. ibid.* p. 322.

and it renders much heat latent, or generates much cold during its liquefaction. In conformity with these views, Cavendish recommended that in using nitric acid and snow as a freezing mixture, the snow should be gradually added to the strong acid, till the mixture ceased to rise in temperature, before any body to be cooled was placed in it; and that then a large addition of snow should be quickly made. By following this method he succeeded, for the first time in this country, in freezing quicksilver. This was effected in February, 1783, when the temperature of the air was as high as 20° or 25° F.

The publication of Hutchins's and Cavendish's observations, besides establishing incontestably the freezing point of mercury, and the limit downwards to its use in thermometers, put an end to many extravagant speculations which had been afloat concerning the enormously low temperature of the colder regions of the globe. These were not only represented as colder by many hundred degrees than the more temperate countries, but the vicissitudes of their climates were declared to be such, that the thermometer might fall in an hour some four or five hundred degrees. Such opinions, founded on a misrepresentation of the cause of the contraction which accompanies the solidification of mercury, necessarily carried error into every branch of physical science, but especially into physiology, and natural history. It now appeared that the enormous natural and artificial colds which had been believed in, had no existence; and Black and Cavendish were both able to predict, even before Mr. Hutchins's experiments were made, that what the one called the "evolution of sensible heat," and the other simply the "generation of heat," rendered it impossible that there could be such rapid and enormous reductions of temperature as Braun thought he had observed.

The remaining papers by Cavendish on heat are two upon freezing mixtures, in continuation of the views contained in the concluding portion of the paper which has just been noticed. The first is entitled "An account of Experiments made by Mr. John McNab at Henley House, Hudson's Bay, relating to Freezing Mixtures. Read [to the Royal Society] February 23rd, 1786."* The experiments recounted in this paper were made at Cavendish's request, and according to his directions, with a view chiefly to test the truth of the opinion he had published concerning the cause of the cold produced by mixing snow with different liquors. It has already been mentioned that he thought it probable, that at certain temperatures nitric acid, instead of dissolving snow, would part with a portion of its water of combination, which would become frozen, so that if the temperature of the materials was equal to this, no additional cold could be produced. There seemed an objection, however, to the truth of his views in a fact observed both by Fahrenheit and Braun, viz. that even frozen nitric acid will produce cold if mixed with snow. Cavendish accordingly thought it desirable to ascertain whether it was possible to freeze the entire mass of the acid, or at least the more concentrated part, without separation of water from it, in the expectation that such congealed acid might dissolve snow and generate cold in so doing. To determine this, and some other questions, he sent to Hudson's Bay bottles of strong and of diluted nitric acid, of oil of vitriol, and of diluted alcohol. The liquids were ordered to be exposed to the cold, and if they froze the temperature was to be ascertained; the more fluid portion which might appear was to be decanted into another bottle, and each

* *Phil. Trans.* 1786, p. 241.

portion sent home so that it might be ascertained by their examination, whether the original liquid had frozen as a homogeneous body, or had separated during congelation into dissimilar parts.

Cavendish also, it will be remembered, drew attention to the fact that snow in certain circumstances produces heat, not cold, when added to nitric acid, and that when employing those materials for freezing mixtures, the snow should be added gradually to the acid till it begins to fall in temperature, and then rapidly so as to secure its maximum effect in producing cold. By the extension of this method he thought a greater cold might be produced than had been observed before; and for this purpose he furnished Mr. McNab with bottles of nitric acid and oil of vitriol, which had been diluted nearly to the point at which they began to generate cold, when mixed with snow.

Another question sought to be determined was, the lowest point to which thermometers filled with other liquids than mercury, could be made to sink. According to Professor Braun, nitric acid and snow caused thermometers filled with oil of sassafras, and other essential oils, to fall to -100° or -124°, whilst a thermometer filled with the most rectified spirits of wine sank to -148°. To test the truth of these statements, a thermometer filled with oil of sassafras, and two others with spirits of wine were furnished to Mr. McNab. The first of those instruments was early deranged by the appearance of a large air-bubble in its ball, so that no further observations could be made with it. The spirit of wine thermometers could by no arrangement of matters be made to sink "by a mixture of snow and spirit of nitre, to a degree near approaching to that mentioned by Professor Braun."

I now notice briefly the results of these different inquiries. Nitric acid was found capable of a kind of congelation in which the whole, and not merely the watery part, freezes. Its freezing point was found to differ greatly according to its strength, and this variation was found to follow a very unexpected law. The acid was also found to admit of being cooled very much below its freezing point without congelation occurring, but when that did happen the temperature immediately rose to the freezing point. During congelation great contraction occurred, so that the crystals which first formed sank rapidly to the bottom, and when the whole liquid was frozen solid, the surface was depressed and full of cracks.

On mixing snow with nitric acid, results were obtained considerably at variance with those anticipated. In two experiments, the addition of snow produced heat, until the mixture arrived pretty exactly at what was found to be the freezing point of the diluted acid; but as soon as it reached that point the addition of more snow began to produce cold. The cause of this, Cavendish inferred to be the following: The freezing point of nitric acid, when diluted to a certain extent, is much less cold than when it is considerably more diluted, and also than when it is not diluted at all; so that there must be a certain degree of strength, not very different from that to which these acids were reduced by dilution, at which they freeze with a less degree of cold than when they are either stronger or weaker. The acids had thus what Cavendish called "a point of easiest freezing;" and he shows that in the experiments referred to, "when they were diluted to the *strength* of easiest freezing, they would also be at the *heat* of easiest freezing." For if the liquid was below that point, so much of the acid would immediately freeze as would raise it to it, and if the liquid was above that point, so much of the congealed acid would dissolve as would sink it down to it.

