THE

NATIONAL LIBRARY.

No. X.

THE

HISTORY OF CHEMISTRY.

BY

THOMAS THOMSON, M.D.

F.R.S. L. & E.; F.L.S.; M.W.S., &c.

REGIUS PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF GLASGOW.

LONDON:
HENRY COLBURN AND RICHARD BENTLEY,
NEW BURLINGTON STREET;
BELL AND BRADFUTE, EDINBURGH;
AND
CUMMING, DUBLIN.

THE

NATIONAL LIBRARY.

No. X.

THE

HISTORY OF CHEMISTRY.

BY

THOMAS THOMSON, M.D.

F.R.S. L. & E.; F.L.S.; F.G.S., &c.

REGIUS PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF GLASGOW.

LONDON:

HENRY COLBURN AND RICHARD BENTLEY.

NEW BURLINGTON STREET;

BELL AND BRADFUTE, EDINBURGH;

AND

CUMMING, DUBLIN.

1831.

THE

HISTORY

OF

CHEMISTRY.

BY

THOMAS THOMSON, M.D.

F.R.S. L. & E.; F.L.S.; F.G.S., &c.

REGIUS PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF GLASGOW.

IN TWO VOLUMES.

VOL. II.

LONDON:
HENRY COLBURN AND RICHARD BENTLEY,
NEW BURLINGTON STREET.

1831.

Reproduced by
DUOPAGE PROCESS
in the
U.S. of America

Micro Photo Division
Bell & Howell Company
Cleveland 12, Ohio

145

C. WHITING, BEAUFORT HOUSE, STRAND.

CONTENTS

OF

THE SECOND VOLUME.

CHAPTER I.

CHAPTER II.

CHAPTER III.

CHAPTER IV.

CHAPTER V.

CHAPTER VI.

HISTORY OF CHEMISTRY.

CHAPTER I.

OF THE FOUNDATION AND PROGRESS OF SCIENTIFIC CHEMISTRY IN GREAT BRITAIN.

WHILE Mr. Cavendish was extending the bounds of pneumatic chemistry, with the caution and precision of a Newton, Dr. Priestley, who had entered on the same career, was proceeding with a degree of rapidity quite unexampled; while from his happy talents and inventive faculties, he contributed no less essentially to the progress of the science, and certainly more than any other British chemist to its popularity.

Joseph Priestley was born in 1733, at Fieldhead, about six miles from Leeds in Yorkshire. His father, Jonas Priestley, was a maker and dresser of woollen cloth, and his mother, the only child of Joseph Swift a farmer in the neighbourhood. Dr. Priestley was the eldest child; and, his mother having children very fast, he was soon committed to the care of his maternal grandfather. He lost his mother when he was only six years of age, and was soon after taken home by his father and sent to school in the neighbourhood. His father being but poor, and encumbered with a large family, his sister, Mrs. Keighley, a woman in good circumstances, and without children, relieved him of all care of his eldest son, by taking him and bringing him up as her own. She was a dissenter, and her house was the resort of all the dissenting clergy in the country. Young Joseph was sent to a public school in the neighbourhood, and, at sixteen, had made considerable progress in Latin, Greek, and Hebrew. Having shown a passion for books and for learning at a very early age, his aunt conceived hopes that he would one day become a dissenting clergyman, which she considered as the first of all professions; and he entered eagerly into her views: but his health declining about this period, and something like phthisical symptoms having come on, he was advised to turn his thoughts to trade, and to settle as a merchant in Lisbon. This induced him to apply to the modern languages; and he learned French, Italian, and German, without a master. Recovering his health, he abandoned his new scheme and resumed his former plan of becoming a clergyman. In 1752 he was sent to the academy of Daventry, to study under Dr. Ashworth, the successor of Dr. Doddridge. He had already made some progress in mechanical philosophy and metaphysics, and dipped into Chaldee, Syriac, and Arabic. At Daventry he spent three years, engaged keenly in studies connected with divinity, and wrote some of his earliest theological tracts. Freedom of discussion was admitted to its full extent in this academy. The two masters espoused different sides upon most controversial subjects, and the scholars were divided into two parties, nearly equally balanced. The discussions, however, were conducted with perfect good humour on both sides; and Dr. Priestley, as he tells us himself, usually supported the heterodox opinion; but he never at any time, as he assures us, advanced arguments which he did not believe to be good, or supported an opinion which he did not consider as true. When he left the academy, he settled at Needham in Suffolk, as an assistant in a small obscure dissenting meeting-house, where his income never exceeded 30*l.* a-year. His hearers fell off, in consequence of their dislike of his theological opinions; and his income underwent a corresponding diminution. He attempted a school; but his scheme failed of success, owing to the bad opinion which his neighbours entertained of his orthodoxy. His situation would have been desperate, had he not been occasionally relieved by sums out of charitable funds, procured by means of Dr. Benson, and Dr. Kippis.

Several vacancies occurred in his vicinity; but he was treated with contempt, and thought unworthy to fill any of them. Even the dissenting clergy in the neighbourhood thought it a degradation to associate with him, and durst not ask him to preach: not from any dislike to his theological opinions; for several of them thought as freely as he did; but because the genteeler part of their audience always absented themselves when he appeared in the pulpit. A good many years afterwards, as he informs us himself, when his reputation was very high, he preached in the same place, and multitudes flocked to hear the very same sermons, which they had formerly listened to with contempt and dislike.

His friends being aware of the disagreeable nature of his situation at Needham, were upon the alert to procure him a better. In 1758, in consequence o the interest of Mr. Gill, he was invited to appear as a candidate for a meeting-house in Sheffield, vacant by the resignation of Mr. Wadsworth. He appeared accordingly and preached, but was not approved of. Mr. Haynes, the other minister, offered to procure him a meeting-house at Nantwich in Cheshire. This situation he accepted, and, to save expenses, he went from Needham to London by sea. At Nantwich he continued three years, and spent his time much more agreeably than he had done at Needham. His opinions were not obnoxious to his hearers, and controversial discussions were never introduced. Here he established a school, and found the business of teaching, contrary to his expectation, an agreeable and even interesting employment. He taught from seven in the morning, till four in the afternoon; and after the school was dismissed, he went to the house of Mr. Tomlinson, an eminent attorney in the neighbourhood, where he taught privately till seven in the evening. Being thus engaged twelve hours every day in teaching, he had little time for private study. It is, indeed, scarcely conceivable how, under such circumstances, he could prepare himself for Sunday. Here, however, his circumstances began to mend. At Needham it required the utmost economy to keep out of debt; but at Nantwich, he was able to purchase a few books and some philosophical instruments, as a small air-pump, an electrical machine, &c. These he taught his eldest scholars to keep in order and manage: and by entertaining their parents and friends with experiments, in which the scholars were generally the operators, and sometimes the lecturers too, he considerably extended the reputation of his school. It was at Nantwich that he wrote his grammar for the use of his school, a book of considerable merit, though its circulation was never extensive. This latter circumstance was probably owing to the superior reputation of Dr. Lowth, who published his well-known grammar about two years afterwards.

Being boarded in the house of Mr. Eddowes, a very sociable and sensible man, and a lover of music, Dr. Priestley was induced to play a little on the English flute; and though he never was a proficient, he informs us that it contributed more or less to his amusement for many years. He recommends the knowledge and practice of music to all studious persons, and thinks it rather an advantage for them if they have no fine ear or exquisite taste, as they will, in consequence, be more easily pleased, and less apt to be offended when the performances they hear are but indifferent.

The academy at Warrington was instituted while Dr. Priestley was at Needham, and he was recommended by Mr. Clark, Dr. Benson, and Dr. Taylor, as tutor in the languages; but Dr. Aiken, whose qualifications were considered as superior, was preferred before him. However, on the death of Dr. Taylor, and the advancement of Dr. Aiken to be tutor in divinity, he was invited to succeed him: this offer he accepted, though his school at Nantwich was likely to be more gainful; for the employment at Warrington was more liberal and less painful. In this situation he continued six years, actively employed in teaching and in literary pursuits. Here he wrote a variety of works, particularly his History of Electricity, which first brought him into notice as an experimental philosopher, and procured him celebrity. After the publication of this work, Dr. Percival of Manchester, then a student at Edinburgh, procured him the title of doctor in laws, from that university. Here he married a daughter of Mr. Isaac Wilkinson, an ironmonger in Wales; a woman whose qualities he has highly extolled, and who died after he went to America.

In the academy he spent his time very happily, but it did not flourish. A quarrel had broken out between Dr. Taylor and the trustees, in consequence of which all the friends of that gentleman were hostile to the institution. This, together with the smallness of his income, 100*l.* a-year, and 15*l.* for each boarder, which precluded him from making any provision for his family, induced him to accept an invitation to take charge of Millhill chapel, at Leeds, where he had a considerable acquaintance, and to which he removed in 1767.

Here he engaged keenly in the study of theology, and produced a great number of works, many of them controversial. Here, too, he commenced his great chemical career, and published his first tract on *air*. He was led accidentally to think of pneumatic chemistry, by living in the immediate vicinity of a brewery. Here, too, he published his history of the Discoveries relative to Light and Colours, as the first part of a general history of experimental philosophy; but the expense of this book was so great, and its sale so limited, that he did not venture to prosecute the undertaking. Here, likewise, he commenced and published three volumes of a periodical work, entitled "The Theological Repository," which he continued after he settled in Birmingham.

After he had been six years at Leeds, the Earl of Shelburne (afterwards Marquis of Lansdowne), engaged him, on the recommendation of Dr. Price, to live with him as a kind of librarian and literary companion, at a salary of 250*l.* a-year, with a house. With his lordship he travelled through Holland, France, and a part of Germany, and spent some time in Paris. He was delighted with this excursion, and expressed himself thoroughly convinced of the great advantages to be derived from foreign travel. The men of science and politicians in Paris were unbelievers, and even professed atheists, and as Dr. Priestley chose to appear before them as a Christian, they told him that he was the first person they had met with, of whose understanding they had any opinion, who was a believer of Christianity; but, upon interrogating them closely, he found that none of them had any knowledge either of the nature or principles of the Christian religion.—While with Lord Shelburne, he published the first three volumes of his Experiments on Air, and had collected materials for a fourth, which he published soon after settling in Birmingham. At this time also he published his attack upon Drs. Reid, Beattie, and Oswald; a book which, he tells us, he finished in a fortnight: but of which he afterwards, in some measure, disapproved. Indeed, it was impossible for any person of candour to approve of the style of that work, and the way in which he treated Dr. Reid, a philosopher certainly much more deeply skilled than himself in metaphysics.

After some years Lord Shelburne began to be weary of his associate, and, on his expressing a wish to settle him in Ireland, Dr. Priestley of his own accord proposed a separation, to which his lordship consented, after settling on him an annuity of 150*l.*, according to a previous stipulation. This annuity he continued regularly to pay during the remainder of the life of Dr. Priestley.

His income being much diminished by his separation from Lord Shelburne, and his family increasing, he found it now difficult to support himself. At this time Mrs. Rayner made him very considerable presents, particularly at one period a sum of 400*l.*; and she continued her contributions to him almost annually. Dr. Fothergill had proposed a subscription, in order that he might prosecute his experiments to their utmost extent, and be enabled to live without sacrificing his time to his pupils. This he accepted. It amounted at first to 40*l.* per annum, and was afterwards much increased. Dr. Watson, Mr Wedgewood, Mr. Galton, and four or five more, were the gentlemen who joined with Dr. Fothergill in this generous subscription.

Soon after, he settled in a meeting-house in Birmingham, and continued for several years engaged in theological and chemical investigations. His apparatus, by the liberality of his friends, had become excellent, and his income was so good that he could prosecute his researches to their full extent. Here he published the three last volumes of his Experiments on Air, and various papers on the same subject in the Philosophical Transactions. Here, too, he continued his Theological Repository, and published a variety of tracts on his peculiar opinions in religion, and upon the history of the primitive church. He now unluckily engaged in controversy with the established clergy of the place; and expressed his opinions on political subjects with a degree of freedom, which, though it would have been of no consequence at any former period, was ill suited to the peculiar circumstances that were introduced into this country by the French revolution, and to the political maxims of Mr. Pitt and his administration. His answer to Mr. Burke's book on the French revolution excited the violent indignation of that extraordinary man, who inveighed against his character repeatedly, and with peculiar virulence, in the house of commons. The clergy of the church of England, too, who began about this time to be alarmed for their establishment, of which Dr. Priestley was the open enemy, were particularly active; the press teemed with their productions against him, and the minds of their hearers seem to have been artificially excited; indeed some of the anecdotes told of the conduct of the clergy of Birmingham, were highly unbecoming their character. Unfortunately, Dr. Priestley did not seem to be aware of the state of the nation, and of the plan of conduct laid down by Mr. Pitt and his political friends; and he was too fond of controversial discussions to yield tamely to the attacks of his antagonists.

These circumstances seem in some measure to explain the disgraceful riots which took place in Birmingham in 1791, on the day of the anniversary of the French revolution. Dr. Priestley's meeting-house and his dwelling-house were burnt; his library and apparatus destroyed, and many manuscripts, the fruits of several years of industry, were consumed in the conflagration. The houses of several of his friends shared the same fate, and his son narrowly escaped death, by the care of a friend who forcibly concealed him for several days. Dr. Priestley was obliged to make his escape to London, and a seat was taken for him in the mail-coach under a borrowed name. Such was the ferment against him that it was believed he would not have been safe any where else; and his friends would not allow him, for several weeks, to walk through the streets.

He was invited to Hackney, to succeed Dr. Price in the meeting-house of that place. He accepted the office, but such was the dread of his unpopularity, that nobody would let him a house, from an apprehension that it would be burnt by the populace as soon as it was known that he inhabited it. He was obliged to get a friend to take a lease of a house in another name; and it was with the utmost difficulty that he could prevail with the landlord to allow the lease to be transferred to him. The members of the Royal Society, of which he was a fellow, declined admitting him into their company; and he was obliged to withdraw his name from the society.

When we look back upon this treatment of a man of Dr. Priestley's character, after an interval of forty years, it cannot fail to strike us with astonishment; and it must be owned, I think, that it reflects an indelible stain upon that period of the history of Great Britain. To suppose that he was in the least degree formidable to so powerful a body as the church of England, backed as it was by the aristocracy, by the ministry, and by the opinions of the people, is perfectly ridiculous. His theological sentiments, indeed, were very different from those of the established church; but so were those of Milton, Locke, and Newton. Nay, some of the members of the church itself entertained opinions, not indeed so decided or so openly expressed as those of Dr. Priestley, but certainly having the same tendency. To be satisfied of this it is only necessary to recollect the book which Dr. Clarke published on the Trinity. Nay, some of the bishops, unless they are very much belied, entertained opinions similar to those of Dr. Clarke. The same observation applies to Dr. Lardner, Dr. Price, and many others of the dissenters. Yet, the church of England never attempted to persecute these respectable and meritorious men, nor did they consider their opinions as at all likely to endanger the stability of the church. Besides, Dr. Horsley had taken up the pen against Dr. Priestley's theological opinions, and had refuted them so completely in the opinion of the members of the church, that it was thought right to reward his meritorious services by a bishopric.

It could hardly, therefore, be the dread of Dr. Priestley's theological opinions that induced the clergy of the church of England to bestir themselves against him with such alacrity. Erroneous opinions advanced and refuted, so far from being injurious, have a powerful tendency to support and strengthen the cause which they were meant to overturn. Or, if there existed any latent suspicion that the refutation of Horsley was not so complete as had been alleged, surely persecution was not the best means of supporting weak arguments; and indeed it was rather calculated to draw the attention of mankind to the theological opinions of Priestley; as has in fact been the consequence.

Neither can the persecutions which Dr. Priestley was subjected to be accounted for by his political opinions, even supposing it not to be true, that in a free country like Great Britain, any man is at liberty to maintain whatever theoretic opinions of government he thinks proper, provided he be a peaceable subject and obey rigorously all the laws of his country.

Dr. Priestley was an advocate for the perfectibility of the human species, or at least its continually increasing tendency to improvement—a doctrine extremely pleasing in itself, and warmly supported by Franklin and Price; but which the wild principles of Condorcet, Godwin, and Beddoes at last brought into discredit. This doctrine was taught by Priestley in the outset of his Treatise on Civil Government, first published in 1768. It is a speculation of so very agreeable a nature, so congenial to our warmest wishes, and so flattering to the prejudices of humanity, that one feels much pain at being obliged to give it up. Perhaps it may be true, and I am willing to hope so, that improvements once made are never entirely lost, unless they are superseded by something much more advantageous, and that therefore the knowledge of the human race, upon the whole, is progressive. But political establishments, at least if we are to judge from the past history of mankind, have their uniform periods of progress and decay. Nations seem incapable of profiting by experience. Every nation seems destined to run the same career, and the history may be comprehended under the following heads: Poverty, liberty, industry, wealth, power, dissipation, anarchy, destruction. We have no example in history of a nation running through this career and again recovering its energy and importance. Greece ran through it more than two thousand years ago: she has been in a state of slavery ever since. An opportunity is now at last given her of recovering her importance: posterity will ascertain whether she wil lembrace it.

Dr. Priestley's short Essay on the First Principles of Civil Government was published in 1768. In it he lays down as the foundation of his reasoning, that "it must be understood, whether it be expressed or not, that all people live in society for their mutual advantage; so that the good and happiness of the members, that is the majority of the members of any state, is the great standard by which every thing relating to that state must be finally determined; and though it may be supposed that a body of people may be bound by a voluntary resignation of all their rights to a single person or to a few, it can never be supposed that the resignation is obligatory on their posterity, because it is manifestly contrary to the good of the whole that it should be so." From this first principle he deduces all his political maxims. Kings, senators, and nobles, are merely the servants of the public; and when they abuse their power, in the people lies the right of deposing and consequently of punishing them. He examines the expediency of hereditary sovereignty, of hereditary rank and privileges, of the duration of parliament, and of the right of voting, with an evident tendency to democratical principles, though he does not express himself very clearly on the subject.

Such were his political principles in 1768, when his book was published. They excited no alarm and drew but little attention; these principles he maintained ever after, or indeed he may be said to have become more moderate instead of violent. Though he approved of a republic in the abstract; yet, considering the prejudices and habits of the people of Great Britain, he laid it down as a principle that their present form of government was best suited to them. He thought, however, that there should be a reform in parliament; and that parliaments should be triennial instead of septennial. He was an enemy to all violent reforms, and thought that the change ought to be brought about gradually and peaceably. When the French revolution broke out he took the side of the patriots, as he had done during the American war; and he wrote a refutation of Mr. Burke's extraordinary performance. Being a dissenter, it is needless to say that he was an advocate for complete religious freedom. He was ever hostile to all religious establishments, and an open enemy to the church of England.

How far these opinions were just and right this is not the place to inquire; but that they were perfectly harmless, and that many other persons in this country during the last century, and even at present, have adopted similar opinions without incurring any odium whatever, and without exciting the jealousy or even the attention of government, is well known to every person. It comes then to be a question of some curiosity at least, to what we are to ascribe the violent persecutions raised against Dr. Priestley. It seems to have been owing chiefly to the alarm caught by the clergy of the established church that their establishment was in danger;—and, considering the ferment excited soon after the breaking out of the French revolution, and the rage for reform, which pervaded all ranks, the almost general alarm of the aristocracy, at least, was not entirely without foundation. I cannot, however, admit that there was occasion for the violent alarm caught by Mr. Pitt and his political friends, and for the very despotic measures which they adopted in consequence. The disease would probably have subsided of itself, or it would have been cured by a much gentler treatment. As Dr. Priestley was an open enemy to the establishment, its clergy naturally conceived a prejudice against him, and this prejudice was violently inflamed by the danger to which they thought themselves exposed; their influence with the ministry was very great, and Mr. Pitt and his friends naturally caught their prejudices and opinions. Mr. Burke, too, who had changed his political principles, and who was inflamed with the burning zeal which distinguishes all converts, was provoked at Dr. Priestley's answer to his book on the French revolution, and took every opportunity to inveigh against him in the house of commons. The conduct of the French, likewise, who made Dr. Priestley a citizen of France, and chose him a member of their assembly, though intended as a compliment, was injurious to him in Great Britain. It was laid hold of by his antagonists to convince the people that he was an enemy to his country; that he had abjured his rights as an Englishman; and that he had adopted the principles of the hereditary enemies of Great Britain. These causes, and not his political opinions, appear to me to account for the persecution which was raised against him.

His sons, disgusted with this persecution of their father, had renounced their native country and gone over to France; and, on the breaking out of the war between this country and the French republic, they emigrated to America. It was this circumstance, joined to the state of insulation in which he lived, that induced Dr. Priestley, after much consideration, to form the resolution of following his sons and emigrating to America. He published his reasons in the preface to a Fast-day Sermon, printed in 1794, one of the gravest and most forcible pieces of composition I have ever read. He left England in April, 1795, and reached New York in June. In America he was received with much respect by persons of all ranks; and was immediately offered the situation of professor of chemistry in the College of Philadelphia; which, however, he declined, as his circumstances, by the liberality of his friends in England, continued independent. He settled, finally, in Northumberland, about 130 miles from Philadelphia, where he built a house, and re-established his library and laboratory, as well as circumstances permitted. Here he published a considerable number of chemical papers, some of them under the form of pamphlets, and the rest in the American Transactions, the New York Medical Repository, and Nicholson's Journal of Natural Philosophy and Chemistry. Here, also, he continued keenly engaged in theological pursuits; and published, or republished, a great variety of books on theological subjects. Here he lost his wife and his youngest and favourite son, who, he had flattered himself, was to succeed him in his literary career:—and here he died, in 1804, after having been confined only two days to bed, and but a few hours after having arranged his literary concerns, inspected some proof-sheets of his last theological work, and given instructions to his son how it should be printed.

During the latter end of the presidency of Mr. Adams, the same kind of odium which had banished Dr. Priestley from England began to prevail in America. He was threatened with being sent out of the country as an alien. Notwithstanding this, he declined being naturalized; resolving, as he said, to die as he had lived, an Englishman. When his friend Mr. Jefferson, whose political opinions coincided with his own, became president, the odium against him wore off, and he became as much respected as ever.

As to the character of Dr. Priestley, it is so well marked by his life and writings, that it is difficult to conceive how it could have been mistaken by many eminent men in this kingdom. Industry was his great characteristic; and this quality, together with a facility of composition, acquired, as he tells us, by a constant habit while young of drawing out an abstract of the sermons which he had preached, and writing a good deal in verse, enabled him to do so much: yet, he informs us that he never was an intense student, and that his evenings were usually passed in amusement or company. He was an early riser, and always lighted his own fire before any one else was stirring: it was then that he composed all his works. It is obvious, from merely glancing into his books, that he was precipitate; and indeed, from the way he went on thinking as he wrote, and writing only one copy, it was impossible he could be otherwise: but, as he was perfectly sincere and anxious to obtain the truth, he freely acknowledged his mistakes as soon as he became sensible of them. This candour is very visible in his philosophical speculations; but in his theological writings it was not so much to be expected. He was generally engaged in controversy in theology; and his antagonists were often insolent, and almost always angry. We all know the effect of such opposition; and need not be surprised that it operated upon Dr. Priestley, as it would do upon any other man. By all accounts his powers of conversation were very great, and his manners in every respect very agreeable. That this must have been the case is obvious from the great number of his friends, and the zeal and ardour with which they continued to serve him, notwithstanding the obloquy under which he lay, and even the danger that might be incurred by appearing to befriend him. As for his moral character, even his worst enemies have been obliged to allow that it was unexceptionable. Many of my readers will perhaps smile, when I say that he was not only a sincere, but a zealous Christian, and would willingly have died a martyr to the cause. Yet I think the fact is of easy proof; and his conduct through life, and especially at his death, affords irrefragable proofs of it. His tenets, indeed, did not coincide with those of the majority of his countrymen; but though he rejected many of the doctrines, he admitted the whole of the sublime morality and the divine origin of the Christian religion; which may charitably be deemed sufficient to constitute a true Christian. Of vanity he seems to have possessed rather more than a usual share; but perhaps he was deficient in pride.

His writings were exceedingly numerous, and treated of science, theology, metaphysics, and politics. Of his theological, metaphysical, and political writings it is not our business in this work to take any notice. His scientific works treat of *electricity*, *optics*, and *chemistry*. As an electrician he was respectable; as an optician, a compiler; as a chemist, a discoverer. He wrote also a book on perspective which I have never had an opportunity of perusing.

It is to his chemical labours that he is chiefly indebted for the great reputation which he acquired. No man ever entered upon any undertaking with less apparent means of success than Dr. Priestley did on the chemical investigation of *airs*. He was unacquainted with chemistry, excepting that he had, some years before, attended an elementary course delivered by Mr. Turner, of Liverpool. He was not in possession of any apparatus, nor acquainted with the method of making chemical experiments; and his circumstances were such, that he could neither lay out a great deal of money on experiments, nor could he hope, without a great deal of expense, to make any material progress in his investigations. These circumstances, which, at first sight, seem so adverse, were, I believe, of considerable service to him, and contributed very much to his ultimate success. The branch of chemistry which he selected was new: an apparatus was to be invented before any thing of importance could be effected; and, as simplicity is essential in every apparatus, *he* was most likely to contrive the best, whose circumstances obliged him to attend to economical considerations.

Pneumatic chemistry had been begun by Mr. Cavendish in his valuable paper on carbonic acid and hydrogen gases, published in the Philosophical Transactions for 1766. The apparatus which he employed was similar to that used about a century before by Dr. Mayow of Oxford. Dr. Priestley contrived the apparatus still used by chemists in pneumatic investigations; it is greatly superior to that of Mr. Cavendish, and, indeed, as convenient as can be desired. Were we indebted to him for nothing else than this apparatus, it would deservedly give him high consideration as a pneumatic chemist.

His discoveries in pneumatic chemistry are so numerous, that I must satisfy myself with a bare outline; to enumerate every thing, would be to transcribe his three volumes, into which he digested his discoveries. His first paper was published in 1772, and was on the method of impregnating water with carbonic acid gas; the experiments contained in it were the consequence of his residing near a brewery in Leeds. This pamphlet was immediately translated into French; and, at a meeting of the College of Physicians in London, they addressed the Lords of the Treasury, pointing out the advantage that might result from water impregnated with carbonic acid gas in cases of scurvy at sea. His next essay was published in the Philosophical Transactions, and procured him the Copleyan medal. His different volumes on air were published in succession, while he lived with Lord Shelburne, and while he was settled at Birmingham. They drew the attention of all Europe, and raised the reputation of this country to a great height.

The first of his discoveries was *nitrous gas*, now called *deutoxide of azote*, which had, indeed, been formed by Dr. Hales; but that philosopher had not attempted to investigate its properties. Dr. Priestley ascertained its properties with much sagacity, and almost immediately applied it to the analysis of air. It contributed very much to all subsequent investigations in pneumatic chemistry, and may be said to have led to our present knowledge of the constitution of the atmosphere.

The next great discovery was *oxygen gas*, which was made by him on the 1st of August, 1774, by heating the red oxide of mercury, and collecting the gaseous matter given out by it. He almost immediately detected the remarkable property which this gas has of supporting combustion better, and animal life longer, than the same volume of common air; and likewise the property which it has of condensing into red fumes when mixed with nitrous gas. Lavoisier, likewise, laid claim to the discovery of oxygen gas; but his claim is entitled to no attention whatever; as Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house, in Paris, and showed him the method of procuring it in the year 1774, which is a considerable time before the date assigned by Lavoisier for his pretended discovery. Scheele, however, actually obtained this gas without any previous knowledge of what Priestley had done; but the book containing this discovery was not published till three years after Priestley's process had become known to the public.

Dr. Priestley first made known sulphurous acid, fluosilicic acid, muriatic acid, and ammonia in the gaseous form; and pointed out easy methods of procuring them: he describes with exactness the most remarkable properties of each. He likewise pointed out the existence of carburetted hydrogen gas; though he made but few experiments to determine its nature. His discovery of protoxide of azote affords a beautiful example of the advantages resulting from his method of investigation, and the sagacity which enabled him to follow out any remarkable appearances which occurred. Carbonic oxide gas was discovered by him while in America, and it was brought forward by him as an incontrovertible refutation of the antiphlogistic theory.

Though he was not strictly the discoverer of hydrogen gas, yet his experiments on it were highly interesting, and contributed essentially to the revolution which chemistry soon after underwent. Nothing, for example, could be more striking, than the reduction of oxide of iron, and the disappearance of the hydrogen when the oxide is heated sufficiently in contact with hydrogen gas. Azotic gas was known before he began his career; but we are indebted to him for most of the properties of it yet known. To him, also, we owe the knowledge of the fact, that an acid is formed when electric sparks are made to pass for some time through a given bulk of common air;

a fact which led afterwards to Mr. Cavendish's great discovery of the composition of nitric acid.

He first discovered the great increase of bulk which takes place when electric sparks are made to pass through ammoniacal gas—a fact which led Berthollet to the analysis of this gas. He merely repeated Priestley's experiment, determined the augmentation of bulk, and the nature of the gases evolved by the action of the electricity. His experiments on the amelioration of atmospherical air by the vegetation of plants, on the oxygen gas given out by their leaves, and on the respiration of animals, are not less curious and interesting.

Such is a short view of the most material facts for which chemistry is indebted to Dr. Priestley. As a discoverer of new substances, his name must always stand very high in the science; but as a reasoner or theorist his position will not be so favourable. It will be observed that almost all his researches and discoveries related to gaseous bodies. He determined the different processes, by means of which the different gases can be procured, the substances which yield them, and the effects which they are capable of producing on other bodies. Of the other departments of chemistry he could hardly be said to know any thing. As a pneumatic chemist he stands high; as an analytical chemist he can scarcely claim any rank whatever. In his famous experiments on the formation of water by detonating mixtures of oxygen and hydrogen in a copper globe, the copper was found acted upon, and a blue liquid was obtained, the nature of which he was unable to ascertain; but Mr. Keir, whose assistance he solicited, determined it to be a solution of nitrate of copper in water. This formation of nitric acid induced him to deny that water was a compound of oxygen and hydrogen. The same acid was formed in the experiments of Mr. Cavendish; but he investigated the circumstances of the formation, and showed that it depended upon the presence of azotic gas in the gaseous mixture. Whenever azotic gas is present, nitric acid is formed, and the quantity of this acid depends upon the relative proportion of the azotic and hydrogen gases in the mixture. When no hydrogen gas is present, nothing is formed but nitric acid: when no azotic gas is present, nothing is formed but water. These facts, determined by Cavendish, invalidate the reasoning of Priestley altogether; and had he possessed the skill, like Cavendish, to determine with sufficient accuracy the proportions of the different gases in his mixtures, and the relative quantities of nitric acid formed, he would have seen the inaccuracy of his own conclusions.

He was a firm believer in the existence of phlogiston; but he seems, at least ultimately, to have adopted the view of Scheele, and many other eminent contemporary chemists—indeed, the view of Cavendish himself—that hydrogen gas is phlogiston in a separate and pure state. Common air he considered as a compound of oxygen and phlogiston. Oxygen, in his opinion, was air quite free from phlogiston, or air in a simple and pure state; while *azotic gas* (the other constituent of common air) was air saturated with phlogiston. Hence he called oxygen *dephlogisticated*, and azote *phlogisticated air*. The facts that when common air is converted into azotic gas its bulk is diminished about one-fifth part, and that azotic gas is lighter than common air or oxygen gas, though not quite unknown to him, do not seem to have drawn much of his attention. He was not accustomed to use a balance in his experiments, nor to attend much to the alterations which took place in the weight of bodies. Had he done so, most of his theoretical opinions would have fallen to the ground.

When a body is allowed to burn in a given quantity of common air, it is known that the quality of the common air is deteriorated; it becomes, in his language, more phlogisticated. This, in his opinion, was owing to an affinity which existed between phlogiston and air. The presence of air is necessary to combustion, in consequence of the affinity which it has for phlogiston. It draws phlogiston out of the burning body, in order to combine with it. When a given bulk of air is saturated with phlogiston, it is converted into azotic gas, or *phlogisticated air*, as he called it; and this air, having no longer any affinity for phlogiston, can no longer attract that principle, and consequently combustion cannot go on in such air.

All combustible bodies, in his opinion, contain hydrogen. Of course the metals contain it as a constituent. The calces of metals are those bodies deprived of phlogiston. To prove the truth of this opinion, he showed that when the oxide of iron is heated in hydrogen gas, that gas is absorbed, while the calx is reduced to the metallic state. Finery cinder, which he employed in these experiments, is, in his opinion, iron not quite free from phlogiston. Hence it still retains a quantity of hydrogen. To prove this, he mixed together finery cinder and carbonates of lime, barytes and strontian, and exposed the mixture to a strong heat; and by this process obtained inflammable gas in abundance. In his opinion every inflammable gas contains hydrogen in abundance. Hence this experiment was adduced by him as a demonstration that hydrogen is a constituent of finery cinder.

All these processes of reasoning, which appear so plausible as Dr. Priestley states them, vanish into nothing, when his experiments are made, and the weights of every thing determined by means of a balance: it is then established that a burning body becomes heavier during its combustion, and that the surrounding air loses just as much weight as the burning body gains. Scheele and Lavoisier showed clearly that the loss of weight sustained by the air is owing to a quantity of oxygen absorbed from it, and condensed in the burning body. Cruikshank first elucidated the nature of the inflammable gas, produced by the heating a mixture of finery cinder and carbonate of lime, or other earthy carbonate. He found that iron filings would answer better than finery cinder. The gas was found to contain no hydrogen, and to be in fact a compound of oxygen and carbon. It was shown to be derived from the carbonic acid of the earthy carbonate, which was deprived of half its oxygen by the iron filings or finery cinder. Thus altered, it no longer preserved its affinity for the lime, but made its escape in the gaseous form, constituting the gas now known by the name of carbonic oxide.

Though the consequence of the Birmingham riots, which obliged Dr. Priestley to leave England and repair to America, is deeply to be lamented, as fixing an indelible disgrace upon the country; perhaps it was not in reality so injurious to Dr. Priestley as may at first sight appear. He had carried his peculiar researches nearly as far as they could go. To arrange and methodize, and deduce from them the legitimate consequences, required the application of a different branch of chemical science, which he had not cultivated, and which his characteristic rapidity, and the time of life to which he had arrived, would have rendered it almost impossible for him to acquire. In all probability, therefore, had he been allowed to prosecute his researches unmolested, his reputation, instead of an increase, might have suffered a diminution, and he might have lost that eminent situation as a man of science which he had so long occupied.

With Dr. Priestley closes this period of the History of British Chemistry—for Mr. Cavendish, though he had not lost his activity, had abandoned that branch of science, and turned his attention to other pursuits.

CHAPTER II.

OF THE PROGRESS OF PHILOSOPHICAL CHEMISTRY IN SWEDEN.

Though Sweden, partly in consequence of her scanty population, and the consequent limited sale of books in that country, and partly from the propensity of her writers to imitate the French, which has prevented that originality in her poets and historians that is requisite for acquiring much eminence —though Sweden, for these reasons, has never reached a very high rank in literature; yet the case has been very different in science. She has produced men of the very first eminence, and has contributed more than her full share in almost every department of science, and in none has she shone with greater lustre than in the department of Chemistry. Even in the latter part of the seventeenth century, before chemistry had, properly speaking, assumed the rank of a science, we find Hierne in Sweden, whose name deserves to be mentioned with respect. Moreover, in the earlier part of the eighteenth century, Brandt, Scheffer, and Wallerius, had distinguished themselves by their writings. Cronstedt, about the middle of the eighteenth century, may be said to have laid the foundation of systematic mineralogy upon chemical principles, by the publication of his System of Mineralogy. But Bergman is entitled to the merit of being the first person who prosecuted chemistry in Sweden on truly philosophical principles, and raised it to that high estimation to which its importance justly entitles it.

Torbern Bergman was born at Catherinberg, in West Gothland, on the 20th of March, 1735. His father, Barthold Bergman, was receiver of the revenues of that district, and his mother, Sara Hägg, the daughter of a Gotheborg merchant. A receiver of the revenues was at that time, in Sweden, a post both disagreeable and hazardous. The creatures of a party which had had the ascendancy in one diet, they were exposed to the persecution of the diet next following, in which an opposite party usually had the predominance. This circumstance induced Bergman to advise his son to turn his attention to the professions of law or divinity, which were at that time the most lucrative in Sweden. After having spent the usual time at school, and acquired those branches of learning commonly taught in Sweden, in the public schools and academies to which Bergman was sent, he went to the University of Upsala, in the autumn of 1752, where he was placed under the guidance of a relation, whose province it was to superintend his studies, and direct them to those pursuits that were likely to lead young Bergman to wealth and distinction. Our young student showed at once a decided predilection for mathematics, and those branches of physics which were connected with mathematics, or depended upon them. But these were precisely the branches of study which his relation was anxious to prevent his indulging in. Bergman attempted at once to indulge his own inclination, and to gratify the wishes of his relation. This obliged him to study with a degree of ardour and perseverance which has few examples. His mathematical and physical studies claimed the first share of his attention; and, after having made such progress in them as would alone have been sufficient to occupy the whole time of an ordinary student—to satisfy his relation, Jonas Victorin, who was at that time a *magister docens* in Upsala, he thought it requisite to study some law books besides, that he might be able to show that he had not neglected his advice, nor abandoned the views which he had held out.

He was in the habit of rising to his studies every morning at four o'clock, and he never went to bed till eleven at night. The first year of his residence at Upsala, he had made himself master of Wolf's Logic, of Wallerius's System of Chemistry, and of twelve books of Euclid's Elements: for he had already studied the first book of that work in the Gymnasium before he went to college. He likewise perused Keil's Lectures on Astronomy, which at that time were considered as the best introduction to physics and astronomy. His relative disapproved of his mathematical and physical studies altogether; but, not being able to put a stop to them, he interdicted the books, and left his young charge merely the choice between law and divinity. Bergman got a small box made, with a drawer, into which he put his mathematical and physical books, and over this box he piled the law books which his relative had urged him to study. At the time of the daily visits of his relative, the mathematical and physical books were carefully locked up in the drawer, and the law books spread upon the table; but no sooner was his presence removed, than the drawer was opened, and the mathematical studies resumed.

This incessant study; this necessity under which he found himself to consult his own inclinations and those of his relative; this double portion of labour, without time for relaxation, exercise, or amusement, proved at last injurious to young Bergman's health. He fell ill, and was obliged to leave the university and return home to his father's house in a state of bad health. There constant and moderate exercise was prescribed him, as the only means of restoring his health. That his time here might not be altogether lost to him, he formed the plan of making his walks subservient to the study of botany and entomology.

At this time Linnæus, after having surmounted obstacles which would have crushed a man of ordinary energy, was in the height of his glory; and was professor of botany and natural history in the University of Upsala. His lectures were attended by crowds of students from every country in Europe: he was enthusiastically admired and adored by his students. This influence on the minds of his pupils was almost unbounded; and at Upsala, every student was a natural historian. Bergman had studied botany before he went to college, and he had acquired a taste for entomology from the lectures of Linnæus himself. Both of these pursuits he continued to follow after his return home to West Gothland; and he made a collection of plants and of insects. Grasses and mosses were the plants to which he turned the most of his attention, and of which he collected the greatest number. But he felt a predilection for the study of insects, which was a field much less explored than the study of plants.

Among the insects which he collected were several not to be found in the *Fauna Suecica*. Of these he sent specimens to Linnæus at Upsala, who was lelighted with the present. All of them were till then unknown as Swedish insects, and several of them were quite new. The following were the insects at this time collected by Bergman, and sent to Upsala, as they were named by Linnæus:

Phalæna.	Bombyx monacha, camelina.
	Noctua Parthenias, conspicillaris.
	Perspicillaris, flavicornis, Plebeia.
	Geometra pennaria.
	Tortrix Bergmanniana, Lediana.
	Tinea Harrisella, Pedella, Punctella.
Tenthredo.	Vitellina, ustulata.
Ichneumon.	Jaculator niger.
Tipula.	Tremula.

When Bergman's health was re-established, he returned to Upsala with full liberty to prosecute his studies according to his own wishes, and to devote the whole of his time to mathematics, physics, and natural history. His relations, finding it in vain to combat his predilections for these studies, thought it better to allow him to indulge them.

He had made himself known to Linnæus by the collection of insects which he had sent him from Catherinberg; and, drawn along by the glory with which Linnæus was surrounded, and the zeal with which his fellow-students prosecuted such studies, he devoted a great deal of his attention to natural history. The first paper which he wrote upon the subject contained a discovery. There was a substance observed in some ponds not far from Upsala, to which the name of *coccus aquaticus* was given, but its nature was unknown. Linnæus had conjectured that it might be the *ovarium* of some insect; but he left the point to be determined by future observations. Bergman ascertained that it was the ovum of a species of leech, and that it contained from ten to twelve young animals. When he stated what he had ascertained to Linnæus, that great naturalist refused to believe it; but Bergman satisfied him of the truth of his discovery by actual observation. Linnæus, thus satisfied, wrote under the paper of Bergman, *Vidi et obstupui*, and sent it to the academy of Stockholm with this flattering panegyric. It was printed in the Memoirs of that learned body for 1756 (p. 199), and was the first paper of Bergman's that was committed to the press.

He continued to prosecute the study of natural history as an amusement; though mathematics and natural philosophy occupied by far the greatest part of his time. Various useful papers of his, connected with entomology, appeared from time to time in the Memoirs of the Stockholm Academy; in particular, a paper on the history of insects which attack fruit-trees, and on the methods of guarding against their ravages: on the method of classing these insects from the forms of their larvæ, a time when it would be most useful for the agriculturist to know, in order to destroy those that are hurtful: a great number of observations on this class of animals, so various in their shape and their organization, and so important for man to know—some of which he has been able to overcome, while others, defended by their small size, and powerful by their vast numbers, still continue their ravages; and which offer so interesting a sight to the philosopher by their labours, their manners, and their foresight.—Bergman was fond of these pursuits, and looked back upon them in afterlife with pleasure. Long after, he used to mention with much satisfaction, that by the use of the method pointed out by him, no fewer than seven millions of destructive insects were destroyed in a single garden, and during the course of a single summer.

About the year 1757 he was appointed tutor to the only son of Count Adolf Frederick Stackelberg, a situation which he filled greatly to the satisfaction both of the father and son, as long as the young count stood in need of an instructor. He took his master's degree in 1758, choosing for the subject of his thesis on *astronomical interpolation.* Soon after, he was appointed *magister docens* in natural philosophy, a situation peculiar to the University of Upsala, and constituting a kind of assistant to the professor. For his promotion to this situation he was obliged to M. Ferner, who saw how well qualified he was for it, and how beneficial his labours would be to the University of Upsala. In 1761 he was appointed *adjunct* in mathematics and physics, which, I presume, means that he was raised to the rank of an associate with the professor of these branches of science. In this situation it was his business to teach these sciences to the students of Upsala, a task for which he was exceedingly well fitted. During this period he published various tracts on different branches of physical science, particularly on the *rainbow*, the crepuscula, the aurora-borealis, the electrical phenomena of Iceland spar, and of the tourmalin. We find his name among the astronomers who observed the first transit of Venus over the sun, in 1761, whose results deserve the greatest confidence.* His observations on the electricity of the tourmalin are important. It was he that first established the true laws that regulate these curious phenomena.

During the whole of this period he had been silently studying chemistry and mineralogy, though nobody suspected that he was engaged in any such pursuits. But in 1767 John Gottschalk Wallerius, who had long filled the chair of chemistry in the

---

* See Phil. Trans., vol. lii. p. 227, and vol. lvi. p. 85.

University of Upsala, with high reputation, resigned his chair. Bergman immediately offered himself as a candidate for the vacant professorship: and, to show that he was qualified for the office, published two dissertations on the Manufacture of Alum, which probably he had previously drawn up, and had lying by him. Wallerius intended to resign his chair in favour of a pupil or relation of his own, whom he had destined to succeed him. He immediately formed a party to oppose the pretensions of Bergman; and his party was so powerful and so malignant, that few doubted of their success: for it was joined by all those who, despairing of equalling the industry and reputation of Bergman, set themselves to oppose and obstruct his success. Such men unhappily exist in all colleges, and the more eminent a professor is, the more is he exposed to their malignant activity. Many of those who cannot themselves rise to any eminence, derive pleasure from the attempt to pull down the eminent to their own level. In these attempts, however, they seldom succeed, unless from some want of prudence and steadiness in the individual whom they assail. Bergman's Dissertations on Alum were severely handled by Wallerius and his party: and such was the influence of the ex-professor, that every body thought Bergman would be crushed by him.

Fortunately, Gustavus III. of Sweden, at that time crown prince, was chancellor of the university. He took up the cause of Bergman, influenced, it is said, by the recommendation of Von Swab, who pledged himself for his qualifications, and was so keen on the subject that he pleaded his cause in person before the senate. Wallerius and his party were of course baffled, and Bergman got the chair.

For this situation his previous studies had fitted him in a peculiar manner. His mathematical, physical, and natural-historical knowledge, so far from being useless, contributed to free him from prejudices, and to emancipate him from that spirit of routine under which chemistry had hitherto suffered. They gave to his ideas a greater degree of precision, and made his views more correct. He saw that mathematics and chemistry divided between them the whole extent of natural science, and that its bounds required to be enlarged, to enable it to embrace all the different branches of science with which it was naturally connected, or which depended upon it. He saw the necessity of banishing from chemistry all vague hypotheses and explanations, and of establishing the science on the firm basis of experiment. He was equally convinced of the necessity of reforming the nomenclature of chemistry, and of bringing it to the same degree of precision that characterized the language of the other branches of natural philosophy.

His first care, after getting the chair, was to make as complete a collection as he could of mineral substances, and to arrange them in order according to the nature of their constituents, as far as they had been determined by experiment. To another cabinet he assigned the Swedish minerals, ranged in a geographical manner according to the different provinces which furnished them.

When I was at Upsala, in 1812, the first of these collections still remained, greatly augmented by his nephew and successor, Afzelius. But no remains existed of the geographical collection. However, there was a very considerable collection of this kind in the apartments of the Swedish school of mines at Stockholm, under the care of Mr. Hjelm, which I had an opportunity of inspecting. It is not improbable that Bergman's collection might have formed the nucleus of this. A geographical collection of minerals, to be of much utility, should exhibit all the different formations which exist in the kingdom: and in a country so uniform in its nature as Sweden, the minerals of one county are very nearly similar to those of the other counties; with the exception of certain peculiarities derived from the mines, or from some formations which may belong exclusively to certain parts of the country, as, for example, the coal formations in the south corner of Sweden, near Helsinburg, and the porphyry rocks, in Elfsdale.

Bergman attempted also to make a collection of models of the apparatus employed in the different chemical manufactories, to be enabled to explain these manufactures with greater clearness to his students. I was informed by M. Ekeberg, who, in 1812, was *magister docens* in chemistry at Upsala, that these models were never numerous. Nor is it likely that they should be, as Sweden cannot boast of any great number of chemical manufactories, and as, in Bergman's time, the processes followed in most of the chemical manufactories of Europe were kept as secret as possible.

Thus it was Bergman's object to exhibit to his pupils specimens of all the different substances which the earth furnishes, with the order in which these productions are arranged on the globe—to show them the uses made of all these different productions—how practice had preceded theory and had succeeded in solving many chemical problems of the most complicated nature.

His lectures are said to have been particularly valuable. He drew around him a considerable number of pupils, who afterwards figured as chemical discoverers themselves. Of all these Assessor Gahn, of Fahlun, was undoubtedly the most remarkable; but Hjelm, Gadolin, the Elhuyarts, and various other individuals, likewise distinguished themselves as chemists.

After his appointment to the chemical chair at Upsala, the remainder of his life passed with very little variety; his whole time was occupied with his favourite studies, and not a year passed that he did not publish some dissertation or other upon some more or less important branch of chemistry. His reputation gradually extended itself over Europe, and he was enrolled among the number of the members of most scientific academies. Among other honourable testimonies of the esteem in which he was held, he was elected rector of the University of Upsala. This university is not merely a literary body, but owns extensive estates, over which it possesses great authority, and, having considerable control over its students, and enjoying considerable immunities and privileges (conferred in former times as an encouragement to learning, though, in reality, they serve only to cramp its energies, and throw barriers in the way of its progress), constitutes, therefore, a kind of republic in the midst of Sweden: the professors being its chiefs. But while, in literary establishments, all the institutions ought to have for an object to maintain peace, and free their members from every occupation unconnected with letters, the constitution of that university obliges its professors to attend to things very inconsistent with their usual functions; while it gives men of influence and ambition a desire to possess the power and patronage, though they may not be qualified to perform the duties, of a professor. Such temptations are very injurious to the true cause of science; and it were to be wished, that no literary body, in any part of the world, were possessed of such powers and privileges. When Bergman was rector, the university was divided into two great parties, the one consisting of the theological and law faculties, and the other of the scientific professors. Bergman's object was to preserve peace and agreement between these two parties, and to convince them that it was the interest of all to unite for the good of the university and the promotion of letters. The period of his magistracy is remarkable in the annals of the university for the small number of deliberations, and the little business recorded in the registers; and for the good sense and good behaviour of the students. The students in Upsala are numerous, and most of them are young men. They had been accustomed frequently to brave or elude the severity of the regulations; but during Bergman's rectorship they were restrained effectually by their respect for his genius, and their admiration of his character and conduct.

When the reputation of Bergman was at its height, in the year 1776, Frederick the Great of Prussia formed the wish to attach him to the Academy of Sciences of Berlin, and made him offers of such a nature that our professor hesitated for a short time as to whether he ought not to accept them. His health had been injured by the assiduity with which he had devoted himself to the double duty of teaching and experimenting. He might look for an alleviation of his ailments, if not a complete recovery, in the milder climate of Prussia, and he would be able to devote himself entirely to his academical duties; but other considerations prevented him from acceding to this proposal, tempting as it was. The King of Sweden had been his benefactor, and it was intimated to him that his leaving the kingdom would afflict that monarch. This information induced him, without further hesitation, to refuse the proposals of the King of Prussia. He requested of the king, his master, not to make him lose the merit of his sacrifice by augmenting his income; but to this demand the King of Sweden very properly refused to accede.

In the year 1771, Professor Bergman married a widow lady, Margaretha Catharina Trast, daughter of a clergyman in the neighbourhood of Upsala. By her he had two sons; but both of them died when infants. This lady survived her husband. The King of Sweden settled on her an annuity of 200 rix dollars, on condition that she gave up the library and apparatus of her late husband to the Royal Society of Upsala.