In further trials the nitric acid was largely diluted with water, and when exposed to a low temperature was found to congeal in part, so as to present the appearance of flakes or spiculæ of ice, floating through a syrupy liquid. This liquid was decanted from the crystals, and its strength determined by saturating it with marble; and the crystals, after liquefaction, were treated in the same way. They were found to consist, in greater part, of pure water, whilst the unfrozen liquid was strongly acid. From these experiments Cavendish drew the conclusion that nitric acid, or as he called it spirit of nitre, "is subject to two kinds of congelation, which we may call the *aqueous* and the *spirituous;* as in the first it is chiefly if not entirely the watery part which freezes, and in the latter the spirit [*i. e.* the acid] itself. Accordingly, when the spirit is cooled to the point of aqueous congelation, it has no tendency to dissolve snow and produce cold thereby, but on the contrary is disposed to part with its own water; whereas its tendency to dissolve snow and produce cold, is by no means destroyed by being cooled to the point of spirituous congelation or even by being actually congealed."

Experiments of a similar nature were made with oil of vitriol, which was frozen at a temperature of $-15°$. It was allowed to melt partially in a warm room, and the fluid part was then decanted, but was not found to differ sensibly in strength from the undecanted portion of the acid. In other experiments snow was added to the diluted oil of vitriol, as in the preceding experiments to the nitric acid, till it began to fall in temperature, which it did speedily. When snow was then rapidly added to it, the temperature of the air being $-39°$, the mixture sunk to $-55\frac{1}{2}°$. In another trial the thermometer descended to $-68\frac{1}{2}°$, and in a third to $-78\frac{1}{2}°$, a greater cold than had *certainly* been produced up to this time. Oil of vitriol was also shown to be capable of the spirituous congelation, and to freeze with a less degree of cold when strong than when much diluted; but Cavendish could not at this time make out that it had any point of easiest freezing. During the congelation, however, of oil of vitriol, he thought, both from his own experiments and from those of others, some separation of its parts took place, so that the congealed portion differed in some respect from the rest, and in consequence froze with a less degree of cold; and as he could detect no difference between the strength of the congealed and uncongealed portions of the oil of vitriol, he thought it must be owing to the presence of some peculiar substance—probably that which makes glacial sulphuric acid differ from common oil of vitriol. He then refers to the separability from the glacial acid, by the gentlest heat, of "a peculiar concrete substance in the form of saline crystals;" but that this was anhydrous sulphuric acid, and that glacial and common oil of vitriol differed from each other only in the proportion of this substance which they contained, he was not aware. A reference is then made to the action of a mixture of oil of vitriol and spirit of nitre on snow, which does not call for special notice, and the paper concludes with an account of experiments on the congelation of spirits of wine. Little cold was found to be produced when the spirits were mixed with snow. When diluted spirit was exposed to the natural cold of the atmosphere, it froze in part. The congealed portion consisted in greater part of water, what had not frozen retaining the spirit.

Not satisfied in some respects with the results we have been considering, Cavendish, with that extraordinary love of accuracy which he carried into every inquiry, had a second series of trials made by Mr. McNab. The report of these is entitled, "An Account of Experiments made by

Mr. John McNab, at Albany Fort, Hudson's Bay, relative to the Freezing of Nitrous and Vitriolic Acids. Read [to the Royal Society] February 28th, 1788."* Cavendish begins this paper by stating that, from Mr. McNab's previous experiments, he had drawn the conclusion, that when nitric acid is of such strength as not to dissolve $\frac{243}{1000}$ of its weight of marble, or when its strength is less than ·243, as he calls it for shortness-sake, it is liable to the aqueous congelation solely; in other words, the water in it freezes, but not the acid. But if of greater strength, it undergoes the spirituous congelation, or the acid itself freezes. The latter congelation occurred at the highest temperature, when the strength was ·411, in which case the freezing point is at $-1\frac{1}{2}°$. If the acid is either stronger or weaker, it requires a greater degree of cold, so that, for example, if its strength be such as ·54, the freezing point goes down to $-31\frac{1}{2}°$; and if it be so much diluted as to have the strength ·243, it goes down to $44\frac{1}{4}°$. Cavendish, however, as he tells us, thought some of these conclusions "were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction;" and therefore he desired Mr. McNab to try some more experiments to ascertain their truth. For this purpose specimens of nitric acid of different strength were sent to Hudson's Bay, and exposed to the cold till they froze. They were then warmed till the greater part was melted and thereafter exposed a second time to the cold, till a considerable portion of the acid had frozen into a more hard and solid ice than could be obtained by a single congelation. The temperature of freezing having been ascertained both times, the uncongealed liquid was separated from the congealed, and each sent back to England for examination. Bottles of oil of vitriol of different strengths were also sent to be treated in the same way, with a view especially to discover whether oil of vitriol has a strength of easiest freezing.

The general result drawn by Cavendish from these new trials is stated as follows: "These experiments confirm the truth of the conclusions I drew from Mr. McNab's former experiments; for, 1st, there is a certain degree of strength at which spirit of nitre freezes with a less degree of cold than when it is either stronger or weaker; and when spirit of nitre of a different strength from that is made to congeal, the frozen part approaches nearer to the foregoing degree of strength than the unfrozen." He proceeds, 2nd, to notice that the numerical results obtained in the second series of experiments came out very nearly the same as in the earlier trials.

Oil of vitriol presented greater anomalies in its behaviour with freezing mixtures than nitric acid had done, and Cavendish thought he was thereby strengthened in the view we have already seen he entertained, concerning the presence of some peculiar substance in the congealed part of oil of vitriol.

The general conclusion to which these experiments led, is thus stated: "From these experiments it should seem, that the freezing point of oil of vitriol answering to different strengths, is nearly as follows:

Strength.	Freezing Point.
,977	+ 1°
,918	— 26°
,846	+ 42°
,758	— 45°

"From hence we may conclude that oil of vitriol has not only a strength of easiest freezing, as Mr. Keir has shown; but that at a strength superior

* *Phil. Trans.* 1788, p. 166.

to this, it has another point of contrary flexure, beyond which, if the strength be increased, the cold necessary to freeze it again begins to diminish."*

The most interesting part, perhaps, of this paper, is an incidental passage in which Cavendish states how he estimated the strength of sulphuric acid. The numbers in the table representing strength, state the weight of marble in 1000ths of the weight of acid which could dissolve it. Cavendish, however, as he tells us, "did not find their strength by actually trying how much marble they would dissolve, as that method is too uncertain, on account of the selenite (sulphate of lime) formed in the operation, and which in good measure defends the marble from the action of the acid." He then proceeds to state, "The method I used was, to find the weight of the plumbum vitriolatum formed by the addition of sugar of lead, and from thence to compute the strength, on the supposition that a quantity of oil of vitriol, sufficient to produce 100 parts of plumbum vitriolatum, will dissolve 33 of marble; as I found by experiment that so much oil of vitriol would saturate as much fixed alkali, as a quantity of nitrous acid sufficient to dissolve 33 of marble. It may be observed that the quantity of alkali necessary to saturate a given quantity of acid, can hardly be determined with much accuracy, for which reason the foregoing less direct method was adopted; especially as the precipitation of plumbum vitriolatum shows the proportional strengths, which is the thing principally wanted, with as great accuracy as any method I know."† The interest of this passage lies in the fact pointed out elsewhere, that Cavendish had detected, not only the law of constant proportion which regulates the quantitative combinations of chemical substances, but had also, at least in one case, perceived and applied the law of reciprocal proportion. It will be seen from the preceding quotation, that he goes upon the principle, that being aware of the quantity of sulphuric acid which will neutralise as much fixed alkali as the quantity of nitric acid able to saturate 33 parts of marble can neutralise, he is thereby made acquainted, without further experiment, with the amount of sulphuric acid requisite to saturate 33 parts of marble, inasmuch as it will be identical with that needed to neutralise the fixed alkali.

* *Phil. Trans.* 1788, pp. 180, 181.
† *Phil. Trans.* 1788, p. 178.

An Attempt to explain some of the principal Phenomena of Electricity, by Means of an Elastic Fluid. Read to the Royal Society, Dec. 19, 1771, *and Jan.* 9, 1772.*

It will suffice to give the title of this well-known and elaborate paper, in which Cavendish upholds a theory of electricity similar to that of Epinus, whose views, however, were entirely unknown to his English contemporary.

An account of some Attempts to imitate the Effects of the Torpedo by Electricity. Read to the Royal Society, Jan. 18, 1775.†

Cavendish begins this paper by pointing out that Walsh's experiments leave little room for doubt "that the phenomena of the torpedo are produced by electricity;" but that, nevertheless, there are difficulties in the way of this conclusion. One of the principal difficulties is "that a shock may be perceived when the fish is held under water," although the electricity has a readier passage through the liquid, than through the body of the person receiving the shock. This difficulty, however, Cavendish shows is not insurmountable; for it is a mistake to imagine, as some electricians do, that electricity always takes the shortest and readiest circuit; whereas, in reality, it takes all the circuits provided for it, only a greater quantity passes through the bodies which oppose least resistance to its passage. This law is then applied to the demonstration of the truth, that though the electricity discharged by the torpedo must pass chiefly through the water by which it is surrounded, a certain quantity will pass through the body of a person who lays his hands on opposite parts of the surface of the animal; or who merely dips his fingers into the water close to its body.

The second great difficulty in the way of the identification of the torpedinal power with electricity, to which Cavendish draws attention, is, that "no one hath ever perceived the shock to be accompanied with any spark or light, or with the least degree of attraction or repulsion." Those phenomena, however, can only be observed when the circuit along which the electricity passes is *interrupted;* but the shock of the torpedo "would never pass through the least sensible space of air, or even through a small brass chain." The torpedo, accordingly, in this respect, does not differ from a Leyden battery, which, if of large size, will give a sharp shock, although "so weakly charged that the electricity will hardly pass through any sensible space of air; and the larger the battery is, the less will this

* *Phil. Trans.* 1771, pp. 584—677.
† *Phil. Trans.* 1776, pp. 196—225.