Bergman's health had been always delicate; indeed he seems never to have completely recovered the effects of his first year's too intense study at Upsala. He struggled on, however, with his ailments; and, by way of relaxation, was accustomed sometimes, in summer, to repair to the waters of Medevi—a celebrated mineral spring in Sweden, situated near the banks of the great inland lake, Wetter. One of these visits seems to have restored him to health for the time. But his malady returned in 1784 with redoubled violence. He was afflicted with hemorrhoids, and his daily loss of blood amounted to about six ounces. This constant drain soon exhausted him, and on the 8th of July, 1784, he died at the baths of Medevi, to which he had repaired in hopes of again benefiting by these waters.

The different tracts which he published; as they have been enumerated by Hjelm, who gave an interesting account of Bergman to the Stockholm Academy in the year 1785, amount to 106. They have been all collected into six octavo volumes entitled "Opuscula Torberni Bergman Physica et Chemica"—with the exception of his notes on Scheffer, his Sciagraphia, and his chapter on Physical Geography, which was translated into French, and published in the Journal des Mines (vol. iii. No. 15, p. 55). His Sciagraphia, which is an attempt to arrange minerals according to their composition, was translated into English by Dr. Withering. His notes on Scheffer were interspersed in an edition of the "Chemiske Föreläsningar" of that chemist, published in 1774, which he seems to have employed as a text-book in his lectures: or, at all events, the work was published for the use of the students of chemistry at Upsala. There was a new edition of it published, after Bergman's death, in the year 1796, to which are appended Bergman's Tables of Affinities.

The most important of Bergman's chemical papers were collected by himself, and constitute the three first volumes of his Opuscula. The three last volumes of that work were published after his death. The fourth volume was published at Leipsic, in 1787, by Hebenstreit, and contains the rest of his chemical papers. The fifth volume was given to the world in 1788, by the same editor. It contains three chemical papers, and the rest of it is made up with papers on natural history, electricity, and other branches of physics, which Bergman had published in the earlier part of his life. The same indefatigable editor published the sixth volume in 1790. It contains three astronomical papers, two chemical, and a long paper on the means of preventing any injurious effects from lightning. This was an oration, delivered before the Royal Academy of Sciences of Stockholm, in 1764, probably at the time of his admission into the academy.

It would serve little purpose in the present state of chemical knowledge, to give a minute analysis of Bergman's papers. To judge of their value, it would be necessary to compare them, not with our present chemical knowledge, but with the state of the science when his papers were published.

A very short general view of his labours will be sufficient to convey an idea of the benefits which the science derived from them.

1. His first paper, entitled "On the Aerial Acid," that is, *carbonic acid*, was published in 1774. In it he gives the properties of this substance in considerable detail, shows that it possesses acid qualities, and that it is capable of combining with the bases, and forming salts. What is very extraordinary, in giving an account of carbonate of lime and carbonate of magnesia, he never mentions the name of Dr. Black; though it is very unlikely that a controversy, which had for years occupied the attention of chemists, should have been unknown to him. Mr. Cavendish's name never once appears in the whole paper; though that philosopher had preceded him by seven or eight years. He informs us, that he had made known his opinions respecting the nature of this substance, to various foreign correspondents, among others to Dr. Priestley, as early as the year 1770, and that Dr. Priestley had mentioned his views on the subject, in a paper inserted in the Philosophical Transactions for 1772. Bergman found the specific gravity of carbonic acid gas rather higher than 1·5, that of air being 1. His result is not far from the truth. He obtained his gas, by mixing calcareous spar with dilute sulphuric acid. He shows that this gas has a sour taste, that it reddens the infusion of litmus, and that it combines with bases. He gives figures of the apparatus which he used. This apparatus demands attention. Though far inferior to the contrivances of Priestley, it answered pretty well, enabling him to collect the gas, and examine its properties.

It is unnecessary to enter into any further details respecting this paper. Whoever will take the trouble to compare it with Cavendish's paper on the same subject, will find that he had been anticipated by that philosopher in a great many of his most important facts. Under these circumstances, I consider as singular his not taking any notice of Cavendish's previous labours.

2. His next paper, "On the Analyses of Mineral Waters," was first published in 1778, being the subject of a thesis, supported by J. P. Scharenberg. This dissertation, which is of great length, is entitled to much praise. He lays therein the foundation of the mode of analyzing waters, such as is followed at present. He points out the use of different reagents, for detecting the presence of the various constituents in mineral water, and then shows how the quantity of each is to be determined. It would be doing great injustice to Bergman, to compare his analyses with those of any modern experimenter. At that time, the science was not in possession of any accurate analyses of the neutral salts, which exist in mineral waters. Bergman undertook these necessary analyses, without which, the determination of the saline constituents of mineral waters was out of the question. His determinations were not indeed accurate, but they were so much better than those that preceded them, and Bergman's character as an experimenter stood so high, that they were long referred to as a standard by chemists. The first attempt to correct them was by Kirwan. But Bergman's superior reputation as a chemist enabled his results still to keep their ground, till his character for accuracy was finally destroyed by the very accurate experiments which the discovery of the atomic theory rendered it necessary to make. These, when once they became generally known, were of course preferred, and Bergman's analyses were laid aside.

It is a curious and humiliating fact, as it shows how much chemical reputation depends upon situation, or accidental circumstances, that Wenzel had, in 1766, in his book on *affinity*, published much more accurate analyses of all these salts, than Bergman's—analyses indeed which were almost perfectly correct, and which have scarcely been surpassed, by the most careful ones of the present day. Yet these admirable experiments scarcely drew the attention of chemists; while the very inferior ones of Bergman were held up as models of perfection.

3. Bergman, not satisfied with pointing out the mode of analyzing mineral waters, attempted to imitate them artificially by chemical processes, and published two essays on the subject; in the first he showed the processes by which cold mineral waters might be imitated, and in the other, the mode of imitating hot mineral waters. The attempt was valuable, and served to extend greatly the chemical knowledge of mineral waters, and of the salts which they contain; but it was made at too early a period of the analytical art, to approach perfection. A similar remark applies to his analysis of sea-water. The water examined was brought by Sparmann from a depth of eighty fathoms, near the latitude of the Canaries: Bergman found in it only common salt, muriate of magnesia, and sulphate of lime. His not having discovered the presence of sulphate of magnesia is a sufficient proof of the imperfection of his analytical methods; the other constituents exist in such small quantity in sea-water that they might easily have been overlooked, but the quantity of sulphate of magnesia in sea-water is considerable.

4. I shall pass over the paper on oxalic acid, which constituted the subject of a thesis, supported in 1776, by John Afzelius Arfvedson. It is now known that oxalic acid was discovered by Scheele, not by Bergman. It is impossible to say how many

of the numerous facts stated in this thesis were ascertained by Scheele, and how many by Afzelius. For, as Afzelius was already a *magister docens* in chemistry, there can be little doubt that he would himself ascertain the facts which were to constitute the foundation of his thesis. It is indeed now known that Bergman himself intrusted all the details of his experiments to his pupils. He was the contriver, while his pupils executed his plans. That Scheele has nowhere laid claim to a discovery of so much importance as that of oxalic acid, and that he allowed Bergman peaceably to bear away the whole credit, constitutes one of the most remarkable facts in the history of chemistry. Moreover, while it reflects so much credit on Scheele for modesty and forbearance, it seems to bear a little hard upon the character of Bergman. When he published the essay in the first volume of his Opuscula, in 1779, why did he not in a note inform the world that Scheele was the true discoverer of this acid? Why did he allow the discovery to be universally assigned to him, without ever mentioning the true state of the case? All this appeared so contrary to the character of Bergman, that I was disposed to doubt the truth of the statement, that Scheele was the discoverer of oxalic acid. When I was at Fahlun, in the year 1812, I took an opportunity of putting the question to Assessor Gahn, who had been the intimate friend of Scheele, and the pupil, and afterwards the friend of Bergman. He assured me that Scheele really was the discoverer of oxalic acid, and ascribed the omission of Bergman to inadvertence. Assessor Gahn showed me a volume of Scheele's letters to him, which he had bound up: they contained the history of all his chemical labours. I have little doubt that an account of oxalic acid would be found in these letters. If the son of Assessor Gahn, in whose possession these letters must now be, would take the trouble to inspect the volume in question, and to publish any notices respecting this acid which they may contain, he would confer an important favour on every person interested in the history of chemistry.

5. The dissertation on the manufacture of alum has been mentioned before. Bergman shows himself well acquainted with the processes followed, at least in Sweden, for making alum. He had no notion of the true constitution of alum; nor was that to be expected, as the discovery was thereby years later in being made. He thought that the reason why alum leys did not crystallize well was, that they contained an excess of acid, and that the addition of potash gave them the property of crystallizing readily, merely by saturating that excess of acid. Alum is a double salt, composed of three integrant particles of sulphate of alumina, and one integrant particle of sulphate of potash, or sulphate of ammonia. In some cases, the alum ore contains all the requisite ingredients. This is the case with the ore at Tolfa, in the neighbourhood of Rome. It seems, also, to be the case with respect to some of the alum ores in Sweden; particularly at Hœnsœter on Kinnekulle, in West Gothland, which I visited in 1812. If any confidence can be put in the statements of the manager of those works, no alkaline salt whatever is added; at least, I understood him to say so when I put the question.

6. In his dissertation on tartar-emetic, he gives an interesting historical account of this salt and its uses. His notions respecting the antimonial preparations best fitted to form it, are not accurate: nor, indeed, could they be expected to be so, till the nature and properties of the different oxides of antimony were accurately known. Antimony forms

three *oxides:* now it is the protoxide alone that is useful in medicine, and that enters into the composition of tartar-emetic; the other two oxides are inert, or nearly so. Bergman was aware that tartar-emetic is a double salt, and that its constituents are tartaric acid, potash, and oxide of antimony; but it was not possible, in 1773, when his dissertation was published, to have determined the true constituents of this salt by analysis.

7. Bergman's paper on magnesia was also a thesis defended in 1775, by Charles Norell, of West Gothland, who in all probability made the experiments described in the essay. In the introduction we have a history of the discovery of magnesia, and he mentions Dr. Black as the person who first accurately made out its peculiar chemical characters, and demonstrated that it differs from lime. This essay contains a pretty full and accurate account of the salts of magnesia, considering the state of chemistry at the time when it was published. There is no attempt to analyze any of the magnesian salts; but, in his treatise on the analysis of mineral waters, he had stated the quantity of magnesia contained in one hundred parts of several of them.

8. His paper on the *shapes of crystals*, published in 1773, contains the germ of the whole theory of crystallization afterwards developed by M. Hauy. He shows how, from a very simple primary form of a mineral, other shapes may proceed, which seem to have no connexion with, or resemblance to the primary form. His view of the subject, so far as it goes, is the very same afterwards adopted by Hauy: and, what is very curious, Hauy and Bergman formed their theory from the very same crystalline shape of calcareous spar—from which, by mechanical divisions, the same rhombic nucleus was extracted by both. Nothing prevented

Bergman from anticipating Hauy but a sufficient quantity of crystals to apply his theory to.*

9. In his paper on silica he gives us a history of the progress of chemical knowledge respecting this substance. Its nature was first accurately pointed out by Pott; though Glauber, and before him Van Helmont, were acquainted with the *liquor silicus*, or the combination of silica and potash, which is soluble in water. Bergman gives a detailed account of its properties; but he does not suspect it to possess acid properties. This great discovery, which has thrown a new light upon mineral bodies, and shown them all to be chemical combinations, was reserved for Mr. Smithson.

10. Bergman's experiments on the precious stones constitute the first rudiments of the method of analyzing stony bodies. His processes are very imperfect, and his apparatus but ill adapted to the purpose. We need not be surprised, therefore, that the results of his analyses are extremely wide of the truth. Yet, if we study his processes, we shall find in them the rudiments of the very methods which we follow at present. The superiority of the modern analyses over those of Bergman must in a great measure be ascribed to the platinum vessels which we now employ, and to the superior purity of the substances which we use as reagents in our analyses. The methods, too, are simplified and perfected. But we must not forget that this paper of Bergman's, imperfect as it is, constitutes the commencement of the art, and that fully as much genius and invention may be requisite to contrive the first rude processes, how imperfect soever they may be, as are required to bring these processes when once invented to a

---

* I shall mention afterwards that the real discoverer of this fact was Assessor Gahn, of Fahlun.

state of comparative perfection. The great step in analyzing minerals is to render them soluble in acids. Bergman first thought of the method for accomplishing this which is still followed, namely, fusing them or heating them to redness with an alkali or alkaline carbonate.

11. The paper on fulminating gold goes a great way to explain the nature of that curious compound He describes the properties of this substance, and the effects of alkaline and acid bodies on it. He shows that it cannot be formed without ammonia, and infers from his experiments that it is a compound of oxide of gold and ammonia. He explains the fulmination by the elastic fluid suddenly generated by the decomposition of the ammonia.

12. The papers on platinum, carbonate of iron, nickel, arsenic, and zinc, do not require many remarks. They add considerably to the knowledge which chemists at that time possessed of these bodies; though the modes of analysis are not such as would be approved of by a modern chemist; nor were the results obtained possessed of much precision.

13. The Essay on the Analysis of Metallic Ores by the wet way, or by solution, constitutes the first attempt to establish a regular method of analyzing metallic ores. The processes are all imperfect, as might be expected from the then existing state of analytical chemistry, and the imperfect knowledge possessed, of the different metallic ores. But this essay constituted a first beginning, for which the author is entitled to great praise. The subject was taken up by Klaproth, and speedily brought to a great degree of improvement by the labours of modern chemists.

14. The experiments on the way in which minerals behave before the blowpipe, which Bergman pub-

lished, were made at Bergman's request by Assessor Gahn, of Fahlun, who was then his pupil. They constitute the first results obtained by that very ingenious and amiable man. He afterwards continued the investigation, and added many improvements, simplifying the reagents and the manner of using them. But he was too indolent a man to commit the results of his investigations to writing. Berzelius, however, had the good sense to see the importance of the facts which Gahn had ascertained. He committed them to writing, and published them for the use of mineralogists. They constitute the book entitled "Berzelius on the Blowpipe," which has been translated into English.

15. The object of the Essay on Metallic Precipitates is to determine the quantity of phlogiston which each metal contains, deduced from the quantity of one metal necessary to precipitate a given weight of another. The experiments are obviously made with little accuracy: indeed they are not susceptible of very great precision. Lavoisier afterwards made use of the same method to determine the quantity of oxygen in the different metallic oxides; but his results were not more successful than those of Bergman.

16. Bergman's paper on iron is one of the most important in his whole works, and contributed very materially to advance the knowledge of the cause of the difference between iron and steel. He employed his pupils to collect specimens of iron from the different Swedish forges, and gave them directions how to select the proper pieces. All these specimens, to the number of eighty-nine, he subjected to a chemical examination, by dissolving them in dilute sulphuric acid. He measured the volume of hydrogen gas, which he obtained by dissolving a given weight of each, and noted the quantity and the

nature of the undissolved residue. The general result of the whole investigation was that pure malleable iron yielded most hydrogen gas; steel less, and cast-iron least of all. Pure malleable iron left the smallest quantity of insoluble matter, steel a greater quantity, and cast-iron the greatest of all. From these experiments he drew conclusions with respect to the difference between iron, steel, and cast-iron. Nothing more was necessary than to apply the antiphlogistic theory to these experiments, (as was done soon after by the French chemists,) in order to draw important conclusions respecting the nature of these bodies. Iron is a simple body; steel is a compound of iron and carbon; and cast-iron of iron and a still greater proportion of carbon. The defective part of the experiments of Bergman in this important paper is his method of determining the quantity of *manganese* in iron. In some specimens he makes the manganese amount to considerably more than a third part of the weight of the whole. Now we know that a mixture of two parts iron and one part manganese is brittle and useless. We are sure, therefore, that no malleable iron whatever can contain any such proportion of manganese. The fact is, that Bergman's mode of separating manganese from iron was defective. What he considered as manganese was chiefly, and might be in many cases altogether, oxide of iron. Many years elapsed before a good process for separating iron from manganese was discovered.

17. Bergman's experiments to ascertain the cause of the brittleness of cold-short iron need not occupy much of our attention. He extracted from it a white powder, by dissolving the cold-short iron in dilute sulphuric acid. This white powder he succeeded in reducing to the state of a white brittle metal, by fusing it with a flux and charcoal.

Klaproth soon after ascertained that this metal was a phosphuret of iron, and that the white powder was a phosphate of iron: and Scheele, with his usual sagacity, hit on a method of analyzing this phosphate, and thus demonstrating its nature. Thus Bergman's experiments led to the knowledge of the fact that cold-short iron owes its brittleness to a quantity of phosphorus which it contains. It ought to be mentioned that Meyer, of Stettin, ascertained the same fact, and made it known to chemists at about the same time with Bergman.

18. The dissertation on the products of volcanoes, first published in 1777, is one of the most striking examples of the sagacity of Bergman which we possess. He takes a view of all the substances certainly known to have been thrown out of volcanoes, attempts to subject them to a chemical analysis, and compares them with the basalt, and greenstone or trap-rocks, the origin of which constituted at that time a keen matter of dispute among geologists. He shows the identity between lavas and basalt and greenstone, and therefore infers the identity of formation. This is obviously the true mode of proceeding, and, had it been adopted at an earlier period, many of those disputes respecting the nature of trap-rocks, which occupied geologists for so long a period, would never have been agitated; or, at least, would have been speedily decided. The whole dissertation is filled with valuable matter, still well entitled to the attention of geologists. His observations on *zeolites*, which he considered as unconnected with volcanic products, were very natural at the time when he wrote; though the subsequent experiments of Sir James Hall, and Mr. Gregory Watt, and, above all, an accurate attention to the scoriæ from different smelting-houses, have thrown a new light on the subject, and have shown the way in

which zeolitic crystals might easily have been formed in melted lava, provided circumstances were favourable. In fact, we find abundant cavities in real lava from Vesuvius, filled with zeolitic crystals.

19. The last of the labours of Bergman which I shall notice here is his Essay on Elective Attractions, which was originally published in 1775, but was much augmented and improved in the third volume of his Opuscula, published in 1783. An English translation of this last edition of the Essay was made by Dr. Beddoes, and was long familiar to the British chemical world. The object of this essay was to elucidate and explain the nature of chemical affinity, and to account for all the apparent anomalies that had been observed. He laid it down as a first principle, that all bodies capable of combining chemically with each other, have an attraction for each other, and that this attraction is a definite and fixed force which may be represented by a number. Now the bodies which have the property of uniting together are chiefly the acids and the alkalies, or bases. Every acid has an attraction for each of the alkalies or bases; but the force of this attraction differs in each. Some bases have a strong attraction for acids, and others a weak; but the attractive force of each may be expressed by numbers.

Now, suppose that an acid *a* is united with a base *m* with a certain force, if we mix the compound *a m* with a certain quantity of the base *n*, which has a stronger attraction for *a* than *m* has, the consequence will be, that *a* will leave *m* and unite with *n*;—*n* having a stronger attraction for *a* than *m* has, will disengage it and take its place. In consequence of this property, which Bergman considered as the foundation of the whole of the science, the strength of affinity of one body for another is determined by these decompositions and combinations. If *n* has a stronger affinity for *a* than *m* has, then if we mix together *a*, *m*, and *n* in the requisite proportions, *a* and *n* will unite together, leaving *m* uncombined: or if we mix *n* with the compound *a m*, *m* will be disengaged. Tables, therefore, may be drawn up, exhibiting the strength of these affinities. At the top of a column is put the name of an *acid* or a *base*, and below it are put the names of all the *bases* or *acids* in the order of their affinity. The following little table will exhibit a specimen of these columns:

Sulphuric Acid.
Barytes
Strontian
Potash
Soda
Lime
Magnesia.

Here sulphuric acid is the substance placed at the head of the column, and under it are the names of the bases capable of uniting with it in the order of their affinity. Barytes, which is highest up, has the strongest affinity, and magnesia, which is lowest down, has the weakest affinity. If sulphuric acid and magnesia were combined together, all the bases whose names occur in the table above magnesia would be able to separate the sulphuric acid from it. Potash would be disengaged from sulphuric acid by barytes and strontian, but not by soda, lime, and magnesia.

Such tables then exhibited to the eye the strength of affinity of all the different bodies that are capable of uniting with one and the same substance, and the order in which decompositions are effected. Bergman drew up tables of affinity according to these views in fifty-nine columns. Each column contained the name of a particular substance, and under it was arranged all the bodies capable of uniting with it, each in the order of its affinity. Now bodies may be made to unite, either by mixing them together, and then exposing them to heat, or by dissolving them in water and mixing the respective solutions together. The first of these ways is usually called the *dry way*, the second the *moist way*. The order of decompositions often varies with the mode employed. On this account, Bergman divided each of his fifty-nine columns into two. In the first, he exhibited the order of decompositions in the moist way, in the second in the dry. He explained also the cases of double decomposition, by means of these unvarying forces acting together or opposing each other—and gave sixty-four cases of such double decompositions.

These views of Bergman's were immediately acceded to by the chemical world, and continued to regulate their processes till Berthollet published his Chemical Statics in 1802. He there called in question the whole doctrine of Bergman, and endeavoured to establish one of the very opposite kind. I shall have occasion to return to the subject when I come to give an account of the services which Berthollet conferred upon chemistry.

I have already observed, that we are under obligations to Bergman, not merely for the improvements which he himself introduced into chemistry, but for the pupils whom he educated as chemists, and the discoveries which were made by those persons, whose exertions he stimulated and encouraged. Among those individuals, whose chemical discoveries were chiefly made known to the world by his means, was Scheele, certainly one of the most extraordinary men, and most sagacious and industrious chemists that ever existed.

Charles William Scheele was born on the 19th of December, 1742, at Stralsund, the capital of Swedish Pomerania, where his father was a tradesman. He received the first part of his education at a private academy in Stralsund, and was afterwards removed to a public school. At a very early period he expressed a strong desire to study pharmacy, and obtained his father's consent to make choice of this profession. He was accordingly bound an apprentice for six years to Mr. Bouch, an apothecary in Gotheborg, and after his time was out, he remained with him still, two years longer.

It was here that he laid the groundwork of all his future celebrity, as we are informed by Mr. Grunberg, who was his fellow-apprentice, and afterwards settled as an apothecary in Stralsund. He was at that time very reserved and serious, but uncommonly diligent. He attended minutely to all the processes, reflected upon them while alone, and studied the writings of Neumann, Lemery, Kunkel, and Stahl, with indefatigable industry. He likewise exercised himself a good deal in drawing and painting, and acquired some proficiency in these accomplishments without a master. Kunkel's Laboratorium was his favourite book, and he was in the habit of repeating experiments out of it secretly during the night-time. On one occasion, as he was employed in making pyrophorus, his fellow-apprentice was malicious enough to put a quantity of fulminating powder into the mixture. The consequence was a violent explosion, which, as it took place in the night, threw the whole family into confusion, and brought a very severe rebuke upon our young chemist. But this did not put a stop to his industry, which he pursued so constantly and judiciously, that, by the time his apprenticeship was ended, there were very few chemists indeed who excelled him in knowledge and practical skill. His fellow-apprentice, Mr. Grunberg, wrote to him in 1774, requesting to know by what means he had become such a proficient in chemistry, and received the following answer: "I look upon you, my dear friend, as my first instructor, and as the author of all I know on the subject, in consequence of your advising me to read Neumann's Chemistry. The perusal of this book first gave me a taste for experimenting, myself; and I very well remember, that upon mixing some oil of cloves and smoking spirit of nitre together, they took fire. However, I kept this matter secret. I have also before my eyes the unfortunate experiment which I made with pyrophorus. Such accidents only served to increase my passion for making experiments."

In 1765 Scheele went to Malmo, to the house of an apothecary, called Mr. Kalstrom. After spending two years in that place, he went to Stockholm, to superintend the apothecary's shop of Mr. Scharenberg. In 1773 he exchanged this situation for another at Upsala, in the house of Mr. Loock. It was here that he accidentally formed an acquaintance with Assessor Gahn, of Fahlun, who was at that time a student at Upsala, and a zealous chemist. Mr. Gahn happening to be one day in the shop of Mr. Loock, that gentleman mentioned to him a circumstance which had lately occurred to him, and of which he was anxious to obtain an explanation. If a quantity of saltpetre be put into a crucible and raised to such a temperature as shall not merely melt it, but occasion an agitation in it like boiling, and if, after a certain time, the crucible be taken out of the fire and allowed to cool, the saltpetre still continues neutral; but its properties are altered: for, if distilled vinegar be poured upon it, red fumes are given out, while vinegar produces no effect upon the saltpetre before it has been thus heated. Mr. Loock wished from Gahn an explanation of the cause of this phenomenon: Gahn was unable to explain it; but promised to put the question to Professor Bergman. He did so accordingly, but Bergman was as unable to find an explanation as himself. On returning a few days after to Mr. Loock's shop, Gahn was informed that there was a young man in the shop who had given an explanation of the phenomenon. This young man was Scheele, who had informed Mr. Loock that there were two species of acids confounded under the name of *spirit of nitre;* what we at present call *nitric* and *hyponitrous* acids. Nitric acid has a stronger affinity for potash than vinegar has; but hyponitrous acid has a weaker. The heat of the fire changes the *nitric* acid of the saltpetre to *hyponitrous:* hence the phenomenon.

Gahn was delighted with the information, and immediately formed an acquaintance with Scheele, which soon ripened into friendship. When he informed Bergman of Scheele's explanation, the professor was equally delighted, and expressed an eager desire to be made acquainted with Scheele; but when Gahn mentioned the circumstance to Scheele, and offered to introduce him to Bergman, our young chemist rejected the proposal with strong feelings of dislike.

It seems, that while Scheele was in Stockholm, he had made experiments on cream of tartar, and had succeeded in separating from it tartaric acid, in a state of purity. He had also determined a number of the properties of tartaric acid, and examined several of the tartrates. He drew up an account of these results, and sent it to Bergman. Bergman, seeing a paper subscribed by the name of a person who was unknown to him, laid it aside without looking at it, and forgot it altogether. Scheele was very much provoked at this contemptuous and unmerited treatment. He drew up another account of his experiments and gave it to Retzius, who sent it to the Stockholm Academy of Sciences (with some additions of his own), in whose Memoirs it was published in the year 1770.* It cost Assessor Gahn considerable trouble to satisfy Scheele that Bergman's conduct was merely the result of inadvertence, and that he had no intention whatever of treating him either with contempt or neglect. After much entreaty, he prevailed upon Scheele to allow him to introduce him to the professor of chemistry. The introduction took place accordingly, and ever after Bergman and Scheele continued steady friends—Bergman facilitating the researches of Scheele by every means in his power.

So high did the character of Scheele speedily rise in Upsala, that when the Duke of Sudermania visited the university soon after, in company with Prince Henry of Prussia, Scheele was appointed by the university to exhibit some chemical processes before him. He fulfilled his charge, and performed in different furnaces several curious and striking experiments. Prince Henry asked him various questions, and expressed satisfaction at the answers given. He was particularly pleased when informed that he was a native of Stralsund. These two princes afterwards stated to the professors that they would take it as a favour if Scheele could have free access to the laboratory of the university whenever he wished to make experiments.

In the year 1775, on the death of Mr. Popler, apothecary at Köping (a small place on the north

---

* Konig. Vetensk. Acad. Handl. 1770, p. 207.

side of the lake Mæler), he was appointed by the Medical College *provisor* of the apothecary's shop. In Sweden all the apothecaries are under the control of the Medical College, and no one can open a shop without undergoing an examination and receiving licence from that learned body. In the course of the examinations which he was obliged to undergo, Scheele gave great proofs of his abilities, and obtained the appointment. In 1777 the widow sold him the shop and business, according to a written agreement made between them; but they still continued housekeeping at their joint expense. He had already distinguished himself by his discovery of fluoric acid, and by his admirable paper on manganese. It is said, too, that it was he who made the experiments on carbonic acid gas, which constitute the substance of Bergman's paper on the subject, and which confirmed and established Bergman's idea that it was an acid. At Köping he continued his researches with unremitting perseverance, and made more discoveries than all the chemists of his time united together. It was here that he made the experiments on air and fire, which constitute the materials of his celebrated work on these subjects. The theory which he formed was indeed erroneous; but the numerous discoveries which the book contains must always excite the admiration of every chemist. His discovery of oxygen gas had been anticipated by Priestley; but his analysis of atmospheric air was new and satisfactory—was peculiarly his own. The processes by means of which he procured oxygen gas were also new, simple, and easy, and are still followed by chemists in general. During his residence at Köping he published a great number of chemical papers, and every one of them contained a discovery. The whole of his time was devoted to chemical investigations. Every action of his life had a tendency to forward the advancement of his favourite science; all his thoughts were turned to the same object; all his letters were devoted to chemical observations and chemical discussions. Crell's Annals was at that time the chief periodical work on chemistry in Germany. He got the numbers regularly as they were published, and was one of Crell's most constant and most valuable correspondents. Every one of his letters published in that work either contains some new chemical fact, or exposes the errors and mistakes of some one or other of Crell's numerous correspondents.

Scheele's outward appearance was by no means prepossessing. He seldom joined in the usual conversations and amusements of society, having neither leisure nor inclination for them. What little time he had to spare from the hurry of his profession was always employed in making experiments. It was only when he received visits from his friends, with whom he could converse on his favourite science, that he indulged himself in a little relaxation. For such intimate friends he had a sincere affection. This regard was extended to all the zealous cultivators of chemistry in every part of the world, whether personally known to him or not. He kept up a correspondence with several; though this correspondence was much limited by his ignorance of all languages except German; for at least he could not write fluently in any other language. His chemical papers were always written in German, and translated into Swedish, before they were inserted in the Memoirs of the Stockholm Academy, where most of them appeared.

He was kind and affable to all. Before he adopted an opinion in science, he reflected maturely on it; but, after he had once embraced it, his opinions were not easily shaken. However, he did not hesitate to give up an opinion as soon as it had been proved to be erroneous. Thus, he entirely renounced the notion which he once entertained that *silica* is a compound of *water* and *fluoric acid;* because it was demonstrated, by Meyer and others, that this *silica* was derived from the glass vessels in which the fluoric acid was prepared; that these glass vessels were speedily corroded into holes; and that, if fluoric acid was prepared in metallic vessels, and not allowed to come in contact with glass or any substance containing silica, it might be mixed with water without any deposition of silica whatever.

It appears also by a letter of his, published in Crell's Annals, that he was satisfied of the accuracy of Mr. Cavendish's experiments, showing that water was a compound of oxygen and hydrogen gases, and of Lavoisier's repetition of them. He attempted to reconcile this fact with his own notion, that heat is a compound of oxygen and hydrogen. But his arguments on that subject, though ingenious, are not satisfactory; and there is little doubt that if he had lived somewhat longer, and had been able to repeat his own experiments, and compare them with those of Cavendish and Lavoisier, he would have given up his own theory and adopted that of Lavoisier, or, at any rate, the explanation of Cavendish, which, being more conformable to his own preconceived notions, might have been embraced by him in preference.

It is said by Dr. Crell that Scheele was invited over to England, with an offer of an easy and advantageous situation; but that his love of quiet and retirement, and his partiality for Sweden, where he had spent the greatest part of his life, threw difficulties in the way of these overtures, and that a change in the English ministry put a stop to them for the time. The invitation, Crell says, was renewed in 1786, with the offer of a salary of 300*l.* a-year; but Scheele's death put a final stop to it. I have very great doubts about the truth of this statement; and, many years ago, during the lifetime of Sir Joseph Banks, Mr. Cavendish, and Mr. Kirwan, I made inquiry about the circumstance; but none of the chemists in Great Britain, who were at that time numerous and highly respectable, had ever heard of any such negotiation. I am utterly at a loss to conceive what one individual in any of the ministries of George III. was either acquainted with the science of chemistry, or at all interested in its progress. They were all so intent upon accomplishing their own objects, or those of their sovereign, that they had neither time nor inclination to think of science, and certainly no money to devote to any of its votaries. What minister in Great Britain ever attempted to cherish the sciences, or to reward those who cultivate them with success? If we except Mr. Montague, who procured the place of master of the Mint for Sir Isaac Newton, I know of no one. While in every other nation in Europe science is directly promoted, and considerable sums are appropriated for its cultivation, and for the support of a certain number of individuals who have shown themselves capable of extending its boundaries, not a single farthing has been devoted to any such purpose in Great Britain. Science has been left entirely to itself; and whatever has been done by way of promoting it has been performed by the unaided exertions of private individuals. George III. himself was a patron of literature and an encourager of *botany.* He might have been disposed to reward the unrivalled eminence which Scheele had attained; but this he could only have done by bestowing on him a pension out of his privy purse. No situation which Scheele could fill was at his disposal. The universities and the church were both shut against a Lutheran; and no pharmaceutical places exist in this country to which Scheele could have been appointed. If any such project ever existed, it must have been an idea which struck some man of science that such a proposal to a man of Scheele's eminence would redound to the credit of the country. But that such a project should have been broached by a British ministry, or by any man of great political influence, is an opinion that no person would adopt who has paid any attention to the history of Great Britain since the Revolution to the present time.

Scheele fell at last a sacrifice to his ardent love for his science. He was unable to abstain from experimenting, and many of his experiments were unavoidably made in his shop, where he was exposed during winter, in the ungenial climate of Sweden, to cold draughts of air. He caught rheumatism in consequence, and the disease was aggravated by his ardour and perseverance in his pursuits. When he purchased the apothecary's shop in which his business was carried on, he had formed the resolution of marrying the widow of his predecessor, and he had only delayed it from the honourable principle of acquiring, in the first place, sufficient property to render such an alliance desirable on her part. At length, in the month of March, 1786, he declared his intention of marrying her; but his disease at this time increased very fast, and his hopes of recovery daily diminished. He was sensible of this; but nevertheless he performed his promise, and married her on the 19th of May, at a time when he lay on his deathbed. On the 21st, he left her by his will the disposal of the whole of his property; and, the same day on which he so tenderly provided for her, he died.

I shall now endeavour to give the reader an idea of the principal chemical discoveries for which we are indebted to Scheele: his papers, with the exception of his book on *air and fire*, which was published separately by Bergman, are all to be found either in the Memoirs of the Stockholm Academy of Science, or in Crell's Journal; they were collected, and a Latin translation of them, made by Godfrey Henry Schaefer, published at Leipsic, in 1788, by Henstreit, the editor of the three last volumes of Bergman's Opuscula. A French translation of them was made in consequence of the exertions of M. Morveau; and an English translation of them, in 1786, by means of Dr. Beddoes, when he was a student in Edinburgh. There are also several German translations, but I have never had an opportunity of seeing them.

1. Scheele's first paper was published by Retzius, in 1770; it gives a method of obtaining pure tartaric acid: the process was to decompose cream of tartar by means of chalk. One half of the tartaric acid unites to the lime, and falls down in the state of a white insoluble powder, being *tartrate of lime*. The cream of tartar, thus deprived of half its acid, is converted into the neutral salt formerly distinguished by the name of *soluble tartar*, from its great solubility in water: it dissolves, and may be obtained in crystals, by the usual method of crystallizing salts. The tartrate of lime is washed with water, and then mixed with a quantity of dilute sulphuric acid, just capable of saturating the lime contained in the tartrate of lime; the mixture is digested for some time; the sulphuric acid displaces the tartaric acid, and combines with the lime; and, as the sulphate of lime is but very little soluble in water, the greatest part of it precipitates, and the clear liquor is drawn off: it consists of tartaric acid, held in solution by water, but not quite free from sulphate of lime. By repeated

concentrations, all the sulphate of lime falls down, and at last the tartaric acid itself is obtained in large crystals. This process is still followed by the manufacturers of this country; for tartaric acid is used to a very considerable extent by the calico-printers, in various processes; for example, it is applied, thickened with gum, to different parts of cloth dyed Turkey red; the cloth is then passed through water containing the requisite quantity of chloride of lime: the tartaric acid, uniting with the lime, sets the chlorine at liberty, which immediately destroys the red colour wherever the tartaric acid has been applied, but leaves all the other parts of the cloth unchanged.

2. The paper on *fluoric acid* appeared in the Memoirs of the Stockholm Academy, for 1771, when Scheele was in Scharenberg's apothecary's shop in Stockholm, where, doubtless, the experiments were made. Three years before, Margraaf had attempted an analysis of fluor spar, but had discovered nothing. Scheele demonstrated that it is a compound of lime and a peculiar acid, to which he gave the name of *fluoric* acid. This acid he obtained in solution in water; it was separated from the fluor spar by sulphuric, muriatic, nitric, and phosphoric acids. When the fluoric acid came in contact with water, a white crust was formed, which proved, on examination, to be silica. Scheele at first thought that this silica was a compound of fluoric acid and water; but it was afterwards proved by Weigleb and by Meyer, that this notion is inaccurate, and that the silica was corroded from the retort into which the fluor spar and sulphuric acid were put. Bergman, who had adopted Scheele's theory of the nature of silica, was so satisfied by these experiments, that he gave it up, as Scheele himself did soon after.

Scheele did not obtain fluoric acid in a state of purity, put only *fluosilicic acid;* nor were chemists acquainted with the properties of fluoric acid till Gay-Lussac and Thenard published their Recherches Physico-chimiques, in 1811.

3. Scheele's experiments on *manganese* were undertaken at the request of Bergman, and occupied him three years; they were published in the Memoirs of the Stockholm Academy, for 1774, and constitute the most memorable and important of all his essays, since they contain the discovery of two new bodies, which have since acted so conspicuous a part, both in promoting the progress of the science, and in improving the manufactures of Europe. These two substances are *chlorine* and *barytes*, the first account of both of which occur in this paper.

The ore of manganese employed in these experiments was the *black oxide*, or *deutoxide*, of manganese, as it is now called. Scheele's method of proceeding was to try the effect of all the different reagents on it. It dissolved in sulphurous and nitrous acids, and the solution was colourless. Dilute sulphuric acid did not act upon it, nor nitric acid; but concentrated sulphuric acid dissolved it by the assistance of heat. The solution of sulphate of manganese in water was colourless and crystallized in very oblique rhomboidal prisms, having a bitter taste. Muriatic acid effervesced with it, when assisted by heat, and the elastic fluid that passed off had a yellowish colour, and the smell of aqua regia. He collected quantities of this elastic fluid *(chlorine)* in bladders, and determined some of its most remarkable properties: it destroyed colours, and tinged the bladder yellow, as nitric acid does. This elastic fluid, in Scheele's opinion, was muriatic acid deprived of phlogiston. By phlogiston Scheele meant, in this place, hydrogen gas. He considered muriatic acid as a compound of chlorine and hydrogen. Now this is the very theory that was established by Davy in consequence of his own experiments and those of Gay-Lussac and Thenard. Scheele's mode of collecting chlorine gas in a bladder, did not enable him to determine its characters with so much precision as was afterwards done. But his accuracy was so great, that every thing which he stated respecting it was correct so far as it went.

Most of the specimens of manganese ore which Scheele examined, contained more or less barytes, as has since been determined, in combination with the oxide. He separated this barytes, and determined its peculiar properties. It dissolved in nitric and muriatic acids, and formed salts capable of crystallizing, and permanent in the air. Neither potash, soda, nor lime, nor any *base* whatever, was capable of precipitating it from these acids. But the alkaline carbonates threw it down in the state of a white powder, which dissolved with effervescence in acids. Sulphuric acid and all the sulphates threw it down in the state of a white powder, which was insoluble in water and in acids. This sulphate cannot be decomposed by any acid or base whatever. The only practicable mode of proceeding is to convert the sulphuric acid into sulphur, by heating the salt with charcoal powder, along with a sufficient quantity of potash, to bring the whole into fusion. The fused mass, edulcorated, is soluble in nitric or muriatic acid, and thus may be freed from charcoal, and the barytes obtained in a state of purity. Scheele detected barytes, also, in the potash made from trees or other smaller vegetables; but at that time he was unacquainted with *sulphate of barytes*, which is so common in various parts of the earth, especially in lead-mines.

To point out all the new facts contained in this admirable essay, it would be necessary to transcribe the whole of it. He shows the remarkable analogy between manganese and metallic oxides. Bergman, in an appendix affixed to Scheele's paper, states his reasons for being satisfied that it is really a metallic oxide. Some years afterwards, Assessor Gahn succeeded in reducing it to the metallic state, and thus dissipating all remaining doubts on the subject.

4. In 1775 he gave a new method of obtaining benzoic acid from benzoin. His method was, to digest the benzoin with pounded chalk and water, till the whole of the acid had combined with lime, and dissolved in the water. It is requisite to take care to prevent the benzoin from running into clots. The liquid thus containing benzoate of lime in solution is filtered, and muriatic acid added in sufficient quantity to saturate the lime. The benzoic acid is separated in white flocks, which may be easily collected and washed. This method, though sufficiently easy, is not followed by practical chemists, at least in this country. The acid when procured by precipitation is not so beautiful as what is procured by sublimation; nor is the process so cheap or so rapid. For these reasons, Scheele's process has not come into general use.

5. During the same year, 1775, his essay on arsenic and its acid was also published in the Memoirs of the Stockholm Academy. In this essay he shows various processes, by means of which white arsenic may be converted into an acid, having a very sour taste, and very soluble in water. This is the acid to which the name of *arsenic acid* has been since given. Scheele describes the properties of this acid, and the salts which it forms, with the different bases. He examines, also, the action of white arsenic upon different bodies, and throws light upon the arsenical salt of Macquer.

6. The object of the little paper on silica, clay,

and alum, published in the Memoirs of the Stockholm Academy, for 1776, is to prove that alumina and silica are two perfectly distinct bodies, possessed of different properties. This he does with his usual felicity of experiment. He shows, also, that alumina and lime are capable of combining together.

7. The same year, and in the same volume of the Stockholm Memoirs, he published his experiments on a urinary calculus. The calculus upon which his experiments were made, happened to be composed of *uric acid*. He determined the properties of this new acid, particularly the characteristic one of dissolving in nitric acid, and leaving a beautiful pink sediment when the solution is gently evaporated to dryness.

8. In 1778 appeared his experiments on molybdena. What is now called *molybdena* is a soft foliated mineral, having the metallic lustre, and composed of two atoms sulphur united to one atom of metallic molybdenum. It was known before, from the experiments of Quest, that this substance contains sulphur. Scheele extracted from it a white powder, which he showed to possess acid properties, though it was insoluble in water. He examined the characters of this acid, called molybdic acid, and the nature of the salts which it is capable of forming by uniting with bases.

9. In the year 1777 was published the Experiments of Scheele on Air and Fire, with an introduction, by way of preface, from Bergman, who seems to have superintended the publication. This work is undoubtedly the most extraordinary production that Scheele has left us; and is really wonderful, if we consider the circumstances under which it was produced. Scheele ascertained that common air is a mixture of two distinct elastic fluids, one of which alone is capable of supporting combustion,

and which, therefore, he calls *empyreal air;* the other, being neither capable of maintaining combustion, nor of being breathed, he called *foul air.* These are the *oxygen* and *azote* of modern chemists. Oxygen he showed to be heavier than common air; bodies burnt in it with much greater splendour than in common air. Azote he found lighter than common air; bodies would not burn in it at all. He showed that metallic *calces*, or metallic *oxides*, as they are now called, contain oxygen as a constituent, and that when they are reduced to the metallic state, oxygen gas is disengaged. In his experiments on fulminating gold he shows, that during the fulmination a quantity of azotic gas is disengaged; and he deduces from a great many curious facts, which are stated at length, that ammonia is a compound of *azote* and *hydrogen*. His apparatus was not nice enough to enable him to determine the proportions of the various ingredients of the bodies which he analyzed: accordingly that is seldom attempted; and when it is, as was the case with common air, the results are very unsatisfactory. He deduces from his experiments, that the volume of oxygen gas, in common air, is between a third and a fourth: we now know that it is exactly a fifth.

In this book, also, we have the first account of sulphuretted hydrogen gas, and of its properties. He gives it the name of stinking sulphureous air.

The observations and new views respecting heat and light in this work are so numerous, that I am obliged to omit them: nor do I think it necessary to advert to his theory, which, when his book was published, was exceedingly plausible, and undoubtedly constituted a great step towards the improvements which soon after followed. His own experiments, had he attended a little more closely to the *weights*, and the alterations of them, would have been sufficient to have overturned the whole doctrine of phlogiston. Upon the whole it may be said, with confidence, that there is no chemical book in existence which contains a greater number of new and important facts than this work of Scheele, at the time it was published. Yet most of his discoveries were made, also, by others. Priestley and Lavoisier, from the superiority of their situations, and their greater means of making their labours speedily known to the public, deprived him of much of that reputation to which, in common circumstances, he would have been entitled. Priestley has been blamed for the rapidity of his publications, and the crude manner in which he ushered his discoveries to the world. But had he kept them by him till he had brought them to a sufficient degree of maturity, it is obvious that he would have been anticipated in the most important of them by Scheele.

10. In the Memoirs of the Stockholm Academy, for 1779, there is a short but curious paper of Scheele, giving an account of some results which he had obtained. If a plate of iron be moistened by a solution of common salt, or of sulphate of soda, and left for some weeks in a moist cellar, an afflorescence of carbonate of soda covers the surface of the plate. The same decomposition of common salt and evolution of soda takes place when unslacked quicklime is moistened with a solution of common salt, and left in a similar situation. These experiments led afterwards to various methods of decomposing common salt, and obtaining from it carbonate of soda. The phenomena themselves are still wrapped up in considerable obscurity. Berthollet attempted an explanation afterwards in his Chemical Statics; but founded on principles not easily admissible.

11. During the same year, his experiments on *plumbago* were published. This substance had been

long employed for making black-lead pencils; but nothing was known concerning its nature. Scheele, with his usual perseverance, tried the effect of all the different reagents, and showed that it consisted chiefly of *carbon*, but was mixed with a certain quantity of iron. It was concluded from these experiments, that plumbago is a carburet of iron. But the quantity of iron differs so enormously in different specimens, that this opinion cannot be admitted. Sometimes the iron amounts only to one-half per cent., and sometimes to thirty per cent. Plumbago, then, is carbon mixed with a variable proportion of iron, or carburet of iron.

12. In 1780 Scheele published his experiments on milk, and showed that sour milk contains a peculiar acid, to which the name of *lactic* acid has been given.

He found that when sugar of milk is dissolved in nitric acid, and the solution allowed to cool, small crystalline grains were deposited. These grains have an acid taste, and combine with bases: they have peculiar properties, and therefore constitute a particular acid, to which the name of *saclactic* was given. It is formed, also, when gum is dissolved in nitric acid; on this account it has been called, *mucic* acid.

13. In 1781 his experiments on a heavy mineral called by the Swedes *tungsten*, were published. This substance had been much noticed on account of its great weight; but nothing was known respecting its nature. Scheele, with his usual skill and perseverance, succeeded in proving that it was a compound of lime and a peculiar acid, to which the name of *tungstic acid* was given. Tungsten was, therefore, a tungstate of lime. Bergman, from its great weight, suspected that tungstic acid was in reality the oxide of a metal, and this conjecture was

afterwards confirmed by the Elhuyarts, who extracted the same acid from wolfram, and succeeded in reducing it to the metallic state.

14. In 1782 and 1783 appeared his experiments on *Prussian blue*, in order to discover the nature of the colouring matter. These experiments were exceedingly numerous, and display uncommon ingenuity and sagacity. He succeeded in demonstrating that *prussic acid*, the name at that time given to the colouring principle, was a compound of *carbon* and *azote*. He pointed out a process for obtaining prussic acid in a separate state, and determined its properties. This paper threw at once a ray of light on one of the obscurest parts of chemistry. If he did not succeed in elucidating this difficult department completely, the fault must not be ascribed to him, but to the state of chemistry when his experiments were made; in fact, it would have been impossible to have gone further, till the nature of the different elastic fluids at that time under investigation had been thoroughly established. Perhaps in 1783 there was scarcely any other individual who could have carried this very difficult investigation so far as it was carried by Scheele.

15. In 1783 appeared his observations on the *sweet principle of oils*. He observed, that when olive oil and litharge are combined together, a sweet substance separates from the oil and floats on the surface. This substance, when treated with nitric acid, yields *oxalic acid*. It was therefore closely connected with sugar in its nature. He obtained the same sweet matter from linseed oil, oil of almonds, of rape-seed, from hogs' lard, and from butter. He therefore concluded that it was a principle contained in all the expressed or fixed oils.

16. In 1784 he pointed out a method by which *citric acid* may be obtained in a state of purity from

lemon-juice. He likewise determined its characters, and showed that it was entitled to rank as a peculiar acid.

It was during the same year that he observed a white earthy matter, which may be obtained by washing rhubarb, in fine powder, with a sufficient quantity of water. This earthy matter he decomposed, and ascertained that it was a neutral salt, composed of oxalic acid, combined with lime. In a subsequent paper he showed, that the same oxalate of lime exists in a great number of roots of various plants.

17. In 1786 he showed that apples contain a peculiar acid, the properties of which he determined, and to which the name of *malic acid* has been given. In the same paper he examined all the common acid fruits of this country—gooseberries, currants, cherries, bilberries, &c., and determined the peculiar acids which they contain. Some owe their acidity to malic acid, some to citric acid, and some to tartaric acid; and not a few hold two, or even three, of these acids at the same time.

The same year he showed that the syderum of Bergman was phosphuret of iron, and the *acidum perlatum* of Proust *biphosphate of soda*.

The only other publication of Scheele, during 1785, was a short notice respecting a new mode of preparing *magnesia alba*. If sulphate of magnesia and common salt, both in solution, be mixed in the requisite proportions, a double decomposition takes place, and there will be formed sulphate of soda and muriate of magnesia. The greatest part of the former salt may be obtained out of the mixed ley by crystallization, and then the magnesia alba may be thrown down, from the muriate of magnesia, by means of an alkaline carbonate. The advantage of this new process is, the procuring of a considerable quantity of sulphate of soda in exchange for common salt, which is a much cheaper substance.

18. The last paper which Scheele published appeared in the Memoirs of the Stockholm Academy, for 1786: in it he gave an account of the characters of gallic acid, and the method of obtaining that acid from nutgalls.

Such is an imperfect sketch of the principal discoveries of Scheele. I have left out of view his controversial papers, which have now lost their interest; and a few others of minor importance, that this notice might not be extended beyond its due length. It will be seen that Scheele extended greatly the number of acids; indeed, he more than doubled the number of these bodies known when he began his chemical labours. The following acids were discovered by him; or, at least, it was he that first accurately pointed out their characters:

Fluoric acid
Molybdic acid
Tungstic acid
Arsenic acid
Lactic acid
Gallic acid
Tartaric acid
Oxalic acid
Citric acid
Malic acid
Saclactic
Chlorine.