space be." Into the proof of this doctrine, Cavendish enters at length, and for the first time lays down that distinction between *intensity* and *quantity* as affecting electrical phenomena, which has since proved so important a guide to the explication of electrical problems. He does not use the word '*intensity*,' but the equivalent terms '*force*' or '*degree*.' These he employs in describing experiments by which it appeared that when Leyden jars of different sizes are electrified "in a given degree," . . . . . "the distance to which the spark will fly is not sensibly affected by the number or size of the jars, but depends only on the force with which they are electrified." In other words, the phenomena referred to, are determined by the *intensity* of the electricity, not by its *quantity*. The peculiar and very ingenious electrometer made use of, is then described; and thereafter experiments are recorded made with jars of the same size unequally charged, from which it appeared that a given quantity of electricity of a certain intensity produces a rather less shock, than twice that quantity with half that intensity. It is further shown, that the rapidity with which the electricity of the torpedo is discharged, renders it impossible that it should move pith-balls suspended near the fish, or otherwise exhibit attraction and repulsion. A description is then given of the artificial torpedo which Cavendish constructed in order to test the truth of his sagacious theories. It was made of wood, with plates of pewter on either side to represent the electrical organs, and covered with sheepskin. A wire attached to each metallic plate, passed through an insulating glass tube and terminated in a brass knob. When this instrument, or 'artificial torpedo,' as Cavendish calls it, was employed, it was first soaked in salt water. One of the knobs was then placed in connexion with the negative side of a large Leyden battery charged with electricity, and the other knob with the positive side. Whilst the battery was thus discharged through the artificial torpedo by an assistant, Cavendish placed his hands in different positions upon it, and observed the result. In these trials, the battery employed was always charged by transferring to it the electricity of a certain number of jars, which were electrified till the balls of an electrometer stood at a given distance, so that the intensity of the electricity was determined with great exactness. On trial it was found that the artificial torpedo, when charged so as to give a shock in *air*, equal in intensity to that of the living fish, gave a shock 'just perceptible' under water; whilst, if charged so as to give a shock under water, equal to that of the real torpedo, it gave too strong a shock in air. To remedy the disproportion thus observed between the strength of the shock in water, and in air, a second artificial torpedo was constructed of sole-leather soaked in salt water, but otherwise like the wooden one, from which it differed only by being a better conductor of electricity. The event, as Cavendish tells us, answered his expectation; for there was a much smaller difference between the shock in air and the shock in water of the leather torpedo, than between the shock in air and in water of the wooden one. Experiments are then described, which proved that the shock received by dipping the hands into water close to the charged torpedo, is occasioned by electricity which passes from hand to hand through the water, not from arm to arm through the body. Various trials are then recorded which showed that the artificial torpedo closely imitated the natural animal in its electrical deportment. Cavendish then proceeds to investigate the cause of the shock of the torpedo not passing through a sensible space of air. The wooden model was used in these trials. It was found that the shock passed freely along tinfoil laid upon sealing wax, but that if the foil was cut across with a penknife, without being otherwise disturbed, the shock could not traverse the minute space in the divided sheet of metal, a phenomenon exactly in accordance with what Mr. Walsh observed in the case of the living torpedo. The electricity of the wooden model traversed a short chain, especially if stretched, and the phenomena then exhibited, were found to be in conformity with those observed with the torpedo and the gymnotus. A difficulty, however, occurred in reference to the action of the living torpedo, as contrasted with the artificial one, inasmuch as the "real torpedo was never known to force his [shock] through a single interval" or link of a metallic chain; whereas the wooden imitation gave a shock through several links of chain. Cavendish refers the difference in this respect to his not having used a sufficiently *large* battery with the model fish, and shows by direct experiment "that the greater the battery is, the less space of air, or the fewer links of a chain, will a shock of a given strength pass across." The general conclusion to which these observations conducted Cavendish, was, that the peculiar physiological sensation, termed emphatically, "the shock," depends somewhat more upon the *quantity* than upon the *intensity* of the electricity passing through the animal 'shocked;' or perhaps, the conclusion arrived at, may be better expressed by saying, that a *large* quantity of electricity possessing a *low* intensity, will produce as severe a shock, as a *small* quantity of electricity possessing a *high* intensity. In illustration of this proposition it is shown that if a large and a small Leyden battery be so charged as to give shocks of equal severity, the electricity of the smaller battery will be found able to travel by a longer route across air or along a chain. The electricity of the smaller battery has thus a higher *intensity*, than that of the larger battery, but it does not on that account give a shock of greater severity. Accordingly, as the living torpedo can give a severe shock, but cannot make this shock pass "through any sensible space of air," the *quantity* of electricity which it develops "must be extremely great," for when electricity possesses a feeble intensity, it must be sent through the body of the animal 'shocked' in large quantity, otherwise it will not produce a shock. Yet if so large a quantity of electricity as the torpedo certainly develops were suddenly transferred from one side of its body to the other, it could not but possess an intensity "sufficient to make it dart through air to a great distance, unless there was something within it [the torpedo] analogous to a very large battery." Cavendish then contends that there is "room in the fish for a battery of a sufficient size," according to the observations of John Hunter, and concludes with reiterating his statement, that even those phenomena exhibited by the artificial torpedo, which differed most from the actions of the living animal, are by no means repugnant to the supposition that the shock is produced by electricity.

It would be difficult to exaggerate the merits of this beautiful essay, on which, however, my limited space does not allow me to dwell at length. Singularly enough, its aim has been entirely misapprehended by several of its critics. Dr. Charles Hutton and others have referred to Cavendish as having pointed out that animal electricity is *peculiar* in its nature, and different from that evolved by inorganic bodies; whereas his aim from first to last is to insist upon the identity of the electricity of the torpedo and the gymnotus with that of the Leyden jar. Faraday has done full justice to Cavendish's merit in this respect. In truth the Third and Fifteenth series of Faraday's Electrical Researches, (January, 1833 and November, 1838,) form a complete commentary on Cavendish's labours, and on those of his able successors who have experimented on the electrical fishes.

I quote a single passage from the Electrical researches in reference to the torpedo; but the reader who wishes to do justice to Cavendish will consult Faraday's entire papers:—"In concluding this summary of the powers of torpedinal electricity, I cannot refrain from pointing out the enormous absolute quantity of electricity which the animal must put in circulation at each effort. It is doubtful whether any common electrical machine has as yet been able to supply electricity sufficient in a reasonable time to cause true electro-chemical decomposition of water; yet the current from the torpedo has done it. The same high proportion is shown by the magnetic effects. These circumstances indicate that the torpedo has power (in the way probably that Cavendish describes) to continue the evolution for a sensible time, so that its successive discharges rather resemble those of a voltaic arrangement, intermitting in its action, than those of a Leyden apparatus, charged and discharged many times in succession. In reality, however, there is *no philosophical difference* between these two cases."*

I add the remark that since the date of Faraday's experiments on the living gymnotus (1838), an interesting addition has been made to our knowledge concerning the electrical fishes. In December, 1844, Dr. James Stark of Edinburgh discovered an electrical organ like that of the torpedo and gymnotus in the tail of the common skate and other Rays.† Professor Goodsir took up the investigation after Dr. Stark‡ and has deposited in the Anatomical Museum of the University of Edinburgh, a beautiful series of dissections of the electrical organ of the skate. In 1846, Dr. C. Robin, apparently unaware of the observations made in Edinburgh in the previous year, announced his discovery of an electrical organ in the Rays.§ It is still doubtful whether the organ in these fishes is rudimentary or a fully developed and active electrical apparatus. I would direct the attention of our experimental electricians to the problem as one worth their investigation.

Besides his two published papers on Electricity, Cavendish has left behind him some twenty packets of manuscript essays, more or less complete, on Mathematical and Experimental Electricity. These papers are at present in the hands of Sir William Snow Harris, who most kindly sent me an abstract of them, with a commentary of great value on their contents. I regret that I cannot do more in this volume than allude to Sir William Harris's communication. It will, I trust, be made public. Meanwhile, I will only mention that Sir William states that "Cavendish had really anticipated all those great facts in common electricity, which were subsequently made known to the scientific world through the investigations and writings of the celebrated Coulomb and other philosophers, and had also obtained the more immediate results of experiments of a more refined kind instituted in our own day."

Professor William Thomson also,‖ who saw Cavendish's Electrical

* *Experimental Researches in Electricity*, by Michael Faraday, vol. i. p. 101, par. 359.

† *Proceedings of the Royal Society of Edinburgh*, December 2, 1844.

‡ *Proceedings of the Royal Society of Edinburgh*, January 6, 1845.

§ *Ann. des Sciences Natur.* 1846, tome vii. pp. 193—302.

‖ Professor of Natural Philosophy in the University of Glasgow.

MSS. whilst in the possession of Sir William Harris, writes to me concerning them, that although he was not able to do more than glance at them, they appeared to him "to contain descriptions of excessively ingenious experiments leading to important quantitative results, with reference to electricity in equilibrium on bodies of various forms and dimensions."

From the concurrent testimony of two such high authorities as Sir William Snow Harris and Professor W. Thomson, it cannot be doubted that the Electrical MSS. of Cavendish would amply repay publication.

*Experiments to determine the Density of the Earth. Read to the Royal Society June 21st, 1798.**

THE object of this paper will appear from the following brief account of the principles which characterised the method suggested by the Rev. John Michell for the determination of the density of the Earth, and were adopted by Cavendish.†

Michell's apparatus was a delicate torsion balance, consisting of a light wooden arm suspended in a horizontal position by a slender wire 40 inches long, and having a leaden ball about 2 inches in diameter hung at either extremity. Two heavy spherical masses of metal were then brought near to the balls, so that their attractions conspired in drawing the arm aside. The deviation of the arm was observed; and the force necessary to produce a given deviation of the arm being calculated from its time of vibration it was found what portion of the weight of either ball was equal to the attraction of the mass of metal placed near it. From the known weight of the mass of metal; the distance of the centres of the mass, and of the ball; and the ascertained attraction, it is easy to determine the attraction of an equal spherical mass of water upon a particle as heavy as the ball placed on its surface; and from this can be found the attraction of a sphere of water of the same diameter as the earth, upon the ball placed on its surface. Now the attraction of this sphere will have to that of the earth, the same ratio as their densities; and as the attraction of the earth is equal to the weight of the ball, it follows that as the calculated attraction is to the weight of the ball, so is the density of water to the earth's density, which is thus determined.

It is unnecessary to criticise this paper at any length, as the late Francis Baily, Esq. has devoted a large quarto volume to a discussion of all the researches which have been made concerning the density of the earth, including his own careful repetition of Cavendish's observations, recorded in the paper under notice.‡ The following extracts will show what Mr. Baily's estimate of Cavendish's experiments is :—"Mr. Cavendish . . . . . proceeds to describe the apparatus which he had erected, and to explain his mode of operation, which appears to have been conducted with great judgment and accuracy. Yet, notwithstanding the precautions which he had taken, he still met with some anomalies for which he could not satisfactorily account, and which appear to have affected the results rather more than he had anticipated. He made several attempts to elucidate this difficulty; yet, although he had evidently hit upon the probable source of the principal anomalies, he does not appear at that time to have taken any effectual steps to remove it, but deferred his intention of pursuing this subject, as well as some other improvements in his apparatus, to a future period.

"The number of his experiments is very few; yet, with one exception, they are very accordant, and show the diligence and care with which they

* *Phil. Trans.* 1798, p. 469.

† In drawing up this abstract I have been kindly assisted by my friend W. Swan, Esq., F.R.S.E., Teacher of Mathematics, Edinburgh.

‡ Experiments with the Torsion Rod for determining the Mean Density of the Earth, forming Vol. XIV. of the Memoirs of the Royal Astronomical Society, by Francis Baily, Esq. Vice-President of the Society. 1843.

have been made. From 17 sets of experiments (being all that are on record) he deduced 23 results; from the mean of which he computes the Density of the Earth to be equal to 5·45.* Some objection, indeed, may be made to the paucity of his experiments, and to certain parts of his mode of proceeding : . . . . . it is but just, however, here to remark, that Cavendish's object in drawing up the Memoir appears to have been more for the purpose of exhibiting a *specimen* of what he considered to be an excellent method of determining this important inquiry, than of deducing a result that should lay claim to the full confidence of the scientific world."†

From allusions in Cavendish's paper it appears that he intended to repeat his experiments with a view to discover the cause of the anomalies which appeared in his earlier researches. As he never, however, put this intention in practice, Mr. Baily, at the request of the Astronomical Society, undertook the repetition of Cavendish's observations. A previous repetition had been made by F. Reich, Professor of Natural Philosophy in the Academy of Mines at Freiberg in Saxony, an account of which was read before the German Scientific Association, which met at Prague in September, 1837. Mr. Baily remarks on this repetition:—

"Reich's experiments also were (like Cavendish's) too few in number; 57 only having been made, from which 14 results have been deduced; the mean of which makes the Density of the Earth equal to 5·44, almost identical with that of Cavendish."‡

Baily's own repetition of Cavendish's experiments commenced on October 19, 1838, with an apparatus constructed generally like that used by Cavendish, but including many ingenious modifications, calculated to render the apparatus more delicate, and, above all, more accurate in its indications.

In spite, however, of all the care and ingenuity bestowed upon the reconstruction of the apparatus of Mr. Baily, the same anomalous motions of the torsion rod showed themselves which had perplexed Cavendish; and so difficult was the detection of the nature of the disturbing force, that eighteen months of labour (during which nearly 1300 experiments were made) were thrown away, before the chief cause of the anomaly was detected.§ The results of these 1300 experiments, as Mr. Baily observes, "although in many cases very consistent amongst themselves, were, upon the whole, so discordant and unsatisfactory, that no confidence could be placed on the general result, as a correct value of the true object of inquiry."‖ The whole experiments accordingly were rejected.

In January, 1841, a new series of trials was commenced with a modification of the apparatus. Professor J. D. Forbes, who agreed "with Cavendish in opinion that one source, at least, of the anomalies might arise from the *radiation of heat* from the masses, ... suggested the propriety of having the masses *gilt*, and also of procuring a *gilt case* as a cover to the torsion box, for the purpose of preventing the effect of radiation, from whatever source it might arise."¶

This alteration was followed with the happiest effects, "for the results soon convinced me," says Mr. Baily, "that the proper mode had been taken for the removal of the principal source of discordance."** The new

* "Cavendish says 5·48; but there is a singular error in his computation."
† *Memoirs of the Royal Astronomical Society*, vol. xiv. pp. 7 and 8.
‡ *Op. cit.* p. 10.
§ *Mem. Astr. Soc.* vol. xiv. p. 41.
‖ *Op. cit.* p. 41.
¶ *Op. et loc. cit.*
** *Ibid.* p. 42.

experiments were continued till May, 1842, and the final result was, that the Mean Density of the Earth is 5·6604.