To him, also, we owe the first knowledge of barytes, and of the characters of manganese. He determined the nature of the constituents of ammonia and prussic acid: he first determined the compound nature of common air, and the properties of the two elastic fluids of which it is composed. What other chemist, either a contemporary or predecessor of Scheele, can be brought in competition with him as a discoverer? And all was performed under the most unpropitious circumstances, and during the continuance of a very short life, for he died in the 44th year of his age.

CHAPTER XI.

PROGRESS OF SCIENTIFIC CHEMISTRY IN FRANCE.

I HAVE already given an account of the state of chemistry in France, during the earlier part of the eighteenth century, as it was cultivated by the Stahlian school. But the new aspect which chemistry put on in Britain in consequence of the discoveries of Black, Cavendish, and Priestley, and the conspicuous part which the gases newly made known was likely to take in the future progress of the science, drew to the study of chemistry, sometime after the middle of the eighteenth century, a man who was destined to produce a complete revolution, and to introduce the same precision, and the same accuracy of deductive reasoning which distinguishes the other branches of natural science. This man was Lavoisier.

Antoine Laurent Lavoisier was born in Paris on the 26th of August, 1743. His father being a man of opulence spared no expense on his education. His taste for the physical sciences was early displayed, and the progress which he made in them was uncommonly rapid. In the year 1764 a prize was offered by the French government for the best and most economical method of lighting the streets of an extensive city. Young Lavoisier, though at that time only twenty-one years of age, drew up a memoir on the subject which obtained the gold medal. This essay was inserted in the Memoirs of the French Academy of Sciences, for 1768. It was during that year, when he was only twenty-five years of age that he became a member of that scientific body. By this time he was become fully conscious of his own strength; but he hesitated for some time to which of the sciences he should devote his attention. He tried pretty early to determine, experimentally, some chemical questions which at that time drew the attention of practical chemists. For example: an elaborate paper of his appeared in the Memoirs of the French Academy, for 1768, on the composition of *gipsum*—a point at that time not settled; but which Lavoisier proved, as Margraaf had done before him, to be a compound of sulphuric acid and lime. In the Memoirs of the Academy, for 1770, two papers of his appeared, the object of which was to determine whether water could, as Margraaf had pretended, be converted into *silica* by long-continued digestion in glass vessels. Lavoisier found, as Margraaf stated, that when water is digested for a long time in a glass retort, a little silica makes its appearance; but he showed that this silica was wholly derived from the retort. Glass, it is well known, is a compound of silica and a fixed alkali. When water is long digested on it the glass is slightly corroded, a little alkali is dissolved in the water and a little silica separated in the form of a powder.

He turned a good deal of his attention also to geology, and made repeated journeys with Guettard into almost every part of France. The object in view was an accurate description of the mineralogical structure of France—an object accomplished to a considerable extent by the indefatigable exertions of Guettard, who published different papers on the subject in the Memoirs of the French Academy, accompanied with geological maps; which were at that time rare.

The mathematical sciences also engrossed a considerable share of his attention. In short he displayed no great predilection for one study more than another, but seemed to grasp at every branch of science with equal avidity. While in this state of suspension he became acquainted with the new and unexpected discoveries of Black, Cavendish, and Priestley, respecting the gases. This opened a new creation to his view, and finally determined him to devote himself to scientific chemistry.

In the year 1774 he published a volume under the title of "Essays Physical and Chemical." It was divided into two parts. The first part contained an historical detail of every thing that had been done on the subject of airs, from the time of Paracelsus down to the year 1774. We have the opinions and experiments of Van Helmont, Boyle, Hales, Boerhaave, Stahl, Venel, Saluces, Black, Macbride, Cavendish, and Priestley. We have the history of Meyer's acidum pingue, and the controversy carried on in Germany, between Jacquin on the one hand, and Crans and Smeth on the other.

In the second part Lavoisier relates his own experiments upon gaseous substances. In the first four chapters he shows the truth of Dr. Black's theory of fixed air. In the 4th and 5th chapters he proves that when metallic calces are reduced, by heating them with charcoal, an elastic fluid is evolved, precisely of the same nature with carbonic acid gas. In the 6th chapter he shows that when metals are calcined their weight increases, and that a portion of air equal to their increase in weight is absorbed from the surrounding atmosphere. He observed that in a given bulk of air calcination goes on to a certain point and then stops altogether, and that air in which metals have been calcined does not support combustion so well as it did before any such process was performed in it. He also burned phosphorus in a given volume of air, observed the diminution of volume of the air and the increase of the weight of the phosphorus.

Nothing in these essays indicates the smallest suspicion that air was a mixture of two distinct fluids, and that only one of them was concerned in combustion and calcination; although this had been already deduced by Scheele from his own experiments, and though Priestley had already discovered the existence and peculiar properties of oxygen gas. It is obvious, however, that Lavoisier was on the way to make these discoveries, and had neither Scheele nor Priestley been fortunate enough to hit upon oxygen gas, it is exceedingly likely that he would himself have been able to have made that discovery.

Dr. Priestley, however, happened to be in Paris towards the end of 1774, and exhibited to Lavoisier, in his own laboratory in Paris, the method of procuring oxygen gas from red oxide of mercury. This discovery altered all his views, and speedily suggested not only the nature of atmospheric air, but also what happens during the calcination of metals and the combustion of burning bodies in general. These opinions when once formed he prosecuted with unwearied industry for more than twelve years, and after a vast number of experiments, conducted with a degree of precision hitherto unattempted in chemical investigations, he boldly undertook to disprove the existence of phlogiston altogether, and to explain all the phenomena hitherto supposed to depend upon that principle by the simple combination or separation of oxygen from bodies.

In these opinions he had for some years no coadjutors or followers, till, in 1785, Berthollet at a meeting of the Academy of Sciences, declared himself a convert. He was followed by M. Fourcroy, and soon after Guyton de Morveau, who was at that time the editor of the chemical department of the Encyclopédie Méthodique, was invited to Paris by Lavoisier and prevailed upon to join the same party. This was followed by a pretty vigorous controversy, in which Lavoisier and his associates gained a signal victory.

Lavoisier, after Buffon and Tillet, was treasurer to the academy, into the accounts of which he introduced both economy and order. He was consulted by the National Convention on the most eligible means of improving the manufacture of assignats, and of augmenting the difficulty of forging them. He turned his attention also to political economy, and between 1778 and 1785 he allotted 240 arpents in the Vendomois to experimental agriculture, and increased the ordinary produce by one-half. In 1791 the Constituent Assembly invited him to draw up a plan for rendering more simple the collection of the taxes, which produced an excellent report, printed under the title of "Territorial Riches of France."

In 1776 he was employed by Turgot to inspect the manufactory of gunpowder; which he made to carry 120 toises, instead of 90. It is pretty generally known, that during the war of the American revolution, the French gunpowder was much superior to the British; but it is perhaps not so generally understood, that for this superiority the French government were indebted to the abilities of Lavoisier. During the war of the French revolution, the quality of the powder of the two nations was reversed; the English being considerably superior to that of the French, and capable of carrying further. This was put to the test in a very remarkable way at Cadiz.

During the horrors of the dictatorship of Robespierre, Lavoisier began to suspect that he would be stripped of his property, and informed Lalande that he was extremely willing to work for his subsistence. It was supposed that he meant to pursue the profession of an apothecary, as most congenial to his studies: but he was accused, along with the other *farmers-general*, of defrauding the revenue, and thrown into prison. During that sanguinary period imprisonment and condemnation were synonymous terms. Accordingly, on the 8th of May, 1794, he suffered on the scaffold, with twenty-eight farmers-general, at the early age of fifty-one. It has been alleged that Fourcroy, who at that time possessed considerable influence, might have saved him had he been disposed to have exerted himself. But this accusation has never been supported by any evidence. Lavoisier was a man of too much eminence to be overlooked, and no accused person at that time could be saved unless he was forgotten. A paper was presented to the tribunal, drawn up by M. Hallé, giving a catalogue of the works, and a recapitulation of the merits of Lavoisier; but it was thrown aside without even being read, and M. Hallé had reason to congratulate himself that his useless attempts to save Lavoisier did not terminate in his own destruction.

Lavoisier was tall, and possessed a countenance full of benignity, through which his genius shone forth conspicuous. He was mild, humane, sociable, obliging, and he displayed an incredible degree of activity. His influence was great, on account of his fortune, his reputation, and the place which he held in the treasury; but all the use which he made of it was to do good. His wife, whom he married in 1771, was Marie-Anna-Pierette-Paulze, daughter of a farmer-general, who was put to death at the same time with her husband; she herself was imprisoned, but saved by the fortunate destruction of the dictator himself, together with his abettors. It would appear that she was able to save a considerable part of her husband's fortune: she afterwards married Count Rumford, whom she survived.

Besides his volume of Physical and Chemical Essays, and his Elements of Chemistry, published in 1789, Lavoisier was the author of no fewer than sixty memoirs, which were published in the volumes of the Academy of Sciences, from 1772, to 1788, or in other periodical works of the time. I shall take a short review of the most important of these memoirs, dividing them into two parts: I. Those that are not connected with his peculiar chemical theory; II. Those which were intended to disprove the existence of phlogiston, and establish the anti-phlogistic theory.

I. I have already mentioned his paper on gypsum, published in the Memoirs of the Academy, for 1768. He proves, by very decisive experiments, that this salt is a compound of sulphuric acid, lime, and water. But this had been already done by Margraaf, in a paper inserted into the Memoirs of the Berlin Academy, for 1750, entitled "An Examination of the constituent parts of the Stones that become luminous." The most remarkable circumstance attending this paper is, that an interval of eighteen years should elapse without Lavoisier's having any knowledge of this important paper of Margraaf; yet he quotes Pott and Cronstedt, who had written on the same subject later than Margraaf, at least Cronstedt. What makes this still more singular and unaccountable is, that a French translation of Margraaf's Opuscula had been published in Paris, in the year 1762. That a man in Lavoisier's circumstances, who, as appears from his paper, had paid considerable attention to chemistry, should not have perused the writings of one of the most eminent chemists that had ever existed, when they were completely within his power, constitutes, I think, one of the most extraordinary phenomena in the history of science.

2. If a want of historical knowledge appears conspicuous in Lavoisier's first chemical paper, the same remark cannot be applied to his second paper, "On the Nature of Water, and the Experiments by which it has been attempted to prove the possibility of changing it into Earth," which was inserted in the Memoirs of the French Academy, for 1770. This memoir is divided into two parts. In the first he gives a history of the progress of opinions on the subject, beginning with Van Helmont's celebrated experiment on the willow; then relating those of Boyle, Triewald, Miller, Eller, Gleditch, Bonnet, Kraft, Alston, Wallerius, Hales, Duhamel, Stahl, Boerhaave, Geoffroy, Margraaf, and Le Roy. This first part is interesting, in an historical point of view, and gives a very complete account of the progress of opinions upon the subject from the very first dawn of scientific chemistry down to his own time. There is, it is true, a remarkable difference between the opinions of his predecessors respecting the conversion of water into earth, and the experiments of Margraaf on the composition of *selenite*. The former were inaccurate, and were recorded by him that they might be refuted; but the experiments of Margraaf were accurate, and of the same nature with his own. The second part of this memoir contains his own experiments, made with much precision, which went to show that the earth was derived from the retort in which the experiments of Margraaf were made, and that we have no proof whatever that water may be converted into earth.

But these experiments of Lavoisier, though they completely disproved the inferences that Margraaf drew from his observations, by no means demonstrated that water might not be converted into different animal and vegetable substances by the processes of digestion. Indeed there can be no doubt that this is the case, and that the oxygen and hydrogen of which it is composed, enter into the composition of by far the greater number of animal and vegetable bodies produced by the action of the functions of living animals and vegetables. We have no evidence that the carbon, another great constituent of vegetable bodies, and the carbon and azote which constitute so great a proportion of animal substances, have their origin from water. They are probably derived from the food of plants and animals, and from the atmosphere which surrounds them, and which contains both of these principles in abundance.

Whether the silica, lime, alumina, magnesia, and iron, that exist in small quantity in plants, be derived from water and the atmosphere, is a question which we are still unable to answer. But the experiments of Schrader, which gained the prize offered by the Berlin Academy, in the year 1800, for the best essay on the following subject: *To determine the earthy constituents of the different kinds of corn, and to ascertain whether these earthy parts are formed by the processes of vegetation*, show at least that we cannot account for their production in any other way. Schrader analyzed the seeds of wheat, rye, barley, and oats, and ascertained the quantity of earthy matter which each contained. He then planted these different seeds in flowers of sulphur, and in oxides of antimony and zinc, watering them regularly with distilled water. They vegetated very well. He then dried the plants, and analyzed what had been the produce of a given weight of seed, and he found that the earthy matter in each was greater than it had been in the seeds from which they sprung. Now as the sulphur and oxides of zinc and antimony could furnish no earthy matter, no other source remains but the water with which the plants were fed, and the atmosphere with which they were surrounded. It may be said, indeed, that earthy matter is always floating about in the atmosphere, and that in this way they may have obtained all the addition of these principles which they contained. This is an objection not easily obviated, and yet it would require to be obviated before the question can be considered as answered.

3. Lavoisier's next paper, inserted in the Memoirs of the Academy, for 1771, was entitled "Calculations and Observations on the Project of the establishment of a Steam-engine to supply Paris with Water." This memoir, though long and valuable, not being strictly speaking chemical, I shall pass over. Mr. Watt's improvements seem to have been unknown to Lavoisier. Indeed as his patent was only taken out in 1769, and as several years elapsed before the merits of his new steam-engine became generally known, Lavoisier's acquaintance with it in 1771 could hardly be expected.

4. In 1772 we find a paper, by Lavoisier, in the Memoirs of the Academy, "On the Use of Spirit of Wine in the analysis of Mineral Waters." He shows how the earthy muriates may be separated from the sulphates by digesting the mixed mass in alcohol. This process no doubt facilitates the separation of the salts from each other: but it is doubtful whether the method does not occasion new inaccuracies that more than compensate the facility of such separations. When different salts are dissolved in water in small quantities, it may very well

happen that they do not decompose each other, being at too great a distance from each other to come within the sphere of mutual action. Thus it is possible that sulphate of soda and muriate of lime may exist together in the same water. But if we concentrate this water very much, and still more, if we evaporate to dryness, the two salts will gradually come into the sphere of mutual action, a double decomposition will take place, and there will be formed sulphate of lime and common salt. If upon the dry residue we pour as much distilled water as was driven off by the evaporation, we shall not be able to dissolve the saline matter deposited; a portion of sulphate of lime will remain in the state of a powder. Yet before the evaporation, all the saline contents of the water were in solution, and they continued in solution till the water was very much concentrated. This is sufficient to show that the nature of the salts was altered by the evaporation. If we digest the dry residue in spirit of wine, we may dissolve a portion of muriate of lime, if the quantity of that salt in the original water was greater than the sulphate of soda was capable of decomposing: but if the quantity was just what the sulphate of soda could decompose, the alcohol will dissolve nothing, if it be strong enough, or nothing but a little common salt, if its specific gravity was above 0·820. We cannot, therefore, depend upon the salts which we obtain after evaporating a mineral water to dryness, being the same as those which existed in the mineral water itself. The nature of the salts must always be determined some other way.

5. In the Memoirs of the Academy, for 1772 (published in 1776), are inserted two elaborate papers of Lavoisier, on the combustion of the diamond. The combustibility of the diamond was suspected by Newton, from its great refractive power. His sus-

picion was confirmed in 1694, by Cosmo III., Grand Duke of Tuscany, who employed Averani and Targioni to try the effect of powerful burning-glasses upon diamonds. They were completely dissipated by the heat. Many years after, the Emperor Francis I. caused various diamonds to be exposed to the heat of furnaces. They also were dissipated, without leaving any trace behind them. M. Darcet, professor of chemistry at the Royal College of Paris, being employed with Count Lauragais in a set of experiments on the manufacture of porcelain, took the opportunity of trying what effect the intense heat of the porcelain furnaces produced upon various bodies. Diamonds were not forgotten. He found that they were completely dissipated by the heat of the furnace, without leaving any traces behind them. Darcet found that a violent heat was not necessary to volatilize diamonds. The heat of an ordinary furnace was quite sufficient. In 1771 a diamond, belonging to M. Godefroi Villetaneuse, was exposed to a strong heat by Macquer. It was placed upon a cupel, and raised to a temperature high enough to melt copper. It was observed to be surrounded with a low red flame, and to be more intensely red than the cupel. In short, it exhibited unequivocal marks of undergoing real combustion.

These experiments were soon after repeated by Lavoisier before a large company of men of rank and science. The real combustion of the diamond was established beyond doubt; and it was ascertained also, that if it be completely excluded from the air, it may be exposed to any temperature that can be raised in a furnace without undergoing any alteration. Hence it is clear that the diamond is not a volatile substance, and that it is dissipated by heat, not by being volatilized, but by being burnt.

The object of Lavoisier in his experiments was to determine the nature of the substance into which the diamond was converted by burning. In the first part he gives as usual a history of every thing which had been done previous to his own experiments on the combustion of the diamond. In the second part we have the result of his own experiments upon the same subject. He placed diamonds on porcelain supports in glass jars standing inverted over water and over mercury; and filled with common air and with oxygen gas.*

The diamonds were consumed by means of burning-glasses. No *water* or *smoke* or *soot* made their appearance, and no alteration took place on the bulk of the air when the experiments were made over mercury. When they were made over water, the bulk of the air was somewhat diminished. It was obvious from this that diamond when burnt in air or oxygen gas, is converted into a gaseous substance, which is absorbed by water. On exposing air in which diamond had been burnt, to lime-water, a portion of it was absorbed, and the lime-water was rendered milky. From this it became evident, that when diamond is burnt, *carbonic acid* is formed, and this was the only product of the combustion that could be discovered.

Lavoisier made similar experiments with charcoal, burning it in air and oxygen gas, by means of a burning-glass. The results were the same: carbonic acid gas was formed in abundance, and nothing else. These experiments might have been employed to support and confirm Lavoisier's peculiar theory, and they were employed by him for that purpose afterwards. But when they were originally pub-

---

* The reader will bear in mind that though the memoir was inserted in the Mem. de l'Acad., for 1772, it was in fact published in 1776, and the experiments were made in 1775 and 1776.

lished, no such intention appeared evident; though doubtless he entertained it.

6. In the second volume of the Journal de Physique, for 1772, there is a short paper by Lavoisier on the conversion of water into ice. M. Desmarets had given the academy an account of Dr. Black's experiments, to determine the latent heat of water. This induced Lavoisier to relate his experiments on the same subject. He does not inform us whether they were made in consequence of his having become acquainted with Dr. Black's theory, though there can be no doubt that this must have been the case. The experiments related in this short paper are not of much consequence. But I have thought it worth while to notice it because it authenticates a date at which Lavoisier was acquainted with Dr. Black's theory of latent heat.

7. In the third volume of the Journal de Physique, there is an account of a set of experiments made by Bourdelin, Malouin, Macquer, Cadet, Lavoisier, and Baumé on the *white-lead ore* of Pullowen. The report is drawn up by Baumé. The nature of the ore is not made out by these experiments. They were mostly made in the dry way, and were chiefly intended to show that the ore was not a chloride of lead. It was most likely a phosphate of lead.

8. In the Memoirs of the Academy, for 1774, we have the experiments of Trudaine, de Montigny, Macquer, Cadet, Lavoisier, and Brisson, with the great burning-glass of M. Trudaine. The results obtained cannot be easily abridged, and are not of sufficient importance to be given in detail.

9. Analysis of some waters brought from Italy by M. Cassini, junior. This short paper appeared in the Memoirs of the Academy, for 1777. The waters in question were brought from alum-pits,

and were found to contain alum and sulphate of iron.

10. In the same volume of the Memoirs of the Academy, appeared his paper "On the Ash employed by the Saltpetre-makers of Paris, and on its use in the Manufacture of Saltpetre." This is a curious and valuable paper; but not sufficiently important to induce me to give an abstract of it here.

11. In the Memoirs of the Academy, for 1777, appeared an elaborate paper, by Lavoisier, "On the Combination of the matter of Fire, with Evaporable Fluids, and the Formation of Elastic aeriform Fluids." In this paper he adopts precisely the same theory as Dr. Black had long before established. It is remarkable that the name of Dr. Black never occurs in the whole paper, though we have seen that Lavoisier had become acquainted with the doctrine of latent heat, at least as early as the year 1772, as he mentioned the circumstance in a short paper inserted that year in the Journal de Physique, and previously read to the academy.

12. In the same volume of the Memoirs of the Academy, we have a paper entitled "Experiments made by Order of the Academy, on the Cold of the year 1775, by Messrs. Bezout, Lavoisier, and Vandermond." It is sufficiently known that the beginning of the year 1776 was distinguished in most parts of Europe by the weather. The object of this paper, however, is rather to determine the accuracy of the different thermometers at that time used in France, than to record the lowest temperature which had been observed. It has some resemblance to a paper drawn up about the same time by Mr. Cavendish, and published in the Philosophical Transactions.

13. In the Memoirs of the Academy, for 1778, appeared a paper entitled "Analysis of the Waters of the Lake Asphaltes, by Messrs. Macquer, Lavoi

sier, and Sage." This water is known to be saturated with *salt*. It is needless to state the result of the analysis contained in this paper, because it is quite inaccurate. Chemical analysis had not at that time made sufficient progress to enable chemists to analyze mineral waters with precision.

The observation of Lavoisier and Guettard, which appeared at the same time, on a species of steatite, which is converted by the fire into a fine biscuit of porcelain, and on two coal-mines, the one in Franche-Comté, the other in Alsace, do not require to be particularly noticed.

14. In the Mem. de l'Académie, for 1780 (published in 1784), we have a paper, by Lavoisier, "On certain Fluids which may be obtained in an aeriform State, at a degree of Heat not much higher than the mean Temperature of the Earth." These fluids are sulphuric ether, alcohol, and water. He points out the boiling temperature of these liquids, and shows that at that temperature the vapour of these bodies possesses the elasticity of common air, and is permanent as long as the high temperature continues. He burnt a mixture of vapour of ether and oxygen gas, and showed that during the combustion carbonic acid gas is formed. Lavoisier's notions respecting these vapours, and what hindered the liquids at the boiling temperature from being all converted into vapour were not quite correct. Our opinions respecting steam and vapours in general were first rectified by Mr. Dalton.

15. In the Mem. de l'Académie, for 1780, appeared also the celebrated paper on *heat*, by Lavoisier and Laplace. The object of this paper was to determine the specific heat of various bodies, and to investigate the proposals that had been made by Dr. Irvine for determining the point at which a thermometer would stand, if plunged into a body destitute of heat. This point is usually called the real zero.

They begin by describing an instrument which they had contrived to measure the quantity of heat which leaves a body while it is cooling a certain number of degrees. To this instrument they gave the name of *calorimeter.* It consisted of a kind of hollow, surrounded on every side by ice. The hot body was put into the centre. The heat which it gave out while cooling was all expended in melting the ice, which was of the temperature of 32°, and the quantity of heat was proportional to the quantity of ice melted. Hence the quantity of ice melted, while equal weights of hot bodies were cooling a certain number of degrees, gave the direct ratios of the specific heats of each. In this way they obtained the following specific heats:

	Specific heat.
Water	1
Sheet-iron	0·109985
Glass without lead (crystal)	0·1929
Mercury	0·029
Quicklime	0·21689
Mixture of 9 water with 16 lime	0·439116
Sulphuric acid of 1·87058	0·334597
4 sulphuric acid, 3 water	0·603162
4 sulphuric acid, 5 water	0·663102
Nitric acid of 1·29895	0·661391
9⅓ nitric acid, 1 lime	0·61895
1 saltpetre, 8 water	0·8167

Their experiments were inconsistent with the conclusions drawn by Dr. Irvine, respecting the real zero, from the diminution of the specific heat, and the heat evolved when sulphuric acid was mixed with various proportions of water, &c. If the experiments of Lavoisier and Laplace approached nearly to accuracy, or, indeed, unless they were quite inaccurate, it is obvious that the conclusions of Irvine must be quite erroneous. It is remarkable that though the experiments of Crawford, and likewise those of Wilcke, and of several others, on specific heat had been published before this paper made its appearance, no allusion whatever is made to these publications. Were we to trust to the information communicated in the paper, the doctrine of specific heat originated with Lavoisier and Laplace. It is true that in the fourth part of the paper, which treats of combustion and respiration, Dr. Crawford's theory of animal heat is mentioned, showing clearly that our authors were acquainted with his book on the subject. And, as this theory is founded on the different specific heats of bodies, there could be no doubt that he was acquainted with that doctrine.

16. In the Mem. de l'Académie, for 1780, occur the two following memoirs:

Report made to the Royal Academy of Sciences on the Prisons. By Messrs. Duhamel, De Montigny, Le Roy, Tenon, Tillet, and Lavoisier.

Report on the Process for separating Gold and Silver. By Messrs. Macquer, Cadet, Lavoisier, Baumé, Cornette, and Berthollet.

17. In the Mem. de l'Académie, for 1781, we find a memoir by Lavoisier and Laplace, on the electricity evolved when bodies are evaporated or sublimed. The result of these experiments was, that when water was evaporated electricity was always evolved. They concluded from these observations, that whenever a body changes its state electricity is always evolved. But when Saussure attempted to repeat these observations, he could not succeed. And, from the recent experiments of Pouillet, it seems to follow that electricity is evolved only when bodies undergo chemical decomposition or combination. Such experiments depend so much upon very minute circumstances, which are apt to escape the attention of the observer, that implicit confidence

cannot be put in them till they have been often repeated, and varied in every possible manner.

18. In the Memoires de l'Académie, for 1781, there is a paper by Lavoisier on the comparative value of the different substances employed as articles of fuel. The substances compared to each other are pit-coal, coke, charcoal, and wood. It would serve no purpose to state the comparison here, as it would not apply to this country; nor, indeed, would it at present apply even to France.

We have, in the same volume, his paper on the mode of illuminating theatres.

19. In the Memoires de l'Académie, for 1782 (printed in 1785), we have a paper by Lavoisier on a method of augmenting considerably the action of fire and of heat. The method which he proposes is a jet of oxygen gas, striking against red-hot charcoal. He gives the result of some trials made in this way. Platinum readily melted. Pieces of ruby or sapphire were softened sufficiently to run together into one stone. Hyacinth lost its colour, and was also softened. Topaz lost its colour, and melted into an opaque enamel. Emeralds and garnets lost their colour, and melted into opaque coloured glasses. Gold and silver were volatilized; all the other metals, and even the metallic oxides, were found to burn. Barytes also burns when exposed to this violent heat. This led Lavoisier to conclude, as Bergman had done before him, that Barytes is a metallic oxide. This opinion has been fully verified by modern chemists. Both silica and alumina were melted. But he could not fuse lime nor magnesia. We are now in possession of a still more powerful source of heat in the oxygen and hydrogen blowpipe, which is capable of fusing both lime and magnesia, and, indeed, every substance which can be raised to the requisite heat without burning

or being volatilized. This subject was prosecuted still further by Lavoisier in another paper inserted in a subsequent volume of the Memoires de l'Académie. He describes the effect on rock-crystal, quartz, sandstone, sand, phosphorescent quartz, milk quartz, agate, chalcedony, cornelian, flint, prase, nephrite, jasper, felspar, &c.

20. In the same volume is inserted a memoir "On the Nature of the aeriform elastic Fluids which are disengaged from certain animal Substances in a state of Fermentation." He found that a quantity of recent human fæces, amounting to about five cubic inches, when kept at a temperature approaching to 60° emitted, every day for a month, about half a cubic inch of gas. This gas was a mixture of eleven parts carbonic acid gas, and one part of an inflammable gas, which burnt with a blue flame, and was therefore probably carbonic oxide. Five cubic inches of old human fæces from a necessary kept in the same temperature, during the first fifteen days emitted about a third of a cubic inch of gas each day, and during each of the second fifteen days, about one fourth of a cubic inch. This gas was a mixture of thirty-eight volumes of carbonic acid gas, and sixty-two volumes of a combustible gas, burning with a blue flame, and probably carbonic oxide.

Fresh fæces do not effervesce with dilute sulphuric acid, but old moist fæces do, and emit about eight times their volume of carbonic acid gas. Quicklime, or caustic potash, mixed with fæces, puts a stop to the evolution of gas, doubtless by preventing all fermentation. During effervescence of fæcal matter the air surrounding it is deprived of a little of its oxygen, probably in consequence of its combining with the nascent inflammable gas which is slowly disengaged.

11. We come now to the new theory of combustion

of which Lavoisier was the author, and upon which his reputation with posterity will ultimately depend. Upon this subject, or at least upon matters more or less intimately connected with it, no fewer than twenty-seven memoirs of his, many of them of a very elaborate nature, and detailing expensive and difficult experiments, appeared in the different volumes of the academy between 1774 and 1788. The analogy between the combustion of bodies and the calcination of metals had been already observed by chemists, and all admitted that both processes were owing to the same cause; namely, the emission of *phlogiston* by the burning or calcining body. The opinion adopted by Lavoisier was, that during burning and calcination nothing whatever left the bodies, but that they simply united with a portion of the air of the atmosphere. When he first conceived this opinion he was ignorant of the nature of atmospheric air, and of the existence of oxygen gas. But after that principle had been discovered, and shown to be a constituent of atmospherical air, he soon recognised that it was the union of oxygen with the burning and calcining body that occasioned the phenomena. Such is the outline of the Lavoisierian theory stated in the simplest and fewest words. It will be requisite to make a few observations on the much-agitated question whether this theory originated with him.

It is now well known that John Rey, a physician at Bugue, in Perigord, published a book in 1630, in order to explain the cause of the increase of weight which lead and tin experience during their calcination. After refuting in succession all the different explanations of this increase of weight which had been advanced, he adds, " To this question, then, supported on the grounds already mentioned, I answer, and maintain with confidence, that the increase of weight arises from the air, which is condensed, rendered heavy and adhesive by the violent and long-continued heat of the furnace. This air mixes itself with the calx (frequent agitation conducing), and attaches itself to the minutest molecules, in the same manner as water renders heavy sand which is agitated with it, and moistens and adheres to the smallest grains." There cannot be the least doubt from this passage that Rey's opinion was precisely the same as the original one of Lavoisier, and had Lavoisier done nothing more than merely state in general terms that during calcination air unites with the calcining bodies, it might have been suspected that he had borrowed his notions from those of Rey. But the discovery of oxygen, and the numerous and decisive proofs which he brought forward that during burning and calcination oxygen unites with the burning and calcining body, and that this oxygen may be again separated and exhibited in its original elastic state oblige us to alter our opinion. And whether we admit that he borrowed his original notion from Rey, or that it suggested itself to his own mind, the case will not be materially altered. For it is not the man who forms the first vague notion of a thing that really adds to the stock of our knowledge, but he who demonstrates its truth and accurately determines its nature.

Rey's book and his opinions were little known. He had not brought over a single convert to his doctrine, a sufficient proof that he had not established it by satisfactory evidence. We may therefore believe Lavoisier's statement, when he assures us that when he first formed his theory he was ignorant of Rey, and never had heard that any such book had been published.

The theory of combustion advanced by Dr. Hook, in 1665, in his Micrographia, approaches still nearer to that of Lavoisier than the theory of Rey, and indeed, so far as he has explained it, the coincidence is exact. According to Hook there exists in common air a certain substance which is like, if not the very same with that which is fixed in saltpetre. This substance has the property of dissolving all combustibles; but only when their temperature is sufficiently raised. The solution takes place with such rapidity that it occasions fire, which in his opinion is mere *motion*. The dissolved substance may be in the state of air, or coagulated in a liquid or solid form. The quantity of this solvent in a given bulk of air is incomparably less than in the same bulk of saltpetre. Hence the reason why a combustible continues burning but a short time in a given bulk of air: the solvent is soon saturated, and then of course the combustion is at an end. This explains why combustion requires a constant supply of fresh air, and why it is promoted by forcing in air with bellows. Hook promised to develop this theory at greater length in a subsequent work; but he never fulfilled his promise; though in his Lampas, published about twelve years afterwards, he gives a beautiful chemical explanation of flame, founded on the very same theory.

From the very general terms in which Hook expresses himself, we cannot judge correctly of the extent of his knowledge. This theory, so far as it goes, coincides exactly with our present notions on the subject. His solvent is oxygen gas, which constitutes one-fifth part of the volume of the air, but exists in much greater quantity in saltpetre. It combines with the burning body, and the compound formed may either be a gas, a liquid, or a solid, according to the nature of the body subjected to combustion.

Lavoisier nowhere alludes to this theory of Hook nor gives the least hint that he had ever heard of it. This is the more surprising, because Hook was a man of great celebrity; and his Micrographia, as containing the original figures and descriptions of many natural objects, is well known, not merely in Great Britain, but on the continent. At the same time it must be recollected that Hook's theory is supported by no evidence; that it is a mere assertion, and that nobody adopted it. Even then, if we were to admit that Lavoisier was acquainted with this theory, it would derogate very little from his merit, which consisted in investigating the phenomena of combustion and calcination, and in showing that oxygen became a constituent of the burnt and calcined bodies.

About ten years after the publication of the Micrographia, Dr. Mayow, of Oxford, published his Essays. In the first of which, De Sal-nitro et Spiritu Nitro-aëreo, he obviously adopts Dr. Hook's theory of combustion, and he applies it with great ingenuity to explain the nature of respiration. Dr. Mayow's book had been forgotten when the attention of men of science was attracted to it by Dr. Beddoes. Dr. Yeats, of Bedford, published a very interesting work on the merits of Mayow, in 1798. It will be admitted at once by every person who takes the trouble of perusing Mayow's tract, that he was not satisfied with mere theory; but proved by actual experiment that air was absorbed during combustion, and altered during respiration. He has given figures of his apparatus, and they are very much of the same nature with those afterwards made use of by Lavoisier. It would be wrong, therefore, to deprive Mayow of the reputation to which he is entitled for his ingeniously-contrived and well-executed experiments. It must be admitted that he proved both the absorption of air during combustion and respiration; but even this does not take much from the fair fame of Lavoisier. The analysis of air and the discovery of oxygen gas really diminish the analogy between the theories of Mayow and Lavoisier, or at any rate the full investigation of the subject and the generalization of it belong exclusively to Lavoisier.

Attempts were made by the other French chemists, about the beginning of the revolution, to associate themselves with Lavoisier, as equally entitled with himself to the merit of the antiphlogistic theory; but Lavoisier himself has disclaimed the partnership. Some years before his death, he had formed the plan of collecting together all his papers relating to the antiphlogistic theory and publishing them in one work; but his death interrupted the project. However, his widow afterwards published the first two volumes of the book, which were complete at the time of his death. In one of these volumes Lavoisier claims for himself the exclusive discovery of the cause of the augmentation of weight which bodies undergo during combustion and calcination. He informs us that a set of experiments, which he made in 1772, upon the different kinds of air which are disengaged in effervescence, and a great number of other chemical operations discovered to him demonstratively the cause of the augmentation of weight which metals experience when exposed to heat. "I was young," says he, "I had newly entered the lists of science, I was desirous of fame, and I thought it necessary to take some steps to secure to myself the property of my discovery. At that time there existed an habitual correspondence between the men of science of France and those of England. There was a kind of rivality between the two nations, which gave importance to new experiments, and which sometimes was the cause that the writers of the one or the other of the nations disputed the discovery with the real author. Consequently, I thought it proper to deposit on the 1st of November, 1772, the following note in the hands of the secretary of the academy. This note was opened on the 1st of May following, and mention of these circumstances marked at the top of the note. It was in the following terms:

"About eight days ago I discovered that sulphur in burning, far from losing, augments in weight; that is to say, that from one pound of sulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air. Phosphorus presents the same phenomenon. This augmentation of weight arises from a great quantity of air, which becomes fixed during the combustion, and which combines with the vapours.

"This discovery, which I confirmed by experiments which I regard as decisive, led me to think that what is observed in the combustion of sulphur and phosphorus, might likewise take place with respect to all the bodies which augment in weight by combustion and calcination; and I was persuaded that the augmentation of weight in the calces of metals proceeded from the same cause. The experiment fully confirmed my conjectures. I operated the reduction of litharge in close vessels with Hales's apparatus, and I observed, that at the moment of the passage of the calx into the metallic state, there was a disengagement of air in considerable quantity, and that this air formed a volume at least one thousand times greater than that of the litharge employed. As this discovery appears to me one of the most interesting which has been made since Stahl, I thought it expedient to secure to myself the property, by depositing the present note in the hands of the secretary of the academy, to remain secret till the period when I shall publish my experiments. "LAVOISIER.

"*Paris, November* 11, 1772."

This note leaves no doubt that Lavoisier had conceived his theory, and confirmed it by experiment, at least as early as November, 1772. But at that time the nature of air and the existence of oxygen were unknown. The theory, therefore, as he understood it at that time, was precisely the same as that of John Rey. It was not till the end of 1774 that his views became more precise, and that he was aware that oxygen is the portion of the air which unites with bodies during combustion, and calcination.

Nothing can be more evident from the whole history of the academy, and of the French chemists during this eventful period, for the progress of the science, that none of them participated in the views of Lavoisier, or had the least intention of giving up the phlogistic theory. It was not till 1785, after his experiments had been almost all published, and after all the difficulties had been removed by the two great discoveries of Mr. Cavendish, that Berthollet declared himself a convert to the Lavoisierian opinions. This was soon followed by others, and within a very few years almost all the chemists and men of science in France enlisted themselves on the same side. Lavoisier's objection, then, to the phrase *La Chimie Française*, is not without reason, the term *Lavoisierian Chemistry* should undoubtedly be substituted for it. This term, *La Chimie Française* was introduced by Fourcroy. Was Fourcroy anxious to clothe himself with the reputation of Lavoisier, and had this any connexion with the violent death of that illustrious man?

The first set of experiments which Lavoisier published on his peculiar views, was entitled, "A Memoir on the Calcination of Tin in close Vessels; and on the Cause of the increase of Weight which the Metal acquires during this Process." It appeared in the Memoirs of the Academy, for 1774. In this paper he gives an account of several experiments which he had made on the calcination of tin in glass retorts, hermetically sealed. He put a quantity of tin (about half a pound) into a glass retort, sometimes of a larger and sometimes of a smaller size, and then drew out the beak into a capillary tube. The retort was now placed upon the sand-bath, and heated till the tin just melted. The extremity of the capillary beak of the retort was now fused so as to seal it hermetically. The object of this heating was to prevent the retort from bursting by the expansion of the air during the process. The retort, with its contents, was now carefully weighed, and the weight noted. It was put again on the sand-bath, and kept melted till the process of calcination refused to advance any further. He observed, that if the retort was small, the calcination always stopped sooner than it did if the retort was large. Or, in other words, the quantity of tin calcined was always proportional to the size of the retort.

After the process was finished, the retort (still hermetically sealed) was again weighed, and was always found to have the same weight exactly as at first. The beak of the retort was now broken off, and a quantity of air entered with a hissing noise. The increase of weight was now noted: it was obviously owing to the air that had rushed in. The weight of air that had been at first driven out by the fusion of the tin had been noted, and it was now found that a considerably greater quantity had entered than had been driven out at first. In some experiments, as much as 10·06 grains, in others 9·87 grains, and in some less than this, when the size of the retort was small. The tin in the retort was mostly unaltered, but a portion of it had been converted into a black powder, weighing in some cases above two ounces. Now it was found in all cases, that the weight of the tin had increased, and the increase of weight was always exactly equal to the diminution of weight which the air in the retort had undergone, measured by the quantity of new air which rushed in when the beak of the retort was broken, minus the air that had been driven out when the tin was originally melted before the retort was hermetically sealed.

Thus Lavoisier proved by these first experiments, that when tin is calcined in close vessels a portion of the air of the vessel disappears, and that the tin increases in weight just as much as is equivalent to the loss of weight which the air has sustained. He therefore inferred, that this portion of air had united with the tin, and that calx of tin is a compound of tin and air. In this first paper there is nothing said about oxygen, nor any allusion to lead to the suspicion that air is a compound of different elastic fluids. These, therefore, were probably the experiments to which Lavoisier alludes in the note which he lodged with the secretary of the academy in November, 1772.

He mentions towards the end of the Memoir that he had made similar experiments with lead; but he does not communicate any of the numerical results: probably because the results were not so striking as those with tin. The heat necessary to melt lead is so high that satisfactory experiments on its calcination could not easily be made in a glass retort.

Lavoisier's next Memoir appeared in the Memoirs of the Academy, for 1775, which were published in 1778. It is entitled, " On the Nature of the Principle which combines with the Metals during their Calcination, and which augments their Weight." He observes that when the metallic calces are reduced to the metallic state it is found necessary to heat them along with charcoal. In such cases a quantity of carbonic acid gas is driven off, which he assures us is the charcoal united to the elastic fluid contained in the calx. He tried to reduce the calx of iron by means of burning-glasses, while placed under large glass receivers standing over mercury; but as the gas thus evolved was mixed with a great deal of common air which was necessarily left in the receiver, he was unable to determine its nature. This induced him to have recourse to red oxide of mercury. He showed in the first place that this substance (*mercurius præcipitatus per se*) was a true calx, by mixing it with charcoal powder in a retort and heating it. The mercury was reduced and abundance of carbonic acid gas was collected in an inverted glass jar standing in a water-cistern into which the beak of the retort was plunged. On heating the red oxide of mercury by itself it was reduced to the metallic state, though not so easily, and at the same time a gas was evolved which possessed the following properties:

1. It did not combine with water by agitation.
2. It did not precipitate lime-water.
3. It did not unite with fixed or volatile alkalies.
4. It did not at all diminish their caustic quality.
5. It would serve again for the calcination of metals.
6. It was diminished like common air by addition of one-third of nitrous gas.
7. It had none of the properties of carbonic acid gas. Far from being fatal, like that gas, to animals, it seemed on the contrary more proper for the purposes of respiration. Candles and burning bodies were

not only not extinguished by it, but burned with an enlarged flame in a very remarkable manner. The light they gave was much greater and clearer than in common air.

He expresses his opinion that the same kind of air would be obtained by heating nitre without addition, and this opinion is founded on the fact that when nitre is detonated with charcoal it gives out abundance of carbonic acid gas.

Thus Lavoisier shows in this paper that the kind of air which unites with metals during their calcination is purer and fitter for combustion than common air. In short it is the gas which Dr. Priestley had discovered in 1774, and which is now known by the name of oxygen gas.

This Memoir deserves a few animadversions. Dr. Priestley discovered oxygen gas in August, 1774; and he informs us in his life, that in the autumn of that year he went to Paris and exhibited to Lavoisier, in his own laboratory the mode of obtaining oxygen gas by heating red oxide of mercury in a gun-barrel, and the properties by which this gas is distinguished—indeed the very properties which Lavoisier himself enumerates in his paper. There can, therefore, be no doubt that Lavoisier was acquainted with oxygen gas in 1774, and that he owed his knowledge of it to Dr. Priestley.

There is some uncertainty about the date of Lavoisier's paper. In the History of the Academy, for 1775, it is merely said about it, "Read at the resumption (*rentrée*) of the Academy, on the 26th of April, by M. Lavoisier," without naming the year. But it could not have been before 1775, because that is the year upon the volume of the Memoirs; and besides, we know from the Journal de Physique (v. 429), that 1775 was the year on which the paper of Lavoisier was read.

Yet in the whole of this paper the name of Dr. Priestley never occurs, nor is the least hint given that he had already obtained oxygen gas by heating red oxide of mercury. So far from it, that it is obviously the intention of the author of the paper to induce his readers to infer that he himself was the discoverer of oxygen gas. For after describing the process by which oxygen gas was obtained by him, he says nothing further remained but to determine its nature, and "I discovered with *much surprise* that it was not capable of combination with water by agitation," &c. Now why the expression of surprise in describing phenomena which had been already shown? And why the omission of all mention of Dr. Priestley's name? I confess that this seems to me capable of no other explanation than a wish to claim for himself the discovery of oxygen gas, though he knew well that that discovery had been previously made by another.

The next set of experiments made by Lavoisier to confirm or extend his theory, was "On the Combustion of Phosphorus, and the Nature of the Acid which results from that Combustion." It appeared in the Memoirs of the Academy, for 1777. The result of these experiments was very striking. When phosphorus is burnt in a given bulk of air in sufficient quantity, about four-fifths of the volume of the air disappears and unites itself with the phosphorus. The residual portion of the air is incapable of supporting combustion or maintaining animal life. Lavoisier gave it the name of *mouffette atmospherique*, and he describes several of its properties. The phosphorus by combining with the portion of air which has disappeared, is converted into phosphoric acid, which is deposited on the inside of the receiver in which the combustion is performed, in the state of fine white flakes. One grain by this process is converted into two and a half grains of phosphoric acid. These observations led to the conclusion that atmospheric air is a mixture or compound of two distinct gases, the one (*oxygen*) absorbed by burning phosphorus, the other (*azote*) not acted on by that principle, and not capable of uniting with or calcining metals. These conclusions had already been drawn by Scheele from similar experiments, but Lavoisier was ignorant of them.

In the second part of this paper, Lavoisier describes the properties of phosphoric acid, and gives an account of the salts which it forms with the different bases. The account of these salts is exceedingly imperfect, and it is remarkable that Lavoisier makes no distinction between phosphate of potash and phosphate of soda; though the different properties of these two salts are not a little striking. But these were not the investigations in which Lavoisier excelled.

The next paper in which the doctrines of the antiphlogistic theory were still further developed, was inserted in the Memoirs of the Academy, for 1777. It is entitled, " On the Combustion of Candles in atmospherical Air, and in Air eminently Respirable." This paper is remarkable, because in it he first notices Dr. Priestley's discovery of oxygen gas; but without any reference to the preceding paper, or any apology for not having alluded in it to the information which he had received from Dr. Priestley.

He begins by saying that it is necessary to distinguish four different kinds of air. 1. Atmospherical air in which we live, and which we breath. 2. Pure air *(oxygen)*, alone fit for breathing, constituting about the fourth of the volume of atmospherical air, and called by Dr. Priestley *dephlogisticated air*. 3. Azotic gas, which constitutes about three-fourths of the volume of atmospherical air, and whose properties are still unknown. 4. Fixed air, which he proposed to call (as Bucquet had done) *acide crayeux, acid of chalk.*

In this paper Lavoisier gives an account of a great many trials that he made by burning candles in given volumes of atmospherical air and oxygen gas enclosed in glass receivers, standing over mercury. The general conclusion which he deduces from these experiments are—that the azotic gas of the air contributes nothing to the burning of the candle; but the whole depends upon the oxygen gas of the air, constituting in his opinion one-fourth of its volume; that during the combustion of a candle in a given volume of air only two-fifths of the oxygen are converted into carbonic acid gas, while the remaining three-fifths remain unaltered; but when the combustion goes on in oxygen gas a much greater proportion (almost the whole) of this gas is converted into carbonic acid gas. Finally, that phosphorus, when burnt in air acts much more powerfully on the oxygen of the air than a lighted candle, absorbing four-fifths of the oxygen and converting it into phosphoric acid.

It is evident that at the time this paper was written, Lavoisier's theory was nearly complete. He considered air as a mixture of three volumes of azotic gas, and one volume of oxygen gas. The last alone was concerned in combustion and calcination. During these processes a portion of the oxygen united with the burning body, and the compound formed constituted the acid or the calx. Thus he was able to account for combustion and calcination without having recourse to phlogiston. It is true that several difficulties still lay in his way, which he was not yet able to obviate, and which prevented any other person from adopting his opinions. One of the greatest of these was the fact that hydrogen gas was evolved during the solution of several metals in dilute sulphuric or muriatic acid; that by this solution these metals were converted into calces, and that calces, when heated in hydrogen gas, were reduced to the metallic state while the hydrogen disappeared. The simplest explanation of these phenomena was the one adopted by chemists at the time. Hydrogen was considered as phlogiston. By dissolving metals in acids, the phlogiston was driven off and the calx remained: by heating the calx in hydrogen, the phlogiston was again absorbed and the calx reduced to the metallic state.

This explanation was so simple and appeared so satisfactory, that it was universally adopted by chemists with the exception of Lavoisier himself. There was a circumstance, however, which satisfied him that this explanation, however plausible, was not correct. The calx was *heavier* than the metal from which it had been produced. And hydrogen, though a light body, was still possessed of weight. It was obviously impossible, then, that the metal could be a combination of the calx and hydrogen. Besides, he had ascertained by direct experiment, that the calces of mercury, tin, and lead are compounds of the respective metals and oxygen. And it was known that when the other calces were heated with charcoal, they were reduced to the metallic state, and at the same time carbonic acid gas is evolved. The very same evolution takes place when calces of mercury, tin, and lead, are heated with charcoal powder. Hence the inference was obvious that carbonic acid is a compound of charcoal and oxygen, and therefore that all calces are compounds of their respective metals and oxygen.

Thus, although Lavoisier was unable to account for the phenomena connected with the evolution and absorption of hydrogen gas, he had conclusive evidence that the orthodox explanation was not the true one. He wisely, therefore, left it to time to throw light upon those parts of the theory that were still obscure.

His next paper, which was likewise inserted in the Memoirs of the Academy, for 1777, had some tendency to throw light on this subject, or at least it elucidated the constitution of sulphuric acid, which bore directly upon the antiphlogistic theory. It was entitled, " On the Solution of Mercury in vitriolic Acid, and on the Resolution of that Acid into aeriform sulphurous Acid, and into Air eminently Respirable."

He had already proved that sulphuric acid is a compound of sulphur and oxygen; and had even shown how the oxygen which the acid contained might be again separated from it, and exhibited in a separate state. Dr. Priestley had by this time made known the method of procuring sulphurous acid gas, by heating a mixture of mercury and sulphuric acid in a phial. This was the process which Lavoisier analyzed in the present paper. He put into a retort a mixture of four ounces mercury and six ounces concentrated sulphuric acid. The beak of the retort was plunged into a mercurial cistern, to collect the sulphurous acid gas as it was evolved; and heat being applied to the belly of the retort, sulphurous acid gas passed over in abundance, and sulphate of mercury was formed. The process was continued till the whole liquid contents of the retort had disappeared: then a strong heat was applied to the salt. In the first place, a quantity of sulphurous acid gas passed over, and lastly a portion of oxygen gas. The quicksilver was reduced to the metallic state. Thus he resolved sulphuric acid into sulphurous acid and oxygen. Hence it followed as a consequence, that sulphurous acid differs from sulphuric merely by containing a smaller quantity of oxygen.

The object of his next paper, published at the same time, was to throw light upon the pyrophorus of Homberg, which was made by kneading alum into a cake, with flour, or some substance containing abundance of carbon, and then exposing the mixture to a strong heat in close vessels, till it ceased to give out smoke. It was known that a pyrophorus thus formed takes fire of its own accord, and burns when it comes in contact with common air. It will not be necessary to enter into a minute analysis of this paper, because, though the experiments were very carefully made, yet it was impossible, at the time when the paper was drawn, to elucidate the phenomena of this pyrophorus in a satisfactory manner. There can be little doubt that the pyrophorus owes its property of catching fire, when in contact with air or oxygen, to a little potassium, which has been reduced to the metallic state by the action of the charcoal and sulphur on the potash in the alum. This substance taking fire, heat enough is produced to set fire to the carbon and sulphur which the pyrophorus contains. Lavoisier ascertained that during its combustion a good deal of carbonic acid was generated.

There appeared likewise another paper by Lavoisier, in the same volume of the academy, which may be mentioned, as it served still further to demonstrate the truth of the antiphlogistic theory. It is entitled, " On the Vitriolization of Martial Pyrites." Iron pyrites is known to be a compound of *iron* and *sulphur*. Sometimes this mineral may be left exposed to the air without undergoing any alteration, while at other times it speedily splits, effloresces, swells, and is converted into sulphate of iron. There are two species of pyrites; the one composed of two atoms of sulphur and one atom of iron, the other of one atom of sulphur and one atom of iron. The first of these is called bisulphuret of iron; the second protosulphuret, or simply sulphuret of iron. The variety of pyrites which undergoes spontaneous decomposition in the air, is known to be a compound, or rather mixture of the two species of pyrites.

Lavoisier put a quantity of the decomposing pyrites under a glass jar, and found that the process went on just as well as in the open air. He found that the air was deprived of the whole of its oxygen by the process, and that nothing was left but azotic gas. Hence the nature of the change became evident. The sulphur, by uniting with oxygen, was converted into sulphuric acid, while the iron became oxide of iron, and both uniting, formed sulphate of iron. There are still some difficulties connected with this change that require to be elucidated.

We have still another paper by Lavoisier, bearing on the antiphlogistic theory, published in the same volume of the Memoirs of the Academy, for 1778, entitled, "On Combustion in general." He establishes that the only air capable of supporting combustion is oxygen gas: that during the burning of bodies in common air, a portion of the oxygen of the atmosphere disappears, and unites with the burning body, and that the new compound formed is either an acid or a metallic calx. When sulphur is burnt, sulphuric acid is formed; when phosphorus, phosphoric acid; and when charcoal, carbonic acid. The calcination of metals is a process analogous to combustion, differing chiefly by the slowness of the process: indeed when it takes place rapidly, actual combustion is produced. After establishing these general principles, which are deduced from his preceding papers, he proceeds to examine the Stahlian theory of phlogiston, and shows that no evidence of the existence of any such principle can be adduced, and that the phenomena can all be explained without having recourse to it. Powerful as these arguments were, they produced no immediate effects. Nobody chose to give up the phlogistic theory to which he had been so long accustomed.

The next two papers of Lavoisier require merely to be mentioned, as they do not bear immediately upon the antiphlogistic theory. They appeared in the Memoirs of the Academy, for 1780. These memoirs were,

1. Second Memoir on the different Combinations of Phosphoric Acid.

2. On a particular Process, by means of which Phosphorus may be converted into phosphoric Acid, without Combustion.

The process here described consisted in throwing phosphorus, by a few grains at a time, into warm nitric acid of the specific gravity 1·29895. It falls to the bottom like melted wax, and dissolves pretty rapidly with effervescence: then another portion is thrown in, and the process is continued till as much phosphorus has been employed as is wanted; then the phosphoric acid may be obtained pure by distilling off the remaining nitric acid with which it is still mixed.

Hitherto Lavoisier had been unable to explain the anomalies respecting hydrogen gas, or to answer the objections urged against his theory in consequence of these anomalies. He had made several attempts to discover what peculiar substance was formed during the combustion of hydrogen, but always without success: at last, in 1783, he resolved to make the experiment upon so large a scale, that whatever the product might be, it should not escape him; but Sir Charles Blagden, who had just gone to Paris, informed him that the experiment for which he was preparing had already been made by Mr. Cavendish, who had ascertained that the product of the combustion of hydrogen was *water*. Lavoisier saw at a glance the vast importance of this discovery for the establishment of the antiphlogistic theory, and with what ease it would enable him to answer all the plausible objections which had been brought forward against his opinions in consequence of the evolution of hydrogen, when metals were calcined by solution in acids, and the absorption of it when metals were reduced in an atmosphere of this gas. He therefore resolved to repeat the experiment of Cavendish with every possible care, and upon a scale sufficiently large to prevent ambiguity. The experiment was made on the 24th of June, 1783, by Lavoisier and Laplace, in the presence of M. Le Roi, M. Vandermonde, and Sir Charles Blagden, who was at that time secretary of the Royal Society. The quantity of water formed was considerable, and they found that water was a compound of

1 volume oxygen
1·91 volume hydrogen.

Not satisfied with this, he soon after made another experiment along with M. Meusnier to decompose water. For this purpose a porcelain tube, filled with iron wire, was heated red-hot by being passed through a furnace, and then the steam of water was made to traverse the red-hot wire. To the further extremity of the porcelain tube a glass tube was luted, which terminated in a water-trough under an inverted glass receiver placed to collect the gas. The steam was decomposed by the red-hot iron wire, its oxygen united to the wire, while the hydrogen passed on and was collected in the water-cistern.

Both of these experiments, though not made till

1783, and though the latter of them was not read to the academy till 1784, were published in the volume of the Memoirs for 1781.

It is easy to see how this important discovery enabled Lavoisier to obviate all the objections to his theory from hydrogen. He showed that it was evolved when zinc or iron was dissolved in dilute sulphuric acid, because the water underwent decomposition, its oxygen uniting to the zinc or iron, and converting it into an oxide, while its hydrogen made its escape in the state of gas. Oxide of iron was reduced when heated in contact with hydrogen gas, because the hydrogen united to the oxygen of the acid and formed water, and of course the iron was reduced to the state of a metal. I consider it unnecessary to enter into a minute detail of these experiments, because, in fact, they added very little to what had been already established by Cavendish. But it was this discovery that contributed more than any thing else to establish the antiphlogistic theory. Accordingly, the great object of Dr. Priestley, and other advocates of the phlogistic theory, was to disprove the fact that water is a compound of oxygen and hydrogen. Scheele admitted the fact that water is a compound of oxygen and hydrogen; and doubtless, had he lived, would have become a convert to the antiphlogistic theory, as Dr. Black actually did. In short, it was the discovery of the compound nature of water that gave the Lavoisierian theory the superiority over that of Stahl. Till the time of this discovery every body opposed the doctrine of Lavoisier; but within a very few years after it, hardly any supporters of phlogiston remained. Nothing could be more fortunate for Lavoisier than this discovery, or afford him greater reason for self-congratulation.

We see the effect of this discovery upon his next paper, "On the Formation of Carbonic Acid," which appeared in the Memoirs of the Academy, for 1781. There, for the first time, he introduces new terms, showing, by that, that he considered his opinions as fully established. To the *dephlogisticated air* of Priestley, or his own *pure air*, he now gives the name of *oxygen*. The fixed air of Black he designates *carbonic acid*, because he considered it as a compound of *carbon* (the pure part of charcoal) and oxygen. The object of this paper is to determine the proportion of the constituents. He details a great many experiments, and deduces from them all, that carbonic acid gas is a compound of

Carbon . . .	0·75
Oxygen . . .	1·93

Now this is a tolerably near approximation to the truth. The true constituents, as determined by modern chemists, being

Carbon . . .	0·75
Oxygen . . .	2·00

The next paper of M. Lavoisier, which appeared in the Memoirs of the Academy, for 1782 (published in 1785), shows how well he appreciated the importance of the discovery of the composition of water. It is entitled, "General Considerations on the Solution of the Metals in Acids." He shows that when metals are dissolved in acids, they are converted into oxides, and that the acid does not combine with the metal, but only with its oxide. When nitric acid is the solvent the oxidizement takes place at the expense of the acid, which is resolved into nitrous gas and oxygen. The nitrous gas makes its escape, and may be collected; but the oxygen unites with the metal and renders it an oxide. He shows this with respect to the solution of mercury in nitric acid. He collected the nitrous gas given out during the solution of the metal in the acid: then evaporated the solution to dryness, and urged the fire till the mercury was converted into red oxide. The fire being still further urged, the red oxide was reduced, and the oxygen gas given off was collected and measured. He showed that the nitrous gas and the oxygen gas thus obtained, added together, formed just the quantity of nitric acid which had disappeared during the process. A similar experiment was made by dissolving iron in nitric acid, and then urging the fire till the iron was freed from every foreign body, and obtained in the state of black oxide.

It is well known that many metals held in solution by acids may be precipitated in the metallic state, by inserting into the solution a plate of some other metal. A portion of that new metal dissolves, and takes the place of the metal originally in solution. Suppose, for example, that we have a neutral solution of copper in sulphuric acid, if we put into the solution a plate of iron, the copper is thrown down in the metallic state, while a certain portion of the iron enters into the solution, combining with the acid instead of the copper. But the copper, while in solution, was in the state of an oxide, and it is precipitated in the metallic state. The iron was in the metallic state; but it enters into the solution in the state of an oxide. It is clear from this that the oxygen, during these precipitations, shifts its place, leaving the copper, and entering into combination with the iron. If, therefore, in such a case we determine the exact quantity of copper thrown down, and the exact quantity of iron dissolved at the same time, it is clear that we shall have the relative weight of each combined with the same weight of oxygen. If, for example, 4 of copper be thrown down by the solution of 3·5 of iron; then it is clear that 3·5 of iron requires just as much oxygen as 4 of copper, to turn both into the oxide that exists in the solution, which is the black oxide of each.

Bergman had made a set of experiments to determine the proportional quantities of phlogiston contained in the different metals, by the relative quantity of each necessary to precipitate a given weight of another from its acid solution. It was the opinion at that time, that metals were compounds of their respective calces and phlogiston. When a metal dissolved in an acid, it was known to be in the state of calx, and therefore had parted with its phlogiston: when another metal was put into this solution it became a calx, and the dissolved metal was precipitated in the metallic state. It had therefore united with the phlogiston of the precipitating metal. It is obvious, that by determining the quantities of the two metals precipitated and dissolved, the relative proportion of phlogiston in each could be determined. Lavoisier saw that these experiments of Bergman would serve equally to determine the relative quantity of oxygen in the different oxides. Accordingly, in a paper inserted in the Memoirs of the Academy, for 1782, he enters into an elaborate examination of Bergman's experiments, with a view to determine this point. But it is unnecessary to state the deductions which he drew, because Bergman's experiments were not sufficiently accurate for the object in view. Indeed, as the mutual precipitation of the metals is a galvanic phenomenon, and as the precipitated metal is seldom quite pure, but an alloy of the precipitating and precipitated metal; and as it is very difficult to dry the more oxidizable metals, as copper and tin, without their absorbing oxygen when they are in a state of very minute division; this mode of experimenting is not precise enough for the object which Lavoisier had in view. Accordingly the table of the composition of the metallic oxides which Lavoisier has drawn up is so very defective, that it is not worth while to transcribe it.

The same remark applies to the table of the affinities of oxygen which Lavoisier drew up and inserted in the Memoirs of the Academy, for the same year. His data were too imperfect, and his knowledge too limited, to put it in his power to draw up any such table with any approach to accuracy. I shall have occasion to resume the subject in a subsequent chapter.

In the same volume of the Memoirs of the Academy, this indefatigable man inserted a paper in order to determine the quantity of oxygen which combines with iron. His method of proceeding was, to burn a given weight of iron in oxygen gas. It is well known that iron wire, under such circumstances, burns with considerable splendour, and that the oxide, by the heat, is fused into a black brittle matter, having somewhat of the metallic lustre. He burnt 145·6 grains of iron in this way, and found that, after combustion, the weight became 192 grains, and 97 French cubic inches of oxygen gas had been absorbed. From this experiment it follows, that the oxide of iron formed by burning iron in oxygen gas is a compound of

Iron	3·5
Oxygen	1·11

This forms a tolerable approximation to the truth. It is now known, that the quantity of oxygen in the oxide of iron formed by the combustion of iron in oxygen gas is not quite uniform in its composition; sometimes it is a compound of

Iron	3½
Oxygen	1⅓

While at other times it consists very nearly of

Iron	3·5
Oxygen	1

proportions between these two extremes. The last of these compounds constitutes what is now known by the name of *protoxide*, or *black oxide of iron.* The first is the composition of the ore of iron so abundant, which is distinguished by the name of *magnetic iron ore.*

Lavoisier was aware that iron combines with more oxygen than exists in the protoxide; indeed, his analysis of peroxide of iron forms a tolerable approximation to the truth; but there is no reason for believing that he was aware that iron is capable of forming only two oxides, and that all intermediate degrees of oxidation are impossible. This was first demonstrated by Proust.

I think it unnecessary to enter into any details respecting two papers of Lavoisier, that made their appearance in the Memoirs of the Academy, for 1783, as they add very little to what he had already done. The first of these describes the experiments which he made to determine the quantity of oxygen which unites with sulphur and phosphorus when they are burnt: it contains no fact which he had not stated in his former papers, unless we are to consider his remark, that the heat given out during the burning of these bodies has no sensible weight, as new.

The other paper is "On Phlogiston;" it is very elaborate, but contains nothing which had not been already advanced in his preceding memoirs. Chemists were so wedded to the phlogistic theory, their prejudices were so strong, and their understandings so fortified against every thing that was likely to change their opinions, that Lavoisier found it necessary to lay the same facts before them again and again, and to place them in every point of view. In this paper he gives a statement of his own theory of combustion, which he had previously done in several preceding papers. He examines the phlogistic theory of Stahl at great length, and refutes it.

In the Memoirs of the Academy, for 1784, Lavoisier published a very elaborate set of experiments on the combustion of alcohol, oil, and different combustible bodies, which gave a beginning to the analysis of vegetable substances, and served as a foundation upon which this most difficult part of chemistry might be reared. He showed that during the combustion of alcohol the oxygen of the air united to the vapour of the alcohol, which underwent decomposition, and was converted into water and carbonic acid. From these experiments he deduced as a consequence, that the constituents of alcohol are carbon, hydrogen, and oxygen, and nothing else; and he endeavoured from his experiments to determine the relative proportions of these different constituents. From these experiments he concluded, that the alcohol which he used in his experiments was a compound of

Carbon . . .	2629·5 part.
Hydrogen . .	725·5
Water . . .	5861

It would serve no purpose to attempt to draw any consequences from these experiments; as Lavoisier does not mention the specific gravity of the alcohol, of course we cannot say how much of the water found was merely united with the alcohol, and how much entered into its composition. The proportion between the carbon and hydrogen, constitutes an approximation to the truth, though not a very near one.

Olive oil he showed to be a compound of hydrogen and carbon, and bees' wax to be a compound of the same constituents, though in a different proportion.

This subject was continued, and his views further extended, in a paper inserted in the Memoirs of the Academy, for 1786, entitled, "Reflections on the Decomposition of Water by Vegetable and Animal Substances." He begins by stating that when charcoal is exposed to a strong heat, it gives out a little carbonic acid gas and a little inflammable air, and after this nothing more can be driven off, however high the temperature be to which it is exposed; but if the charcoal be left for some time in contact with the atmosphere it will again give out a little carbonic acid gas and inflammable gas when heated, and this process may be repeated till the whole charcoal disappears. This is owing to the presence of a little moisture which the charcoal imbibes from the air. The water is decomposed when the charcoal is heated and converted into carbonic acid and inflammable gas. When vegetable substances are heated in a retort, the water which they contain undergoes a similar decomposition, the carbon which forms one of their constituents combines with the oxygen and produces carbonic acid, while the hydrogen, the other constituent of the water, flies off in the state of gas combined with a certain quantity of carbon. Hence the substances obtained when vegetable or animal substances are distilled did not exist ready formed in the body operated on; but proceeded from the double decompositions which took place by the mutual action of the constituents of the water, sugar, mucus, &c., which the vegetable body contains. The oil, the acid, &c., extracted by distilling vegetable bodies did not exist in them, but are formed during the mutual action of the constituents upon each other, promoted as their action is by the heat. These views were quite new and perfectly just, and threw a new light on the nature of vegetable substances and on the products obtained by distilling them. It showed the futility of all the pretended analyses of vegetable substances, which chemists had performed by simply subjecting them to distillation, and the error of drawing any conclusions respecting the constituents of vegetable substances from the results of their distillation, except indeed with respect to their elementary constituents. Thus when by distilling a vegetable substance we obtain water, oil, acetic acid, carbonic acid, and carburetted hydrogen, we must not conclude that these principles existed in the substance, but merely that it contained carbon, hydrogen, and oxygen, in such proportions as to yield all these principles by decompositions.

As nitric acid acts upon metals in a very different way from sulphuric and muriatic acids, and as it is a much better solvent of metals in general than any other, it was an object of great importance towards completing the antiphlogistic theory to obtain an accurate knowledge of its constituents. Though Lavoisier did not succeed in this, yet he made at least a certain progress, which enabled him to explain the phenomena, at that time known, with considerable clearness, and to answer all the objections to the antiphlogistic theory from the action of nitric acid on metals. His first paper on the subject was published in the Memoirs of the Academy, for 1776. He put a quantity of nitric acid and mercury into a retort with a long beak, which he plunged into the water-trough. An effervescence took place and gas passed over in abundance, and was collected in a glass jar; the mercury being dissolved the retort was still further heated, till every thing liquid passed over into the receiver, and a dry yellow salt remained. The beak of the retort was now again plunged into the water-trough, and the salt heated till all the nitric acid which it contained was decomposed, and nothing remained in the retort but red oxide of mercury. During this last process much more gas was collected. All the gas obtained during the solution of the mercury and the decomposition of the salt was nitrous gas. The red oxide of mercury was now heated to redness, oxygen gas was emitted in abundance, and the mercury was reduced to the metallic state: its weight was found the very same as at first. It is clear, therefore, that the nitrous gas and the oxygen gas were derived, not from the mercury but from the nitric acid, and that the nitric acid had been decomposed into nitrous gas and oxygen: the nitrous gas had made its escape in the form of gas, and the oxygen had remained united to the metal.

From these experiments it follows clearly, that nitric acid is a compound of nitrous gas and oxygen. The nature of nitrous gas itself Lavoisier did not succeed in ascertaining. It passed with him for a simple substance; but what he did ascertain enabled him to explain the action of nitric acid on metals. When nitric acid is poured upon a metal which it is capable of dissolving, copper for example, or mercury, the oxygen of the acid unites to the metal, and converts into an oxide, while the nitrous gas, the other constituent of the acid, makes its escape in the gaseous form. The oxide combines with and is dissolved by another portion of the acid which escapes decomposition.

It was discovered by Dr. Priestley, that when nitrous gas and oxygen gas are mixed together in certain proportions, they instantly unite, and are converted into nitrous acid. If this mixture be made over water, the volume of the gases is instantly diminished, because the nitrous acid formed loses its elasticity, and is absorbed by the water. When nitrous gas is mixed with air containing oxygen gas, the diminution of volume after mixture is greater the more oxygen gas is present in the air. This induced Dr. Priestley to employ nitrous gas as a test of the purity of common air. He mixed together equal volumes of the nitrous gas and air to be examined, and he judged of the purity of the air by the degree of condensation: the greater the diminution of bulk, the greater did he consider the proportion of oxygen in the air under examination to be. This method of proceeding was immediately adopted by chemists and physicians; but there was a want of uniformity in the mode of proceeding, and a considerable diversity in the results. M. Lavoisier endeavoured to improve the process, in a paper inserted in the Memoirs of the Academy, for 1782; but his method did not answer the purpose intended: it was Mr. Cavendish that first pointed out an accurate mode of testing air by means of nitrous gas, and who showed that the proportions of oxygen and azotic gas in common air are invariable.

Lavoisier, in the course of his investigations, had proved that carbonic acid is a compound of carbon and oxygen; sulphuric acid, of sulphur and oxygen; phosphoric acid, of phosphorus and oxygen; and nitric acid, of nitrous gas and oxygen. Neither the carbon, the sulphur, the phosphorus, nor the nitrous gas, possessed any acid properties when uncombined; but they acquired these properties when they were united to oxygen. He observed further, that all the acids known in his time which had been decomposed were found to contain oxygen, and when they were deprived of oxygen, they lost their acid properties. These facts led him to conclude, that oxygen is an essential constituent in all acids, and that it is the principle which bestows acidity or the true acidifying principle. This was the reason why he distinguished it by the name of oxygen.[*] These views were fully developed by Lavoisier, in a paper inserted in the Memoirs of the Academy, for 1778,

---

[*] From ὀξυς, sour, and γινομαι, which he defined the *producer of acids*, the *acidifying principle.*

entitled, "General Considerations on the Nature of Acids, and on the Principles of which they are composed." When this paper was published, Lavoisier's views were exceedingly plausible. They were gradually adopted by chemists in general, and for a number of years may be considered to have constituted a part of the generally-received doctrines. But the discovery of the nature of chlorine, and the subsequent facts brought to light respecting iodine, bromine, and cyanogen, have demonstrated that it is inaccurate; that many powerful acids exist which contain no oxygen, and that there is no one substance to which the name of acidifying principle can with justice be given. To this subject we shall again revert, when we come to treat of the more modern discoveries.

Long as the account is which we have given of the labours of Lavoisier, the subject is not yet exhausted. Two other papers of his remain to be noticed, which throw considerable light on some important functions of the living body: we allude to his experiments on *respiration* and *perspiration*.

It was known, that if an animal was confined beyond a certain limited time in a given volume of atmospherical air, it died of suffocation, in consequence of the air becoming unfit for breathing; and that if another animal was put into this air, thus rendered noxious by breathing, its life was destroyed almost in an instant. Dr. Priestley had thrown some light upon this subject by showing that air, in which an animal had breathed for some time, possessed the property of rendering lime-water turbid, and therefore contained carbonic acid gas. He considered the process of breathing as exactly analogous to the calcination of metals, or the combustion of burning bodies. Both, in his opinion acted by giving out phlogiston; which, uniting with the air of the atmosphere, converted it into phlogisticated air. Priestley found, that if plants were made to vegetate for some time in air that had been rendered unfit for supporting animal life by respiration, it lost the property of extinguishing a candle, and animals could breathe it again without injury. He concluded from this that animals, by breathing, phlogisticated air, but that plants, by vegetating, dephlogisticated air: the former communicated phlogiston to it, the latter took phlogiston from it.

After Lavoisier had satisfied himself that air is a mixture of oxygen and azote, and that oxygen alone is concerned in the processes of calcination and combustion, being absorbed and combined with the substances undergoing calcination and combustion, it was impossible for him to avoid drawing similar conclusions with respect to the breathing of animals. Accordingly, he made experiments on the subject, and the result was published in the Memoirs of the Academy, for 1777. From these experiments he drew the following conclusions:

1. The only portion of atmospherical air which is useful in breathing is the oxygen. The azote is drawn into the lungs along with the oxygen, but it is thrown out again unaltered.

2. The oxygen gas, on the contrary, is gradually, by breathing, converted into carbonic acid; and air becomes unfit for respiration when a certain portion of its oxygen is converted into carbonic acid gas.

3. Respiration is therefore exactly analogous to calcination. When air is rendered unfit for supporting life by respiration, if the carbonic acid gas formed be withdrawn by means of lime-water, or caustic alkali, the azote remaining is precisely the same, in its nature, as what remains after air is exhausted of its oxygen by being employed for calcining metals.

In this first paper Lavoisier went no further than establishing these general principles; but he afterwards made experiments to determine the exact amount of the changes which were produced in air by breathing, and endeavoured to establish an accurate theory of respiration. To this subject we shall have occasion to revert again, when we give an account of the attempts made to determine the phenomena of respiration by more modern experimenters.

Lavoisier's experiments on *perspiration* were made during the frenzy of the French revolution, when Robespierre had usurped the supreme power, and when it was the object of those at the head of affairs to destroy all the marks of civilization and science which remained in the country. His experiments were scarcely completed when he was thrown into prison, and though he requested a prolongation of his life for a short time, till he could have the means of drawing up a statement of their results, the request was barbarously refused. He has therefore left no account of them whatever behind him. But Seguin, who was associated with him in making these experiments, was fortunately overlooked, and escaped the dreadful times of the reign of terror: he afterwards drew up an account of the results, which has prevented them from being wholly lost to chemists and physiologists.

Seguin was usually the person experimented on. A varnished silk bag, perfectly air-tight, was procured, within which he was enclosed, except a slit over against the mouth, which was left open for breathing; and the edges of the bag were accurately cemented round the mouth, by means of a mixture of turpentine and pitch. Thus every thing emitted by the body was retained in the bag, except what made its escape from the lungs by respiration. By weighing himself in a delicate balance at the commencement of the experiment, and again after he had continued for some time in the bag, the quantity of matter carried off by respiration was determined. By weighing himself without this varnished covering, and repeating the operation after the same interval of time had elapsed, as in the former experiment, he determined the loss of weight occasioned by *perspiration* and *respiration* together. The loss of weight indicated by the first experiment being subtracted from that given by the second, the quantity of matter lost by *perspiration* through the pores of the skin was determined. The following facts were ascertained by these experiments:

1. The maximum of matter perspired in a minute amounted to 26·25 grains troy; the minimum to nine grains; which gives 17·63 grains, at a medium, in the minute, or 52·89 ounces in twenty-four hours.

2. The amount of perspiration is increased by drink, but not by solid food.

3. Perspiration is at its minimum immediately after a repast; it reaches its maximum during digestion.

Such is an epitome of the chemical labours of M. Lavoisier. When we consider that this prodigious number of experiments and memoirs were all performed and drawn up within the short period of twenty years, we shall be able to form some idea of the almost incredible activity of this extraordinary man: the steadiness with which he kept his own peculiar opinions in view, and the good temper which he knew how to maintain in all his publications, though his opinions were not only not supported, but actually opposed by the whole body of chemists in existence, does him infinite credit, and was undoubtedly the wisest line of conduct which he could possibly have adopted. The difficulties connected with the evolution and absorption of hydrogen, constituted the stronghold of the phlogistians. But Mr. Cavendish's discovery, that water is a compound of oxygen and hydrogen, was a death-blow to the doctrine of Stahl. Soon after this discovery was fully established, or during the year 1785, M. Berthollet, a member of the academy, and fast rising to the eminence which he afterwards acquired, declared himself a convert to the Lavoisierian theory. His example was immediately followed by M. Fourcroy, also a member of the academy, who had succeeded Macquer as professor of chemistry in the Jardin du Roi.

M. Fourcroy, who was perfectly aware of the strong feeling of patriotism which, at that time, actuated almost every man of science in France, hit upon a most infallible way of giving currency to the new opinions. To the theory of Lavoisier he gave the name of *La Chimie Française* (French Chemistry). This name was not much relished by Lavoisier, as, in his opinion, it deprived him of the credit which was his due; but it certainly contributed, more than any thing else, to give the new opinions currency, at least, in France; they became at once a national concern, and those who still adhered to the old opinions, were hooted and stigmatized as enemies to the glory of their country. One of the most eminent of those who still adhered to the phlogistic theory was M. Guyton de Morveau, a nobleman of Burgundy, who had been educated as a lawyer, and who filled a conspicuous situation in the Parliament of Dijon: he had cultivated chemistry with great zeal, and was at that time the editor of the chemical part of the Encyclopédie Méthodique. In the first half-volume of the chemical part of this dictionary, which had just appeared, Morveau had supported the doctrine of phlogiston, and opposed the opinions of Lavoisier with much zeal and considerable skill:

on this account, it became an object of considerable consequence to satisfy Morveau that his opinions were inaccurate, and to make him a convert to the antiphlogistic theory; for the whole matter was managed as if it had been a political intrigue, rather than a philosophical inquiry.

Morveau was accordingly invited to Paris, and Lavoisier succeeded without difficulty in bringing him over to his own opinions. We are ignorant of the means which he took; no doubt friendly discussion and the repetition of the requisite experiments, would be sufficient to satisfy a man so well acquainted with the subject, and whose mode of thinking was so liberal as Morveau. Into the middle of the second half-volume of the chemical part of the Encyclopédie Méthodique he introduced a long advertisement, announcing this change in his opinions, and assigning his reasons for it.

The chemical nomenclature at that time in use had originated with the medical chemists, and contained a multiplicity of unwieldy and unmeaning, and even absurd terms. It had answered the purposes of chemists tolerably well while the science was in its infancy; but the number of new substances brought into view had of late years become so great, that the old names could not be applied to them without the utmost straining: and the chemical terms in use were so little systematic that it required a considerable stretch of memory to retain them. These evils were generally acknowledged and lamented, and various attempts had been made to correct them. Bergman, for instance, had contrived a new nomenclature, confined chiefly to the salts and adapted to the Latin language. Dr. Black had done the same thing: his nomenclature possessed both elegance and neatness, and was, in several respects, superior to the terms ultimately adopted; but with his usual indolence and disregard of reputation, he satisfied himself merely with drawing it up in the form of a table and exhibiting it to his class. Morveau contrived a new nomenclature of the salts, and published it in 1783; and it appears to have been seen and approved of by Bergman.

The old chemical phraseology as far as it had any meaning was entirely conformable to the phlogistic theory. This was so much the case that it was with difficulty that Lavoisier was able to render his opinions intelligible by means of it. Indeed it would have been out of his power to have conveyed his meaning to his readers, had he not invented and employed a certain number of new terms. Lavoisier, aware of the defects of the chemical nomenclature, and sensible of the advantage which his own doctrine would acquire when dressed up in a language exactly suited to his views, was easily prevailed upon by Morveau to join with him in forming a new nomenclature to be henceforth employed exclusively by the antiphlogistians, as they called themselves. For this purpose they associated with themselves Berthollet, and Fourcroy. We do not know what part each took in this important undertaking; but, if we are to judge from appearances, the new nomenclature was almost exclusively the work of Lavoisier and Morveau. Lavoisier undoubtedly contrived the general phrases, and the names applied to the simple substances, so far as they were new, because he had employed the greater number of them in his writings before the new nomenclature was concocted. That the mode of naming the salts originated with Morveau is obvious; for it differs but little from the nomenclature of the salts published by him four years before.

The new nomenclature was published by Lavoisier and his associates in 1787, and it was ever after employed by them in all their writings. Aware of the importance of having a periodical work in which they could register and make known their opinions, they established the *Annales de Chimie*, as a sort of counterpoise to the *Journal de Physique*, the editor of which, M. Delametherie, continued a zealous votary of phlogiston to the end of his life. This new nomenclature very soon made its way into every part of Europe, and became the common language of chemists, in spite of the prejudices entertained against it, and the opposition which it every where met with. In the year 1796, or nine years after the appearance of the new nomenclature, when I attended the chemistry-class in the College of Edinburgh, it was not only in common use among the students, but was employed by Dr. Black, the professor of chemistry, himself; and I have no doubt that he had introduced it into his lectures several years before. This extraordinary rapidity with which the new chemical language came into use, was doubtless owing to two circumstances. First, the very defective, vague, and barbarous state of the old chemical nomenclature: for although, in consequence of the prodigious progress which the science of chemistry has made since the time of Lavoisier, his nomenclature is now nearly as inadequate to express our ideas as that of Stahl was to express his; yet, at the time of its appearance, its superiority over the old nomenclature was so great, that it was immediately felt and acknowledged by all those who were acquiring the science, who are the most likely to be free from prejudices, and who, in the course of a few years, must constitute the great body of those who are interested in the science. 2. The second circumstance, to which the rapid triumph of the new nomenclature was owing, is the superiority of

Lavoisier's theory over that of Stahl. The subsequent progress of the science has betrayed many weak points in Lavoisier's opinions; yet its superiority over that of Stahl was so obvious, and the mode of interrogating nature introduced by him was so good, and so well calculated to advance the science, that no unprejudiced person, who was at sufficient pains to examine both, could hesitate about preferring that of Lavoisier. It was therefore generally embraced by all the young chemists in every country; and they became, at the same time, partial to the new nomenclature, by which only that theory could be explained in an intelligible manner.

When the new nomenclature was published, there were only three nations in Europe who could be considered as holding a distinguished place as cultivators of chemistry: France, Germany, and Great Britain. For Sweden had just lost her two great chemists, Bergman and Scheele, and had been obliged, in consequence, to descend from the high chemical rank which she had formerly occupied. In France the fashion, and of course almost the whole nation, were on the side of the new chemistry. Macquer, who had been a stanch phlogistian to the last, was just dead. Monnet was closing his laborious career. Baumé continued to adhere to the old opinions; but he was old, and his chemical skill, which had never been *accurate*, was totally eclipsed by the more elaborate researches of Lavoisier and his friends. Delametherie was a keen phlogistian, a man of some abilities, of remarkable honesty and integrity, and editor of the Journal de Physique, at that time a popular and widely-circulating scientific journal. But his habits, disposition, and conduct, were by no means suited to the taste of his countrymen, or conformable to the practice of his contemporaries. The consequence was, that he was shut out of all the scientific coteries of Paris; and that his opinions, however strongly, or rather violently expressed, failed to produce the intended effect. Indeed, as his views were generally inaccurate, and expressed without any regard to the rules of good manners, they in all probability rather served to promote than to injure the cause of his opponents. Lavoisier and his friends appear to have considered the subject in this light: they never answered any of his attacks, or indeed took any notice of them. France, then, from the date of the publication of the new nomenclature, might be considered as enlisted on the side of the antiphlogistic theory.

The case was very different in Germany. The national prejudices of the Germans were naturally enlisted on the side of Stahl, who was their countryman, and whose reputation would be materially injured by the refutation of his theory. The cause of phlogiston, accordingly, was taken up by several German chemists, and supported with a good deal of vigour; and a controversy was carried on for some years in Germany between the old chemists who adhered to the doctrine of Stahl, and the young chemists who had embraced the theory of Lavoisier. Gren, who was at that time the editor of a chemical journal, deservedly held in high estimation, and whose reputation as a chemist stood rather high in Germany, finding it impossible to defend the Stahlian theory as it had been originally laid down, introduced a new modification of phlogiston, and attempted to maintain it against the antiphlogistians. The death of Gren and of Wiegleb, who were the great champions of phlogiston, left the field open to the antiphlogistians, who soon took possession of all the universities and scientific journals in Germany. The most eminent chemist in Germany, or perhaps in Europe at that time, was Martin Henry

Klaproth, professor of chemistry at Berlin, to whom analytical chemistry lies under the greatest obligations. In the year 1792 he proposed to the Academy of Sciences of Berlin, of which he was a member, to repeat all the requisite experiments before them, that the members of the academy might be able to determine for themselves which of the two theories deserved the preference. This proposal was acceded to. All the fundamental experiments were repeated by Klaproth with the most scrupulous attention to accuracy: the result was a full conviction, on the part of Klaproth and the academy, that the Lavoisierian theory was the true one. Thus the Berlin Academy became antiphlogistians in 1792: and as Berlin has always been the focus of chemistry in Germany, the determination of such a learned body must have had a powerful effect in accelerating the propagation of the new theory through that vast country.

In Great Britain the investigation of gaseous bodies, to which the new doctrines were owing, had originated. Dr. Black had begun the inquiry—Mr. Cavendish had prosecuted it with unparalleled accuracy—and Dr. Priestley had made known a great number of new gaseous bodies, which had hitherto escaped the attention of chemists. As the British chemists had contributed more than those of any other nation to the production of the new facts on which Lavoisier's theory was founded, it was natural to expect that they would have embraced that theory more readily than the chemists of any other nation: but the matter of fact was somewhat different. Dr. Black, indeed, with his characteristic candour, speedily embraced the opinions, and even adopted the new nomenclature: but Mr. Cavendish new modelled the phlogistic theory, and published a defence of phlogiston, which it was impossible at that time to refute. The French chemists had the good sense not to attempt to overturn it. Mr. Cavendish after this laid aside the cultivation of chemistry altogether, and never acknowledged himself a convert to the new doctrines.

Dr. Priestley continued a zealous advocate for phlogiston till the very last, and published what he called a refutation of the antiphlogistic theory about the beginning of the present century: but Dr. Priestley, notwithstanding his merit as a discoverer and a man of genius, was never, strictly speaking, entitled to the name of chemist; as he was never able to make a chemical analysis. In his famous experiments, for example, on the composition of water, he was obliged to procure the assistance of Mr. Keir to determine the nature of the blue-coloured liquid which he had obtained, and which Mr. Keir showed to be nitrate of copper. Besides, Dr. Priestley, though perfectly honest and candid, was so hasty in his decisions, and so apt to form his opinions without duly considering the subject, that his chemical theories are almost all erroneous and sometimes quite absurd.

Mr. Kirwan, who had acquired a high reputation, partly by his *mineralogy*, and partly by his experiments on the composition of the salts, undertook the task of refuting the antiphlogistic theory, and with that view published a work to which he gave the name of "An Essay on Phlogiston and the Composition of Acids." In that book he maintained an opinion which seems to have been pretty generally adopted by the most eminent chemists of the time; namely, that phlogiston is the same thing with what is at present called *hydrogen*, and which, when Kirwan wrote, was called light *inflammable air*. Of course Mr. Kirwan undertook to prove that every combustible substance and every metal contains hydrogen as a constituent, and that hydrogen escapes in every case of combustion and calcination. On the other hand, when calces are reduced to the metallic state hydrogen is absorbed. The book was divided into thirteen sections. In the first the specific gravity of the gases was stated according to the best data then existing. The second section treats of the composition of acids, and the composition and decomposition of water. The third section treats of sulphuric acid; the fourth, of nitric acid; the fifth, of muriatic acid; the sixth, of aqua regia; the seventh, of phosphoric acid; the eighth, of oxalic acid; the ninth, of the calcination and reduction of metals and the formation of fixed air; the tenth, of the dissolution of metals; the eleventh, of the precipitation of metals by each other; the twelfth, of the properties of iron and steel; while the thirteenth sums up the whole argument by way of conclusion.

In this work Mr. Kirwan admitted the truth of M. Lavoisier's theory, that during combustion and calcination, oxygen united with the burning and calcining body. He admitted also that water is a compound of oxygen and hydrogen. Now these admissions, which, however, it was scarcely possible for a man of candour to refuse, rendered the whole of his arguments in favour of the identity of hydrogen and phlogiston, and of the existence of hydrogen in all combustible bodies, exceedingly inconclusive. Kirwan's book was laid hold of by the French chemists, as affording them an excellent opportunity of showing the superiority of the new opinions over the old. Kirwan's view of the subject was that which had been taken by Bergman and Scheele, and indeed by every chemist of eminence who still adhered to the phlogistic system. A satisfactory refutation of it, therefore, would be a death-blow to phlogiston and would place the antiphlogistic theory upon a basis so secure that it would be henceforth impossible to shake it.

Kirwan's work on phlogiston was accordingly translated into French, and published in Paris. At the end of each section was placed an examination and refutation of the argument contained in it by some one of the French chemists, who had now associated themselves in order to support the antiphlogistic theory. The introduction, together with the second, third, and eleventh sections were examined and refuted by M. Lavoisier; the fourth, the fifth, and sixth sections fell to the share of M. Berthollet; the seventh and thirteenth sections were undertaken by M. de Morveau; the eighth, ninth, and tenth, by M. De Fourcroy; while the twelfth section, on iron and steel was animadverted on by M. Monge. These refutations were conducted with so much urbanity of manner, and were at the same time so complete, that they produced all the effects expected from them. Mr. Kirwan, with a degree of candour and liberality of which, unfortunately, very few examples can be produced, renounced his old opinions, abandoned phlogiston, and adopted the antiphlogistic doctrines of his opponents. But his advanced age, and a different mode of experimenting from what he had been accustomed to, induced him to withdraw himself entirely from experimental science and to devote the evening of his life to metaphysical and logical and moral investigations.

Thus, soon after the year 1790, a kind of interregnum took place in British chemistry. Almost all the old British chemists had relinquished the science, or been driven out of the field by the superior prowess of their antagonists. Dr. Austin and Dr. Pearson will, perhaps, be pointed out as exceptions. They undoubtedly contributed somewhat to the progress of the science. But they were arranged on the side of the antiphlogistians. Dr. Crawford, who had done so much for the theory of heat, was about this time ruined in his circumstances by the bankruptcy of a house to which he had intrusted his property. This circumstance preyed upon a mind which had a natural tendency to morbid sensibility, and induced this amiable and excellent man to put an end to his existence. Dr. Higgins had acquired some celebrity as an experimenter and teacher; but his disputes with Dr. Priestley, and his laying claim to discoveries which certainly did not belong to him, had injured his reputation, and led him to desert the field of science. Dr. Black was an invalid, Mr. Cavendish had renounced the cultivation of chemistry, and Dr. Priestley had been obliged to escape from the iron hand of theological and political bigotry, by leaving the country. He did little as an experimenter after he went to America; and, perhaps, had he remained in England, his reputation would rather have diminished than increased. He was an admirable pioneer, and as such, contributed more than any one to the revolution which chemistry underwent; though he was himself utterly unable to rear a permanent structure capable, like the Newtonian theory, of withstanding all manner of attacks, and becoming only the firmer and stronger the more it is examined. Mr. Keir, of Birmingham, was a man of great eloquence, and possessed of all the chemical knowledge which characterized the votaries of phlogiston. In the year 1789 he attempted to stem the current of the new opinions by publishing a dictionary of chemistry, in which all the controversial points were to be fully discussed, and the antiphlogistic theory examined and refuted. Of this dictionary only one part appeared, constituting a very thin volume of two hundred and eight quarto pages, and treating almost entirely of *acids*.

Finding that the sale of this work did not answer his expectations, and probably feeling, as he proceeded, that the task of refuting the antiphlogistic opinions was much more difficult, and much more hopeless than he expected, he renounced the undertaking, and abandoned altogether the pursuit of chemistry.

It will be proper in this place to introduce some account of the most eminent of those French chemists who embraced the theory of Lavoisier, and assisted him in establishing his opinions.

Claude-Louis Berthollet was born at Talloire, near Annecy, in Savoy, on the 9th of December, 1748. He finished his school education at Chambéry, and afterwards studied at the College of Turin, a celebrated establishment, where many men of great scientific celebrity have been educated. Here he attached himself to medicine, and after obtaining a degree he repaired to Paris, which was destined to be the future theatre of his speculations and pursuits.

In Paris he had not a single acquaintance, nor did he bring with him a single introductory letter; but understanding that M. Tronchin, at that time a distinguished medical practitioner in Paris, was a native of Geneva, he thought he might consider him as in some measure a countryman. On this slender ground he waited on M. Tronchin, and what is rather surprising, and reflects great credit on both, this acquaintance, begun in so uncommon a way, soon ripened into friendship. Tronchin interested himself for his young *protégée*, and soon got him into the situation of physician in ordinary to the Duke of Orleans, father of him who cut so conspicuous a figure in the French revolution, under the name of M. Egalité. In this situation he devoted himself to the study of chemistry, and soon made himself known by his publications on the subject.

In 1781 he was elected a member of the Academy of Sciences of Paris: one of his competitors was M. Fourcroy. No doubt Berthollet owed his election to the influence of the Duke of Orleans. In the year 1784 he was again a competitor with M. de Fourcroy for the chemical chair at the Jardin du Roi, left vacant by the death of Macquer. The chair was in the gift of M. Buffon, whose vanity is said to have been piqued because the Duke of Orleans, who supported Berthollet's interest, did not pay him sufficient court. This induced him to give the chair to Fourcroy; and the choice was a fortunate one, as his uncommon vivacity and rapid elocution particularly fitted him for addressing a Parisian audience. The chemistry-class at the Jardin du Roi immediately became celebrated, and attracted immense crowds of admiring auditors.

But the influence of the Duke of Orleans was sufficient to procure for Berthollet another situation which Macquer had held. This was government commissary and superintendent of the dyeing processes. It was this situation which naturally turned his attention to the phenomena of dyeing, and occasioned afterwards his book on dyeing; which at the time of its publication was excellent, and exhibited a much better theory of dyeing, and a better account of the practical part of the art than any work which had previously appeared. The arts of dyeing and calico-printing have been very much improved since the time that Berthollet's book was written; yet if we except Bancroft's work on the permanent colours, nothing very important has been published on the subject since that period. We are at present almost as much in want of a good work on dyeing as we were when Berthollet's book appeared.

In the year 1785 Berthollet, at a meeting of the Academy of Sciences, informed that learned body that he had become a convert to the antiphlogistic doctrines of Lavoisier. There was one point, however, upon which he entertained a different opinion from Lavoisier, and this difference of opinion continued to the last. Berthollet did not consider oxygen as the acidifying principle. On the contrary, he was of opinion that acids existed which contained no oxygen whatever. As an example, he mentioned sulphuretted hydrogen, which possessed the properties of an acid, reddening vegetable blues, and combining with and neutralizing bases, and yet it was a compound of sulphur and hydrogen, and contained no oxygen whatever. It is now admitted that Berthollet was accurate in his opinion, and that oxygen is not of itself an acidifying principle.

Berthollet continued in the uninterrupted prosecution of his studies, and had raised himself a very high reputation when the French revolution burst upon the world in all its magnificence. It is not our business here to enter into any historical details, but merely to remind the reader that all the great powers of Europe combined to attack France, assisted by a formidable army of French emigrants assembled at Coblentz. The Austrian and Prussian armies hemmed her in by land, while the British fleets surrounded her by sea, and thus shut her out from all communication with other nations. Thus France was thrown at once upon her own resources. She had been in the habit of importing her saltpetre, and her iron, and many other necessary implements of war: these supplies were suddenly withdrawn; and it was expected that France, thus deprived of all her resources, would be obliged to submit to any terms imposed upon her by her adversaries. At this time she summoned her men of science to her assistance, and the call was speedily answered. Berthollet and Monge were particularly active, and saved the French nation from destruction by their activity, intelligence, and zeal. Berthollet traversed France from one extremity to the other; pointed out the mode of extracting saltpetre from the soil, and of purifying it. Saltpetre-works were instantly established in every part of France, and gunpowder made of it in prodigious quantity, and with incredible activity. Berthollet even attempted to manufacture a new species of gunpowder still more powerful than the old, by substituting chlorate of potash for saltpetre: but it was found too formidable a substance to be made with safety.

The demand for cannon, muskets, sabres, &c., was equally urgent and equally difficult to be supplied. A committee of men of science, of which Berthollet and Monge were the leading members, was established, and by them the mode of smelting iron, and of converting it into steel, was instantly communicated, and numerous manufactories of these indispensable articles rose like magic in every part of France.

This was the most important period of the life of of Berthollet. It was in all probability his zeal, activity, sagacity, and honesty, which saved France from being overrun by foreign troops. But perhaps the moral conduct of Berthollet was not less conspicuous than his other qualities. During the reign of terror, a short time before the 9th Thermidor, when it was the system to raise up pretended plots, to give pretexts for putting to death those that were obnoxious to Robespierre and his friends, a hasty notice was given at a sitting of the Committee of Public Safety, that a conspiracy had just been discovered to destroy the soldiers, by poisoning the brandy which was just going to be served out to them previous to an engagement. It was said that the sick in the hospitals who had tasted this brandy, all perished in consequence of it. Immediate orders were issued to arrest those previously marked for execution. A quantity of the brandy was sent to Berthollet to be examined. He was informed, at the same time, that Robespierre wanted a conspiracy to be established, and all knew that opposition to his will was certain destruction. Having finished his analysis, Berthollet drew up his results in a Report, which he accompanied with a written explanation of his views; and he there stated, in the plainest language, that nothing poisonous was mixed with the brandy, but that it had been diluted with water holding small particles of slate in suspension, an ingredient which filtration would remove. This report deranged the plans of the Committee of Public Safety. They sent for the author, to convince him of the inaccuracy of his analysis, and to persuade him to alter its results. Finding that he remained unshaken in his opinion, Robespierre exclaimed, " What, Sir! darest thou affirm that the muddy brandy is free from poison!" Berthollet immediately filtered a glass of it in his presence, and drank it off. " Thou art daring, Sir, to drink that liquor," exclaimed the ferocious president of the committee. " I dared much more," replied Berthollet, " when I signed my name to that Report." There can be no doubt that he would have paid the penalty of this undaunted honesty with his life, but that fortunately the Committee of Public Safety could not at that time dispense with his services.

In the year 1792 Berthollet was named one of the commissioners of the Mint, into the processes of which he introduced considerable improvements. In 1794 he was appointed a member of the Commission of Agriculture and the Arts: and in the course of the same year he was chosen professor of chemistry at the Polytechnic School and also in the Normal School. But his turn of mind did not fit him for a public teacher. He expected too much information to be possessed by his hearers, and did not, therefore, dwell sufficiently upon the elementary details. His pupils were not able to follow his metaphysical disquisitions on subjects totally new to them; hence, instead of inspiring them with a love for chemistry, he filled them with langour and disgust.

In 1795, at the organization of the Institute, which was intended to include all men of talent or celebrity in France, we find Berthollet taking a most active lead; and the records of the Institute afford abundant evidence of the perseverance and assiduity with which he laboured for its interests. Of the committees to which all original memoirs are in the first place referred, we find Berthollet, oftener than any other person, a member, and his signature to the report of each work stands generally first.

In the year 1796, after the subjugation of Italy by Bonaparte, Berthollet and Monge were selected by the Directory to proceed to that country, in order to select those works of science and art with which the Louvre was to be filled and adorned. While engaged in the prosecution of that duty, they became acquainted with the victorious general. He easily saw the importance of their friendship, and therefore cultivated it with care; and was happy afterwards to possess them, along with nearly a hundred other philosophers, as his companions in his celebrated expedition to Egypt, expecting no doubt an eclat from such a halo of surrounding science, as might favour the development of his schemes of future greatness. On this expedition, which promised so favourably for the French nation, and which was intended to inflict a mortal stab upon the commercial greatness of Great Britain, Bonaparte set out in the year 1798, accompanied by a crowd of the most eminent men of science that France could boast of. That they might co-operate more effectually in the cause of knowledge, these gentlemen formed themselves into a society, named "The Institute of Egypt," which was constituted on the same plan as the National Institute of France. Their first meeting was on the 6th Fructidor (24th of August, 1798; and after that they continued to assemble, at stated intervals. At these meetings papers were read, by the respective members, on the climate, the inhabitants, and the natural and artificial productions of the country to which they had gone. These memoirs were published in 1800, in Paris, in a single volume entitled, "Memoirs of the Institute of Egypt."