It only remains to notice the papers on the Density of the Earth, which have appeared since the publication of Mr. Baily's volume in 1843. In 1847, G. W. Hearn, Esq. read a paper to the Royal Society "On the Cause of the Discrepancies observed by Mr. Baily with the Cavendish Apparatus for determining the Mean Density of the Earth." The object of this communication was to draw attention to the probability of the anomalous vibrations of the torsion-rod, being in part occasioned by the magnetic or diamagnetic condition of the masses.* In 1849, Prof. J. D. Forbes re-directed attention to a method of determining the density of the earth suggested by Prof. Robison, by taking advantage of the high tide which rises in the Bay of Fundy, Nova Scotia. "The object was to determine the earth's density by the attraction of the tide-wave on a plummet or spirit-level, on the same principle as Maskelyne's experiment on Schiehallien, but with the superior advantages arising from the perfect homogeneity of the attracting mass, and from the circumstance that all the observations might be made at a single station. The experiment might, in short, appear to unite the advantages both of Maskelyne's and Cavendish's methods of determining the earth's density." Prof. Forbes has made the calculation approximately for an assumed height of the tide-wave. Robison reports the water in Fundy Bay to rise 100 feet at spring tide. Professor Forbes accordingly has "calculated the horizontal attraction of a semicylinder of water 100 feet thick, and of about two, four, and eight miles radius upon a point at the extremity of the axis of such a semicylinder." The influence, however, of a tide-wave 100 feet thick, with a radius of 40,000 feet, upon a plumb-line, would produce a deviation of only $0''·53$ (fifty-three hundredths of a second). "Even the greatest of these calculated deviations affords no ground for hoping that the method of Robison could be applied with any success to determine the earth's density."†

It is not a little curious that Robison had been anticipated by Cavendish in suggesting this method of procedure. Among the Cavendish MSS. is a parcel in Cavendish's handwriting, entitled "Attraction," and containing a variety of packets of papers on various matters connected with the estimation of the earth's density. One of these packets is entitled, "Paper given to Maskelyne relating to Attraction and Form of Earth. No. 6." In this paper Cavendish calculates the deviation which the tide in the Bristol Channel would occasion on a plumb-line, on the supposition that the Channel is ten miles broad, and that the tide rises fifty feet; and comes to the conclusion that the mass of water "would make the plumb-line deviate $1\frac{3}{4}$ seconds, if the mean density of the earth is the same as that of the surface." (P. 9.) It further appears that Boscovich has preceded Cavendish; for the latter, in another part of the paper, says, "Since I saw you [Maskelyne], I have looked again into Boscovich's book ('De Littoraria Expeditione, &c.'), and find that . . . . . he supposes the arm of the sea to be 100 miles broad, in which case he says the plumb-line will deviate $2''\ 38'''$." (P. 10.) From these passages it will be seen that Boscovich, Cavendish, and Maskelyne were aware of the method generally believed to have been suggested by Robison. I do not know the exact date of the communication of Cavendish to Maskelyne, but a letter

* See Report in *Athenæum*, March 27th, 1847.

† The extracts in the text are taken from Prof. Forbes's interesting *Note regarding an experiment suggested by Professor Robison.* (Proceed. of R.S.E., vol. ii. (1849) p. 244.)

from the latter to the former remains among the Cavendish MSS., dated January 5, 1783. It refers to Cavendish's "Rules and directions for the choice of hills having a considerable attraction," which he had furnished to Maskelyne. Cavendish was a member of the "Committee of Attraction," appointed by the Royal Society to assist Maskelyne in his search for a mountain suitable for his experiment, and the account quoted must have been given before June, 1774, when he began his observations on Schehallien.* Robison does not explicitly claim the method described by him as his own device. Perhaps, however, he devised the process for himself; at all events, the suggestion of Fundy Bay as specially suitable for trying the supposed experiment was his, and so was the application of a syphon to indicate the attraction.

The remaining papers have been referred to in the Personal Narrative. I will only further remark concerning them, that they are all important. That on the *Height of a Luminous Arch*† has been commented on by Dalton.‡ The paper on the *Civil Year of the Hindoos*§ should be read in connexion with a work of high authority, with the loan of which I have been favoured by James Dalmahoy, Esq. It is entitled "*Kala Sankalita,* a collection of memoirs on the various modes according to which the nations of the southern parts of India divide time, &c. By Lieutenant-Colonel John Warren. Madras, 1825."

Cavendish's latest published paper, that, namely, on the *Division of Astronomical Instruments,*‖ is commented on in the "Encyclopædia Brittanica," art. *Graduation.* Finally, I may notice that among the Cavendish MSS. I have found a paper on the Density of the Atmosphere of the Earth and of Jupiter. In this Cavendish supposes the air to consist of "particles disposed as in the angular points of cubes." He then shows, that if the density of the air be diminished beyond a certain limit, depending on the weight of particles of air, its elasticity will not be sufficient to support the weight of the next row of particles. The necessary consequence of this hypothesis is, that the atmosphere has a definite limit; and this conclusion seems evidently to anticipate Wollaston's speculations on the same subject.¶

* Weld's *History of the Royal Society*, vol. ii. pp. 79—80.
† *Phil. Trans.* 1790, pp. 101 and 105.
‡ *Meteorological Essays*, 2nd. ed., p. 146.
§ *Phil. Trans.* 1792, pp. 383—399.
‖ *Ibid.* 1809, pp. 221—231.
¶ *Ibid.* 1822, p. 89, and *Trans. R.S.E.*, vol. xvi. part 1, p. 79.