The history of the Institute of Egypt, as related by Cuvier, is not a little singular, and deserves to be stated. Bonaparte, during his occasional intercourse with Berthollet in Italy, was delighted with the simplicity of his manners, joined to a force and depth of thinking which he soon perceived to characterize our chemist. When he returned to Paris, where he enjoyed some months of comparative leisure, he resolved to employ his spare time in studying chemistry under Berthollet. It was at this period that his illustrious pupil imparted to our philosopher his intended expedition to Egypt, of which no whisper was to be spread abroad till the blow was ready to fall; and he begged of him not merely to accompany the army himself, but to choose such men of talent and experience as he conceived fitted to find there an employment worthy of the country which they visited, and of that which sent them forth. To invite men to a hazardous expedition, the nature and destination of which he was not permitted to disclose, was rather a delicate task; yet Berthollet undertook it. He could simply inform them that he would himself accompany them; yet such was the universal esteem in which he was held, such was the confidence universally placed in his honesty and integrity, that all the men of science agreed at once, and without hesitation, to embark on an unknown expedition, the dangers of which he was to share along with them. Had it not been for the link which Berthollet supplied between the commander-in-chief and the men of science, it would have been impossible to have united, as was done on this occasion, the advancement of knowledge with the progress of the French arms.

During the whole of this expedition, Berthollet and Monge distinguished themselves by their firm friendship, and by their mutually braving every danger to which any of the common soldiers could be exposed. Indeed, so intimate was their association that many of the army conceived Berthollet and Monge to be one individual; and it is no small proof of the intimacy of these philosophers with Bonaparte, that the soldiers had a dislike at this double personage, from a persuasion that it had been at his suggestion that they were led into a country which they detested. It happened on one occasion that a boat, in which Berthollet and some others were conveyed up the Nile, was assailed by a troop of Mamelukes, who poured their small shot into it from the banks. In the midst of this perilous voyage, M. Berthollet began very coolly to pick up stones and stuff his pockets with them. When his motive for this conduct was asked, "I am desirous," said he, "that in case of my being shot, my body may sink at once to the bottom of this river, and may escape the insults of these barbarians."

In a conjuncture where a courage of a rarer kind was required, Berthollet was not found wanting. The plague broke out in the French army, and this, added to the many fatigues they had previously endured, the diseases under which they were already labouring, would, it was feared, lead to insurrection on the one hand, or totally sink the spirits of the men on the other. Acre had been besieged for many weeks in vain. Bonaparte and his army had been able to accomplish nothing against it: he was anxious to conceal from his army this disastrous intelligence. When the opinion of Berthollet was asked in council, he spoke at once the plain, though unwelcome truth. He was instantly assailed by the most violent reproaches. "In a week," said he, "my opinion will be unfortunately but too well vindicated." It was as he foretold: and when nothing but a hasty retreat could save the wretched remains of the army of Egypt, the carriage of Berthollet was seized for the convenience of some wounded officers. On this, he travelled on foot, and without the smallest discomposure, across twenty leagues of the desert.

When Napoleon abandoned the army of Egypt, and traversed half the Mediterranean in a single vessel, Berthollet was his companion. After he had put himself at the head of the French government, and had acquired an extent of power, which no modern European potentate had ever before realized, he never forgot his associate. He was in the habit of placing all chemical discoveries to his account, to the frequent annoyance of our chemist; and when an unsatisfactory answer was given him upon any scientific subject, he was in the habit of saying, "Well; I shall ask this of Berthollet." But he did not limit his affection to these proofs of regard. Having been informed that Berthollet's earnest pursuits of science had led him into expenses which had considerably deranged his fortune, he sent for him, and said, in a tone of affectionate reproach, "M. Berthollet, I have always one hundred thousand crowns at the service of my friends." And, in fact, this sum was immediately presented to him.

Upon his return from Egypt, Berthollet was nominated a senator by the first consul; and afterwards received the distinction of grand officer of the Legion of Honour; grand cross of the Order of Reunion; titulary of the Senatory of Montpellier; and, under the emperor, he was created a peer of France, receiving the title of Count. The advancement to these offices produced no change in the manners of Berthollet. Of this he gave a striking proof, by adopting, as his armorial bearing (at the time that others eagerly blazoned some exploit), the plain unadorned figure of his faithful and affectionate dog. He was no courtier before he received these honours, and he remained equally simple and unassuming, and not less devoted to science after they were conferred.

As we advance towards the latter period of his life, we find the same ardent zeal in the cause of science which had glowed in his early youth, accompanied by the same generous warmth of heart that he ever possessed, and which displayed itself in his many intimate friendships still subsisting, though mellowed by the hand of time. At this period La Place lived at Arcueil, a small village about three miles from Paris. Between him and Berthollet there had long subsisted a warm affection, founded on mutual esteem. To be near this illustrious man Berthollet purchased a country-seat.

in the village: there he established a very complete laboratory, fit for conducting all kinds of experiments in every branch of natural philosophy. Here he collected round him a number of distinguished young men, who knew that in his house their ardour would at once receive fresh impulse and direction from the example of Berthollet. These youthful philosophers were organized by him into a society, to which the name of Société d'Arcueil was given. M. Berthollet was himself the president, and the other members were La Place, Biot, Gay-Lussac, Thenard, Collet-Descotils, Decandolle, Humboldt, and A. B. Berthollet. This society published three volumes of very valuable memoirs. The energy of this society was unfortunately paralyzed by an untoward event, which imbittered the latter days of this amiable man. His only son, M. A. B. Berthollet, in whom his happiness was wrapped up, was unfortunately afflicted with a lowness of spirits which rendered his life wholly insupportable to him. Retiring to a small room, he locked the door, closed up every chink and crevice which might admit the air, carried writing materials to a table, on which he placed a second-watch, and then seated himself before it. He now marked precisely the hour, and lighted a brasier of charcoal beside him. He continued to note down the series of sensations he then experienced in succession, detailing the approach and rapid progress of delirium; until, as time went on, the writing became confused and illegible, and the young victim dropped dead upon the floor.

After this event the spirits of the old man never again rose. Occasionally some discovery, extending the limits of his favourite science, engrossed his interest and attention for a short time: but such intervals were rare, and shortlived. The restoration of the Bourbons, and the downfal of his friend and patron Napoleon, added to his sufferings by diminishing his income, and reducing him from a state of affluence to comparative embarrassment. But he was now old, and the end of his life was approaching. In 1822 he was attacked by a slight fever, which left behind it a number of boils: these were soon followed by a gangrenous ulcer of uncommon size. Under this he suffered for several months with surprising fortitude. He himself, as a physician, knew the extent of his danger, felt the inevitable progress of the malady, and calmly regarded the slow approach of death. At length, after a tedious period of suffering, in which his equanimity had never once been shaken, he died on the 6th of November, when he had nearly completed the seventy-fourth year of his age.

His papers are exceedingly numerous, and of a very miscellaneous nature, amounting to more than eighty. The earlier were chiefly inserted into the various volumes of the Memoirs of the Academy. He furnished many papers to the Annales de Chimie and the Journal de Physique, and was also a frequent contributor to the Society of Arcueil, in the different volumes of whose transactions several memoirs of his are to be found. He was the author likewise of two separate works, comprising each two octavo volumes. These were his Elements of the Art of Dyeing, first published in 1791, in a single volume: but the new and enlarged edition of 1814 was in two volumes; and his Essay on Chemical Statics, published about the beginning of the present century. I shall notice his most important papers.

His earlier memoirs on sulphurous acid, on volatile alkali, and on the decomposition of nitre, were encumbered by the phlogistic theory, which at that time he defended with great zeal, though he afterwards retracted these his first opinions upon all these subjects. Except his paper on soaps, in which he shows that they are chemical compounds of an oil (acting the part of an acid) and an alkaline base, and his proof that phosphoric acid exists ready formed in the body (a fact long before demonstrated by Gahn and Scheele), his papers published before he became an antiphlogistian are of inferior merit.

In 1785 he demonstrated the nature and proportion of the constituents of ammonia, or volatile alkali. This substance had been collected in the gaseous form by the indefatigable Priestley, who had shown also that when electric sparks are made to pass for some time through a given volume of this gas, its bulk is nearly doubled. Berthollet merely repeated this experiment of Priestley, and analyzed the new gases evolved by the action of electricity. This gas he found a mixture of three volumes hydrogen and one volume azotic gas: hence it was evident that ammoniacal gas is a compound of three volumes of hydrogen and one volume of azotic gas united together, and condensed into two volumes. The same discovery was made about the same time by Dr. Austin, and published in the Philosophical Transactions. Both sets of experiments were made without any knowledge of what was done by the other: but it is admitted, on all hands, that Berthollet had the priority in point of time.

It was about this time, likewise, that he published his first paper on chlorine. He observed, that when water, impregnated with chlorine, is exposed to the light of the sun, the water loses its colour, while, at the same time, a quantity of oxygen gas is given out. If we now examine the water, we find that it contains no chlorine, but merely a little muriatic acid. This fact, which is undoubted, led him to conclude that chlorine is decomposed by the action of solar light, and that its two elements are muriatic acid and oxygen. This led to the notion that the basis of muriatic acid is capable of combining with various doses of oxygen, and of forming various acids, one of which is chlorine: on that account it was called *oxygenized muriatic acid* by the French chemists, which unwieldy appellation was afterwards shortened by Kirwan into *oxymuriatic acid*.

Berthollet observed that when a current of chlorine gas is passed through a solution of carbonate of potash an effervescence takes place owing to the disengagement of carbonic acid gas. By-and-by crystals are deposited in fine silky scales, which possess the property of detonating with combustible bodies still more violently than saltpetre. Berthollet examined these crystals and showed that they were compounds of potash with an acid containing much more oxygen than oxymuriatic acid. He considered its basis as muriatic acid, and distinguished it by the name of hyper-oxymuriatic acid.

It was not till the year 1810, that the inaccuracy of these opinions was established. Gay-Lussac and Thenard attempted in vain to extract oxygen from chlorine. They showed that not a trace of that principle could be detected. Next year Davy took up the subject and concluded from his experiments that *chlorine* is a simple substance, that muriatic acid is a compound of chlorine and hydrogen, and hyper-oxymuriatic acid of chlorine and oxygen. Gay-Lussac obtained this acid in a separate state, and gave it the name of *chloric acid*, by which it is now known.

Scheele, in his original experiments on chlorine, had noticed the property which it has of destroying vegetable colours. Berthollet examined this property with care, and found it so remarkable that he proposed it as a substitute for exposure to the sun in bleaching. This suggestion alone would have immortalized Berthollet had he done nothing else; since its effect upon some of the most important of the manufactures of Great Britain has been scarcely inferior to that of the steam-engine itself. Mr. Watt happened to be in Paris when the idea suggested itself to Berthollet. He not only communicated it to Mr. Watt, but showed him the process in all its simplicity. It consisted in nothing else than in steeping the cloth to be bleached in water impregnated with chlorine gas. Mr. Watt, on his return to Great Britain, prepared a quantity of this liquor, and sent it to his father-in-law, Mr. Macgregor, who was a bleacher in the neighbourhood of Glasgow. He employed it successfully, and thus was the first individual who tried the new process of bleaching in Great Britain. For a number of years the bleachers in Lancashire and the neighbourhood of Glasgow were occupied in bringing the process to perfection. The disagreeable smell of the chlorine was a a great annoyance. This was attempted to be got rid of by dissolving potash in the water to be impregnated with chlorine; but it was found to injure considerably the bleaching powers of the gas. The next method tried was to mix the water with quicklime, and then to pass a current of chlorine through it. The quicklime was dissolved, and the liquor thus constituted was found to answer very well. The last improvement was to combine the chlorine with dry lime. At first two atoms of lime were united to one atom of chlorine; but of late years it is a compound of one atom of lime, and one of chlorine. This chloride is simply dissolved in water, and the cloth to be bleached is steeped in it. For all these improvements, which have brought the method of bleaching by means of chlorine to great simplicity and perfection, the bleachers are indebted to Knox, Tennant, and Mackintosh, of Glasgow; by whose indefatigable exertions the mode of manufacturing chloride of lime has been brought to a state of perfection.

Berthollet's experiments on prussic acid and the prussiates deserve also to be mentioned, as having a tendency to rectify some of the ideas at that time entertained by chemists, and to advance their knowledge of one of the most difficult departments of chemical investigation. In consequence of his experiments on the nature and constituents of sulphuretted hydrogen, he had already concluded that it was an acid, and that it was destitute of oxygen: this had induced him to refuse his assent to the hypothesis of Lavoisier, that *oxygen* is the *acidifying principle*. Scheele, in his celebrated experiments on prussic acid, had succeeded in ascertaining that its constituents were carbon and azote; but he had not been able to make a rigid analysis of that acid, and consequently to demonstrate that oxygen did not enter into it as a constituent. Berthollet took up the subject, and though his analysis was also incomplete, he satisfied himself, and rendered it exceedingly probable, that the only constituents of this acid were, carbon, azote, and hydrogen, and that oxygen did not enter into it as a constituent. This was another reason for rejecting the notion of *oxygen* as an acidifying principle. Here were two acids capable of neutralizing bases, namely, sulphuretted hydrogen and prussic acid, and yet neither of them contained oxygen. He found that when prussic acid was treated with chlorine, its properties were altered; it acquired a different smell and taste, and no longer precipitated iron blue, but green. From his opinion respecting the nature of chlorine, that it was a compound of muriatic acid and oxygen, he naturally concluded that by this process he had formed a new prussic acid by adding oxygen to the old constituents. He therefore called this new substance *oxyprussic acid.* It has been proved by the more recent experiments of Gay-Lussac, that the new acid of Berthollet is a compound of *cyanogen* (the prussic acid deprived of hydrogen) and *chlorine;* it is now called *chloro-cyanic acid*, and is known to possess the characters assigned it by Berthollet: it constitutes, therefore, a new example of an acid destitute of oxygen. Berthollet was the first person who obtained prussiate of potash in regular crystals; the salt was known long before, but had been always used in a state of solution.

Berthollet's discovery of fulminating silver, and his method of obtaining pure hydrated potash and soda, by means of alcohol, deserve to be mentioned. This last process was of considerable importance to analytical chemistry. Before he published his process, these substances in a state of purity were not known.

I think it unnecessary to enter into any details respecting his experiments on sulphuretted hydrogen, and the hydrosulphurets and sulphurets. They contributed essentially to elucidate that obscure part of chemistry. But his success was not perfect; nor did we understand completely the nature of these compounds, till the nature of the alkaline bases had been explained by the discoveries of Davy.

The only other work of Berthollet, which I think it necessary to notice here, is his book entitled "Chemical Statics," which he published in 1803. He had previously drawn up some interesting papers on the subject, which were published in the Memoirs of the Institute. Though chemical affinity constitutes confessedly the basis of the science, it had been almost completely overlooked by Lavoisier, who had done nothing more on the subject than drawn up some tables of affinity, founded on very imperfect data. Morveau had attempted a more profound investigation of the subject in the article *Affinité*, inserted in the chemical part of the Encyclopédie Méthodique. His object was, in imitation of Buffon, who had preceded him in the same investigation, to prove that chemical affinity is merely a case of the *attraction of gravitation*. But it is beyond our reach, in the present state of our knowledge, to determine the amount of attraction which the atoms of bodies exert with respect to each other. This was seen by Newton, and also by Bergman, who satisfied themselves with considering it as an attraction, without attempting to determine its amount; though Newton, with his usual sagacity, was inclined, from the phenomena of light, to consider the attraction of affinity as much stronger than that of gravitation, or at least as increasing much more rapidly, as the distances between the attracting particles diminished.

Bergman, who had paid great attention to the subject, considered affinity as a certain determinate attraction, which the atoms of different bodies exerted towards each other. This attraction varies in intensity between every two bodies, though it is constant between each pair. The consequence is, that these intensities may be denoted by numbers. Thus, suppose a body m, and the atoms of six other bodies, a, b, c, d, e, f, to have an affinity for m, the forces by which they are attracted towards each other may be represented by the numbers x, $x+1$, $x+2$, $x+3$, $x+4$, $x+5$. And the attractions may be represented thus:

Attraction between m & $a = x$
m & $b = x+1$
m & $c = x+2$
m & $d = x+3$
m & $e = x+4$
m & $f = x+5$

Suppose we have the compound $m\ a$, if we present b, it will unite with *m* and displace *a*, because the attraction between *m* and *a* is only x, while that between *m* & *b* is x+1: c will displace *b*; *d* will displace c, and so on, for the same reason. On this account Bergman considered affinity as an *elective attraction*, and in his opinion the intensity may always be estimated by decomposition. That substance which displaces another from a third, has a greater affinity than the body which is displaced. If *b* displace *a* from the compound *a m*, then *b* has a greater affinity for *m* than *a* has.

The object of Berthollet in his Chemical Statics, was to combat this opinion of Bergman, which had been embraced without examination by chemists in general. If affinity be an attraction, Berthollet considered it as evident that it never could occasion decomposition. Suppose *a* to have an affinity for *m*, and *b* to have an affinity for the same substances. Let the affinity between *b* and *m* be greater than that between *a m*. Let *b* be mixed with a solution of the compound *a m*, then in that case *b* would unite with *a m*, and form the triple compound *a m b*. Both *a* and *b* would at once unite with *m*. No reason can be assigned why *a* should separate from *m*, and *b* take its place. Berthollet admitted that in fact such decompositions often happened; but he accounted for them from other causes, and not from the superior affinity of one body over another. Suppose we have a solution of *sulphate of soda* in water. This salt is a compound of *sulphuric acid* and *soda*; two substances between which a strong affinity subsists, and which therefore always unites whenever they come in contact. Suppose we have dissolved in another portion of water, a quantity of barytes, just sufficient to saturate the sulphuric acid in the sulphate of soda. If we mix these two solutions together. The barytes will combine with the sulphuric acid and the compound (*sulphate of barytes*) will fall to the bottom, leaving a pure solution of soda in the water. In this case the barytes has seized all the sulphuric acid, and displaced the soda. The reason of this, according to Berthollet, is not that barytes has a stronger affinity for sulphuric acid than soda has; but because sulphate of barytes is insoluble in water. It therefore falls down, and of course the sulphuric acid is withdrawn from the soda. But if we add to a solution of sulphate of soda as much potash as will saturate all the sulphuric acid, no such decomposition will take place; at least, we have no evidence that it does. Both the alkalies, in this case, will unite to the acid and form a triple compound, consisting of potash, sulphuric acid, and soda. Let us now concentrate the solution by evaporation, and crystals of sulphate of potash will fall down. The reason is, that sulphate of potash is not nearly so soluble in water as sulphate of soda. Hence it separates; not because sulphuric acid has a greater affinity for potash than for soda, but because sulphate of potash is a much less soluble salt than sulphate of soda.

This mode of reasoning of Berthollet is plausible, but not convincing: it is merely an *argumentum ad ignorantiam*. We can only prove the decomposition by separating the salts from each other, and this can only be done by their difference of solubility. But cases occur in which we can judge that decomposition has taken place from some other phenomena than precipitation. For example, *nitrate of copper* is a *blue* salt, while *muriate of copper* is *green*. If into a solution of nitrate of copper we pour muriatic acid, no precipitation appears, but the colour changes from blue to green. Is not this an evidence that the muriatic acid has displaced the nitric, and that the salt held in solution is not nitrate of copper, as it was at first, but muriate of copper?

Berthollet accounts for all decompositions which take place when a third body is added, either by insolubility or by *elasticity:* as, for example, when sulphuric acid is poured into a solution of carbonate of ammonia, the carbonic acid all flies off, in consequence of its elasticity, and the sulphuric acid combines with the ammonia in its place. I confess that this explanation, of the reason why the carbonic acid flies off, appears to me very defective. The ammonia and carbonic acid are united by a force quite sufficient to overcome the elasticity of the carbonic acid. Accordingly, it exhibits no tendency to escape. Now, why should the elasticity of the acid cause it to escape when sulphuric acid is added? It certainly could not do so, unless it has weakened the affinity by which it is kept united to the ammonia. Now this is the very point for which Bergman contends. The subject will claim our attention afterwards, when we come to the electro-chemical discoveries, which distinguished the first ten years of the present century.

Another opinion supported by Berthollet in his Chemical Statics is, that quantity may be made to overcome force; or, in other words, that if we mix a great quantity of a substance which has a weaker affinity with a small quantity of a substance which has a stronger affinity, the body having the weaker affinity will be able to overcome the other, and combine with a third body in place of it. He gave a number of instances of this; particularly, he showed that a large quantity of potash, when mixed with a small quantity of sulphate of barytes, is able to deprive the barytes of a portion of its sulphuric acid. In this way he accounted for the decomposition of the common salt, by carbonate of lime in the soda lakes in Egypt; and the decomposition of the same salt by iron, as noticed by Scheele.

I must acknowledge myself not quite satisfied with Berthollet's reasoning on this subject. No doubt if two atoms of a body having a weaker affinity, and one atom of a body having a stronger affinity, were placed at equal distances from an atom of a third body, the force of the two atoms might overcome that of the one atom. And it is possible that such cases may occasionally occur: but such a balance of distances must be rare and accidental. I cannot but think that all the cases adduced by Berthollet are of a complicated nature, and admit of an explanation independent of the efficacy of mass. And at any rate, abundance of instances might be stated, in which mass appears to have no preponderating effect whatever. Chemical decomposition is a phenomenon of so complicated a nature, that it is more than doubtful whether we are yet in possession of data sufficient to enable us to analyze the process with accuracy.

Another opinion brought forward by Berthollet in his work was of a startling nature, and occasioned a controversy between him and Proust which was carried on for some years with great spirit, but with perfect decorum and good manners on both sides. Berthollet affirmed that bodies were capable of uniting with each other in all possible proportions, and that there is no such thing as a definite compound, unless it has been produced by some accidental circumstances, as insolubility, volatility, &c. Thus every metal is capable of uniting with all possible doses of oxygen. So that instead of one or two oxides of every metal, an infinite number of oxides of each metal exist. Proust affirmed that all compounds are definite. Iron, says he, unites with oxygen only in two proportions; we have either a compound of 3·5 iron and 1 oxygen, or of 3·5 iron and 1·5 oxygen. The first constitutes the *black*, and the second the *red* oxide of iron; and beside these there is no other. Every one is now satisfied that Proust's view of the subject was correct, and Berthollet's erroneous. But a better opportunity will occur hereafter to explain this subject, or at least to give the information respecting it which we at present possess.

Berthollet in this book points out the quantity of each base necessary to neutralize a given weight of acid, and he considers the strength of affinity as inversely that quantity. Now of all the bases known when Berthollet wrote, ammonia is capable of saturating the greatest quantity of acid. Hence he considered its affinity for acids as stronger than that of any other base. Barytes, on the contrary, saturates the smallest quantity of acid; therefore its affinity for acids is smallest. Now ammonia is separated from acids by all the other bases; while there is not one capable of separating barytes. It is surprising that the notoriety of this fact did not induce him to hesitate, before he came to so problematical a conclusion. Mr. Kirwan had already considered the force of affinity as directly proportional to the quantity of base necessary to saturate a given weight of acid. When we consider the subject metaphysically, Berthollet's opinion is most plausible; for it is surely natural to consider that body as the strongest which produces the greatest effect. Now when we deprive an acid of its properties, or neutralize it by adding a base, one would be disposed to consider that base as acting with most energy, which with the smallest quantity of matter is capable of producing a given effect. This was the way that Berthollet reasoned. But if we attend to the power which one base has of displacing another, we shall find it very nearly proportional to the weight of it necessary to saturate a given weight of acid; or, at least those bases act most powerfully in displacing others of which the greatest quantity is necessary to saturate a given weight of acid. Kirwan's opinion, therefore, was more conformable to the order of decomposition. These two opposite views of the subject show clearly that neither Kirwan nor Berthollet had the smallest conception of the atomic theory; and, consequently, that the allegation of Mr. Higgens, that he had explained the atomic theory in his book on phlogiston, published in the year 1789, was not well founded. Whether Berthollet had read that book I do not know, but there can be no doubt that it was perused by Kirwan; who, however, did not receive from it the smallest notions respecting the atomic theory. Had he imbibed any such notions, he never would have considered chemical affinity as capable of being measured by the weight of base capable of neutralizing a given weight of acid.

Berthollet was not only a man of great energy of character, but of the most liberal feelings and benevolence. The only exception to this is his treatment of M. Clement. This gentleman, in company with M. Desormes, had examined the carbonic oxide of Priestley, and had shown as Cruikshanks had done before them, that it is a compound of carbon and oxygen, and that it contains no hydrogen whatever. Berthollet examined the same gas, and he published a paper to prove that it was a triple compound of oxygen, carbon, and hydrogen. This occasioned a controversy, which chemists have finally determined in favour of the opinion of Clement and Desormes. Berthollet, during this discussion, did not on every occasion treat his opponents with his accustomed temper and liberality; and ever after he opposed all attempts on the part of Clement to be admitted a member of the Institute. Whether there was any other reason for this conduct on the part of Berthollet, besides difference of opinion respecting the composition of carbonic oxide, I do not know: nor would it be right to condemn him without a more exact knowledge of all the circumstances han I can pretend to.

Antoine François de Fourcroy, was born at Paris on the 15th of June, 1755. His family had long resided in the capital, and several of his ancestors had distinguished themselves at the bar. But the branch from which he sprung had gradually sunk into poverty. His father exercised in Paris the trade of an apothecary, in consequence of a charge which he held in the house of the Duke of Orleans. The corporation of apothecaries having obtained the general suppression of all such charges, M. de Fourcroy, the father, was obliged to renounce his mode of livelihood; and his son grew up in the midst of the poverty produced by the monopoly of the privileged bodies in Paris. He felt this situation the more keenly, because he possessed from nature an extreme sensibility of temper. When he lost his mother, at the age of seven years, he attempted to throw himself into her grave. The care of an elder sister preserved him with difficulty till he reached the age at which it was usual to be sent to college. There he was unlucky enough to meet with a brutal master, who conceived an aversion for him and treated him with cruelty: the consequence was, a dislike to study; and he quitted the college at the age of fourteen, somewhat less informed than when he went to it.

His poverty now was such that he was obliged to endeavour to support himself by becoming writing-master. He had even some thoughts of going on the stage; but was prevented by the hisses bestowed on a friend of his who had unadvisedly entered upon that perilous career, and was treated in consequence without mercy by the audience. While uncertain what plan to follow, the advice of Viq. d'Azyr induced him to commence the study of medicine.

This great anatomist was an acquaintance of M. de Fourcroy, the father. Struck with the appearance of his son, and the courage with which he struggled with his bad fortune, he conceived an affection for him, and promised to direct his studies, and even to assist him during their progress. The study of medicine to a man in his situation was by no means an easy task. He was obliged to lodge in a garret, so low in the roof that he could only stand upright in the middle of the room. Beside him lodged a water-carrier with twelve children. Fourcroy acted as physician to this numerous family, and in recompence was always supplied with abundance of water. He contrived to support himself by giving lessons to other students, by facilitating the researches of richer writers, and by some translations which he sold to a bookseller. For these he was only half paid; but the conscientious bookseller offered thirty years afterwards to make up the deficiency, when his creditor was become director-general of public instruction.

Fourcroy studied with so much zeal and ardour that he soon became well acquainted with the subject of medicine. But this was not sufficient. It was necessary to get a doctor's degree, and all the expenses at that time amounted to 250*l*. An old physician, Dr. Diest, had left funds to the faculty to give a gratuitous degree and licence, once every two years, to the poor student who should best deserve them. Fourcroy was the most conspicuous student at that time in Paris. He would therefore have reaped the benefit of this benevolent institution had it not been for the unlucky situation in which he was placed. There happened to exist a quarrel between the faculty charged with the education of medical men and the granting of degrees, and a society recently formed by government for the improvement of the medical art. This dispute had been carried to a great length, and had attracted the attention of all the frivolous and idle inhabitants of Paris. Viq. d'Azyr was secretary to the society, and of course one of its most active champions; and was, in consequence, particularly obnoxious to the faculty of medicine at Paris. Fourcroy was unluckily the acknowledged *protégée* of this eminent anatomist. This was sufficient to induce the faculty of medicine to refuse him a gratuitous degree. He would have been excluded in consequence of this from entering on the career of a practitioner, had not the society, enraged at this treatment, and influenced by a violent party spirit, formed a subscription, and contributed the necessary expenses.

It was no longer possible to refuse M. de Fourcroy the degree of doctor, when he was thus enabled to pay for it. But above the simple degree of doctor there was another, entitled *docteur regent*, which depended entirely on the votes of the faculty. It was unanimously refused to M. de Fourcroy. This refusal put it out of his power afterwards to commence teacher in the medical school, and gave the medical faculty the melancholy satisfaction of not being able to enroll among their number the most celebrated professor in Paris. This violent and unjust conduct of the faculty of medicine made a deep impression on the mind of Fourcroy, and contributed not a little to the subsequent downfal of that powerful body.

Fourcroy being thus entitled to practise in Paris, his success depended entirely on the reputation which he could contrive to establish. For this purpose he devoted himself to the sciences connected with medicine, as the shortest and most certain road by which he could reach his object. His first writings showed no predilection for any particular branch of science. He wrote upon *chemistry*, *anatomy*, and *natural history*. He published an Abridgment of the History of Insects, and a Description of the Bursæ Mucosæ of the Tendons. This last piece seems to have given him the greatest celebrity; for in 1785 he was admitted, in consequence of it, into the academy as an anatomist. But the reputation of Bucquet, at that time very high, gradually drew his particular attention to chemistry, and he retained this predilection during the rest of his life.

Bucquet was at that time professor of chemistry in the Medical School of Paris, and was greatly celebrated and followed on account of his eloquence, and the elegance of his language. Fourcroy became in the first place his pupil, and afterwards his particular friend. One day, when a sudden attack of disease prevented him from lecturing as usual, he entreated Fourcroy to supply his place. Our young chemist at first declined, and alleged his ignorance of the method of addressing a public audience. But, overcome by the persuasions of Bucquet, he at last consented: and in this, his first essay, he spoke two hours without disorder or hesitation, and acquitted himself to the satisfaction of his whole audience. Bucquet soon after substituted him in his place, and it was in his laboratory and in his class-room that he first made himself acquainted with chemistry. He was enabled at the death of Bucquet, in consequence of an advantageous marriage that he had made, to purchase the apparatus and cabinet of his master; and although the faculty of medicine would not allow him to succeed to the chair of Bucquet, they could not prevent him from succeeding to his reputation.

There was a kind of college which had been established in the Jardin du Roi, which at that time was under the superintendence of Buffon, and Macquer was the professor of chemistry in this institution. On the death of this chemist, in 1784, both Berthollet and Fourcroy offered themselves as candidates for the vacant chair. The voice of the public was so loud in favour of Fourcroy, that he was appointed to the situation in spite of the high character of his antagonist and the political influence which was exerted in his favour. He filled this chair for twenty-five years, with a reputation for eloquence continually on the increase. Such were the crowds, both of men and women, who flocked to hear him, that it was twice necessary to enlarge the size of the lecture room.

After the revolution had made some progress, he was named a member of the National Convention in the autumn of the memorable year 1793. It was during the reign of terror, when the Convention itself, and with it all France, was under the absolute dominion of one of the most sanguinary monsters that ever existed: it was almost equally dangerous for the members of the Convention to remain silent, or to take an active part in the business of that assembly. Fourcroy never opened his mouth in the Convention till after the death of Robespierre; at this period he had influence enough to save the lives of some men of merit: among others, of Darcet, who did not know the obligation under which he lay to him till long after; at last his own life was threatened, and his influence, of course, completely annihilated.

It was during this unfortunate and disgraceful period, that many eminent men lost their lives; among others, Lavoisier; and Fourcroy is accused of having contributed to the death of this illustrious chemist: but Cuvier entirely acquits him of this atrocious charge, and assures us that it was urged against him merely out of envy at his subsequent elevation. "If in the rigorous researches which we have made," says Cuvier in his Eloge of Fourcroy, "we had found the smallest proof of an atrocity so horrible, no human power could have induced us to sully our mouths with his Eloge, or to have pronounced it within the walls of this temple, which ought to be no less sacred to honour than to genius."

Fourcroy began to acquire influence only after the 9th Thermidor, when the nation was wearied with destruction, and when efforts were making to restore those monuments of science, and those public institutions for education, which during the wantonness and folly of the revolution had been overturned and destroyed. Fourcroy was particularly active in this renovation, and it was to him, chiefly, that the schools established in France for the education of youth are to be ascribed. The Convention had destroyed all the colleges, universities, and academies throughout France. The effects of this absurd abolition soon became visible; the army stood in need of surgeons and physicians, and there were none educated to supply the vacant places: three new schools were founded for educating medical men; they were nobly endowed. The term *schools of medicine* was proscribed as too aristocratical; they were distinguished by the ridiculous appellation of *schools of health*. The *Polytechnic School* was next instituted, as a kind of preparation for the exercise of the military profession, where young men could be instructed in mathematics and natural philosophy, to make them fit for entering the schools of the artillery, of engineers, and of the marine. The *Central Schools* was another institution for which France was indebted to the efforts of Fourcroy. The idea was good, though it was very imperfectly executed. It was to establish a kind of university in every department, for which the young men were to be prepared by a sufficient number of inferior schools scattered through the department. But unfortunately these inferior schools were never properly established or endowed; and even the central schools themselves were never supplied with proper masters. Indeed, it was found impossible to furnish such a number of masters at once. On that account, an institution was established in Paris, called the *Normal School*, for the express purpose of educating a sufficient number of masters to supply the different central schools.

Fourcroy, either as a member of the Convention or of the *Council of the Ancients*, took an active part in all these institutions, as far as regarded the plan and the establishment. He was equally concerned in the establishment of the Institute and of the *Musée d'Histoire Naturelle.* This last was endowed with the utmost liberality, and Fourcroy was one of the first professors; as he was also in the School of Medicine and the Polytechnic School. He was equally concerned in the restoration of the university, which constituted one of the most useful parts of Bonaparte's reign.

The violent exertions which he made in the numerous situations which he filled, and the prodigious activity which he displayed, gradually undermined his constitution. He himself was sensible of his approaching death, and announced it to his friends as an event which would speedily take place. On the 16th of December, 1809, after signing some despatches, he suddenly cried out, *Je* *suis mort (I am dead),* and dropped lifeless on the ground.

He was twice married: first to Mademoiselle Bettinger, by whom he had two children, a son and a daughter, who survived him. He was married for the second time to Madame Belleville, the widow of Vailly, by whom he had no family. He left but little fortune behind him; and two maiden sisters, who lived with him, depended afterwards for their support on his friend M. Vauquelin.

Notwithstanding the vast quantity of papers which he published, it will be admitted, without dispute, that the prodigious reputation which he enjoyed during his lifetime was more owing to his eloquence than to his eminence as a chemist—though even as a chemist he was far above mediocrity. He must have possessed an uncommon facility of writing. Five successive editions of his System of Chemistry appeared, each of them gradually increasing in size and value: the first being in two volumes and the last in ten. This last edition he wrote in sixteen months: it contains much valuable information, and doubtless contributed considerably to the general diffusion of chemical knowledge. Its style is perhaps too diffuse, and the spirit of generalizing from particular, and often ill-authenticated facts, is carried to a vicious length. Perhaps the best of all his productions is his Philosophy of Chemistry. It is remarkable for its conciseness, its perspicuity, and the neatness of its arrangement.

Besides these works, and the periodical publication entitled "Le Médecin éclairé," of which he was the editor, there are above one hundred and sixty papers on chemical subjects, with his name attached to them, which appeared in the Memoirs of the Academy and of the Institute; in the Annales de Chimie, or the Annales de Musée d'Histoire Naturelle; of which last work he was the original projector. Many of these papers contained analyses both animal, vegetable, and mineral, of very considerable value. In most of them, the name of Vauquelin is associated with his own as the author; and the general opinion is, that the experiments were all made by Vauquelin; but that the papers themselves were drawn up by Fourcroy.

It would serve little purpose to go over this long list of papers; because, though they contributed essentially to the progress of chemistry, yet they exhibit but few of those striking discoveries, which at once alter the face of the science, by throwing a flood of light on every thing around them. I shall merely notice a few of what I consider as his best papers.

1. He ascertained that the most common biliary calculi are composed of a substance similar to spermaceti. This substance, in consequence of a subsequent discovery which he made during the removal of the dead bodies from the burial-ground of the Innocents at Paris; namely, that these bodies are converted into a fatty matter, he called *adipocire*. It has since been distinguished by the name of *cholestine;* and has been shown to possess properties different from those of adipocire and spermaceti.

2. It is to him that we are indebted for the first knowledge of the fact, that the salts of magnesia and ammonia have the property of uniting together, and forming double salts.

3. His dissertation on the sulphate of mercury contains some good observations. The same remark applies to his paper on the action of ammonia on the sulphate, nitrate, and muriate of mercury. He first described the double salts which are formed.

4. The analysis of urine would have been valuable had not almost all the facts contained in it been

anticipated by a paper of Dr. Wollaston, published in the Philosophical Transactions. It is to him that we are indebted for almost all the additions to our knowledge of calculi since the publication of Scheele's original paper on the subject.

5. I may mention the process of Fourcroy and Vauquelin for obtaining pure barytes, by exposing nitrate of barytes to a red heat, as a good one. They discovered the existence of phosphate of magnesia in bones, of phosphorus in the brain and in the milts of fishes, and of a considerable quantity of saccharine matter in the bulb of the common onion; which, by undergoing a kind of spontaneous fermentation was converted into *manna*.

In these, and many other similar discoveries, which I think it unnecessary to notice, we do not know what fell to the share of Fourcroy and what to Vauquelin; but there is one merit at least to which Fourcroy is certainly entitled, and it is no small one: he formed and brought forward Vauquelin, and proved to him, ever after, a most steady and indefatigable friend. This is bestowing no small panegyric on his character; for it would have been impossible to have retained such a friend through all the horrors of the French revolution, if his own qualities had not been such as to merit so steady an attachment.

Louis Bernard Guyton de Morveau was born at Dijon on the 4th of January, 1737. His father, Anthony Guyton, was professor of civil law in the University of Dijon, and descended from an ancient and respectable family. At the age of seven he showed an uncommon mechanical turn: being with his father at a small village near Dijon, he there happened to meet a public officer returning from a sale, whence he had brought back a clock that had remained unsold on account of its very bad condition. Morveau supplicated his father to buy it. The purchase was made for six francs. Young Morveau took it to pieces and cleaned it, supplied some parts that were wanting, and put it up again without any assistance. In 1799 this very clock was resold at a higher price, together with the estate and house in which it had been originally placed; having during the whole of that time continued to go in the most satisfactory manner. When only eight years of age, he took his mother's watch to pieces, cleaned it, and put it up again to the satisfaction of all parties.

After finishing his preliminary studies in his father's house, he went to college, and terminated his attendance on it at the age of sixteen. About this time he was instructed in botany by M. Michault, a friend of his father, and a naturalist of some eminence. He now commenced law student in the University of Dijon; and, after three years of intense application, he went to Paris to acquire a knowledge of the practice of the law.

While in Paris, he not only attended to law, but cultivated at the same time several branches of polite literature. In 1756 he paid a visit to Voltaire, at Ferney. This seems to have inspired him with a love of poetry, particularly of the descriptive and satiric kind. About a year afterwards, when only twenty, he published a poem called "Le Rat Iconoclaste, ou le Jesuite croquée." It was intended to throw ridicule on a well-known anecdote of the day, and to assist in blowing the fire that already threatened destruction to the obnoxious order of Jesuits. The adventure alluded to was this: Some nuns, who felt a strong predilection for a Jesuit, their spiritual director, were engaged in their accustomed Christmas occupation of modelling a representation of a religious mystery, decorated with several small statues representing the holy personages connected with the subject, and among them that of the ghostly father; but, to mark their favourite, his statue was made of loaf sugar. The following day was destined for the triumph of the Jesuit: but, meanwhile, a rat had devoured the valuable puppet. The poem is written after the agreeable manner of the celebrated poem, "Ververt."

At the age of twenty-four he had already pleaded several important causes at the bar, when the office of advocate-general, at the parliament of Dijon, was advertised for sale. At that time all public situations, however important, were sold to the best bidder. His father having ascertained that this place would be acceptable to his son, purchased it for forty thousand francs. The reputation of the young advocate, and his engaging manners, facilitated the bargain.

In 1764 he was admitted an honorary member of the Academy of Sciences, Arts, and Belles Lettres, of Dijon. Two months after, he presented to the assembled chamber of the parliament of Burgundy, a memoir on public instruction, with a plan for a college, on the principles detailed in his work. The encomiums which every public journal of the time passed on this production, and the flattering letters which he received, were unequivocal proofs of its value. In this memoir he endeavoured to prove that man is *bad* or *good*, according to the education which he has received. This doctrine was contrary to the creed of Diderot, who affirmed, in his Essay on the Life of Seneca, that nature makes wicked persons, and that the best institutions cannot render them good. But this mischievous opinion was successfully refuted by Morveau, in a letter to an anonymous friend.

The exact sciences were so ill taught, and lamely cultivated at Dijon, during the time of his university education, that after his admission into the academy his notions on mechanics and natural philosophy were scanty and inaccurate. Dr. Chardenon was in the habit of reading memoirs on chemical subjects; and on one occasion Morveau thought it necessary to hazard some remarks which were ill received by the doctor, who sneeringly told him that having obtained such success in literature, he had better rest satisfied with the reputation so justly acquired, and leave chemistry to those who knew more of the matter.

Provoked at this violent remark, he resolved upon taking an honourable revenge. He therefore applied himself to the study of Macquer's Theoretical and Practical Chemistry, and of the Manual of Chemistry which Beaumé had just published. To the last chemist he also sent an extensive order for chemical preparations and utensils, with a view of forming a small laboratory near his office. He began by repeating many of Beaumé's experiments, and then trying his inexperienced hand at original researches. He soon found himself strong enough to attack the doctor. The latter had just been reading a memoir on the analysis of different kinds of oil; and Morveau combated some of his opinions with so much skill and sagacity, as astonished every one present. After the meeting, Dr. Chardenon addressed him thus: "You are born to be an honour to chemistry. So much knowledge could only have been gained by genius united with perseverance. Follow your new pursuit, and confer with me in your difficulties."

But this new pursuit did not prevent Morveau from continuing to cultivate literature with success. He wrote an *Eloge* of Charles V. of France, surnamed *the Wise*, which had been given out as the subject of a prize, by the academy. A few months afterwards, at the opening of the session of parliament, he delivered a discourse on the actual state of jurisprudence; on which subject, three years after, he composed a more extensive and complete work. No code of laws demanded reform more urgently than those of France, and none saw more clearly the necessity of such a reformation.

About this time a young gentleman of Dijon had taken into his house an adept, who offered, upon being furnished with the requisite materials, to produce gold in abundance; but, after six months of expensive and tedious operations (during which period the roguish pretender had secretly distilled many oils, &c., which he disposed of for his own profit), the gentleman beginning to doubt the sincerity of his instructer, dismissed him from his service and sold the whole of his apparatus and materials to Morveau for a trifling sum.

Soon after he repaired to Paris, to visit the scientific establishments of that metropolis, and to purchase preparations and apparatus which he still wanted to enable him to pursue with effect his favourite study. For this purpose he applied to Beaumé, then one of the most conspicuous of the French chemists. Pleased with his ardour, Beaumé inquired what courses of chemistry he had attended. " None," was the answer.—" How then could you have learned to make experiments, and above all, how could you have acquired the requisite dexterity ?"—" Practice," replied the young chemist, " has been my master; melted crucibles and broken retorts my tutors."—" In that case," said Beaumé, " you have not learned, you have invented."

About this time Dr. Chardenon read a paper before the Dijon Academy on the causes of the augmentation of weight which metals experience when calcined. He combated the different explanations which had been already advanced, and then proceeded to show that it might be accounted for in a satisfactory manner by the *abstraction* of phlogiston. This drew the attention of Morveau to the subject: he made a set of experiments a few months afterwards, and read a paper on the *phenomena of the air during combustion.* It was soon after that he made a set of experiments on the time taken by different substances to absorb or emit a given quantity of heat. These experiments, if properly followed out, would have led to the discovery of *specific heat;* but in his hands they seem to have been unproductive.

In the year 1772 he published a collection of scientific essays under the title of "Digressions Académiques." The memoirs on *phlogiston, crystallization,* and *solution,* found in this book deserve particular attention, and show the superiority of Morveau over most of the chemists of the time.

About this time an event happened which deserves to be stated. It had been customary in one of the churches of Dijon to bury considerable numbers of dead bodies. From these an infectious exhalation had proceeded, which had brought on a malignant disorder, and threatened the inhabitants of Dijon with something like the plague. Ali attempts to put an end to this infectious matter had failed, when Morveau tried the following method with complete success: A mixture of common salt and sulphuric acid in a wide-mouthed vessel was put upon chafing-dishes in various parts of the church. The doors and windows were closed and left in this state for twenty-four hours. They were then thrown open, and the chafing-dishes with the mixtures removed. Every remains of the bad smell was gone, and the church was rendered quite clean and free from infection. The same process was tried soon after in the prisons of Dijon, and with the same success. Afterwards chlorine gas was substituted for muriatic acid gas, and found still more efficacious. The present practice is to employ chloride of lime, or chloride of soda, for the purpose of fumigating infected apartments, and the process is found still more effectual than the muriatic acid gas, as originally employed by Morveau. The nitric acid fumes, proposed by Dr. Carmichael Smith, are also efficacious, but the application of them is much more troublesome and more expensive than of chloride of lime, which costs very little.

In the year 1774 it occurred to Morveau, that a course of lectures on chemistry, delivered in his native city, might be useful. Application being made to the proper authorities, the permission was obtained, and the necessary funds for supplying a laboratory granted. These lectures were begun on the 29th of April, 1776, and seem to have been of the very best kind. Every thing was stated with great clearness, and illustrated by a sufficient number of experiments. His fame now began to extend, and his name to be known to men of science in every part of Europe; and, in consequence, he began to experience the fate of almost all eminent men—to be exposed to the attacks of the malignant and the envious. The experiments which he exhibited to determine the properties of *carbonic acid gas* drew upon him the animadversions of several medical men, who affirmed that this gas was nothing else than a peculiar state of sulphuric acid. Morveau answered these animadversions in two pamphlets, and completely refuted them.

About this time he got metallic conductors erected on the house of the Academy at Dijon. On this account he was attacked violently for his presumption in disarming the hand of the Supreme Being. A multitude of fanatics assembled to pull down the conductors, and they would probably have done much mischief, had it not been for the address of M. Maret, the secretary, who assured them that the astonishing virtue of the apparatus resided in the gilded point, which had purposely been sent from Rome by the holy father! Will it excite any surprise, that within less than twenty years after this the mass of the French people not only renounced the Christian religion, and the spiritual dominion of the pope, but declared themselves atheists!

In 1777 Morveau published the first volume of a course of chemistry, which was afterwards followed by three other volumes, and is known by the name of "Elémens de Chimie de l'Académie de Dijon." This book was received with universal approbation, and must have contributed very much to increase the value of his lectures. Indeed, a text-book is essential towards a successful course of lectures: it puts it in the power of the students to understand the lecture if they be at the requisite pains; and gives them a means of clearing up their difficulties, when any such occur. I do not hesitate to say, that a course of chemical lectures is twice as valuable when the students are furnished with a good text-book, as when they are left to interpret the lectures by their own unassisted exertions.

Soon after he undertook the establishment of a manufacture of saltpetre upon a large scale. For this he received the thanks of M. Necker, who was at that time minister of finance, in the name of the King of France. This manufactory he afterwards gave up to M. Courtois, whose son still carries it on, and is advantageously known to the public as the discoverer of *iodine*.

His next object was to make a collection of minerals, and to make himself acquainted with the science of mineralogy. All this was soon accomplished. In 1777 he was charged to examine the slate-quarries and the coal-mines of Burgundy, for which purpose he performed a mineralogical tour through the province. In 1779 he discovered a lead-mine in that country, and a few years afterwards, when the attention of chemists had been drawn to sulphate of barytes and its base, by the Swedish chemists, he sought for it in Burgundy, and found it in considerable quantity at Thôte. This enabled him to draw up a description of the mineral, and to determine the characters of the base, to which he gave the name of *barote;* afterwards altered to that of barytes. This paper was published in the third volume of the Memoirs of the Dijon Academy. In this paper he describes his method of decomposing sulphate of barytes, by heating it with charcoal—a method now very frequently followed.

In the year 1779 he was applied to by Pankouke, who meditated the great project of the *Encyclopédie Méthodique*, to undertake the chemical articles in that immense dictionary, and the demand was supported by a letter from Buffon, whose request he did not think that he could with propriety refuse. The engagement was signed between them in September, 1780. The first half-volume of the chemical part of this Encyclopédie did not appear till 1786, and Morveau must have been employed during the interval in the necessary study and researches. Indeed, it is obvious, from many of the articles, that he had spent a good deal of time in experiments of research.

The state of the chemical nomenclature was at that period peculiarly barbarous and defective. He found himself stopped at every corner for want of words to express his meaning. This state of things he resolved to correct, and accordingly in 1782 published his first essay on a new chemical nomenclature. No sooner did this essay appear than it was attacked by almost all the chemists of Paris, and by none more zealously than by the chemical members of the academy. Undismayed by the violence of his antagonists, and satisfied with the rectitude of his views, and the necessity of the reform, he went directly to Paris to answer the objections in person. He not only succeeded in convincing his antagonists of the necessity of reform; but a few years afterwards prevailed upon the most eminent chemical members of the academy, Lavoisier, Berthollet, and Fourcroy, to unite with him in rendering the reform still more complete and successful. He drew up a memoir, exhibiting a plan of a methodical chemical nomenclature, which was read at a meeting of the Academy of Sciences, in 1787. Morveau, then, was in reality the author of the new chemical nomenclature, if we except a few terms, which had been already employed by Lavoisier. Had he done nothing more for the science than this, it would deservedly have immortalized his name. For every one must be sensible how much the new nomenclature contributed to the subsequent rapid extension of chemical science.

It was during the repeated conferences held with Lavoisier and the other two associates that Morveau became satisfied of the truth of Lavoisier's new doctrine, and that he was induced to abandon the phlogistic theory. We do not know the methods employed to convert him. Doubtless both reasoning and experiment were made use of for the purpose.

It was during this period that Morveau published a French translation of the Opuscula of Bergman. A society of friends, under his encouragement, translated the chemical memoirs of Scheele and many other foreign books of importance, which by their means were made known to the men of science in France.

In 1783, in consequence of a favourable report by Macquer, Morveau obtained permission to establish a manufactory of carbonate of soda, the first of the kind ever attempted in France. It was during the same year that he published his collection of pleadings at the bar, among which we find his Discours sur la Bonhomie, delivered at the opening of the sessions at Dijon, with which he took leave of his fellow-magistrates, surrendering the insignia of office, as he had determined to quit the profession of the law.