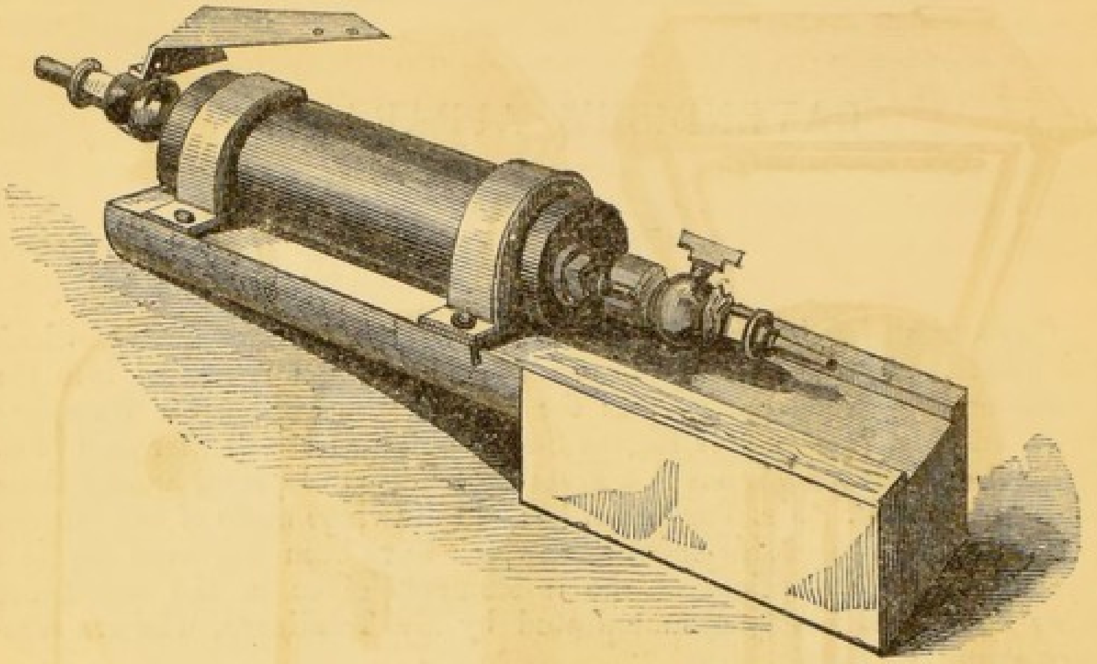
CAVENDISH'S APPARATUS.

Cavendish left behind him an immense amount of apparatus, which was inherited by Lord George Cavendish. From him it passed into the possession of various parties, but the Earl of Burlington, Sir Humphry Davy, and Mr. Newman of Regent Street, obtained the greater part of it. By them or their heirs it has to a great extent been dispersed among those likely to value it. Mr. Tomlinson has had drawings made of two curious specimens of Cavendish's apparatus, which have been engraved for this volume. The descriptions of the instruments given in the succeeding pages have been kindly communicated by Mr. Tomlinson, who has carefully examined them.

I have been unable to discover any reference to the brass Eudiometer in Cavendish's published or unpublished writings, but it seems probable that it was employed in the course of the investigations which led to the discovery of the composition of water and of nitric acid. I have applied to all the parties known to me who possess Cavendish's apparatus, with a view to learn whether the glass globe-eudiometers which he describes in his paper of 1784 (ante, pp. 42, 43), are still in existence. As yet, I have discovered no traces of these vessels. The Earl of Burlington, however, writes to me, that he thinks it possible that relics of them may be found among a collection of Cavendish's apparatus, which is at present, from circumstances, inaccessible. Lord Burlington has kindly engaged to send the relics of the Water-Eudiometers, should he discover them, to the Museum of the Royal Society, in which they would doubtless find an appropriate place beside Newton's Telescope and Davy's Safety-lamp.

Cavendish seems to have taken greater interest in thermometers than in any other instruments applicable to the extension of physical science. His house at Clapham Common was crowded with them. The late Professor Daniell possessed two, which were provided with wooden scales, on which the degrees had been marked by Cavendish himself. In the collection of apparatus belonging to the Natural Philosophy Chair of Edinburgh, is a singular instrument of Cavendish's, presented to Professor J. D. Forbes by Dr. Davy. It may be called a Balance Thermometer. Its construction is such that a glass thermometer turns upon an axis, and moves an index, according as the expansion or contraction of the included quicksilver renders heavier either extremity of the balanced tube.

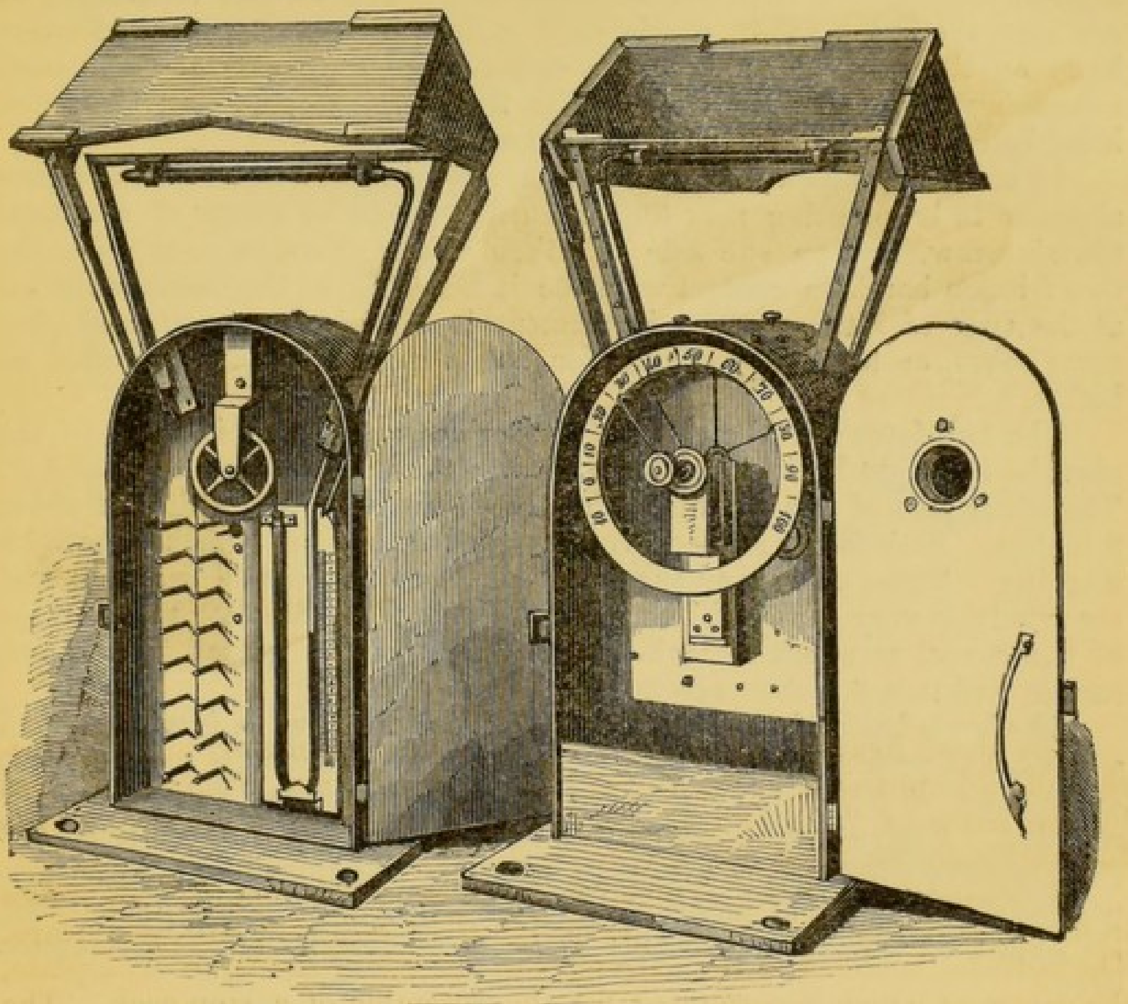
METALLIC EUDIOMETER.



The above figure is copied from an instrument belonging to the collection of old apparatus in the Royal Institution of Great Britain. It goes by the name of "Cavendish's Eudiometer," and was presented to the Institution by Sir Humphry Davy.

It consists of a brass cylinder 6 inches in length, and about 2 inches in diameter, fastened down to a piece of deal by means of two iron straps. The cylinder is closed at the extremity to the left of the figure by a plate, the centre of which is perforated for the reception of a stop-cock. The cylinder appears to have been exhausted of air through this stop-cock, and then to have been connected with a gas-bag or gasometer for the purpose of filling it after every explosion had created a partial vacuum within it. The handle of the stop-cock is furnished with a bent piece of metal, added probably for the sake of increasing the leverage, so as to give the operator perfect command over the stop-cock. The extremity of the cylinder to the right of the figure is furnished with a shallow cap, in the centre of which is a stop-cock furnished with a drawn-out jet. This stop-cock served the purpose of drawing off the water resulting from the combustion of the mixed gases; and the jet may have been added for the purpose of effecting the combination by means of fire. There is, however, a special contrivance for firing the gases by electricity. By the side of the opening for the stop-cock is another opening, into which is screwed a perforated nut, containing a short piece of thermometer tube, the bore of which is filled with a wire passing into the cylinder.

REGISTER THERMOMETER.



The above figures represent a front and back view of Cavendish's Register Thermometer. This instrument was presented by Sir Humphry Davy to Professor Brande, and is included in the collection of old apparatus in the Royal Institution.

This instrument consists essentially of a large glass tube containing alcohol, the expansion and contraction of which acts upon mercury contained in the recurved or inverted syphon-termination of the tube. A considerable portion of the alcohol tube is exposed to the atmosphere, but it is sheltered from the rain by a roof-like cover. The only opening in this tube is at the top of the left-hand limb of the syphon. The surface of the mercury here carries an ivory float, from the top of which proceeds a silken line, and this, passing twice round the periphery of a wheel grooved for the purpose, falls down and hangs loosely, with a small balance weight at its extremity.

The register is performed in the following manner: The axis of the wheel which carries the cord carries also a light index hand: this hand moves in a vertical plane some distance behind the graduated circle; but near the top of this hand projects a short horizontal piece carrying a vertical needle which in the right hand figure is now pointing at 50°. On either side of this index is a friction needle, which accompanies the index to the extreme limit of its range, and stops there. In the figure we may suppose the index hand to have advanced to nearly 80° on one side, and, having pushed the friction hand thus far, the alcohol began to contract, the index to recede, thus leaving the friction hand to record the highest temperature that had been attained. The alcohol continuing to contract, the index would recede until it came in contact with the friction hand on the other side: here the extreme limit appears to have been something below 40°; the temperature then beginning to rise, the index hand had reached 50° at the time of observation.

In order to make a new observation, a bent lever (which in the figure appears to be pointing near 30°) is turned round by means of a central thumb screw, first on one side and then on the other, so as to place the two friction hands in contact with the index hand at the point indicated at the time of setting. The instrument is then left to itself, and after some hours, or next day, the friction hands will be found separated as before.

The two faces are glazed with plate glass, and a hole is made in the glass in the right hand figure for allowing the thumb-screw to pass out. The lower part of this glass is covered with tin-foil to the height of about 2 inches. Both faces are moreover provided with doors, which close with a spring. These doors, and the outer case, are of sheet and bar iron, and the whole is very heavy. The whole height, from the base to the ridge of the cover, is about 18 inches; the height of the glazed part is $11\frac{1}{2}$ inches, and it is 6 inches across.

The brass back of the instrument (left hand figure) is furnished with a number of projecting brass pegs: the top row consists of 4 pegs, the two outer ones of which have sharp points; all the others being blunt or rounded. I have not been able to discover the object of those pegs, but Professor Brande informs me, that when the instrument came first into possession a number of pieces of bibulous paper were stuck upon them, the object of which was probably to keep the interior of the instrument dry.

The instrument is now quite out of order: the alcohol appears to have oozed out, probably by capillary action; the glass is also in some places corroded by the mercury, as we often see in old instruments in which impure mercury has been used.

FINIS.

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