On the 25th of April, 1784, Morveau, accompanied by President Virly, ascended from Dijon in a balloon, which he had himself constructed, and repeated the ascent on the 12th of June following, with a view of ascertaining the possibility of directing these aerostatic machines, by an apparatus of his own contrivance: The capacity of the balloon was 10,498,074 French cubic feet. The effect produced by this bold undertaking by two of the most distinguished characters in the town was beyond description. Such ascents were then quite new, and looked upon with a kind of reverential awe. Though Morveau failed in his attempts to direct these aerial vessels, yet his method was ingenious and exceedingly plausible.

In 1786 Dr. Maret, secretary to the Dijon Academy, having fallen a victim to an epidemic disease, which he had in vain attempted to arrest, Morveau was appointed perpetual secretary and chancellor of the institution. Soon after this the first half-volume of the chemical part of the Encyclopédie Méthodique made its appearance, and drew the attention of every person interested in the science of chemistry. No chemical treatise had hitherto appeared worthy of being compared to it. The article *Acid*, which occupies a considerable part, is truely admirable; and whether we consider the historical details, the completeness of the accounts, the accuracy of the description of the experiments, or the elegance of the style, constitutes a complete model of what such a work should be. I may, perhaps, be partial, as it was from this book that I imbibed my own first notions in chemistry, but I never perused any book with more delight, and when I compared it with the best chemical books of the time, whether German, French, or English, its superiority became still more striking.

In the article *Acier*, Morveau had come to the very same conclusions, with respect to the nature of *steel*, as had been come to by Berthollet, Monge, and Vandermonde, in their celebrated paper on the subject, just published in the Memoirs of the Academy. His own article had been printed, though not published, before the appearance of the Memoir of the Academicians. This induced him to send an explanation to Berthollet, which was speedily published in the Journal de Physique.

In September, 1787, he received a visit from Lavoisier, Berthollet, Fourcroy, Monge, and Vandermonde. Dr. Beddoes, who was travelling through France at the time, and happened to be in Dijon, joined the party. The object of the meeting was to discuss several experiments explanatory of the new doctrine. In 1789 an attempt was made to get him admitted as a member of the Academy of Sciences; but it failed, notwithstanding the strenuous exertions of Berthollet and his other chemical friends.

The French revolution had now broken out, occasioned by the wants of the state on the one hand, and the resolute determination of the clergy and the nobility on the other, not to submit to bear any share in the public burdens. During the early part of this revolution Morveau took no part whatever in politics. In 1790, when France was divided into departments, he was named one of a commission by the National Assembly for the formation of the department of the Côte d'Or. On the 25th of August, 1791, he received from the Academy of Sciences the annual prize of 2000 francs, for the most useful work published in the course of the year. This was decreed him for his Dictionary of Chemistry, in the Encyclopédie Méthodique. Aware of the pressing necessities of the state, Morveau seized the opportunity of showing his desire of contributing towards its relief, by making a patriotic offering of the whole amount of his prize.

When the election of the second Constitutional Assembly took place, he was nominated a member by the electoral college of his department. A few months before, his name had appeared among the list of members proposed by the assembly, for the election of a governor to the heir-apparent. All this, together with the dignity of solicitor-general of the department to which he had recently been raised, not permitting him to continue his chemical lectures at Dijon, of which he had already delivered fifteen gratuitous courses, he resigned his chair in favour of Dr. Chaussier, afterwards a distinguished professor at the Faculty of Medicine of Paris; and, bidding adieu to his native city, proceeded to Paris.

On the ever memorable 16th of January, 1793, he voted with the majority of deputies. He was therefore, in consequence of this vote, a regicide. During the same year he resigned, in favour of the republic, his pension of two thousand francs, together with the arrears of that pension.

In 1794 he received from government different commissions to act with the French armies in the Low Countries. Charged with the direction of a great aerostatic machine for warlike purposes, he superintended that one in which the chief of the staff of General Jourdan and himself ascended during the battle of Fleurus, and which so materially contributed to the success of the French arms on that day. On his return from his various missions, he received from the three committees of the executive government an invitation to co-operate with several learned men in the instruction of the *central schools*, and was named professor of chemistry at the *Ecole Centrale des Travaux publiques*, since better known under the name of the *Polytechnic School*.

In 1795 he was re-elected member of the Council of Five Hundred, by the electoral assemblies of Sarthe and Ile et Vilaine. The executive government, at this time, decreed the formation of the National Institute, and named him one of the forty-eight members chosen by government to form the nucleus of that scientific body.

In 1797 he resigned all his public situations, and once more attached himself exclusively to science and to the establishments for public instruction. In 1798 he was appointed a provisional director of the Polytechnic School, to supply the place of Monge, who was then in Egypt. He continued to exercise its duties during eighteen months, to the complete satisfaction of every person connected with that establishment. With much delicacy and disinterestedness, he declined accepting the salary of 2000 francs attached to this situation, which he thought belonged to the proper director, though absent from his duties.

In 1799 Bonaparte appointed him one of the administrators-general of the Mint; and the year following he was made director of the Polytechnic

School. In 1803 he received the cross of the Legion of Honour, then recently instituted; and in 1805 was made an officer of the same order. These honours were intended as a reward for the advantage which had accrued from the mineral acid fumigations which he had first suggested. In 1811 he was created a baron of the French empire.

After having taught in the *Ecole Polytechnique* for sixteen years, he obtained leave, on applying to the proper authorities, to withdraw into the retired station of private life, crowned with years and reputation, and followed with the blessings of the numerous pupils whom he had brought up in the career of science. In this situation he continued about three years, during which he witnessed the downfal of Bonaparte, and the restoration of the Bourbons. On the 21st of December, 1815, he was seized with a total exhaustion of strength; and, after an illness of three days only, expired in the arms of his disconsolate wife, and a few trusty friends, having nearly completed the eightieth year of his age. On the 3d of January, 1816, his remains were followed to the grave by the members of the Institute, and many other distinguished men: and Berthollet, one of his colleagues, pronounced a short but impressive funeral oration on his departed friend.

Morveau had married Madame Picardet, the widow of a Dijon academician, who had distinguished himself by numerous scientific translations from the Swedish, German, and English languages. The marriage took place after they were both advanced in life, and he left no children behind him. His publications on chemical subjects were exceedingly numerous, and he contributed as much as any of his contemporaries to the extension of the science; but as he was not the author of any striking chemical discoveries, it would be tedious to give a catalogue of his numerous productions which were scattered through the Dijon Memoirs, the Journal de Physique, and the Annales de Chimie.

CHAPTER IV.

PROGRESS OF ANALYTICAL CHEMISTRY.

ANALYSIS, or the art of determining the constituents of which every compound is composed, constitutes the essence of chemistry: it was therefore attempted as soon as the science put on any thing like a systematic form. At first, with very little success; but as knowledge became more and more general, chemists became more expert, and something like regular analysis began to appear. Thus, Brandt showed that *white vitriol* is a compound of sulphuric acid and oxide of zinc; and Margraaf, that *selenite* or *gypsum* is a compound of sulphuric acid and lime. Dr. Black made analyses of several of the salts of magnesia, so far at least as to determine the nature of the constituents. For hardly any attempt was made in that early period of the art to determine the weight of the respective constituents. The first person who attempted to lay down rules for the regular analysis of minerals, and to reduce these rules to practice, was Bergman. This he did in his papers "De Docimasia Minerarum Humida," "De Terra Gemmarum," and "De Terra Tourmalini," published between the years 1777 and 1780.

To analyze a mineral, or to separate it into its constituent parts, it is necessary in the first place, to be able to dissolve it in an acid. Bergman showed that most minerals become soluble in muriatic acid if they be reduced to a very fine powder, and then heated to redness, or fused with an alkaline carbonate. After obtaining a solution in this way he pointed out methods by which the different constituents may be separated one after another, and their relative quantities determined. The fusion with an alkaline carbonate required a strong red heat. An earthenware crucible could not be employed, because at a fusing temperature it would be corroded by the alkaline carbonate, and thus the mineral under analysis would be contaminated by the addition of a quantity of foreign matter. Bergman employed an iron crucible. This effectually prevented the addition of any earthy matter. But at a red heat the iron crucible itself is apt to be corroded by the action of the alkali, and thus the mineral under analysis becomes contaminated with a quantity of that metal. This iron might easily be separated again by known methods, and would therefore be of comparatively small consequence, provided we were sure that the mineral under examination contained no iron; but when that happens (and it is a very frequent occurrence), an error is occasioned which we cannot obviate. Klaproth made a vast improvement in the art of analysis, by substituting crucibles of fine silver for the iron crucibles of Bergman. The only difficulty attending their use was, that they were apt to melt unless great caution was used in heating them. Dr. Wollaston introduced crucibles of platinum about the beginning of the present century. It is from that period that we may date the commencement of accurate analyzing.

Bergman's processes, as might have been expected, were rude and imperfect. It was Klaproth who first systematized chemical analysis and brought the art to such a state, that the processes followed could be imitated by others with nearly the same results, thus offering a guarantee for the accuracy of the process.

Martin Henry Klaproth, to whom chemistry lies under so many and such deep obligations, was born at Wernigerode, on the 1st of December, 1743. His father had the misfortune to lose his whole goods by a great fire, on the 30th of June, 1751, so that he was able to do little or nothing for the education of his children. Martin was the second of three brothers, the eldest of whom became a clergyman, and the youngest private secretary at war, and keeper of the archives of the cabinet of Berlin. Martin survived both his brothers. He procured such meagre instruction in the Latin language as the school of Wernigerode afforded, and he was obliged to procure his small school-fees by singing as one of the church choir. It was at first his intention to study theology; but the unmerited hard treatment which he met with at school so disinclined him to study, that he determined, in his sixteenth year, to learn the trade of an apothecary. Five years which he was forced to spend as an apprentice, and two as an assistant in the public laboratory in Quedlinburg, furnished him with but little scientific information, and gave him little else than a certain mechanical adroitness in the most common pharmaceutical preparations.

He always regarded as the epoch of his scientific instruction, the two years which he spent in the public laboratory at Hanover, from Easter 1766, till the same time in 1768. It was there that he first met with some chemical books of merit, especially those of Spielman, and Cartheuser, in which a higher scientific spirit already breathed. He was now anxious to go to Berlin, of which he had formed a high idea from the works of Pott, Henkel, Rose, and Margraaf. An opportunity presenting itself about Easter, 1768, he was placed as assistant in the laboratory of Wendland, at the sign of the Golden Angel, in the Street of the Moors. Here he employed all the time which a conscientious discharge of the duties of his station left him, in completing his own scientific education. And as he considered a profounder acquaintance with the ancient languages, than he had been able to pick up at the school of Wernigerode, indispensable for a complete scientific education, he applied himself with great zeal to the study of the Greek and Latin languages, and was assisted in his studies by Mr. Poppelbourn, at that time a preacher.

About Michaelmas, 1770, he went to Dantzig, as assistant in the public laboratory: but in March of the following year he returned to Berlin, as assistant in the office of the elder Valentine Rose, who was one of the most distinguished chemists of his day. But this connexion did not continue long; for Rose died in 1771. On his deathbed he requested Klaproth to undertake the superintendence of his office. Klaproth not only superintended this office for nine years with the most exemplary fidelity and conscientiousness, but undertook the education of the two sons of Rose, as if he had been their father. The younger died before reaching the age of manhood: the elder became his intimate friend, and the associate of all his scientific researches. For several years before the death of Rose (which happened in 1808) they wrought together, and Klaproth was seldom satisfied with the results of his experiments till they had been repeated by Rose.

In the year 1780 Klaproth went through his trials for the office of apothecary with distinguished applause. His thesis, "On Phosphorus and distilled Waters," was printed in the Berlin Miscellanies for

1782. Soon after this, Klaproth bought what had formerly been the Flemming laboratory in Spandau-street: and he married Sophia Christiana Lekman, with whom he lived till 1803 (when she died) in a happy state. They had three daughters and a son, who survived their parents. He continued in possession of this laboratory, in which he had arranged a small work-room of his own, till the year 1800, when he purchased the room of the Academical Chemists, in which he was enabled, at the expense of the academy, to furnish a better and more spacious apartment for his labours, for his mineralogical and chemical collection, and for his lectures.

As soon as he had brought the first arrangements of his office to perfection—an office which, under his inspection and management, became the model of a laboratory, conducted upon the most excellent principles, and governed with the most conscientious integrity, he published in the various periodical works of Germany, such as "Crell's Chemical Annals," the "Writings of the Society for the promotion of Natural Knowledge," "Selle's Contributions to the Science of Nature and of Medicine," "Köhler's Journal," &c.; a multitude of papers which soon drew the attention of chemists; for example, his Essay on Copal—on the Elastic Stone—on Proust's Sel perlée—on the Green Lead Spar of Tschoppau—on the best Method of preparing Ammonia—on the Carbonate of Barytes—on the Wolfram of Cornwall—on Wood Tin—on the Violet Schorl—on the celebrated Aerial Gold—on Apatite, &c. All these papers, which secured him a high reputation as a chemist, appeared before 1788, when he was chosen an ordinary member of the physical class of the Royal Berlin Academy of Sciences. The Royal Academy of Arts had elected him a member a year earlier. From this time, every volume of the

Memoirs of the Academy, and many other periodical works besides, contained numerous papers by this accomplished chemist; and there is not one of them which does not furnish us with a more exact knowledge of some one of the productions of nature or art. He has either corrected false representations, or extended views that were before partially known, or has revealed the composition and mixture of the parts of bodies, and has made us acquainted with a variety of new elementary substances. Amidst all these labours, it is difficult to say whether we should most admire the fortunate genius, which, in all cases, readily and easily divined the point where any thing of importance lay concealed; or the acuteness which enabled him to find the best means of accomplishing his object; or the unceasing labour and incomparable exactness with which he developed it; or the pure scientific feeling under which he acted, and which was removed at the utmost possible distance from every selfish, every avaricious, and every contentious purpose.

In the year 1795 he began to collect his chemical works which lay scattered among so many periodical publications,and gave them to the world under the title of " Beitrage zur Chemischen Kenntniss der Mineralkörper" (Contributions to the Chemical Knowledge of Mineral Bodies). Of this work, which consists of six volumes, the last was published in 1815, about a year before the author's death. It contains no fewer than two hundred and seven treatises, the most valuable part of all that Klaproth had done for chemistry and mineralogy. It is a pity that the sale of this work did not permit the publication of a seventh volume, which would have included the rest of his papers, which he had not collected, and given us a good index to the whole work, which would have been of great importance to the practical chemist. There is, indeed, an index to the first five volumes; but it is meagre and defective, containing little else than the names ot the substances on which his experiments were made.

Besides his own works, the interest which he took in the labours of others deserves to be noticed. He superintended a new edition of Gren's Manual of Chemistry, remarkable not so much for what he added as for what he took away and corrected. The part which he took in Wolff's Chemical Dictionary was of great importance. The composition of every particular treatise was by Professor Wolff; but Klaproth read over every important article before it was printed, and assisted the editor on all occasions with the treasures of his experience and knowledge. Nor was he less useful to Fischer in his translation of Berthollet on Affinity and on Chemical Statics.

These meritorious services, and the lustre which his character and discoveries conferred on his country were duly appreciated by his sovereign. In 1782 he had been made assessor in the Supreme College of Medicine and of Health, which then existed. At a more recent period he enjoyed the same rank in the Supreme Council of Medicine and of Health; and when this college was subverted, in 1810, he became a member of the medical deputation attached to the ministry of the interior. He was also a member of the perpetual court commission for medicines. His lectures, too, procured for him several municipal situations. As soon as the public became acquainted with his great chemical acquirements he was permitted to give yearly two private courses of lectures on chemistry; one for the officers of the royal artillery corps, the other for officers not connected with the army, who wished to accomplish themselves for some practical employment. Both of these lectures assumed afterwards a municipal character. The former led to his appointment as professsor of the Artillery Academy instituted at Tempelhoff; and, after its dissolution, to his situation as professor in the Royal War School. The other lecture procured for him the professorship of chemistry in the Royal Mining Institutute. On the establishment of the university, Klaproth's lectures became those of the university, and he himself was appointed ordinary professor of chemistry, and member of the academical senate. From 1797 to 1810 he was an active member of a small scientific society, which met yearly during a few weeks for the purpose of discussing the more recondite mysteries of the science. In the year 1811, the King of Prussia added to all his other honours the order of the Red Eagle of the third class.

Klaproth spent the whole of a long life in the most active and conscientious discharge of all the duties of his station, and in an uninterrupted course of experimental investigations. He died at Berlin on the 1st of January, 1817, in the 70th year of his age.

Among the remarkable traits in his character was his incorruptible regard for every thing that he believed to be true, honourable, and good; his pure love of science, with no reference whatever to any selfish, ambitious, and avaricious feeling; his rare modesty, undebased by the slightest vainglory or boasting. He was benevolently disposed towards all men, and never did a slighting or contemptuous word respecting any person fall from him. When forced to blame, he did it briefly, and without bitterness, for his blame always applied to actions, not to persons. His friendship was never the result of selfish calculation, but was founded on his opinion of the personal worth of the individual. Amidst all the unpleasant accidents of his life, which were far from few, he evinced the greatest firmness of mind. In his common behaviour he was pleasant and composed, and was indeed rather inclined to a joke. To all this may be added a true religious feeling, so uncommon among men of science of his day. His religion consisted not in words and forms, not in positive doctrines, nor in ecclesiastical observances, which, however, he believed to be necessary and honourable; but in a zealous and conscientious discharge of all his duties, not only of those which are imposed by the laws of men, but of those holy duties of love and charity, which no human law, but only that of God can command, and without which the most enlightened of men is but "as sounding brass, or a tinkling cymbal." He early showed this religious feeling by the honourable care which he bestowed on the education of the children of Valentine Rose. Nor did he show less care at an after-period towards his assistants and apprentices, to whom he refused no instruction, and in whose success he took the most active concern. He took a pleasure in every thing that was good and excellent, and felt a lively interest in every undertaking which he believed to be of general utility. He was equally removed from the superstition and infidelity of his age, and carried the principles of religion, not on his lips, but in the inmost feelings of his heart, from whence they emanated in actions which pervaded and ennobled his whole being and conduct.

When we take a view of the benefits which Klaproth conferred upon chemistry, we must not look so much at the new elementary substances which he discovered, though they must not be forgotten, as at the new analytical methods which he introduced, the precision, and neatness, and order, and regularity with which his analyses were conducted, and the scrupulous fidelity with which every thing was faithfully stated as he found it.

1. When a mineral is subjected to analysis, whatever care we take to collect all the constituents, and to weigh them without losing any portion whatever, it is generally found that the sum of the constituents obtained fall a little short of the weight of the mineral employed in the analysis. Thus, if we take 100 grains of any mineral, and analyze it, the weights of all the constituents obtained added together will rarely make up 100 grains, but generally somewhat less; perhaps only 99, or even 98 grains. But some cases occur, when the analysis of 100 grains of a mineral gives us constituents that weigh, when added together, more than 100 grains; perhaps 105, or, in some rare cases, as much as 110. It was the custom with Bergman, and other analysts of his time, to consider this deficiency or surplus as owing to errors in the analysis, and therefore to slur it over in the statement of the analysis, by bringing the weight of the constituents, by calculation, to amount exactly to 100 grains. Klaproth introduced the method of stating the results exactly as he got them. He gives the weight of mineral employed in all his analyses, and the weight of each constituent extracted. These weights, added together, generally show a loss, varying from two per cent. to a half per cent. This improvement may appear at first sight trifling; yet I am persuaded that to it we are indebted for most of the subsequent improvements introduced into analytical chemistry. If the loss sustained was too great, it was obvious either that the analysis had been badly performed, or that the mineral contains some constituent which had been overlooked, and not obtained. This laid him under the necessity of repeating the analysis; and if the loss continued, he naturally looked out for

some constituent which his analysis had not enabled him to obtain. It was in this way that he discovered the presence of potash in minerals; and Dr. Kennedy afterwards, by following out his processes, discovered soda as a constituent. It was in this way that water, phosphoric acid, arsenic acid, fluoric acid, boracic acid, &c., were also found to exist as constituents in various mineral bodies, which, but for the accurate mode of notation introduced by Klaproth, would have been overlooked and neglected.

2. When Klaproth first began to analyze mineral bodies, he found it extremely difficult to bring them into a state capable of being dissolved in acids, without which an accurate analysis was impossible. Accordingly corundum, adamantine spar, and the zircon, or hyacinth, baffled his attempts for a considerable time, and induced him to consider the earth of corundum as of a peculiar nature. He obviated this difficulty by reducing the mineral to an extremely fine powder, and, after digesting it in caustic potash ley till all the water was dissipated, raising the temperature, and bringing the whole into a state of fusion. This fusion must be performed in a silver crucible. Corundum, and every other mineral which had remained insoluble after fusion with an alkaline carbonate, was found to yield to this new process. This was an improvement of considerable importance. All those stony minerals which contain a notable proportion of silica, in general become soluble after having been kept for some time in a state of ignition with twice their weight of carbonate of soda. At that temperature the silica of the mineral unites with the soda, and the carbonic acid is expelled. But when the quantity of silica is small, or when it is totally absent, heating with carbonate of soda does not answer so well. With such minerals, caustic potash or soda may be substituted with ad-

vantage; and there are some of them that cannot be analyzed without having recourse to that agent. I have succeeded in analyzing corundum and chrysoberyl, neither of which, when pure, contain any silica, by simply heating them in carbonate of soda; but the process does not succeed unless the minerals be reduced to an exceedingly minute powder.

3. When Klaproth discovered potash in the idocrase, and in some other minerals, it became obvious that the old mode of rendering minerals soluble in acids by heating them with caustic potash, or an alkaline carbonate, could answer only for determining the quantity of silica, and of earths or oxides, which the mineral contained; but that it could not be used when the object was to determine its potash. This led him to substitute *carbonate of barytes* instead of potash or soda, or their carbonates. After having ascertained the quantity of silica, and of earths, and metallic oxides, which the mineral contained, his last process to determine the potash in it was conducted in this way: A portion of the mineral reduced to a fine powder was mixed with four or five times its weight of carbonate of barytes, and kept for some time (in a platinum crucible) in a red heat. By this process, the whole becomes soluble in muriatic acid. The muriatic acid solution is freed from silica, and afterwards from barytes, and all the earths and oxides which it contains, by means of carbonate of ammonia. The liquid, thus freed from every thing but the alkali, which is held in solution by the muriatic acid, and the ammonia, used as a precipitant, is evaporated to dryness, and the dry mass, cautiously heated in a platinum crucible till the ammoniacal salts are driven off. Nothing now remains but the potash, or soda, in combination with muriatic acid. The addition of muriate of platinum enables us to determine whether the alkali

be potash or soda: if it be potash, it occasions a yellow precipitate; but nothing falls if the alkali be soda.

This method of analyzing minerals containing potash or soda is commonly ascribed to Rose. Fescher, in his Eloge of Klaproth, informs us that Klaproth said to him, more than once, that he was not quite sure whether he himself, or Rose, had the greatest share in bringing this method to a state of perfection. From this, I think it not unlikely that the original suggestion might have been owing to Rose, but that it was Klaproth who first put it to the test of experiment.

The objection to this mode of analyzing is the high price of the carbonate of barytes. This is partly obviated by recovering the barytes in the state of carbonate; and this, in general, may be done, without much loss. Berthier has proposed to substitute oxide of lead for carbonate of barytes. It answers very well, is sufficiently cheap, and does not injure the crucible, provided the oxide of lead be mixed previously with a little nitrate of lead, to oxidize any fragments of metallic lead which it may happen to contain. Berthier's mode, therefore, in point of cheapness, is preferable to that of Klaproth. It is equally efficacious and equally accurate. There are some other processes which I myself prefer to either of these, because I find them equally easy, and still less expensive than either carbonate of barytes or oxide of lead. Davy's method with boracic acid is exceptionable, on account of the difficulty of separating the boracic acid completely again.

4. The mode of separating iron and manganese from each other employed by Bergman was so defective, that no confidence whatever can be placed in his results. Even the methods suggested by Vauquelin, though better, are still defective. But

the process followed by Klaproth is susceptible of very great precision. He has (we shall suppose) the mixture of iron and manganese to be separated from each other, in solution, in muriatic acid. The first step of the process is to convert the protoxide of iron (should it be in that state) into peroxide. For this purpose, a little nitric acid is added to the solution, and the whole heated for some time. The liquid is now to be rendered as neutral as possible; first, by driving off as much of the excess of acid as possible, by concentrating the liquid; and then by completing the neutralization, by adding very dilute ammonia, till no more can be added without occasioning a permanent precipitation. Into the liquid thus neutralized, succinate or benzoate of ammonia is dropped, as long as any precipitate appears. By this means, the whole peroxide of iron is thrown down in combination with succinic, or benzoic acid, while the whole manganese remains in solution. The liquid being filtered, to separate the benzoate of iron, the manganese may now (if nothing else be in the liquid) be thrown down by an alkaline carbonate; or, if the liquid contain magnesia, or any other earthy matter, by hydrosulphuret of ammonia, or chloride of lime.

This process was the contrivance of Gehlen; but it was made known to the public by Klaproth, who ever after employed it in his analyses. Gehlen employed succinate of ammonia; but Hisinger afterwards showed that benzoate of ammonia might be substituted without any diminution of the accuracy of the separation. This last salt, being much cheaper than succinate of ammonia, answers better in this country. In Germany, the succinic acid is the cheaper of the two, and therefore the best.

5. But it was not by new processes alone that Klaproth improved the mode of analysis, though

they were numerous and important; the improvements in the apparatus contributed not less essentially to the success of his experiments. When he had to do with very hard minerals, he employed a mortar of flint, or rather of agate. This mortar he, in the first place, analyzed, to determine exactly the nature of the constituents. He then weighed it. When a very hard body is pounded in such a mortar, a portion of the mortar is rubbed off, and mixed with the pounded mineral. What the quantity thus abraded was, he determined by weighing the mortar at the end of the process. The loss of weight gave the portion of the mortar abraded; and this portion must be mixed with the pounded mineral.

When a hard stone is pounded in an agate mortar it is scarcely possible to avoid losing a little of it. The best method of proceeding is to mix the matter to be pounded (previously reduced to a coarse powder in a diamond mortar) with a little water. This both facilitates the trituration, and prevents any of the dust from flying away; and not more than a couple of grains of the mineral should be pounded at once. Still, owing to very obvious causes, a little of the mineral is sure to be lost during the pounding. When the process is finished, the whole powder is to be exposed to a red heat in a platinum crucible, and weighed. Supposing no loss, the weight should be equal to the quantity of the mineral pounded together with the portion abraded from the mortar. But almost always the weight will be found less than this. Suppose the original weight of the mineral before pounding was a, and the quantity abraded from the mortar 1; then, if nothing were lost, the weight should be $a + 1$; but we actually find it only b, a quantity less than $a + 1$. To determine the weight of matter abraded from the mortar contained in this powder, we say $a + 1 : b :: 1 : x$, the quantity from the mortar in our powder, and $x = \frac{b}{a+1}$. In performing the analysis, Klaproth attended to this quantity, which was silica, and subtracted it. Such minute attention may appear, at first sight superfluous; but it is not so. In analyzing sapphire, chrysoberyl, and some other very hard minerals, the quantity of silica abraded from the mortar sometimes amounts to five per cent. of the weight of the mineral; and if we were not to attend to the way in which this silica has been introduced into the powder, we should give an erroneous view of the constitution of the mineral under analysis. All the analyses of chrysoberyl hitherto published, give a considerable quantity of silica as a constituent of it. This silica, if really found by the analysts, must have been introduced from the mortar, for pure chrysoberyl contains no silica whatever, but is a definite compound of glucina, alumina, and oxide of iron.

When Klaproth operated with fire, he always selected his vessels, whether of earthenware, glass, plumbago, iron, silver, or platinum, upon fixed principles; and showed more distinctly than chemists had previously been aware of, what an effect the vessel frequently has upon the result. He also prepared his reagents with great care, to ensure their purity; for obtaining several of which in their most perfect state, he invented several efficient methods. It is to the extreme care with which he selected his minerals for analysis, and to the purity of his reagents, and the fitness of his vessels for the objects in view, that the great accuracy of his analyses is to be, in a great measure, ascribed. He must also have possessed considerable dexterity in operating, for when he had in view to determine any particular point with accuracy, his results came, in general, exceedingly near the truth. I may notice, as an example of this, his analysis of sulphate of barytes, which was within about one-and-a-half per cent. of absolute correctness. When we consider the looseness of the data which chemists were then obliged to use, we cannot but be surprised at the smallness of the error. Berzelius, in possession of better data, and possessed of much dexterity, and a good apparatus, when he analyzed this salt many years afterwards, committed an error of a half per cent.

Klaproth, during a very laborious life, wholly devoted to analytical chemistry, entirely altered the face of mineralogy. When he began his labours, chemists were not acquainted with the true composition of a single mineral. He analyzed above 200 species, and the greater number of them with so much accuracy, that his successors have, in most cases, confirmed the results which he obtained. The analyses least to be depended on, are of those minerals which contain both lime and magnesia; for his process for separating lime and magnesia from each other was not a good one; nor am I sure that he always succeeded completely in separating silica and magnesia from each other. This branch of analysis was first properly elucidated by Mr. Chenevix.

6. Analytical chemistry was, in fact, systematized by Klaproth; and it is by studying his numerous and varied analyses, that modern chemists have learned this very essential, but somewhat difficult art; and have been able, by means of still more accurate data than he possessed, to bring it to a still greater degree of perfection. But it must not be forgotten, that Klaproth was in reality the creator of this art, and that on that account the greatest part of the credit due to the progress that has been made in it belongs to him.

It would be invidious to point out the particular analyses which are least exact; perhaps they ought rather to be ascribed to an unfortunate selection of specimens, than to any want of care or skill in the operator. But, during his analytical processes, he discovered a variety of new elementary substances which it may be proper to enumerate.

In 1789 he examined a mineral called *pechblende*, and found in it the oxide of a new metal, to which he gave the name of *uranium*. He determined its characters, reduced it to the metallic state, and described its properties. It was afterwards examined by Richter, Bucholz, Arfvedson, and Berzelius.

It was in the same year, 1789, that he published his analysis of the zircon; he showed it to be a compound of silica and a new earth, to which he gave the name of zirconia. He determined the properties of this new earth, and showed how it might be separated from other bodies and obtained in a state of purity. It has been since ascertained, that it is a metallic oxide, and the metallic basis of it is now distinguished by the name of *zirconium*. In 1795 he showed that the *hyacinth* is composed of the same ingredients as the zircon; and that both, in fact, constitute only one species. This last analysis was repeated by Morveau, and has been often confirmed by modern analytical chemists.

It was in 1795 that he analyzed what was at that time called *red schorl*, and now *titanite*. He showed that it was the oxide of a new metallic body, to which he gave the name of *titanium*. He described the properties of this new body, and pointed out its distinctive characters. It must not be omitted, however, that he did not succeed in obtaining oxide of titanium, or *titanic acid*, as it is now called, in a state of purity. He was not able to separate a quantity of oxide of iron, with which it was united, and which gave it a reddish colour. It was first obtained pure by H. Rose, the son of his friend and pupil, who took so considerable a part in his scientfic investigations.

Titanium, in the metallic state, was some years ago discovered by Dr. Wollaston, in the slag at the bottom of the iron furnace, at Merthyr Tydvil, in Wales. It is a yellow-coloured, brittle, but very hard metal, possessed of considerable beauty; but not yet applied to any useful purpose.

In 1797 he examined the menachanite, a black sand from Cornwall, which had been subjected to a chemical analysis by Gregor, in 1791, who had extracted from it a new metallic substance, which Kirwan distinguished by the name of *menachine.* Klaproth ascertained that the new metal of Gregor was the very same as his own titanium, and that menachanite is a compound of titanic acid and oxide of iron. Thus Mr. Gregor had anticipated him in the discovery of titanium, though he was not aware of the circumstance till two years after his own experiments had been published.

In the year 1793 he published a comparative set of experiments on the nature of carbonates of barytes and strontian; showing that their bases are two different earths, and not the same, as had been hitherto supposed in Germany. This was the first publication on strontian which appeared on the continent; and Klaproth seems to have been ignorant of what had been already done on it in Great Britain; at least, he takes no notice of it in his paper, and it was not his character to slur over the labours of other chemists, when they were known to him. Strontian was first mentioned as a peculiar earth by Dr. Crawford, in his paper on the medicinal properties of the muriate of barytes, published in 1790. The experiments on which he founded his opinions were made, he informs us, by Mr. Cruikshanks. A paper on the same subject, by Dr. Hope, was read to the Royal Society of Edinburgh, in 1793; but they had been begun in 1791. In this paper Dr. Hope establishes the peculiar characters of strontian, and describes its salts with much precision.

Klaproth had been again anticipated in his experiments on strontian; but he could not have become aware of this till afterwards. For his own experiments were given to the public before those of Dr. Hope.

On the 25th of January, 1798, his paper on the gold ores of Transylvania was read at a meeting of the Academy of Sciences at Berlin. During his analysis of these ores, he detected a new white metal, to which he gave the name of *tellurium*. Of this metal he describes the properties, and points out its distinguishing characters.

These ores had been examined by Muller, of Reichenstein, in the year 1782; and he had extracted from them a metal which he considered as differing from every other. Not putting full confidence in his own skill, he sent a specimen of his new metal to Bergman, requesting him to examine it and give his opinion respecting its nature. All that Bergman did was to show that the metallic body which he had got was not antimony, to which alone, of all known metals, it bore any resemblance. It might be inferred from this, that Muller's metal was new. But the subject was lost sight of, till the publication of Klaproth's experiments, in 1802, recalled it to the recollection of chemists. Indeed, Klaproth relates all that Muller had done, with the most perfect fairness.

In the year 1804 he published the analysis of a red-coloured mineral, from Bastnäs in Sweden, which had been at one time confounded with tungsten; but which the Elhuyarts had shown to contain none of that metal. Klaproth showed that it contained a new substance, as one of its constituents, which he considered as a new earth, and which he called *ochroita*, because it forms coloured salts with acids. Two years after, another analysis of the same mineral was published by Berzelius and Hisinger. They considered the new substance which the mineral contained as a metallic oxide, and to the unknown metallic base they gave the name of *cerium*, which has been adopted by chemists in preference to Klaproth's name. The characters of oxide of cerium given by Berzelius and Hisinger, agree with those given by Klaproth to ochroita, in all the essential circumstances. Of course Klaproth must be considered as the discoverer of this new body. The distinction between *earth* and *metallic oxide* is now known to be an imaginary one. All the substances formerly called earths are, in fact, metallic oxides.

Besides these new substances, which he detected by his own labours, he repeated the analyses of others, and confirmed and extended the discoveries they had made. Thus, when Vauquelin discovered the new earth *glucina*, in the emerald and beryl, he repeated the analysis of these minerals, confirmed the discovery of Vauquelin, and gave a detailed account of the characters and properties of glucina. Gadolin had discovered another new earth in the mineral called gadolinite. This discovery was confirmed by the analysis of Ekeberg, who distinguished the new earth by the name of yttria. Klaproth immediately repeated the analysis of the gadolinite, confirmed the results of Ekeberg's analysis, and examined and described the properties of *yttria.*

When Dr. Kennedy discovered soda in basalt, Klaproth repeated the analysis of this mineral, and confirmed the results obtained by the Edinburgh analyst.

But it would occupy too much room, if I were to enumerate every example of such conduct. Whoever will take the trouble to examine the different volumes of the Beitrage, will find several others not less striking or less useful.

The service which Klaproth performed for mineralogy, in Germany, was performed equally in France by the important labours of M. Vauquelin. It was in France, in consequence of the exertions of Romé de Lisle, and the mathematical investigations of the Abbé Hauy, respecting the structure of crystals, which were gradually extended over the whole mineral kingdom, that the reform in mineralogy, which has now become in some measure general, originated. Hauy laid it down as a first principle, that every mineral species is composed of the same constituents united in the same proportion. He therefore considered it as an object of great importance, to procure an exact chemical analysis of every mineral species. Hitherto no exact analysis of minerals had been performed by French chemists; for Sage, who was the chemical mineralogist connected with the academy, satisfied himself with ascertaining the nature of the constituents of minerals, without determining their proportions. But Vauquelin soon displayed a knowledge of the mode of analysis, and a dexterity in the use of the apparatus which he employed, little less remarkable than that of Klaproth himself.

Of Vauquelin's history I can give but a very imperfect account, as I have not yet had an opportunity of seeing any particulars of his life. He was a peasant-boy of Normandy, with whom Fourcroy accidentally met. He was pleased with his quickness and parts, and delighted with the honesty and integrity of his character. He took him with him to Paris, and gave him the superintendence of his laboratory. His chemical knowledge speedily became great, and his practice in experimenting gave him skill and dexterity: he seems to have performed all the analytical experiments which Fourcroy was in the habit of publishing. He speedily became known by his publications and discoveries. When the scientific institutions were restored or established, after the death of Robespierre, Vauquelin became a member of the Institute and chemist to the School of Mines. He was made also assay-master of the Mint. He was a professor of chemistry in Paris, and delivered, likewise, private lectures, and took in practical pupils into his laboratory. His laboratory was of considerable size, and he was in the habit of preparing both medicines and chemical reagents for sale. It was he chiefly that supplied the French chemists with phosphorus, &c., which cannot be conveniently prepared in a laboratory fitted up solely for scientific purposes.

Vauquelin was by far the most industrious of all the French chemists, and has published more papers, consisting of mineral, vegetable, and animal analyses, than any other chemist without exception. When he had the charge of the laboratory of the School of Mines, Hauy was in the habit of giving him specimens of all the different minerals which he wished analyzed. The analyses were conducted with consummate skill, and we owe to him a great number of improvements in the methods of analysis. He is not entitled to the same credit as Klaproth, because he had the advantage of many analyses of Klaproth to serve him as a guide. But he had no model before him in France; and both the apparatus used by him, and the reagents which he employed, were of his own contrivance and preparation. I have sometimes suspected that his reagents were not always very pure; but I believe the true reason of the unsatisfactory nature of many of his analyses, is the bad choice made of the specimens selected for analysis. It is obvious from his papers, that Vauquelin was not a mineralogist; for he never attempts a description of the mineral which he subjects to analysis, satisfying himself with the specimen put into his hands by Hauy. Where that specimen was pure, as was the case with emerald and beryl, his analysis is very good; but when the specimen was impure or ill-chosen, then the result obtained could not convey a just notion of the constituents of the mineral. That Hauy would not be very difficult to please in his selection of specimens, I think myself entitled to infer from the specimens of minerals contained in his own cabinet, many of which were by no means well selected. I think, therefore, that the numerous analyses published by Vauquelin, in which the constituents assigned by him are not those, or, at least, not in the same proportions, as have been found by succeeding analysts, are to be ascribed, not to errors in the analysis, which, on the contrary, he always performed carefully, and with the requisite attention to precision, but to the bad selection of specimens put into his hand by Hauy, or those other indviduals who furnished him with the specimens which he employed in his analyses. This circumstance is very much to be deplored; because it puts it out of our power to confide in an analysis of Vauquelin, till it has been repeated and confirmed by somebody else.

Vauquelin not only improved the analytical methods, and reduced the art to a greater degree of simplicity and precision, but he discovered, likewise, new elementary bodies.

The red lead ore of Siberia had early drawn the attention of chemists, on account of its beauty; and various attempts had been made to analyze it.

Among others, Vauquelin tried his skill upon it, in 1789, in concert with M. Macquart, who had brought specimens of it from Siberia; but at that time he did not succeed in determining the nature of the acid with which the oxide of lead was combined in it. He examined it again in 1797, and now succeeded in separating an acid to which, from the beautiful coloured salts which it forms, he gave the name of *chromic*. He determined the properties of this acid, and showed that its basis was a new metal to which he gave the name of *chromium*. He succeeded in obtaining this metal in a separate state, and showed that its protoxide is an exceedingly beautiful green powder. This discovery has been of very great importance to different branches of manufacture in this country. The green oxide is used pretty extensively in painting green on porcelain. It constitutes an exceedingly beautiful green pigment, very permanent, and easily applied. The chromic acid, when combined with oxide of lead, forms either a yellow or an orange colour upon cotton cloth, both very fixed and exceedingly beautiful colours. In that way it is extensively used by the calico-printers; and the bichromate of potash is prepared, in a crystalline form, to a very considerable amount, both in Glasgow and Lancashire, and doubtless in other places.

Vauquelin was requested by Hauy to analyze the *beryl*, a beautiful light-green mineral, crystallized in six-sided prisms, which occurs not unfrequently in granite rocks, especially in Siberia. He found it to consist chiefly of silica, united to alumina, and to another earthy body, very like alumina in many of its properties, but differing in others. To this new earth he gave the name of *glucina*, on account of the sweet taste of its salts; a name not very appropriate, as alumina, yttria, lead, protoxide of chromium, and even protoxide of iron, form salts which are distinguished by a sweet taste likewise. This discovery of glucina confers honour on Vauquelin, as it shows the care with which his analyses must have been conducted. A careless experimenter might easily have confounded *glucina* with *alumina*. Vauquelin's mode of distinguishing them was, to add sulphate of potash to their solution in sulphuric acid If the earth in solution was alumina, crystals of alum would form in the course of a short time; but if the earth was glucina, no such crystals would make their appearance, alumina being the basis of alum, and not glucina. He showed, too, that glucina is easily dissolved in a solution of carbonate of ammonia, while alumina is not sensibly taken up by that solution.

Vauquelin died in 1829, after having reached a good old age. His character was of the very best kind, and his conduct had always been most exemplary. He never interfered with politics, and steered his way through the bloody period of the revolution, uncontaminated by the vices or violence of any party, and respected and esteemed by every person.

Mr. Chenevix deserves also to be mentioned as an improver of analytical chemistry. He was an Irish gentleman, who happened to be in Paris during the reign of terror, and was thrown into prison and put into the same apartment with several French chemists, whose whole conversation turned upon chemical subjects. He caught the infection, and, after getting out of prison, began to study the subject with much energy and success, and soon distinguished himself as an analytical chemist.

His analysis of corundum and sapphire, and his observations on the affinity between magnesia and silica, are valuable, and led to considerable improvements in the method of analysis. His analyses of the arseniates of copper, though he demonstrated that several different species exist, are not so much to be depended on; because his method of separating and estimating the quantity of arsenic acid is not good. This difficult branch of analysis was not fully understood till afterwards.

Chenevix was for several years a most laborious and meritorious chemical experimenter. It is much to be regretted that he should have been induced, in consequence of the mistake into which he fell respecting palladium, to abandon chemistry altogether. Palladium was originally made known to the public by an anonymous handbill which was circulated in London, announcing that *palladium*, or new silver, was on sale at Mrs. Forster's, and describing its properties. Chenevix, in consequence of the unusual way in which the discovery was announced, naturally considered it as an imposition on the public. He went to Mrs. Forster's, and purchased the whole palladium in her possession, and set about examining it, prepossessed with the idea that it was an alloy of some two known metals. After a laborious set of experiments, he considered that he had ascertained it to be a compound of platinum and mercury, or an amalgam of platinum made in a peculiar way, which he describes. This paper was read at a meeting of the Royal Society by Dr. Wollaston, who was secretary, and afterwards published in their Transactions. Soon after this publication, another anonymous handbill was circulated, offering a considerable price for every grain of palladium *made* by Mr. Chenevix's process, or by any other process whatever. No person appearing to claim the money thus offered, Dr. Wollaston, about a year after, in a paper read to the Royal Society, acknowledged himself to have been the discoverer of palladium, and related the process by which he had obtained it from the solution of crude platina in aqua regia. There could be no doubt after this, that palladium was a peculiar metal, and that Chenevix, in his experiments, had fallen into some mistake, probably by inadvertently employing a solution of palladium, instead of a solution of his amalgam of platinum; and thus giving the properties of the one solution to the other. It is very much to be regretted, that Dr. Wollaston allowed Mr. Chenevix's paper to be printed, without informing him, in the first place, of the true history of palladium: and I think that if he had been aware of the bad consequences that were to follow, and that it would ultimately occasion the loss of Mr. Chenevix to the science, he would have acted in a different manner. I have more than once conversed with Dr. Wollaston on the subject, and he assured me that he did every thing that he could do, short of betraying his secret, to prevent Mr. Chenevix from publishing his paper; that he had called upon, and assured him, that he himself had attempted his process without being able to succeed, and that he was satisfied that he had fallen into some mistake. As Mr. Chenevix still persisted in his conviction of the accuracy of his own experiments after repeated warnings, perhaps it is not very surprising that Dr. Wollaston allowed him to publish his paper, though; had he been aware of the consequences to their full extent, I am persuaded that he would not have done so. It comes to be a question whether, had Dr. Wollaston informed him of the whole secret, Mr. Chenevix would have been convinced.

Another chemist, to whom the art of analyzing minerals lies under great obligations, is Dr. Frederick Stromeyer, professor of chemistry and pharmacy, in the University of Gottingen. He was originally a botanist, and only turned his attention to chemistry when he had the offer of the chemical chair at Gottingen. He then went to Paris, and studied practical chemistry for some years in Vauquelin's laboratory. He has devoted most of his attention to the analysis of minerals; and in the year 1821 published a volume of analyses under the title of "Untersuchungen über die Mischung der Mineralkörper und anderer damit verwandten Substanzen." It contains thirty analyses, which constitute perfect models of analytical sagacity and accuracy. After Klaproth's Beitrage, no book can be named more highly deserving the study of the analytical chemist than Stromeyer's Untersuchungen.

The first paper in this work contains the analysis of arragonite. Chemists had not been able to discover any difference in the chemical constitution of arragonite and calcareous spar, both being compounds of

Lime	3·5
Carbonic acid .	2·75

Yet the minerals differ from each other in their hardness, specific gravity, and in the shape of their crystals. Many attempts had been made to account for this difference in characters between these two minerals, but in vain. Mr. Holme showed that arragonite contained about one per cent. of water, which is wanting in calcareous spar; and that when arragonite is heated, it crumbles into powder, which is not the case with calcareous spar. But it is not easy to conceive how the addition of one per cent. of water should increase the specific gravity and the hardness, and quite alter the shape of the crystals of calcareous spar. Stromeyer made a vast number of experiments upon arragonite, with very great care, and the result was, that the arragonite from Bastenes, near Dax, in the department of Landes, and likewise that from Molina, in Arragon, was a compound of

96 carbonate of lime
4 carbonate of strontian.

This amounts to about thirty-five atoms of carbonate of lime, and one atom of carbonate of strontian. Now as the hardness and specific gravity of carbonate of strontian is greater than that of carbonate of lime, we can see a reason why arragonite should be heavier and harder than calcareous spar. More late researches upon different varieties of arragonite enabled him to ascertain that this mineral exists with different proportions of carbonate of strontian. Some varieties contain only 2 per cent., some only 1 per cent., and some only 0·75, or even 0·5 per cent.; but he found no specimen among the great number which he analyzed totally destitute of carbonate of strontian. It is true that Vauquelina fterwards examined several varieties in which he could detect no strontian whatever; but as Vauquelin's mineralogical knowledge was very deficient, it comes to be a question, whether the minerals analyzed by him were really arragonites, or only varieties of calcareous spar.

To Professor Stromeyer we are likewise indebted for the discovery of the new metal called *cadmium;* and the discovery does great credit to his sagacity and analytical skill. He is inspector-general of the apothecaries for the kingdom of Hanover. While discharging the duties of his office at Hildesheim, in the year 1817, he found that the carbonate of zinc had been substituted for the oxide of zinc, ordered in the Hanoverian Pharmacopœia. This carbonate of zinc was manufactured at Salzgitter. On inquiry he learned from Mr. Jost, who managed that manufactory, that they had been obliged to substitute the carbonate for the oxide of zinc, because the oxide had a yellow colour which rendered it unsaleable. On examining this oxide, Stromeyer found that it owed its yellow colour to the presence of a small quantity of the oxide of a new metal, which he separated, reduced, and examined, and to which he gave the name of *cadmium*, because it occurs usually associated with zinc. The quantity of cadmium which he was able to obtain from this oxide of zinc was but small. A fortunate circumstance, however, supplied him with an additional quantity, and enabled him to carry his examination of cadmium to a still greater length. During the apothecaries' visitation in the state of Magdeburg, there was found, in the possession of several apothecaries, a preparation of zinc from Silesia, made in Hermann's laboratory at Schönebeck, which was confiscated on the supposition that it contained arsenic, because its solution gave a yellow precipitate with sulphuretted hydrogen, which was considered as orpiment. This statement could not be indifferent to Mr. Hermann, as it affected the credit of his manufactory; especially as the medicinal counsellor, Roloff, who had assisted at the visitation, had drawn up a statement of the circumstances which occasioned the confiscation, and caused it to be published in Hofeland's Medical Journal. He subjected the suspected oxide to a careful examination; but he could not succeed in detecting any arsenic in it. He then requested Roloff to repeat his experiments. This he did; and now perceived that the precipitate, which he had taken for orpiment, was not so in reality, but owed its existence to the presence of another metallic oxide, different from arsenic and probably new. Specimens of this oxide of zinc, and of the yellow precipitate, were sent to Stromeyer for examination, who readily recognised the presence of cadmium, and was able to extract from it a considerable quantity of that metal.

It is now nine years since the first volume of the

Untersuchungen was published. All those who are interested in analytical chemistry are anxious for the continuance of that admirable work. By this time he must have collected ample materials for an additional volume; and it could not but add considerably to a reputation already deservedly high.

There is no living chemist, to whom analytical chemistry lies under greater obligations than to Berzelius, whether we consider the number or the exactness of the analyses which he has made.

Jacob Berzelius was educated at Upsala, when Professor Afzelius, a nephew of Bergman, filled the chemical chair, and Ekeberg was *magister docens* in chemistry. Afzelius began his chemical career with considerable *éclat*, his paper on sulphate of barytes being possessed of very considerable merit. But he is said to have soon lost his health, and to have sunk, in consequence, into listless inactivity.

Andrew Gustavus Ekeberg was born in Stockholm, on the 16th of January, 1767. His father was a captain in the Swedish navy. He was educated at Calmar; and in 1784 went to Upsala, where he devoted himself chiefly to the study of mathematics. He took his degree in 1788, when he wrote a thesis "De Oleis Seminum expressis." In 1789 he went to Berlin; and on his return, in 1790, he gave a specimen of his poetical talents, by publishing a poem entitled "Tal öfver Freden emellan Sverige och Ryssland" (Discourse about the Peace between Sweden and Russia). After this he turned his attention to chemistry; and in 1794 was made *chemiæ docens*. In this situation he continued till 1813, when he died on the 11th of February. He had been in such bad health for some time before his death, as to be quite unable to discharge the duties of his situation. He published but little, and that little consisted almost entirely of chemical analyses.

His first attempt was on phosphate of lime; then he wrote a paper on the analysis of the topaz, the object of which was to explain Klaproth's method of dissolving hard stony bodies.

He made an analysis of gadolinite, and determined the chemical properties of yttria. During these experiments he discovered the new metal to which he gave the name of *tantalum*, and which Dr. Wollaston afterwards showed to be the same with the *columbium* of Mr. Hatchett. He also published an analysis of the automalite, of an ore of titanium, and of the mineral water of Medevi. In this last analysis he was assisted by Berzelius, who was then quite unknown to the chemical world.

Berzelius has been much more industrious than his chemical contemporaries at Upsala. His first publication was a work in two volumes on animal chemistry, chiefly a compilation, with the exception of his experiments on the analysis of blood, which constitute an introduction to the second volume. This book was published in 1806 and 1808. In the year 1806 he and Hisinger began a periodical work, entitled "Afhandlingar i Fysik, Kemi och Mineralogi," of which six volumes in all were published, the last in 1818. In this work there occur forty-seven papers by Berzelius, some of them of great length and importance, which will be noticed afterwards; but by far the greatest part of them consist of mineral analyses. We have the analysis of cerium by Hisinger and Berzelius, together with an account of the chemical characters of the two oxides of cerium. In the fourth volume he gives us a new chemical arrangement of minerals, founded on the supposition that they are all chemical compounds in definite proportions. Mr. Smithson had thrown out the opinion that *silica* is an acid: which opinion was taken up by Berzelius, who showed, by decisive experiments, that it enters into definite combinations with most of the bases. This happy idea enabled him to show, that most of the stony minerals are definite compounds of silica, with certain earths or metallic oxides. This system has undergone several modifications since he first gave it to the world; and I think it more than doubtful whether his last correction of it, published in the Memoirs of the Stockholm Academy, for 1824, be quite as good as the first, which he published in 1815. The first arrangement was founded on the bases, the last upon the acids with which these bases are united. He was induced to alter his arrangement, in consequence of Mitcherlich's doctrine of isomorphism But I conceive that the alterations which exist in the constitution of pyroxene, amphibole, garnet, and a few other minerals, might be explained in a very simple way, without admitting this doctrine of isomorphism; which if it do not, like Berthollet's hypothesis of indefinite combinations, overturn the whole principles of chemistry, seems scarcely consistent with what we know respecting chemical combination.

In the same volume we have a set of experiments on columbium, and its characters when reduced to the metallic state; together with an analysis of all the minerals containing columbicum that were known in the year 1815.

We have also a new examination of the properties of yttria, together with the analysis of a number of minerals, containing both cerium and yttria, and the mode of separating these two substances from each other by means of sulphate of potash.

In the sixth volume we have his discovery of selenium, with an account of selenic acid, and the different compounds which it forms.

Since the year 1818 his papers have been all published in the Memoirs of the Stockholm Academy;

but he has taken care to have translations of them inserted into Poggensdorf's Annalen, and the Annales de Chimie et de Physique.

In the Stockholm Memoirs, for 1819, we have his analysis of wavellite, showing that this mineral is a hydrous phosphate of alumina. The same analysis and discovery had been made by Fuchs, who published his results in 1818; but probably Berzelius had not seen the paper; at least he takes no notice of it. We have also in the same volume his analysis of euclase, of silicate of zinc, and his paper on the prussiates.

In the Memoirs for 1820 we have, besides three others, his paper on the mode of analyzing the ores of nickel. In the Memoirs for 1821 we have his paper on the alkaline sulphurets, and his analysis of achmite. The specimen selected for this analysis was probably impure; for two successive analyses of it, made in my laboratory by Captain Lehunt, gave a considerable difference in the proportion of the constituents, and a different formula for the composition than that resulting from the constituents found by Berzelius.

In the Memoirs for 1822 we have his analysis of the mineral waters of Carlsbad. In 1823 he published his experiments on uranium, which were meant as a confirmation and extension of the examination of this substance previously made by Arfvedson. In the same year appeared his experiments on fluoric acid and its combinations, constituting one of the most curious and important of all the numerous additions which he has made to analytical chemistry. In 1824 we have his analysis of phosphate of yttria, a mineral found in Norway; of polymignite, a mineral from the neighbourhood of Christiania, where it occurs in the zircon sienite, and remarkable for the great number of bases which it contains united to titanic acid; namely, zirconia, oxide of iron, lime, oxide of manganese, oxide of cerium, and yttria. We have also his analysis of arseniate of iron, from Brazil and from Cornwall; and of chabasite from Ferro. In this last analysis he mentions chabasites from Scotland, containing soda instead of lime. The only chabasites in Scotland, that I know of, occur in the neighbourhood of Glasgow; and in none of these have I found any soda. But I have found soda instead of lime in chabasites from the north of Ireland, always crystallized in the form to which Hauy has given the name of *trirhomboidale.* I think, therefore, that the chabasites analyzed by Arfvedson, to which Berzelius refers, must have been from Ireland, and not from Scotland; and I think it may be a question whether this form of crystal, if it should always be found to contain soda instead of lime, ought not to constitute a peculiar species.

In 1826 we have his very elaborate and valuable paper on sulphur salts. In this paper he shows that sulphur is capable of combining with bodies, in the same way as oxygen, and of converting the acidifiable bases into acids, and the alkalifiable bases into alkalies. These sulphur acids and alkalies unite with each other, and form a new class of saline bodies, which may be distinguished by the name of *sulphur salts.* This subject has been since carried a good deal further by M. H. Rose, who has by means of it thrown much light on some mineral species hitherto quite inexplicable. Thus, what is called *nickel glance,* is a sulphur salt of nickel. The acid is a compound of sulphur and arsenic, the base a compound of sulphur and nickel. Its composition may be represented thus:

1 atom disulphide of arsenic
1 atom disulphide of nickel.

In like manner glance cobalt is

1 atom disulphide of arsenic
1 atom disulphide of nickel.

Zinkenite is composed of

3 atoms sulphide of antimony
1 atom sulphide of lead;

and jamesonite of

2½ atoms sulphide of antimony
1 atom sulphide of lead.

Feather ore of antimony, hitherto confounded with sulphuret of antimony, is a compound of

5 atoms sulphide of antimony
3 atoms sulphide of lead.

Gray copper ore, which has hitherto appeared so difficult to be reduced to any thing like regularity, is composed of

1 atom sulphide of antimony or arsenic
2 atoms sulphide of copper or silver.

Dark red silver ore is composed of

1 atom sulphide of antimony
1 atom sulphide of silver;

and light red silver ore of

2 atoms sesquisulphide of arsenic
3 atoms sulphide of silver.

These specimens show how much light the doctrine of sulphur salts has thrown on the mineral kingdom.

In 1828 he published his experimental investigation of the characters and compounds of palladium, rhodium, osmium, and iridium; and upon the mode of analyzing the different ores of platinum.

One of the greatest improvements which Berzelius has introduced into analytical chemistry, is his mode of separating those bodies which become acid when united to oxygen, as sulphur, selenium, arsenic, &c., from those that become alkaline, as copper, lead, silver, &c. His method is to put the alloy or ore to be analyzed into a glass tube, and to pass over it a current of dry chlorine gas, while the powder in the tube is heated by a lamp. The acidifiable bodies are volatile, and pass over along the tube into a vessel of water placed to receive them, while the alkalifiable bodies remain fixed in the tube. This mode of analysis has been considerably improved by Rose, who availed himself of it in his analysis of gray copper ore, and other similar compounds.

Analytical chemistry lies under obligations to Berzelius, not merely for what he has done himself, but for what has been done by those pupils who were educated in his laboratory. Bonsdorf, Nordenskiöld, C. G. Gmelin, Rose, Wöhler, Arfvedson, have given us some of the finest examples of analytical investigations with which the science is furnished.

P. A. Von Bonsdorf was a professor of Abo, and after that university was burnt down, he moved to the new locality in which it was planted by the Russian government. His analysis of the minerals which crystallize in the form of the amphibole, constitutes a model for the young analysts to study, whether we consider the precision of the analyses, or the methods by which the different constituents were separated and estimated. His analysis of red silver ore first demonstrated that the metals in it were not in the state of oxides. The nature of the combination was first completely explained by Rose, after Berzelius's paper on the sulphur salts had made its appearance. His paper on the acid properties of several of the chlorides, has served considerably to extend and to rectify the views first proposed by Berzelius respecting the different classes of salts.

Nils Nordenskiöld is superintendent of the mines in Finland: his "Bidrag till närmare kännedom af Finland's Mineralier och Geognosie" was published in 1820. It contains a description and analysis of fourteen species of Lapland minerals, several of them new, and all of them interesting. The analyses were conducted in Berzelius's laboratory, and are excellent. In 1827 he published a tabular view of the mineral species, arranged chemically, in which he gives the crystalline form, hardness, and specific gravity, together with the chemical formulas for the composition.

C. G. Gmelin is professor of chemistry at Tubingen; he has devoted the whole of his attention to chemical analysis, and has published a great number of excellent ones, particularly in Schweigger's Journal. His analysis of helvine, and of the tourmalin, may be specified as particularly valuable. In this last mineral, he demonstrated the presence of boracic acid. Leopold Gmelin, professor of chemistry at Heidelberg, has also distinguished himself as an analytical chemist. His System of Chemistry, which is at present publishing, promises to be the best and most perfect which Germany has produced.

Henry Rose, of Berlin, is the son of that M. Rose who was educated by Klaproth, and afterwards became the intimate friend and fellow-labourer of that illustrious chemist. He has devoted himself to analytical chemistry with indefatigable zeal, and has favoured us with a prodigious number of new and admirably-conducted analyses. His analyses of pyroxenes, of the ores of titanium, of gray copper ore, of silver glance, of red silver ore, miargyrite, polybasite, &c., may be mentioned as examples. In 1829 he published a volume on analytical chemistry, which is by far the most complete and valuable work of the kind that has hitherto appeared; and ought to be carefully studied by all those who wish to make themselves masters of the difficult, but necessary art of analyzing compound bodies.*

---

* An excellent English translation of this book with several important additions by the author, has just been published by Mr. Griffin.

Wöhler is professor of chemistry in the Polytechnic School of Berlin; he does not appear to have turned his attention to analytical chemistry, but rather towards extending our knowledge of the compounds which the different simple bodies are capable of forming with each other. His discovery of cyanic acid may be mentioned as a specimen. He is active and young; much, therefore, may be expected from him.

Augustus Arfvedson has distinguished himself by the discovery of the new fixed alkali, lithia, in petalite and spodumene. It has been lately ascertained at Moscow, by M. R. Hermann, and the experiments have been repeated and confirmed by Berzelius, that lithia is a much lighter substance than it was found to be by Arfvedson, its atomic weight being only 1·75. We have from Arfvedson an important set of experiments on uranium and its oxides, and on the action of hydrogen on the metallic sulphurets. He has likewise analyzed a considerable number of minerals with great care; but of late years he seems to have lost his activity. His analysis of chrysoberyl does not possess the accuracy of the rest: by some inadvertence, he has taken a compound of glucina and alumina for silica.

I ought to have included Walmstedt and Trollé-Wachmeister among the Swedish chemists who have contributed important papers towards the progress of analytical chemistry, the memoir of the former on chrysolite, and of the latter on the garnets, being peculiarly valuable. But it would extend this work to an almost interminable length, if I were to particularize every meritorious experimenter. This must plead my excuse for having omitted the names of Bucholz, Gehlen, Fuchs, Dumesnil, Dobereiner, Kupfer, and various other meritorious chemists who have contributed so much to the perfecting of the chemical analysis of the mineral kingdom. But it would be unpardonable to leave out the name of M. Mitcherlich, professor of chemistry in Berlin, and successor of Klaproth, who was also a pupil of Berzelius. He has opened a new branch of chemistry to our consideration. His papers on isomorphous bodies, on the crystalline forms of various sets of salts, on the artificial formation of various minerals, do him immortal honour, and will hand him down to posterity as a fit successor of his illustrious predecessors in the chemical chair of Berlin—a city in which an uninterrupted series of firstrate chemists have followed each other for more than a century; and where, thanks to the fostering care of the Prussian government, the number was never greater than at the present moment.

The most eminent analytical chemists at present in France are, Laugier, a nephew and successor of Fourcroy, as professor of chemistry in the Jardin du Roi, and Berthier, who has long had the superintendence of the laboratory of the School of Mines. Laugier has not published many analyses to the world, but those with which he has favoured us appear to have been made with great care, and are in general very accurate. Berthier is a much more active man; and has not merely given us many analyses, but has made various important improvements in the analytical processes. His mode of separating arsenic acid, and determining its weight, is now generally followed; and I can state from experience that his method of fusing minerals with oxide of lead, when the object is to detect an alkali, is both accurate and easy. Berthier is young, and active, and zealous; we may therefore expect a great deal from him hereafter.

The chemists in great Britain have never hitherto distinguished themselves much in analytical chemistry. This I conceive is owing to the mode of education which has been hitherto unhappily followed. Till within these very few years, practical chemistry has been nowhere taught. The consequence has been, that every chemist must discover processes for himself; and a long time elapses before he acquires the requisite dexterity and skill. About the beginning of the present century, Dr. Kennedy, of Edinburgh, was an enthusiastic and dexterous analyst; but unfortunately he was lost to the science by a premature death, after giving a very few, but these masterly, analyses to the public. About the same time, Charles Hatchett, Esq., was an active chemist, and published not a few very excellent analyses; but unfortunately this most amiable and accomplished man has been lost to science for more than a quarter of a century; the baneful effects of wealth, and the cares of a lucrative and extensive business, having completely weaned him from scientific pursuits. Mr. Gregor, of Cornwall, was an accurate man, and attended only to analytical chemistry: his analyses were not numerous, but they were in general excellent. Unfortunately the science was deprived of his services by a premature death. The same observation applies equally to Mr. Edward Howard, whose analyses of meteoric stones form an era in this branch of chemistry. He was not only a skilful chemist, but was possessed of a persevering industry which peculiarly fitted him for making a figure as a practical chemist. Of modern British analytical chemists, undoubtedly the first is Mr. Richard Philips; to whom we are indebted for not a few analyses, conducted with great chemical skill, and performed with great accuracy. Unfortunately, of late years he has done little, having been withdrawn from science by the necessity of providing for a large family, which can hardly be done, in this country, except by turning one's attention to trade or manufactures. The same remark applies to Dr. Henry, who has contributed so much to our knowledge of gaseous bodies, and whose analytical skill, had it been wholly devoted to scientific investigations, would have raised his reputation, as a discoverer, much higher than it has attained; although the celebrity of Dr. Henry, even under the disadvantages of being a manufacturing chemist, is deservedly very high. Of the young chemists who have but recently started in the path of analytical investigation, we expect the most from Dr. Turner, of the London University. His analyses of the ores of manganese are admirable specimens of skill and accuracy, and have completely elucidated a branch of mineralogy which, before his experiments, and the descriptions of Haidinger appeared, was buried in impenetrable darkness.

No man that Great Britain has produced was better fitted to have figured as an analytical chemist, both by his uncommon chemical skill, and the powers of his mind, which were of the highest order, than Mr. Smithson Tennant, had he not been in some measure prevented by a delicate frame of body, which produced in him a state of indolence somewhat similar to that of Dr. Black. His discovery of osmium and iridium, and his analysis of emery and magnesian limestone, may be mentioned as proofs of what he could have accomplished had his health allowed him a greater degree of exertion. His experiments on the diamond first demonstrated that it was composed of pure carbon; while his discovery of phosphuret of lime has furnished lecturers on chemistry with one of the most brilliant and beautiful of those exhibitions which they are in the habit of making to attract the attention of their students.

Smithson Tennant was the only child of the Rev. Calvert Tennant, youngest son of a respectable family in Wensleydale, near Richmond, in Yorkshire, and vicar of Selby in that county. He was born on the 30th of November, 1761: he had the misfortune to lose his father when he was only nine years of age; and before he attained the age of manhood he was deprived likewise of his mother, by a very unfortunate accident: she was thrown from her horse while riding with her son, and killed on the spot. His education, after his father's death, was irregular, and apparently neglected; he was sent successively to different schools in Yorkshire, at Scorton, Tadcaster, and Beverley. He gave many proofs while young of a particular turn for chemistry and natural philosophy, both by reading all books of that description which fell in his way, and by making various little experiments which the perusal of these books suggested. His first experiment was made at nine years of age, when he prepared a quantity of gunpowder for fireworks, according to directions contained in some scientific book to which he had access.

In the choice of a profession, his attention was naturally directed towards medicine, as being more nearly allied to his philosophical pursuits. He went accordingly to Edinburgh, about the year 1781, where he laid the foundation of his chemical knowledge under Dr. Black. In 1782 he was entered a member of Christ's College, Cambridge, where he began, from that time, to reside. He was first entered as a pensioner; but disliking the ordinary discipline and routine of an academical life, he obtained an exemption from those restraints, by becoming a fellow commoner. During his residence at Cambridge his chief attention was bestowed on chemistry and botany; though he made himself also acquainted with the elementary parts of mathematics, and had mastered the most important parts of Newton's Principia.

In 1784 he travelled into Denmark and Sweden, chiefly with the view of becoming personally acquainted with Scheele, for whom he had imbibed a high admiration. He was much gratified by what he saw of this extraordinary man, and was particularly struck with the simplicity of the apparatus with which his great experiments had been performed. On his return to England he took great pleasure in showing his friends at Cambridge various mineralogical specimens, which had been presented to him by Scheele, and in exhibiting several interesting experiments which he had learned from that great chemist. A year or two afterwards he went to France, to become personally acquainted with the most eminent of the French chemists. Thence he went to Holland and the Netherlands, at that time in a state of insurrection against Joseph II.

In 1786 he left Christ's College along with Professor Hermann, and removed with him to Emmanuel College. In 1788 he took his first degree as bachelor of physic, and soon after quitted Cambridge and came to reside in London. In 1791 he made his celebrated analysis of carbonic acid, which fully confirmed the opinions previously stated by Lavoisier respecting the constituents of this substance. His mode was to pass phosphorus through red-hot carbonate of lime. The phosphorus was acidified, and charcoal deposited. It was during these experiments that he discovered phosphuret of lime.

In 1792 he again visited Paris; but, from circumstances, being afraid of a convulsion, he was fortunate enough to leave that city the day before the memorable 10th of August. He travelled through Italy, and then passed through part of Germany.

On his return to Paris, in the beginning of 1793, he was deeply impressed with the gloom and desolation arising from the system of terror then beginning to prevail in that capital. On calling at the house of M. Delametherie, of whose simplicity and moderation he had a high opinion, he found the doors and windows closed, as if the owner were absent. Being at length admitted, he found his friend sitting in a back room, by candle-light, with the shutters closed in the middle of the day. On his departure, after a hurried and anxious conversation, his friend conjured him not to come again, as the knowledge of his being there might be attended with serious consequences to them both. To the honour of Delametherie, it deserves to be stated, that through all the inquisitions of the revolution, he preserved for his friend property of considerable value, which Mr. Tennant had intrusted to his care.

On his return from the continent, he took lodgings in the Temple, where he continued to reside during the rest of his life. He still continued the study of medicine, and attended the hospitals, but became more indifferent about entering into practice. He took, however, a doctor's degree at Cambridge in 1796; but resolved, as his fortune was independent, to relinquish all idea of practice, as not likely to contribute to his happiness. Exquisite sensibility was a striking feature in his character, and it would, as he very properly conceived, have made him peculiarly unfit for the exercise of the medical profession. It may be worth while to relate an example of his practical benevolence which happened about this time.

He had a steward in the country, in whom he had long placed implicit confidence, and who was considerably indebted to him. In consequence of this man's becoming embarrassed in his circumstances, Mr. Tennant went into the country to examine his accounts. A time and place were appointed for him to produce his books, and show the extent of the deficiency; but the unfortunate steward felt himself unequal to the task of such an explanation, and in a fit of despair put an end to his existence. Touched by this melancholy event, Mr. Tennant used his utmost exertions for the relief and protection of the family whom he had left, and not only forgave them the debt, but afforded them pecuniary assistance, and continued ever afterwards to be their friend and benefactor.

During the year 1796 he made his experiments to prove that the diamond is pure carbon. His method was to heat it in a gold tube, with saltpetre. The diamond was converted into carbonic acid gas, which combined with the potash from the saltpetre, and by the evolution of which the quantity of carbon, in a given weight of diamond, might be estimated. A characteristic trait of Mr. Tennant occurred during the course of this experiment, which I relate on the authority of Dr. Wollaston, who was present as an assistant, and who related the fact to me. Mr. Tennant was in the habit of taking a ride on horseback every day at a certain hour. The tube containing the diamond and saltpetre were actually heating, and the experiment considerably advanced, when, suddenly recollecting that his hour for riding was come, he left the completion of the process to Dr. Wollaston, and went out as usual to take his ride.

In the year 1797, in consequence of a visit to a friend in Lincolnshire, where he witnessed the activity with which improvements in farming operations were at that time going on, he was induced to purchase some land in that country, in order to commence farming operations. In 1799 he bought a considerable tract of waste land in Somersetshire, near the village of Cheddar, where he built a small house, in which, during the remainder of his life, he was in the habit of spending some months every summer, besides occasional visits at other times of the year. These farming speculations, as might have been anticipated from the indolent and careless habits of Mr. Tennant, were not very successful. Yet it appears from the papers which he left behind him, that he paid considerable attention to agriculture, that he had read the best books on the subject, and collected many facts on it during his different journeys through various parts of England. In the course of these inquiries he had discovered that there were two kinds of limestone known in the midland counties of England, one of which yielded a lime injurious to vegetation. He showed, in 1799, that the presence of carbonate of magnesia is the cause of the bad qualities of this latter kind of limestone. He found that the magnesian limestone forms an extensive stratum in the midland counties, and that it occurs also in primitive districts under the name of dolomite.

He infers from the slow solubility of this limestone in acids, that it is a double salt composed of carbonate of lime and carbonate of magnesia in chemical combination. He found that grain would scarcely germinate, and that it soon perished in moistened carbonate of magnesia: hence he concluded that magnesia is really injurious to vegetation. Upon this principle he accounted for the injurious effects of the magnesian limestone when employed as a manure.

In 1802 he showed that emery is merely a variety of corundum, or of the precious stone known by the name of sapphire.

During the same year, while endeavouring to make an alloy of lead with the powder which remains after treating crude platinum with aqua regia, he observed remarkable properties in this powder, and found that it contained a new metal. While he was engaged in the investigation, Descotils had turned his attention to the same powder, and had discovered that it contained a metal which gives a red colour to the ammoniacal precipitate of platinum. Soon after, Vauquelin, having treated the powder with alkali, obtained a volatile metallic oxide, which he considered as the same metal that had been observed by Descotils. In 1804 Mr. Tennant showed that this powder contains two new metals, to which he gave the name of *osmium* and *iridium*.

Mr. Tennant's health, by this time, had become delicate, and he seldom went to bed without a certain quantity of fever, which often obliged him to get up during the night and expose himself to the cold air. To keep himself in any degree in health, he found it necessary to take a great deal of exercise on horseback. He was always an awkward and a bad horseman, so that these rides were sometimes a little hazardous; and I have more than once heard him say, that a fall from his horse would some day prove fatal to him. In 1809 he was thrown from his horse near Brighton, and had his collar-bone broken.

In the year 1812 he was prevailed upon to deliver a few lectures on the principles of mineralogy, to a number of his friends, among whom were many ladies, and a considerable number of men of science and information. These lectures were completely successful, and raised his reputation very much among his friends as a lecturer. He particularly excelled in the investigation of minerals by the blowpipe; and I have heard him repeatedly say, that he was indebted for the first knowledge of the mode of using that valuable instrument to Assessor Gahn of Fahlun.

In 1813, a vacancy occurring in the chemical professorship at Cambridge, he was solicited to become a candidate. His friends exerted themselves in his favour with unexampled energy; and all opposition being withdrawn, he was elected professor in May, 1813.

After the general pacification in 1814 he went to France, and repaired to the southern provinces of that kingdom. He visited Lyons, Nismes, Avignon, Marseilles, and Montpellier. He returned to Paris in November, much gratified by his southern tour. He was to have returned to England about the latter end of the year; but he continued to linger on till the February following. On the 15th of that month he went to Calais; but the wind blew directly into Calais harbour, and continued unfavourable for several days. After waiting till the 20th he went to Boulogne, in order to take the chance of a better passage from that port. He embarked on board a vessel on the 22d, but the wind was still adverse, and blew so violently that the vessel was obliged to put back. When Mr. Tennant came ashore, he said that "it was in vain to struggle with the elements, and that he was not yet tired of life." It was determined, in case the wind should abate, to make another trial in the evening. During the interval Mr. Tennant proposed to his fellow-traveller, Baron Bulow, that they should hire horses and take a ride. They rode at first along the sea-side; but, on Mr. Tennant's suggestion, they went afterwards to Bonaparte's pillar, which stands on an eminence about a league from the sea-shore, and which, having been to see it the day before, he was desirous of showing to Baron Bulow. On their return from thence they deviated a little from the road, in order to look at a small fort near the pillar, the entrance to which was over a fosse twenty feet deep. On the side towards them, there was a standing bridge for some way, till it joined a drawbridge, which turned on a pivot. The end next the fort rested on the ground. On the side next to them it was usually fastened by a bolt; but the bolt had been stolen about a fortnight before, and was not replaced. As the bridge was too narrow for them to go abreast, the baron said he would go first, and attempted to ride over it; but perceiving that it was beginning to sink, he made an effort to pass the centre, and called out to warn his companion of his danger; but it was too late: they were both precipitated into the trench. The baron, though much stunned, fortunately escaped without any serious hurt; but on recovering his senses, and looking round for Mr. Tennant, he found him lying under his horse nearly lifeless. He was taken, however, to the Civil Hospital, as the nearest place ready to receive him. After a short interval, he seemed in some slight degree to recover his senses, and made an effort to speak, but without effect, and died within the hour. His remains were interred a few days after in the public cemetery at Boulogne, being attended to the grave by most of the English residents.

There is another branch of investigation intimately connected with analytical chemistry, the improvements in which have been attended with great advantage, both to mineralogists and chemists. I mean the use of the blowpipe, to make a kind of miniature analysis of minerals in the dry way; so far, at least, as to determine the nature of the constituents of the mineral under examination. This is attended with many advantages, as a preliminary to a rigid analysis by solution. By informing us of the nature of the constituents, it enables us to form a plan of the analysis beforehand, which, in many cases, saves the trouble and the tediousness of two separate analytical investigations; for when we set about analyzing a mineral, of the nature of which we are entirely ignorant, two separate sets of experiments are in most cases indispensable. We must examine the mineral, in the first place, to determine the nature of its constituents. These being known, we can form a plan of an analysis, by means of which we can separate and estimate in succession the amount of each constituent of the mineral. Now a judicious use of the blowpipe often enables us to determine the nature of the constituents in a few minutes, and thus saves the trouble of the preliminary analysis.

The blowpipe is a tube employed by goldsmiths in soldering. By means of it, they force the flame of a candle or lamp against any particular point which they wish to heat. This enables them to solder trinkets of various kinds, without affecting any other part except the portion which is required to be heated. Cronstedt and Engestroem first thought of applying this little instrument to the examination of minerals. A small fragment of the mineral to be examined, not nearly so large as the head of a small pin, was put upon a piece of charcoal, and the flame of a candle was made to play upon it by means of a blowpipe, so as to raise it to a white heat. They observed whether it decrepitated, or was dissipated, or melted; and whatever the effect produced was, they were enabled from it to draw consequences respecting the nature of the mineral under examination.

The importance of this instrument struck Bergman, and induced him to wish for a complete examination of the action of the heat of the blowpipe upon all different minerals, either tried *per se* upon charcoal, or mixed with various fluxes; for three different substances had been chosen as fluxes, namely, *carbonate of soda*, *borax*, and *biphosphate of soda*; or, at least, what was in fact an equivalent for this last substance, *ammonio-phosphate of soda*, or *microcosmic salt*, at that time extracted from urine. This salt is a compound of one integrant particle of phosphate of soda, and one integrant particle of phosphate of ammonia. When heated before the blowpipe it fuses, and the water of crystallization, together with the ammonia, are gradually dissipated, so that at last nothing remains but biphosphate of soda. These fluxes have been found to act with considerable energy on most minerals. The carbonate of soda readily fuses with those that contain much silica, while the borax and biphosphate of soda act most powerfully on the bases, not sensibly affecting the silica, which remains unaltered in the fused bead. A mixture of borax and carbonate of soda upon charcoal in general enables us to reduce the metallic oxides to the state of metals, provided we understand the way of applying the flame properly. Bergman employed Gahn, who was at that time his pupil, and whose skill he was well acquainted with, to make the requisite experiments. The result of these experiments was drawn up into a paper, which Bergman sent to Baron Born in 1777, and they were published by him at Vienna in 1779. This valuable publication threw a new light upon the application of the blowpipe to the assaying of minerals; and for every thing new which it contained Bergman was indebted to Gahn, who had made the experiments.

John Gottlieb Gahn, the intimate friend of Bergman and of Scheele, was one of the best-informed men, and one whose manners were the most simple, unaffected, and pleasing, of all the men of science with whom I ever came in contact. I spent a few days with him at Fahlun, in 1812, and they were some of the most delightful days that I ever passed in my life. His fund of information was inexhaustible, and was only excelled by the charming simplicity of his manners, and by the benevolence and goodness of heart which beamed in his countenance. He was born on the 17th of August, 1745, at the Woxna iron-works, in South Helsingland, where his father, Hans Jacob Gahn, was treasurer to the government of Stora Kopperberg. His grandfather, or great-grandfather, he told me, had emigrated from Scotland; and he mentioned several families in Scotland to which he was related. After completing his school education at Westerås, he went, in the year 1760, to the University of Upsala. He had already shown a decided bias towards the study of chemistry, mineralogy, and natural philosophy; and, like most men of science in Sweden, where philosophical instrument-makers are scarcely to be found, he had accustomed himself to handle the different tools, and to supply himself in that manner with all the different pieces of apparatus which he required for his investigations. He seems to have spent nearly ten years at Upsala, during which time he acquired a very profound knowledge in chemistry, and made various important discoveries, which his modesty or his indifference to fame made him allow others to pass as their own. The discovery of the rhomboidal nucleus of carbonate of lime in a six-sided prism of that mineral, which he let fall, and which was accidentally broken, constitutes the foundation of Hauy's system of crystallization. He communicated the fact to Bergman, who published it as his own in the second volume of his Opuscula, without any mention of Gahn's name.

The earth of bones had been considered as a peculiar simple earth; but Gahn ascertained, by analysis, that it was a compound of phosphoric acid and lime; and this discovery he communicated to Scheele, who, in his paper on fluor spar, published in 1771, observed, in the seventeenth section, in which he is describing the effect of phosphoric acid on fluor spar, "It has lately been discovered that the earth of bones, or of horns, is calcareous earth combined with phosphoric acid." In consequence of this remark, in which the name of Gahn does not appear, it was long supposed that Scheele, and not Gahn, was the author of this important discovery.

It was during this period that he demonstrated the metallic nature of manganese, and examined the properties of the metal. This discovery was announced as his, at the time, by Bergman, and was almost the only one of the immense number of new facts which he had ascertained that was publicly known to be his.

On the death of his father he was left in rather narrow circumstances, which obliged him to turn his immediate attention to mining and metallurgy. To acquire a practical knowledge of mining he associated with the common miners, and continued to work like them till he had acquired all the practical dexterity and knowledge which actual labour could give. In 1770 he was commissioned by the College of Mines to institute a course of experiments, with a view to improve the method of smelting copper, at Fahlun. The consequence of this investigation was a complete regeneration of the whole system, so as to save a great deal both of time and fuel.

Sometime after, he became a partner in some extensive works at Stora Kopperberg, where he settled as a superintendent. From 1770, when he first settled at Fahlun, down to 1785, he took a deep interest in the improvement of the chemical works in that place and neighbourhood. He established manufactories of sulphur, sulphuric acid, and red ochre.

In 1780 the Royal College of Mines, as a testimony of their sense of the value of Gahn's improvements, presented him with a gold medal of merit. In 1782 he received a royal patent as mining master. In 1784 he was appointed assessor in the Royal College of Mines, in which capacity he officiated as often as his other vocations permitted him to reside in Stockholm. The same year he married Anna Maria Bergstrom, with whom he enjoyed for thirty-one years a life of uninterrupted happiness. By his wife he had a son and two daughters.

In the year 1773 he had been elected chemical stipendiary to the Royal College of Mines, and he continued to hold this appointment till the year 1814. During the whole of this period the solution of almost every difficult problem remitted to the college devolved upon him. In 1795 he was chosen a member of the committee for directing the general affairs of the kingdom. In 1810 he was made one of the committee for the general maintenance of the poor. In 1812 he was elected an active associate of the Royal Academy for Agriculture; and in 1816 he became a member of the committee for organizing the plan of a Mining Institute. In 1818 he was chosen a member of the committee of the Mint; but from this situation he was shortly after, at his own request, permitted to withdraw.

His wife died in 1815, and from that period his health, which had never been robust, visibly declined. Nature occasionally made an effort to shake off the disease; but it constantly returned with increasing strength, until, in the autumn of 1818, the decay became more rapid in its progress, and more decided in its character. He became gradually weaker, and on the 8th of December, 1818, died without a struggle, and seemingly without pain.

Ever after the experiments on the blowpipe which Gahn performed at the request of Bergman, his attention had been turned to that piece of apparatus;

and during the course of a long life he had introduced so many improvements, that he was enabled, by means of the blowpipe, to determine in a few minutes the constituents of almost any mineral. He had gone over almost all the mineral kingdom, and determined the behaviour of almost every mineral before the blowpipe, both by itself and when mixed with the different fluxes and reagents which he had invented for the purpose of detecting the different constituents; but, from his characteristic unwillingness to commit his observations and experiments to writing, or to draw them up into a regular memoir, had not Berzelius offered himself as an assistant, they would probably have been lost. By his means a short treatise on the blowpipe, with minute directions how to use the different contrivances which he had invented, was drawn up and inserted in the second volume of Berzelius's Chemistry. Berzelius and he afterwards examined all the minerals known, or at least which they could procure, before the blowpipe; and the result of the whole constituted the materials of Berzelius's treatise on the blowpipe, which has been translated into German, French, and English. It may be considered as containing the sum of all the improvements which Gahn had made on the use of the blowpipe, together with all the facts that he had collected respecting the phenomena exhibited by minerals before the blowpipe. It constitutes an exceedingly useful and valuable book, and ought to make a part of the library of every analytical chemist.

Dr. Wollaston had paid as much attention to the blowpipe as Gahn, and had introduced so many improvements into its use, that he was able, by means of it, to determine the nature of the constituents of any mineral in the course of a few minutes. He was fond of such analytical experiments, and was generally applied to by every person who thought himself possessed of a new mineral, in order to be enabled to state what its constituents were. The London mineralogists if the race be not extinct, must sorely feel the want of the man to whom they were in the habit of applying on all occasions, and to whom they never applied in vain.

Dr. William Hyde Wollaston, was the son of the Reverend Dr. Wollaston, a clergyman of some rank in the church of England, and possessed of a competent fortune. He was a man of abilities, and rather eminent as an astronomer. His grandfather was the celebrated author of the Religion of Nature delineated. Dr. William Hyde Wollaston was born about the year 1767, and was one of fifteen children, who all reached the age of manhood. His constitution was naturally feeble; but by leading a life of the strictest sobriety and abstemiousness he kept himself in a state fit for mental exertion. He was educated at Cambridge, where he was at one time a fellow. After studying medicine by attending the hospitals and lectures in London, and taking his degree of doctor at Cambridge, he settled at Bury St. Edmund's, where he practised as a physician for some years. He then went to London, became a fellow of the Royal College of Physicians, and commenced practitioner in the metropolis. A vacancy occurring in St. George's Hospital, he offered himself for the place of physician to that institution; but another individual, whom he considered his inferior in knowledge and science, having been preferred before him, he threw up the profession of medicine altogether, and devoted the rest of his life to scientific pursuits. His income, in consequence of the large family of his father, was of necessity small. In order to improve it he turned his thoughts to the manufacture of platinum, in which he succeeded so well, that he must have, by means of it, realized considerable sums. It was he who first succeeded in reducing it into ingots in a state of purity and fit for every kind of use: it was employed, in consequence, for making vessels for chemical purposes; and it is to its introduction that we are to ascribe the present accuracy of chemical investigations. It has been gradually introduced into the sulphuric acid manufactories, as a substitute for glass retorts.

Dr. Wollaston had a particular turn for contriving pieces of apparatus for scientific purposes. His reflecting goniometer was a most valuable present to mineralogists, and it is by its means that crystallography has acquired the great degree of perfection which it has recently exhibited. He contrived a very simple apparatus for ascertaining the power of various bodies to refract light. His camera lucida furnished those who were ignorant of drawing with a convenient method of delineating natural objects. His periscopic glasses must have been found useful, for they sold rather extensively: and his sliding rule for chemical equivalents furnished a ready method for calculating the proportions of one substance necessary to decompose a given weight of another.

Dr. Wollaston's knowledge was more varied, and his taste less exclusive than any other philosopher of his time, except Mr. Cavendish: but optics and chemistry are the two sciences which lie under the greatest obligations to him. His first chemical paper on urinary calculi at once added a vast deal to what had been previously known. He first pointed out the constituents of the mulberry calculi, showing them to be composed of oxalate of lime and animal matter. He first distinguished the nature of the triple phosphates. It was he who first ascertained the nature of the cystic oxides, and of the chalkstones, which appear occasionally in the joints of gouty patients. To him we owe the first demonstration of the identity of galvanism and common electricity; and the first explanation of the cause of the different phenomena exhibited by galvanic and common electricity. To him we are indebted for the discovery of palladium and rhodium, and the first account of the properties and characters of these two metals. He first showed that oxalic acid and potash unite in three different proportions, constituting oxalate, binoxalate, and quadroxalate of potash. Many other chemical facts, first ascertained by him, are to be found in the numerous papers of his scattered over the last forty volumes of the Philosophical Transactions: and perhaps not the least valuable of them is his description of the mode of reducing platinum from the raw state, and bringing it into the state of an ingot.

Dr. Wollaston died in the month of January, 1829, in consequence of a tumour formed in the brain, near, if I remember right, the thalami nervorum opticorum. There is reason to suspect that this tumour had been some time in forming. He had, without exception, the sharpest eye that I have ever seen: he could write with a diamond upon glass in a character so small, that nothing could be distinguished by the naked eye but a ragged line; yet when the letters were viewed through a microscope, they were beautifully regular and quite legible. He retained his senses to almost the last moment of his life: when he lay apparently senseless, and his friends were anxiously solicitous whether he still retained his understanding, he informed them, by writing, that his senses were still perfectly entire. Few individuals ever enjoyed a greater share of general respect and confidence, or had fewer enemies, than Dr. Wollaston. He was at first shy and distant, and remarkably circumspect, but he grew insensibly more and more agreeable as you got better acquainted with him, till at last you formed for him the most sincere friendship, and your acquaintance ended in the warmest and closest attachment.

CHAPTER V.

OF ELECTRO-CHEMISTRY.

Electricity, like chemistry, is a modern science; for it can scarcely claim an older origin than the termination of the first quarter of the preceding century; and during the last half of that century, and a small portion of the present, it participated with chemistry in the zeal and activity with which it was cultivated by the philosophers of Europe and America. For many years it was not suspected that any connexion existed between chemistry and electricity; though some of the meteorological phenomena, especially the production of clouds and the formation of rain, which are obviously connected with chemistry, seem likewise to claim some connexion with the agency of electricity.

The discovery of the intimate relation between chemistry and electricity was one of the consequences of a controversy carried on about the year 1790 between Galvani and Volta, two Italian philosophers, whose discoveries will render their names immortal. Galvani, who was a professor of anatomy, was engaged in speculations respecting muscular motion. He was of opinion that a peculiar fluid was secreted in the brain, which was sent along the nerves to all the different parts of the body. This nervous fluid possessed many characters analogous to those of electricity: the muscles were capable of being charged with it somewhat like a Leyden phial; and it was by the discharge of this accumulation, by the voluntary power of the nerves, that muscular motion was produced. He accidently discovered, that if the crural nerve going into the muscles of a frog, and the crural muscles, be laid bare immediately after death, and a piece of zinc be placed in contact with the nerve, and a piece of silver or copper with the muscle; when these two pieces of metal are made to touch each other, violent convulsions are produced in the muscle, which cause the limb to move. He conceived that these convulsions were produced by the discharge of the nervous energy from the muscles, in consequence of the conducting power of the metals.

Volta, who repeated these experiments, explained them in a different manner. According to him, the convulsions were produced by the passage of a current of common electricity through the limb of the frog, which was thrown into a state of convulsion merely in consequence of its irritability. This irritability vanishes after the death of the muscle; accordingly it is only while the principle of life remains that the convulsions can be produced. Every metallic conductor, according to him, possesses a certain electricity which is peculiar to it, either positive or negative, though the quantity is so small, as to be imperceptible, in the common state of the metal. But if a metal, naturally positive, be placed in contact, while insulated, with a metal naturally negative, the charge of electricity in both is increased by induction, and becomes perceptible when the two metals are separated and presented to a sufficiently delicate electrometer. Thus zinc is naturally positive, and copper and silver naturally negative. If we take two discs of copper and zinc, to the centre of each of which a varnished glass handle is cemented, and after keeping them for a short time in contact, separate them by the handles, and apply each to a sufficiently delicate electrometer, we shall find that the zinc is positive, and the silver or copper disc negative. When the silver and copper are placed in contact while lying on the nerve and muscles of the leg of a frog, the zinc becomes positive, and the silver negative, by induction; but, as the animal substance is a conductor, this state cannot continue: the two electricities pass through the conducting muscles and nerve, and neutralize one another. And it is this current which occasions the convulsions.

Such was Volta's simple explanation of the convulsions produced in galvanic experiments in the limb of a frog. Galvani was far from allowing the accuracy of it; and, in order to obviate the objection to his reasoning advanced by Volta from the necessity of employing two metals, he showed that the convulsions might, in certain cases, be produced by one metal. Volta showed that a very small quantity of one metal, either alloyed with, or merely in contact with another, were capable of inducing the two electricities. But in order to prove in the most unanswerable manner that the two electricities were induced when two different metals were placed in contact, he contrived the following piece of apparatus:

He procured a number (say 50) of pieces of zinc, about the size of a crown-piece, and as many pieces of copper, and thirdly, the same number of pieces of card of the same size. The cards were steeped in a solution of salt, so as to be moist. He lays upon the table a piece of zinc, places over it a piece of copper, and then a piece of moist card. Over the card is placed a second piece of zinc, then a piece of copper, then a piece of wet card. In this way all the pieces are piled upon each other in exactly the same order, namely, zinc, copper, card; zinc, copper, card; zinc, copper, card. So that the lowest plate is zinc and the uppermost is copper (for the last wet card may be omitted). In this way there are fifty pairs of zinc and copper plates in contact, each separated by a piece of wet card, which is a conductor of electricity. If you now moisten a finger of each hand with water, and apply one wet finger to the lowest zinc plate, and the other to the highest copper plate, the moment the fingers come in contact with the plates an electric shock is felt, the intensity of which increases with the number of pairs of plates in the pile. This is what is called the Galvanic, or rather the Voltaic pile. It was made known to the public in a paper by Volta, inserted in the Philosophical Transactions for 1800. This pile was gradually improved, by substituting troughs, first of baked wood, and afterwards of porcelain, divided into as many cells as there were pairs of plates. The size of the plates was increased; they were made square, and instead of all being in contact, it was found sufficient if they were soldered together by means of metallic slips rising from one side of each square. The two plates thus soldered were slipped over the diaphragm separating the contiguous cells, so that the zinc plate was in one cell and the copper in the other. Care was taken that the pairs were introduced all looking one way, so that a copper plate had always a zinc plate immediately opposite to it. The cells were filled with conducting liquid. brine, or a solution of salt in vinegar, or dilute muriatic, sulphuric, or nitric acid, might be employed; but dilute nitric acid was found to answer best, and the energy of the battery is directly proportional to the strength of the nitric acid employed.

Messrs. Nicholson and Carlisle were the first persons who repeated Volta's experiments with this apparatus, which speedily drew the attention of all Europe. They ascertained that the zinc end of the pile was positive and the copper end negative. Happening to put a drop of water on the uppermost plate, and to put into it the extremity of a gold wire connected with the undermost plate, they observed an extrication of air-bubbles from the wire. This led them to suspect that the water was decomposed. To determine the point, they collected a little of the gas extricated and found it hydrogen. They then attached a gold wire to the zinc end of the pile, and another gold wire to the copper end, and plunged the two wires into a glass of water, taking care not to allow them to touch each other. Gas was extricated from both wires. On collecting that from the wire attached to the zinc end, it was found to be *oxygen gas*, while that from the copper end was hydrogen gas. The volume of hydrogen gas extricated was just double that of the oxygen gas; and the two gases being mixed, and an electric spark passed through them, they burnt with an explosion, and were completely converted into water. Thus it was demonstrated that water was decomposed by the action of the pile, and that the oxygen was extricated from the positive pile and the hydrogen from the negative. This held when the communicating wires were gold or platinum; but if they were of copper, silver, iron, lead, tin, or zinc, then only hydrogen gas was extricated from the negative wire. The positive wire extricated little or no gas; but it was rapidly oxidized. Thus the connexion between chemical decompositions and electrical currents was first established.

It was soon after observed by Henry, Haldane, Davy, and other experimenters, that other chemical compounds were decomposed by the electrical currents as well as water. Ammonia, for example, nitric acid, and various salts, were decomposed by it. In the year 1803 an important set of experiments was published by Berzelius and Hisinger. They decomposed eleven different salts, by exposing them to the action of a current of electricity. The salts were dissolved in water, and iron or silver wires from the two poles of the pile were plunged into the solution. In every one of these decompositions, the acid was deposited round the positive wire, and the base of the salt round the negative wire. When ammonia was decomposed by the action of galvanic electricity, the azotic gas separated from the positive wire, and the hydrogen gas from the negative.

But it was Davy that first completely elucidated the chemical decompositions produced by galvanic electricity, who first explained the laws by which these decompositions were regulated, and who employed galvanism as an instrument for decomposing various compounds, which had hitherto resisted all the efforts of chemists to reduce them to their elements. These discoveries threw a blaze of light upon the obscurest parts of chemistry, and secured for the author of them an immortal reputation.

Humphry Davy, to whom these splendid discoveries were owing, was born at Penzance, in Cornwall, in the year 1778. He displayed from his very infancy a spirit of research, and a brilliancy of fancy, which augured, even at that early period, what he was one day to be. When very young, he was bound apprentice to an apothecary in his native town. Even at that time, his scientific acquirements were so great, that they drew the attention of Mr. Davis Gilbert, the late distinguished president of the Royal Society. It was by his advice that he resolved to devote himself to chemistry, as the pursuit best calculated to procure him celebrity. About this time Mr. Gregory Watt, youngest son of the celebrated improver of the steam-engine, happening to be at Penzance, met with young Davy, and was delighted with the uncommon knowledge which he displayed, at the brilliancy of his fancy, and the great dexterity and ardour with which, under circumstances the most unfavourable, he was prosecuting his scientific investigations. These circumstances made an indelible impression on his mind, and led him to recommend Davy as the best person to superintend the Bristol Institution for trying the medicinal effects of the gases.

After the discovery of the different gases, and the investigation of their properties by Dr. Priestley, it occurred to various individuals, nearly about the same time, that the employment of certain gases, or at least of mixtures of certain gases, with common air in respiration, instead of common air, might be powerful means of curing diseases. Dr. Beddoes, at that time professor of chemistry at Oxford, was one of the keenest supporters of these opinions. Mr. Watt, of Birmingham, and Mr. Wedgewood, entertained similar sentiments. About the beginning of the present century, a sum of money was raised by subscription, to put these opinions to the test of experiment; and, as Dr. Beddoes had settled as a physician in Bristol, it was agreed upon that the experimental investigation should take place at Bristol. But Dr. Beddoes was not qualified to superintend an institution of the kind: it was necessary to procure a young man of zeal and genius, who would take such an interest in the investigation as would compensate for the badness of the apparatus and the defects of the arrangements. The greatest part of the money had been subscribed by Mr. Wedgewood and Mr. Watt: their influence of course would be greatest in recommending a proper superintendent. Gregory Watt thought of Mr. Davy, whom he had lately been so highly pleased with, and recommended him with much zeal to superintend the undertaking. This recommendation being seconded by that of Mr. Davis Gilbert, who was so well acquainted with the scientific acquirements and genius of Davy, proved successful, and Davy accordingly got the appointment. At Bristol he was employed about a year in investigating the effects of the gases when employed in respiration. But he did not by any means confine himself to this, which was the primary object of the institution; but investigated the properties and determined the composition of nitric acid, ammonia, protoxide of azote and deutoxide of azote. The fruit of his investigations was published in 1800, in a volume entitled, "Researches, Chemical and Philosophical; chiefly concerning Nitrous Oxide, or Dephlogisticated Nitrous Air, and its Respiration." This work gave him at once a high reputation as a chemist, and was really a wonderful performance, when the circumstances under which it was produced are taken into consideration. He had discovered the intoxicating effects which protoxide of azote (nitrous oxide) produces when breathed, and had tried their effects upon a great number of individuals. This fortunate discovery perhaps contributed more to his celebrity, and to his subsequent success, than all the sterling merit of the rest of his researches—so great is the effect of display upon the greater part of mankind.

A few years before, a philosophical institution had been established in London, under the auspices of Count Rumford, which had received the name of the Royal Institution. Lectures on chemistry and natural philosophy were delivered in this institution; a laboratory was provided, and a library established:

The first professor appointed to this institution, Dr. Garnet, had been induced, in consequence of some disagreement between him and Count Rumford, to throw up his situation. Many candidates started for it; but Davy, in consequence of the celebrity which he had acquired by his researches, or perhaps by the intoxicating effects of protoxide of azote, which he had discovered, was, fortunately for the institution and for the reputation of England, preferred to them all. He was appointed professor of chemistry, and Dr. Thomas Young professor of natural philosophy, in the year 1801. Davy, either from the more popular nature of his subject, or from his greater oratorical powers, became at once a popular lecturer, and always lectured to a crowded room; while Dr. Young, though both a profound and clear lecturer, could scarcely command an audience of a dozen. It was here that Davy laboured with unwearied industry during eleven years, and acquired by his discoveries the highest reputation of any chemist in Europe.

In 1811 he was knighted, and soon after married Mrs. Apreece, a widow lady, daughter of Mr. Ker, who had been secretary to Lord Rodney, and had made a fortune in the West Indies. He was soon after created a baronet. About this time he resigned his situation as professor of chemistry in the Royal Institution, and went to the continent. He remained for some years in France and Italy. In the year 1821, when Sir Joseph Banks died, a very considerable number of the fellows offered their votes to Dr. Wollaston; but he declined standing as a candidate for the president's chair. Sir Humphry Davy, on the other hand, was anxious to obtain that honourable situation, and was accordingly elected president by a very great majority of votes on the 30th of November, 1821. This honourable situation he filled about seven years; but his health declining, he was induced to resign in 1828, and to go to Italy. Here he continued till 1829, when feeling himself getting worse, and wishing to draw his last breath in his own country, he began to turn his way homewards; but at Geneva he felt himself so ill, that he was unable to proceed further: here he took to his bed, and here he died on the 29th of May, 1829.

It was his celebrated paper "On some chemical Agencies of Electricity," inserted in the Philosophical Transactions for 1807, that laid the foundation of the high reputation which he so deservedly acquired. I consider this paper not merely as the best of all his own productions, but as the finest and completest specimen of inductive reasoning which appeared during the age in which he lived. It had been already observed, that when two platinum wires from the two poles of a galvanic pile are plunged each into a vessel of water, and the two vessels united by means of wet asbestos, or any other conducting substance, an *acid* appeared round the positive wire and an *alkali* round the negative wire. The alkali was said by some to be *soda*, by others to be *ammonia*. The acid was variously stated to be *nitric acid*, *muriatic acid*, or even *chlorine*. Davy demonstrated, by decisive experiments, that in all cases the acid and alkali are derived from the decomposition of some salt contained either in the water or in the vessel containing the water. Most commonly the salt decomposed is common salt, because it exists in water and in agate, basalt, and various other stony bodies, which he employed as vessels. When the same agate cup was used in successive experiments, the quantity of acid and alkali evolved diminished each time, and at last no appreciable quantity could be perceived. When glass vessels were used, soda was disengaged at the expense of the glass, which was sensibly corroded. When the water into which the wires were dipped was perfectly pure, and when the vessel containing it was free from every trace of saline matter, no acid or alkali made its appearance, and nothing was evolved except the constituents of water, namely, oxygen and hydrogen; the oxygen appearing round the positive wire, and the hydrogen round the negative wire.

When a salt was put into the vessel in which the positive wire dipped, the vessel into which the negative wire dipped being filled with pure water, and the two vessels being united by means of a slip of moistened asbestos, the acid of the salt made its appearance round the positive wire, and the alkali round the negative wire, before it could be detected in the intermediate space; but if an intermediate vessel, containing a substance for which the alkali has a strong affinity, be placed between these two vessels, the whole being united by means of slips of asbestos, then great part, or even the whole of the alkali, was stopped in this intermediate vessel. Thus, if the salt was nitrate of barytes, and sulphuric acid was placed in the intermediate vessel, much sulphate of barytes was deposited in the intermediate vessel, and very little or even no barytes made its appearance round the negative wire. Upon this subject a most minute, extensive, and satisfactory series of experiments was made by Davy, leaving no doubt whatever of the accuracy of the fact.

The conclusions which he drew from these experiments are, that all substances which have a chemical affinity for each other, are in different states of electricity, and that the degree of affinity is proportional to the intensity of these opposite states.

When such a compound body is placed in contact with the poles of a galvanic battery, the positive pole attracts the constituent, which is negative, and repels the positive. The negative acts in the opposite way, attracting the positive constituent and repelling the negative. The more powerful the battery, the greater is the force of these attractions and repulsions. We may, therefore, by increasing the energy of a battery sufficiently, enable it to decompose any compound whatever, the negative constituent being attracted by the positive pole, and the positive constituent by the negative pole. Oxygen, chlorine, bromine, iodine, cyanogen, and acids, are *negative* bodies; for they always appear round the *positive* pole of the battery, and are therefore attracted to it: while hydrogen, azote, carbon, selenium, metals, alkalies, earths, and oxide bases, are deposited round the negative pole, and consequently are *positive*.

According to this view of the subject, chemical affinity is merely a case of the attractions exerted by bodies in different states of electricity. Volta first broached the idea, that every body possesses naturally a certain state of electricity. Davy went a step further, and concluded, that the attractions which exist between the atoms of different bodies are merely the consequence of these different states of electricity. The proof of this opinion is founded on the fact, that if we present to a compound, sufficiently strong electrical poles, it will be separated into its constituents, and one of these constituents will invariably make its way to the positive and the other to the negative pole. Now bodies in a state of electrical excitement always attract those that are in the opposite state.

If electricity be considered as consisting of two distinct fluids, which attract each other with a force inversely, as the square of the distance, while the particles of each fluid repel each other with a force varying according to the same law, then we must conclude that the atoms of each body are covered externally with a coating of some one electric fluid to a greater or smaller extent. Oxygen and the other supporters of combustion are covered with a coating of negative electricity; while hydrogen, carbon, and the metals, are covered with a coating of positive electricity. What is the cause of the adherence of the electricity to these atoms we cannot explain. It is not owing to an attraction similar to gravitation; for electricity never penetrates into the interior of bodies, but spreads itself only on the surface, and the quantity of it which can accumulate is not proportional to the quantity of matter but to the extent of surface. But whatever be the cause, the adhesion is strong, and seemingly cannot be overcome. If we were to suppose an atom of any body, of oxygen for example, to remain uncombined with any other body, but surrounded by electricity, it is obvious that the coating of negative electricity on its surface would be gradually neutralized by its attracting and combining with positive electricity. But let us suppose an atom of oxygen and an atom of hydrogen to be united together, it is obvious that the positive electricity of the one atom would powerfully attract the negative electricity of the other, and *vice versâ*. And if these respective electricities cannot leave the atoms, the two atoms will remain firmly united, and the opposite electrical intensities will in some measure neutralize each other, and thus prevent them from being neutralized by electricity from any other quarter. But a current of the opposite electricities passing through such a compound, might neutralize the electricity in each, and thus putting an end to their attractions, occasion decomposition.

Such is a very imperfect outline of the electrical theory of affinity first proposed by Davy to account for the decompositions produced by electricity. It has been universally adopted by chemists; and some progress has been made in explaining and accounting for the different phenomena. It would be improper, in a work of this kind, to enter further into the subject. Those who are interested in such discussions will find a good deal of information in the first volume of Berzelius's Treatise on Chemistry, in the introduction to the Traité de Chimie appliqué aux Arts, by Dumas, or in the introduction to my System of Chemistry, at present in the press.

Davy having thus got possession of an engine, by means of which the compounds, whose constituents adhered to each other might be separated, immediately applied it to the decomposition of potash and soda; bodies which were admitted to be compounds, though all attempts to analyze them had hitherto failed. His attempt was successful. When a platinum wire from the negative pole of a strong battery in full action was applied to a lump of potash, slightly moistened, and lying on a platinum tray attached to the positive pole of the battery, small globules of a white metal soon appeared at its extremity. This white metal he speedily proved to be the basis of potash. He gave it the name of *potassium*, and very soon proved, that potash is a compound of five parts by weight of this metal and one part of oxygen. Potash, then, is a metallic oxide. He proved soon after that soda is a compound of oxygen and another white metal, to which he gave the name of *sodium*. Lime is a compound of *calcium* and oxygen, magnesia of *magnesium* and oxygen, barytes of *barium* and oxygen, and strontian of *strontium* and oxygen. In short, the fixed alkalies and alkaline earths, are metallic oxides. When *lithia* was afterwards discovered by Arfvedson, Davy succeeded in decomposing it also by the galvanic battery, and resolving it into oxygen and a white metal, to which the name of *lithium* was given.

Davy did not succeed so well in decomposing alumina, glucina, yttria, and zirconia, by the galvanic battery: they were not sufficiently good conductors of electricity; but nobody entertained any doubt that they also were metallic oxides. They have been all at length decomposed, and their bases obtained by the joint action of chlorine and potassium, and it has been demonstrated, that they also are metallic oxides. Thus it has been ascertained, in consequence of Davy's original discovery of the powers of the galvanic battery, that all the bases formerly distinguished into the four classes of alkalies, alkaline earths, earths proper, and metallic oxides, belong in fact only to one class, and are all metallic oxides.

Important as these discoveries are, and sufficient as they would have been to immortalize the author of them, they are not the only ones for which we are indebted to Sir Humphry Davy. His experiments on *chlorine* are not less interesting or less important in their consequences. I have already mentioned in a former chapter, that Berthollet made a set of experiments on chlorine, from which he had drawn as a conclusion, that it is a compound of oxygen and muriatic acid, in consequence of which it got the name of *oxymuriatic acid*. This opinion of Berthollet had been universally adopted by chemists, and admitted by them as a fundamental principle, till Gay-Lussac and Thenard endeavoured, unsuccessfully, to decompose this gas, or to resolve it into muriatic acid and chlorine. They showed, in the clearest manner, that such a resolution was impossible, and that no direct evidence could be adduced to prove that oxygen was one of its constituents. The conclusion to which they came was, that muriatic acid gas contained water as an essential constituent; and they succeeded by this hypothesis in accounting for all the different phenomena which they had observed. They even made an experiment to determine the quantity of water thus combined. They passed muriatic acid through hot litharge (protoxide of lead); muriate of lead was formed, and abundance of water made its appearance and was collected. They did not attempt to determine the proportions; but we can now easily calculate the quantity of water which would be deposited when a given weight of muriatic acid gas is absorbed by a given weight of litharge. Suppose we have fourteen parts of oxide of lead: to convert it into muriate of lead, 4·625 parts (by weight) of muriatic acid would be necessary, and during the formation of the muriate of lead there would be deposited 1·125 parts of water. So that from this experiment it might be concluded, that about one-fourth of the weight of muriatic acid gas is water.

The very curious and important facts respecting chlorine and muriatic acid gas which they had ascertained, were made known by Gay-Lussac and Thenard to the Institute, on the 27th of February, 1809, and an abstract of them was published in the second volume of the Mémoires d'Arcueil. There can be little doubt that it was in consequence of these curious and important experiments of the French chemists that Davy's attention was again turned to muriatic acid gas. He had already, in 1808, shown that when potassium is heated in muriatic acid gas, muriate of potash is formed, and a quantity of hydrogen gas evolved, amounting to more than one-third of the muriatic acid gas employed, and he had shown, that in no case can muriatic acid be obtained from chlorine, unless water or its elements be present. This last conclusion had been amply confirmed by the new investigations of Gay-Lussac and Thenard. In 1810 Davy again resumed the examination of the subject, and in July of that year read a paper to the Royal Society, to prove that *chlorine* is a simple substance, and that muriatic acid is a compound of *chlorine* and *hydrogen*.

This was introducing an alteration in chemical theory of the same kind, and nearly as important, as was introduced by Lavoisier, with respect to the action of oxygen in the processes of combustion and calcination. It had been previously supposed that sulphur, phosphorus, charcoal, and metals, were compounds; one of the constituents of which was phlogiston, and the other the acids or oxides which remained after the combustion or calcination had taken place. Lavoisier showed that the sulphur, phosphorus, charcoal, and metals, were simple substances; and that the acids or calces formed were compounds of these simple bodies and oxygen. In like manner, Davy showed that chlorine, instead of being a compound of muriatic acid and oxygen, was, in fact, a simple substance, and muriatic acid a compound of chlorine and hydrogen. This new doctrine immediately overturned the Lavoisierian hypothesis respecting oxygen as the acidifying principle, and altered all the previously received notions respecting the muriates. What had been called *muriates* were, in fact, combinations of chlorine with the combustible or metal, and were analagous to oxides. Thus, when muriatic acid gas was made to act upon hot litharge, a double decomposition took place, the chlorine united to the lead, while the hydrogen of the muriatic acid united with the oxygen of the litharge, and formed water. Hence the reason of the appearance of water in this case; and hence it was obvious that what had been called muriate of lead, was, in reality, a compound of chlorine and metallic lead. It ought, therefore, to be called, not muriate of lead, but chloride of lead.

It was not likely that this new opinion of Davy should be adopted by chemists in general, without a struggle to support the old opinions. But the feebleness of the controversy which ensued, affords a striking proof how much chemistry had advanced since the days of Lavoisier, and how free from prejudices chemists had become. One would have expected that the French chemists would have made the greatest resistance to the admission of these new opinions; because they had a direct tendency to diminish the reputation of two of their most eminent chemists, Lavoisier and Berthollet. But the fact was not so: the French chemists showed a degree of candour and liberality which redounds highly to their credit. Berthollet did not enter at all into the controversy. Gay-Lussac and Thenard, in their Recherches Physico-chimiques, published in 1811, state their reasons for preferring the old hypothesis to the new, but with great modesty and fairness; and, within less than a year after, they both adopted the opinion of Davy, that chlorine is a simple substance, and muriatic acid a compound of hydrogen and chlorine.

The only opponents to the new doctrine who appeared against it, were Dr. John Murray, of Edinburgh, and Professor Berzelius, of Stockholm. Dr. Murray was a man of excellent abilities, and a very zealous cultivator of chemistry; but his health had been always very delicate, which had prevented him from dedicating so much of his time to experimenting as he otherwise would have been inclined to do. The only experimental investigations into which he entered was the analysis of some mineral waters.

His powers of elocution were great. He was, in consequence, a popular and very useful lecturer. He published animadversions upon the new doctrine respecting *chlorine*, in Nicholson's Journal; and his observations were answered by Dr. John Davy.

Dr. John Davy was the brother of Sir Humphry, and had shown, by his paper on fluoric acid and on the chlorides, that he possessed the same dexterity and the same powers of inductive reasoning, which had given so much celebrity to his brother. The controversy between him and Dr. Murray was carried on for some time with much spirit and ingenuity on both sides, and was productive of some advantage to the science of chemistry, by the discovery of phosgene gas or chlorocarbonic acid, which was made by Dr. Davy. It is needless to say to what side the victory fell. The whole chemical world has for several years unanimously adopted the theory of Davy; showing sufficiently the opinion entertained respecting the arguments advanced by either party. Berzelius supported the old opinion respecting the compound nature of chlorine, in a paper which he published in the Annals of Philosophy. No person thought it worth while to answer his arguments, though Sir Humphry Davy made a few animadversions upon one or two of his experiments. The discovery of iodine, which took place almost immediately after, afforded so close an analogy with chlorine, and the nature of the compounds which it forms was so obvious and so well made out, that chemists were immediately satisfied; and they furnished so satisfactory an answer to all the objections of Berzelius, that I am not aware of any person, either in Great Britain or in France, who adopted his opinions. I have not the same means of knowing the impression which his paper made upon the chemists of Germany and Sweden. Berzelius continued for several years a very zealous opponent to the new doctrine, that chlorine is a simple substance. But he became at last satisfied of the futility of his own objections, and the inaccuracy of his reasoning. About the year 1820 he embraced the opinion of Davy, and is now one of its most zealous defenders. Dr. Murray has been dead for many years, and Berzelius has renounced his notion, that muriatic acid is a compound of oxygen and an unknown combustible basis. We may say then, I believe with justice, that at present all the chemical world adopts the notion that chlorine is a simple substance, and muriatic acid a compound of chlorine and hydrogen.

The recent discovery of bromine, by Balard, has added another strong analogy in favour of Davy's theory; as has likewise the discovery by Gay-Lussac respecting prussic acid. At present, then, (not reckoning sulphuretted and telluretted hydrogen gas), we are acquainted with four acids which contain no oxygen, but are compounds of hydrogen with another negative body. These are

Muriatic acid, composed of chlorine and hydrogen
Hydriodic acid . . . iodine and hydrogen
Hydrobromic acid . . bromine and hydrogen
Prussic acid . . . . cyanogen and hydrogen.

So that even if we were to leave out of view the chlorine acids, the sulphur acids, &c., no doubt can be entertained that many acids exist which contain no oxygen. Acids are compounds of electro-negative bodies and a base, and in them all the electro-negative electricity continues to predominate.

Next to Sir Humphry Davy, the two chemists who have most advanced electro-chemistry are Gay-Lussac and Thenard. About the year 1808, when the attention of men of science was particularly drawn towards the galvanic battery, in consequence of the splendid discoveries of Sir Humphry Davy, Bonaparte, who was at that time Emperor of France, consigned a sufficient sum of money to Count Cessac, governor of the Polytechnic School, to construct a powerful galvanic battery; and Gay-Lussac and Thenard were appointed to make the requisite experiments with this battery. It was impossible that a better choice could have been made. These gentlemen undertook a most elaborate and extensive set of experiments, the result of which was published in 1811, in two octavo volumes, under the title of "Recherches Physico-chimiques, faites sur la Pile; sur la Preparation chimique et les Propriétés du Potassium et du Sodium; sur la Décomposition de l'Acide boracique; sur les Acides fluorique, muriatique, et muriatique oxygené; sur l'Action chimique de la Lumière; sur l'Analyse vegetale et animale, &c." It would be difficult to name any chemical book that contains a greater number of new facts, or which contains so great a collection of important information, or which has contributed more to the advancement of chemical science.

The first part contains a very minute and interesting examination of the galvanic battery, and upon what circumstances its energy depends. They tried the effect of various liquid conductors, varied the strength of the acids and of the saline solutions. This division of their labours contains much valuable information for the practical electro-chemist, though it would be inconsistent with the plan of this work to enter into details.

The next division of the work relates to potassium. Davy had hitherto produced that metal only in minute quantities by the action of the galvanic battery upon potash. But Gay-Lussac and Thenard contrived a process by which it can be prepared on a large scale by chemical decomposition. Their method was, to put into a bent gun-barrel, well coated externally with clay, and passed through a furnace, a quantity of clean iron-filings. To one extremity of this barrel was fitted a tube containing a quantity of caustic potash. This tube was either shut at one end by a stopper, or by a glass tube luted to it, and plunged under the surface of mercury. To the other extremity of the gun-barrel was also luted a tube, which plunged into a vessel containing mercury. Heat was applied to the gun-barrel till it was heated to whiteness; then, by means of a choffer, the caustic potash was melted and made to trickle slowly into the white-hot iron-filings. At this temperature the potash undergoes decomposition, the iron uniting with its oxygen. The potassium is disengaged, and being volatile is deposited at a distance from the hot part of the tube, where it is collected after the process is finished.

Being thus in possession, both of potassium and sodium in considerable quantities, they were enabled to examine its properties more in detail than Davy had done: but such was the care and industry with which Davy's experiments had been made that very little remained to be done. The specific gravity of the two metals was determined with more precision than it was possible for Davy to do. They determined the action of these metals on water, and measured the quantity of hydrogen gas given out with more precision than Davy could. They discovered also, by heating these metals in oxygen gas, that they were capable of uniting with an additional dose of oxygen, and of forming peroxides of potassium and sodium. These oxides have a yellow colour, and give out the surplus oxygen, and are brought back to the state of potash and soda when they are plunged into water. They exposed a great variety of substances to the action of potassium, and brought to light a vast number of curious and important facts, tending to throw new light on the properties and characters of that curious metallic substance.

By heating together anhydrous boracic acid and potassium in a copper tube, they succeeded in decomposing the acid, and in showing it to be a compound of oxygen, and a black matter like charcoal, to which the name of *boron* has been given. They examined the properties of boron in detail, but did not succeed in determining with exactness the proportions of the constituents of boracic acid. The subsequent experiments of Davy, though not exact, come a good deal nearer the truth.

Their experiments on fluoric acid are exceedingly valuable. They first obtained that acid in a state of purity, and ascertained its properties. Their attempts to decompose it as well as those of Davy, ended in disappointment. But Ampere conceived the idea that this acid, like muriatic acid, is a compound of hydrogen with an unknown supporter of combustion, to which the name *fluorine* was given. This opinion was adopted by Davy, and his experiments, though they do not absolutely prove the truth of the opinion, give it at least considerable probability, and have disposed chemists in general to adopt it. The subsequent researches of Berzelius, while they have added a great deal to our former knowledge respecting fluoric acid and its compounds, have all tended to confirm and establish the doctrine that it is a hydracid, and similar in its nature to the other hydracids. But such is the tendency of fluorine to combine with every substance, that hitherto it has been impossible to obtain it in an insulated state. We want therefore, still, a decisive proof of the accuracy of the opinion.

To the experiments of Gay-Lussac and Thenard on chlorine and muriatic acid, I have already alluded in a former part of this chapter. It was during their investigations connected with this subject, that they discovered *fluoboric* acid gas, which certainly adds considerably to the probability of the theory of Ampere respecting the nature of fluoric acid.

I pass over a vast number of other new and important facts and observations contained in this admirable work, which ought to be studied with minute attention by every person who aspires at becoming a chemist.

Besides the numerous discoveries contained in the Recherches Physico-chimique, Gay-Lussac is the author of two of so much importance that it would be wrong to omit them. He showed that cyanogen is one of the constituents of prussic acid; succeeded in determining the composition of cyanogen, and showing it to be a compound of two atoms of carbon and one atom of azote. Prussic acid is a compound of one atom of hydrogen and one atom of cyanogen. Sulpho-cyanic acid, discovered by Mr. Porrett, is a compound of one atom sulphuric, and one atom cyanogen; chloro-cyanic acid, discovered by Berthollet, is a compound of one atom chlorine and one atom cyanogen; while cyanic acid, discovered by Wöhler, is a compound of one atom oxygen and one atom cyanogen. I take no notice of the fulminic acid; because, although Gay-Lussac's experiments are exceedingly ingenious, and his reasoning very plausible, it is not quite convincing; especially as the results obtained by Mr. Edmund Davy, and detailed by him in his late interesting memoir on this subject, are somewhat different.

The other discovery of Gay-Lussac is his demonstration of the peculiar nature of iodine, his account of iodic and hydriodic acids, and of many other compounds into which that curious substance enters as a constituent. Sir H. Davy was occupied with iodine at the same time with Gay-Lussac; and his sagacity and inventive powers were too great to allow him to work upon such a substance without discovering many new and interesting facts.

To M. Thenard we are indebted for the discovery of the important fact, that hydrogen is capable of combining with twice as much oxygen as exists in water, and determining the properties of this curious liquid which has been called deutoxide of hydrogen. It possesses bleaching properties in perfection, and I think it likely that chlorine owes its bleaching powers to the formation of a little deutoxide of hydrogen in consequence of its action on water.

The mantle of Davy seems in some measure to have descended on Mr. Faraday, who occupies his old place at the Royal Institution. He has shown equal industry, much sagacity, and great powers of invention. The most important discovery connected with electro-magnetism, next to the great fact, for which we are indebted to Professor Œrstedt of Copenhagen, is due to Mr. Faraday; I mean the rotation of the electric wires round the magnet. To him we owe the knowledge of the fact, that several of the gases can be condensed into liquids by the united action of pressure and cold, which has removed the barrier that separated gaseous bodies from vapours, and shown us that all owe their elasticity to the same cause. To him also we owe the knowledge of the important fact, that chlorine is capable of combining with carbon. This has considerably improved the history of chlorine and served still further to throw new light on the analogy which exists between all the supporters of combustion. They are doubtless all of them capable of combining with every one of the other simple bodies, and of forming compounds with them. For they are all negative bodies; while the other simple substances without exception, when compared to them, possess positive properties. We must therefore view the history of chemistry as incomplete, till we have become acquainted with the compounds of every supporter with every simple base.

CHAPTER VI.

OF THE ATOMIC THEORY.

I COME now to the last improvement which chemistry has received—an improvement which has given a degree of accuracy to chemical experimenting almost approaching to mathematical precision, which has simplified prodigiously our views respecting chemical combinations; which has enabled manufacturers to introduce theoretical improvements into their processes, and to regulate with almost perfect precision the relative quantities of the various constituents necessary to produce the intended effects. The consequence of this is, that nothing is wasted, nothing is thrown away. Chemical products have become not only better in quality, but more abundant and much cheaper. I allude to the atomic theory still only in its infancy, but already productive of the most important benefits. It is destined one day to produce still more wonderful effects, and to render chemistry not only the most delightful, but the most useful and indispensable, of all the sciences.

Like all other great improvements in science, the atomic theory developed itself by degrees, and several of the older chemists ascertained facts which might, had they been aware of their importance, have led them to conclusions similar to those of the moderns. The very attempt to analyze the salts was an acknowledgment that bodies united with each other in definite proportions: and these definite proportions, had they been followed out, would have led ultimately to the doctrine of atoms. For how could it be, that six parts of potash were always saturated by five parts of sulphuric acid and 6·75 parts of nitric acid? How came it that five of sulphuric acid always went as far in saturating potash as 6·75 of nitric acid? It was known, that in chemical combinations it was the ultimate particles of matter that combined. The simple explanation, therefore, would have been—that the weight of an ultimate particle of sulphuric acid was only five, while that of an ultimate particle of nitric acid was 6·75. Had such an inference been drawn, it would have led directly to the atomic theory.

The atomic theory in chemistry has many points of resemblance to the fluxionary calculus in mathematics. Both give us the ratios of quantities; both reduce investigations that would be otherwise extremely difficult, or almost impossible, to the utmost simplicity; and what is still more curious, both have been subjected to the same kind of ridicule by those who have not put themselves to the trouble of studying them with such attention as to understand them completely. The minute philosopher of Berkeley, *mutatis mutandis*, might be applied to the atomic theory with as much justice as to the fluxionary calculus; and I have heard more than one individual attempt to throw ridicule upon the atomic theory by nearly the same kind of arguments.

The first chemists, then, who attempted to analyze the salts may be considered as contributing towards laying the foundation of the atomic theory, though they were not themselves aware of the importance of the structure which might have been raised upon their experiments, had they been made with the requisite precision.

Bergman was the first chemist who attempted regular analyses of salts. It was he that first tried to establish regular formulas for the analyses of mineral waters, stones, and ores, by the means of solution and precipitation. Hence a knowledge of the constituents of the salts was necessary, before his formulas could be applied to practice. It was to supply this requisite information that he set about analyzing the salts, and his results were long considered by chemists as exact, and employed by them to determine the results of their analyses. We now know that these analytical results of Bergman are far from accurate; they have accordingly been laid aside as useless: but this knowledge has been derived from the progress of the atomic theory.

The first accurate set of experiments to analyze the salts was made by Wenzel, and published by him in 1777, in a small volume entitled "Lehre von der Verwandschaft der Körper," or, "Theory of the Affinities of Bodies." These analyses of Wenzel are infinitely more accurate than those of Bergman, and indeed in many cases are equally precise with the best which we have even at the present day. Yet the book fell almost dead-born from the press; Wenzel's results never obtained the confidence of chemists, nor is his name ever quoted as an authority. Wenzel was struck with a phenomenon, which had indeed been noticed by preceding chemists; but they had not drawn the advantages from it which it was capable of affording. There are several saline solutions which, when mixed with each other, completely decompose each other, so that two new salts are produced. Thus, if we mix together solutions of nitrate of lead and sulphate of soda in the requisite proportions, the sulphuric acid of the latter salt will combine with the oxide of lead of the former, and will form with it sulphate of lead, which will precipitate to the bottom in the state of an insoluble powder, while the nitric acid formerly united to the oxide of lead, will combine with the soda formerly in union with the sulphuric acid, and form nitrate of soda, which being soluble, will remain in solution in the liquid. Thus, instead of the two old salts,

Sulphate of soda
Nitrate of lead,

we obtain the two new salts,

Sulphate of lead
Nitrate of soda.

If we mix the two salts in the requisite proportions, the decomposition will be complete; but if there be an excess of one of the salts, that excess will still remain in solution without affecting the result. If we suppose the two salts anhydrous, then the proportions necessary for complete decomposition are,

Sulphate of soda	9
Nitrate of lead	20·75
	29·75

and the quantities of the new salts formed will be

Sulphate of lead	19
Nitrate of soda	10·75
	29·75

We see that the absolute weights of the two sets of salts are the same: all that has happened is, that both the acids and both the bases have exchanged situations. Now if, instead of mixing these two salts together in the preceding proportions, we employ

Sulphate of soda	9
Nitrate of lead	25·75

That is to say, if we employ 5 parts of nitrate of lead more than is sufficient for the purpose; we shall have exactly the same decompositions as before; but the 5 of excess of nitrate of lead will remain in solution, mixed with the nitrate of soda. There will be precipitated as before,

Sulphate of lead 19

and there will remain in solution a mixture of

Nitrate of soda 10·75
Nitrate of lead 5

The phenomena are precisely the same as before; the additional 5 of nitrate of lead have occasioned no alteration; the decomposition has gone on just as if they had not been present.

Now the phenomena which drew the particular attention of Wenzel is, that if the salts were neutral before being mixed, the neutrality was not affected by the decomposition which took place on their mixture.* A salt is said to be neutral when it neither possesses the characters of an acid or an alkali. Acids *redden* vegetable *blues*, while alkalies render them *green*. A neutral salt produces no effect whatever upon vegetable blues. This observation of Wenzel is very important: it is obvious that the salts, after their decomposition, could not have remained neutral unless the elements of the two salts had been such that the bases in each just saturated the acids in either of the salts.

The constituents of the two salts are as follows:

9	sulphate of soda	5	sulphuric acid
		4	soda,
20·75	nitrate of lead	6·75	nitric acid
		14	oxide of lead.

---

* This observation is not without exception. It does not hold when one of the salts is a phosphate or an arseniate, and this is the cause of the difficulty attending the analysis of these genera of salts.

Now it is clear, that unless 5 sulphuric acid were just saturated by 4 soda and by 14 oxide of lead; and 6·75 of nitric acid by the same 4 soda and 14 oxide of lead, the salts, aft their decomposition, could not have preserved their neutrality. Had 4 soda required only 5·75 of nitric acid, or had 14 oxide of lead required only 4 sulphuric acid, to saturate them, the liquid, after decomposition, would have contained an excess of acid. As no such excess exists, it is clear that in saturating an acid, 4 soda goes exactly as far as 14 oxide of lead; and that, in saturating a base, 5 sulphuric acid goes just as far as 6·75 nitric acid.

Nothing can exhibit in a more striking point of view, the almost despotic power of fashion and authority over the minds even of men of science, and the small number of them that venture to think for themselves, than the fact, that this most important and luminous explanation of Wenzel, confirmed by much more accurate experiments than any which chemistry had yet seen, is scarcely noticed by any of his contemporaries, and seems not to have attracted the smallest attention. In science, it is as unfortunate for a man to get before the age in which he lives, as to continue behind it. The admirable explanation of combustion by Hooke, and the important experiments on combustion and respiration by Mayow, were lost upon their contemporaries, and procured them little or no reputation whatever; while the same theory, and the same experiments, advanced by Lavoisier and Priestley, a century later, when the minds of men of science were prepared to receive them, raised them to the very first rank among philosophers, and produced a revolution in chemistry. So much concern has fortune, not merely in the success of kings and conquerors, but in the reputation acquired by men of science.

In the year 1792 another labourer, in the same department of chemistry, appeared: this was Jeremiah Benjamin Richter, a Prussian chemist, of whose history I know nothing more than that his publications were printed and published in Breslau, from which I infer that he was a native of, or at least resided in, Silesia. He calls himself Assessor of the Royal Prussian Mines and Smeltinghouses, and Arcanist of the Commission of Berlin Porcelain Manufacture. He died in the prime of life, on the 4th of May, 1807. In the year 1792 he published a work entitled "Anfansgründe der Stochyometrie; oder, Messkunst Chymischer Elemente" (Elements of Stochiometry; or, the Mathematics of the Chemical Elements). A second and third volume of this work appeared in 1793, and a fourth volume in 1794. The object of this book was a rigid analysis of the different salts, founded on the fact just mentioned, that when two salts decompose each other, the salts newly formed are neutral as well as those which have been decomposed. He took up the subject nearly in the same way as Wenzel had done, but carried the subject much further; and endeavoured to determine the capacity of saturation of each acid and base, and to attach numbers to each, indicating the weights which mutually saturate each other. He gave the whole subject a mathematical dress, and endeavoured to show that the same relation existed, between the numbers representing the capacity of saturation of these bodies, as does between certain classes of figurate numbers. When we strip the subject of the mystical form under which he presented it, the labours of Richter may be exhibited under the two following tables, which represent the capacity of saturation of the acids and bases, according to his experiments.

1. ACIDS.	
Fluoric acid . .	427
Carbonic . . .	577
Sebacic . . .	706
Muriatic . . .	712
Oxalic . . .	755
Phosphoric . .	979
Formic . . .	988
Sulphuric . .	1000
Succinic . . .	1209
Nitric	1405
Acetic	1480
Citric	1683
Tartaric . . .	1694

2. BASES.	
Alumina . . .	525
Magnesia . .	615
Ammonia . .	672
Lime	793
Soda	859
Strontian . . .	1329
Potash . . .	1605
Barytes . . .	2222

To understand this table, it is only necessary to observe, that if we take the quantity of any of the acids placed after it in the table, that quantity will be exactly saturated by the weight of each base put after it in the second column: thus, 1000 of sulphuric acid will be just saturated by 525 alumina, 615 magnesia, 672 ammonia, 793 lime, and so on. On the other hand, the quantity of any base placed after its name in the second column, will be just saturated by the weight of each acid placed after its name in the first column: thus, 793 parts of lime will be just saturated by 427 of fluoric acid, 577 of carbonic acid, 706 of sebacic acid, and so on.

This work of Richter was followed by a periodical work entitled "Ueber die neuern Gegenstande der Chymie" (On the New Objects of Chemistry). This work was begun in the year 1792, and continued in twelve different numbers, or volumes, to the time of his death in 1807.*

---

* I have only seen eleven parts of this work, the last of which appeared in 1802; but I believe that a twelfth part was published afterwards.

Richter's labours in this important field produced as little attention as those of Wenzel. Gehlen wrote a short panegyric upon him at his death, praising his views and pointing out their importance; but I am not aware of any individual, either in Germany or elsewhere, who adopted Richter's opinions during his lifetime, or even seemed aware of their importance, unless we are to except Berthollet, who mentions them with approbation in his Chemical Statics. This inattention was partly owing to the great want of accuracy which it is impossible not be sensible of in Richter's experiments. He operated upon too large quantities of matter, which indeed was the common defect of the times, and was first checked by Dr. Wollaston. The dispute between the phlogistians and the antiphlogistians, which was not fully settled in Richter's time, drew the attention of chemists to another branch of the subject. Richter in some measure went before the age in which he lived, and had his labours not been recalled to our recollection by the introduction of atomic theory, he would probably have been forgotten, like Hooke and Mayow, and only brought again under review after the new discoveries in the science had put it in the power of chemists in general to appreciate the value of his labours.

It is to Mr. Dalton that we are indebted for the happy and simple idea from which the atomic theory originated.

John Dalton, to whose lot it has fallen to produce such an alteration and improvement in chemistry, was born in Westmorland, and belongs to that small and virtuous sect known in this country by the name of Quakers. When very young he lived with Mr. Gough of Kendal, a blind philosopher, to whom he read, and whom he assisted in his philosophical investigations It was here, probably, that he acquired a considerable part of his education, particularly his taste for mathematics. For Mr. Gough was remarkably fond of mathematical investigations, and has published several mathematical papers that do him credit. From Kendal Mr. Dalton went to Manchester, about the beginning of the present century, and commenced teaching elementary mathematics to such young men as felt inclined to acquire some knowledge of that important subject. In this way, together with a few courses of lectures on chemistry, which he has occasionally given at the Royal Institution in London, at the Institution in Birmingham, in Manchester, and once in Edinburgh and in Glasgow, he has contrived to support himself for more than thirty years, if not in affluence, at least in perfect independence. And as his desires have always been of the most moderate kind, his income has always been equal to his wants. In a country like this, where so much wealth abounds, and where so handsome a yearly income was subscribed to enable Dr. Priestley to prosecute his investigations undisturbed and undistracted by the necessity of providing for the daily wants of his family, there is little doubt that Mr. Dalton, had he so chosen it, might, in point of pecuniary circumstances, have exhibited a much more brilliant figure. But he has displayed a much nobler mind by the career which he has chosen—equally regardless of riches as the most celebrated sages of antiquity, and as much respected and beloved by his friends, even in the rich commercial town of Manchester, as if he were one of the greatest and most influential men in the country. Towards the end of the last century, a literary and scientific society had been established in Manchester, of which Mr. Thomas Henry, the translator of Lavoisier's Essays, and who distinguished himself so much in promoting the introduction of the new mode of bleaching into Lancashire, was long president. Mr. Dalton, who had already distinguished himself by his meteorological observations, and particularly by his account of the Aurora Borealis, soon became a member of the society; and in the fifth volume of their Memoirs, part II., published in 1802, six papers of his were inserted, which laid the foundation of his future celebrity. These papers were chiefly connected with meteorological subjects; but by far the most important of them all was the one entitled "Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; on Evaporation; and on the Expansion of Gases by Heat."

From a careful examination of all the circumstances, he considered himself as entitled to infer, that when two elastic fluids or gases, A and B, are mixed together, there is no mutual repulsion among their particles; that is, the particles of A do not repel those of B, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind. This doctrine is of so startling a nature and so contrary to the opinions previously received, that chemists have not been much disposed to admit it. But at the same time it must be confessed, that no one has hitherto been able completely to refute it. The consequences of admitting it are obvious: we should be able to account for a fact which has been long known, though no very satisfactory reason for it had been assigned; namely, that if two gases be placed in two separate vessels, communicating by a narrow orifice, and left at perfect rest in a place where the temperature never varies, if we examine them after a certain interval of time we shall find both equally diffused through both vessels. If we fill a glass phial with hydrogen gas and another phial with common air or carbonic acid gas and unite the two phials by a narrow glass tube two feet long, filled with common air, and place the phial containing the hydrogen gas uppermost, and the other perpendicularly below it, the hydrogen, though lightest, will not remain in the upper phial, nor the carbonic acid, though heaviest, in the undermost phial; but we shall find both gases equally diffused through both phials.

But the second of these essays is by far the most important. In it he establishes, by the most unexceptionable evidence, that water, when it evaporates, is always converted into an elastic fluid, similar in its properties to air. But that the distance between the particles is greater the lower the temperature is at which the water evaporates. The elasticity of this vapour increases as the temperature increases. At 32° it is capable of balancing a column of mercury about half an inch in height, and at 212° it balances a column thirty inches high, or it is then equal to the pressure of the atmosphere. He determined the elasticity of vapour at all temperatures from 32° to 212°, pointed out the method of determining the quantity of vapour that at any time exists in the atmosphere, the effect which it has upon the volume of air, and the mode of determining its quantity. Finally, he determined, experimentally, the rate of evaporation from the surface of water at all temperatures from 32° to 212°. These investigations have been of infinite use to chemists in all their investigations respecting the specific gravity of gases, and have enabled them to resolve various interesting problems, both respecting specific gravity, evaporation, rain and respiration, which, had it not been for the principles laid down in this essay, would have eluded their grasp.

In the last essay contained in this paper he has shown that all elastic fluids expand the same quantity by the same addition of heat, and this expansion is very nearly 1-480th part for every degree of Fahrenheit's thermometer. In this last branch of the subject Mr. Dalton was followed by Gay-Lussac, who, about half a year after the appearance of his Essays, published a paper in the Annales de Chimie, showing that the expansion of all elastic fluids, when equally heated, is the same. Mr. Dalton concluded that the expansion of all elastic fluids by heat is equable. And this opinion has been since confirmed by the important experiments of Dulong and Petit, which have thrown much additional light on the subject.

In the year 1804, on the 26th of August, I spent a day or two at Manchester, and was much with Mr. Dalton. At that time he explained to me his notions respecting the composition of bodies. I wrote down at the time the opinions which he offered, and the following account is taken literally from my journal of that date:

The ultimate particles of all simple bodies are *atoms* incapable of further division. These atoms (at least viewed along with their atmospheres of heat) are all spheres, and are each of them possessed of particular weights, which may be denoted by numbers. For the greater clearness he represented the atoms of the simple bodies by symbols. The following are his symbols for four simple bodies, together with the numbers attached to them by him in 1804:

	Relative weights.
○ Oxygen	6·5
⊙ Hydrogen	1

		Relative weights.
●	Carbon	5
⦶	Azote	5

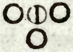
The following symbols represent the way in which he thought these atoms were combined to form certain binary compounds, with the weight of an integrant particle of each compound:

		Weights.
○⊙	Water	7·5
○⦶	Nitrous gas	11·5
●⊙	Olefiant gas	6
⦶⊙	Ammonia	6
○●	Carbonic oxide . . .	11·5

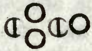
The following were the symbols by which he represented the composition of certain tertiary compounds:

		Weights.
○●○	Carbonic acid . . .	18
○⦶○	Nitrous oxide . . .	16·5
●⊙●	Ether	11
⊙●⊙	Carburetted hydrogen	7
○⦶○	Nitric acid	18

A quaternary compound:

	Oxynitric acid . .	24·5

A quinquenary compound:

	Nitrous acid . .	29·5

A sextenary compound:

	Alcohol	23·5

These symbols are sufficient to give the reader an idea of the notions entertained by Dalton respecting the nature of compounds. Water is a compound of one atom oxygen and one atom hydrogen as is obvious from the symbol ○⊙. Its weight 7·5 is that of an atom of oxygen and an atom of hydrogen united together. In the same way carbonic oxide is a compound of one atom oxygen and one atom carbon. Its symbol is ○●, and its weight 11·5 is equal to an atom of oxygen and an atom of carbon added together. Carbonic acid is a tertiary compound, or it consists of three atoms united together; namely, two atoms of oxygen and one atom of carbon. Its symbol is ○●○, and its weight 18. A bare inspection of the symbols and weights will make Mr. Dalton's notions respecting the constitution of every body in the table evident to every reader.

It was this happy idea of representing the atoms and constitution of bodies by symbols that gave Mr. Dalton's opinions so much clearness. I was delighted with the new light which immediately struck my mind, and saw at a glance the immense importance of such a theory, when fully developed. Mr. Dalton informed me that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gases, at that time imperfectly understood, and the constitution of which was first fully developed by Mr. Dalton himself. It was obvious from the experiments which he made upon them, that the constituents of both were carbon and hydrogen, and nothing else. He found further, that if we reckon the carbon in each the same, then carburetted hydrogen gas contains exactly twice as much hydrogen as olefiant gas does. This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas as a compound of one atom of carbon and one atom of hydrogen; and carburetted hydrogen of one atom of carbon and two atoms of hydrogen. The idea thus conceived was applied to carbonic oxide, water ammonia, &c.; and numbers representing the atomic weights of oxygen, azote, &c., deduced from the best analytical experiments which chemistry then possessed.

Let not the reader suppose that this was an easy task. Chemistry at that time did not possess a single analysis which could be considered as even approaching to accuracy. A vast number of facts had been ascertained, and a fine foundation laid for future investigation; but nothing, as far as weight and measure were concerned, deserving the least confidence, existed. We need not be surprised, then, that Mr. Dalton's first numbers were not exact. It required infinite sagacity, and not a little labour, to come so near the truth as he did. How could accurate analyses of gases be made when there was not a single gas whose specific gravity was known, with even an approach to accuracy; the preceding investigations of Dalton himself paved the way for accuracy in this indispensable department; but still accurate results had not yet been obtained.

In the third edition of my System of Chemistry, published in 1807, I introduced a short sketch of Mr. Dalton's theory, and thus made it known to the chemical world. The same year a paper of mine on *oxalic acid* was published in the Philosophical Transactions, in which I showed that oxalic acid unites in two proportions with strontian, forming an *oxalate* and *binoxalate;* and that, supposing the strontian in both salts to be the same, the oxalic acid in the latter is exactly twice as much as in the former. About the same time, Dr. Wollaston showed that bicarbonate of potash contains just twice the quantity of carbonic acid that exists in carbonate of potash; and that there are three oxalates of potash; viz., *oxalate*, *binoxalate*, and *quadroxalate;* the weight of acids in each of which are as the numbers 1, 2, 4.

These facts gradually drew the attention of chemists to Mr. Dalton's views. There were, however, some of our most eminent chemists who were very hostile to the atomic theory. The most conspicuous of these was Sir Humphry Davy. In the autumn of 1807 I had a long conversation with him at the Royal Institution, but could not convince him that there was any truth in the hypothesis. A few days after I dined with him at the Royal Society Club, at the Crown and Anchor, in the Strand. Dr. Wollaston was present at the dinner. After dinner every member of the club left the tavern, except Dr. Wollaston, Mr. Davy, and myself, who staid behind and had tea. We sat about an hour and a half together, and our whole conversation was about the atomic theory. Dr. Wollaston was a convert as well as myself; and we tried to convince Davy of the inaccuracy of his opinions; but, so far from being convinced, he went away, if possible, more prejudiced against it than ever. Soon after, Davy met Mr. Davis Gilbert, the late distinguished president of the Royal Society; and he amused him with a caricature description of the atomic theory, which he exhibited in so ridiculous a light, that Mr. Gilbert was astonished how any man of sense or science could be taken in with such a tissue of absurdities. Mr. Gilbert called on Dr. Wollaston (probably to discover what could have induced a man of Dr. Wollaston's sagacity and caution to adopt such opinions), and was not sparing in laying the absurdities of the theory, such as they had been represented to him by Davy, in the broadest point of view. Dr. Wollaston begged Mr. Gilbert to sit down, and listen to a few facts which he would state to him. He then went over all the principal facts at that time known respecting the salts; mentioned the alkaline carbonates and bicarbonates, the oxalate, binoxalate, and quadroxalate of potash, carbonic oxide and carbonic acid, olefiant gas, and carburetted hydrogen; and doubtless many other similar compounds, in which the proportion of one of the constituents increases in a regular ratio. Mr. Gilbert went away a convert to the truth of the atomic theory; and he had the merit of convincing Davy that his former opinions on the subject were wrong. What arguments he employed I do not know; but they must have been convincing ones, for Davy ever after became a strenuous supporter of the atomic theory. The only alteration which he made was to substitute *proportion* for Dalton's word, *atom*. Dr. Wollaston substituted for it the term *equivalent*. The object of these substitutions was to avoid all theoretical annunciations. But, in fact, these terms, *proportion*, *equivalent*, are neither of them so convenient as the term *atom:* and, unless we adopt the hypothesis with which Dalton set out, namely, that the ultimate particles of bodies are *atoms* incapable of further division, and that chemical combination consists in the union of these atoms with each other, we lose all the new light which the atomic theory throws upon chemistry, and bring our notions back to the obscurity of the days of Bergman and of Berthollet.

In the year 1808 Mr. Dalton published the first volume of his New System of Chemical Philosophy. This volume consists chiefly of two chapters: the first, on *heat*, occupies 140 pages. In it he treats of all the effects of heat, and shows the same sagacity and originality which characterize all his writings. Even when his opinions on a subject are not correct, his reasoning is so ingenious and original, and the new facts which he contrives to bring forward so important, that we are always pleased and always instructed. The second chapter, on the *constitution* *of bodies*, occupies 70 pages. The chief object of it is to combat the peculiar notions respecting elastic fluids, which had been advanced by Berthollet, and supported by Dr. Murray, of Edinburgh. In the third chapter, on *chemical synthesis*, which occupies only a few pages, he gives us the outlines of the atomic theory, such as he had conceived it. In a plate at the end of the volume he exhibits the symbols and atomic weights of thirty-seven bodies, twenty of which were then considered as simple, and the other seventeen as compound. The following table shows the atomic weight of the simple bodies, as he at that time had determined them from the best analytical experiments that had been made:

	Weight of atom.		Weight of atom.
Hydrogen	1	Strontian	46
Azote	5	Barytes	68
Carbon	5	Iron	38
Oxygen	7	Zinc	56
Phosphorus	9	Copper	56
Sulphur	13	Lead	95
Magnesia	20	Silver	100
Lime	23	Platinum	100
Soda	28	Gold	140
Potash	42	Mercury	167

He had made choice of hydrogen for unity, because it is the lightest of all bodies. He was of opinion that the atomic weights of all other bodies are multiples of hydrogen; and, accordingly, they are all expressed in whole numbers. He had raised the atomic weight of oxygen from 6·5 to 7, from a more careful examination of the experiments on the component parts of water. Davy, from a more accurate set of experiments, soon after raised the number for oxygen to 7·5: and Dr. Prout, from a still more careful investigation of the relative specific gravities of oxygen and hydrogen, showed that if the atom of hydrogen be 1, that of oxygen must be 8. Every thing conspires to prove that this is the true ratio between the atomic weights of oxygen and hydrogen.

In 1810 appeared the second volume of Mr. Dalton's New System of Chemical Philosophy. In it he examines the elementary principles, or simple bodies, namely, oxygen, hydrogen, azote, carbon, sulphur, phosphorus, and the metals; and the compounds consisting of two elements, namely, the compounds of oxygen with hydrogen, azote, carbon, sulphur, phosphorus; of hydrogen with azote, carbon, sulphur, phosphorus. Finally he treats of the fixed alkalies and earths. All these combinations are treated of with infinite sagacity; and he endeavours to determine the atomic weights of the different elementary substances. Nothing can exceed the ingenuity of his reasoning. But unfortunately at that time very few accurate chemical analyses existed; and in chemistry no reasoning, however ingenious, can compensate for this indispensable datum. Accordingly his table of atomic weights at the end this second volume, though much more complete than that at the end of the first volume, is still exceedingly defective; indeed no one number can be considered as perfectly correct.

The third volume of the New System of Chemical Philosophy was only published in 1827; but the greatest part of it had been printed nearly ten years before. It treats of the metallic oxides, the sulphurets, phosphurets, carburets, and alloys. Doubtless many of the facts contained in it were new when the sheets were put to the press; but during the interval between the printing and publication, almost the whole of them had not merely been anticipated, but the subject carried much further. By far the most important part of the volume is the Appendix, consisting of about ninety pages, in which he discusses, with his usual sagacity, various important points connected with heat and vapour. In page 352 he gives a new table of the atomic weights of bodies, much more copious than those contained in the two preceding volumes; and into which he has introduced the corrections necessary from the numerous correct analyses which had been made in the interval. He still adheres to the ratio 1:7 as the correct difference between the weights of the atoms of hydrogen and oxygen. This shows very clearly that he has not attended to the new facts which have been brought forward on the subject. No person who has attended to the experiments made on the specific gravity of these two gases during the last twelve years, could admit that these specific gravities are to each other as 1 to 14. If 1 to 16 be not the exact ratio, it will surely be admitted on all hands that it is infinitely near it.

Mr. Dalton represented the weight of an atom of hydrogen by 1, because it is the lightest of bodies. In this he has been followed by the chemists of the Royal Institution, by Mr. Philips, Dr. Henry, and Dr. Turner, and perhaps some others whose names I do not at present recollect. Dr. Wollaston, in his paper on Chemical Equivalents, represented the atomic weight of oxygen by 1, because it enters into a greater number of combinations than any other substance; and this plan has been adopted by Berzelius, by myself, and by the greater number, if not the whole, of the chemists on the continent. Perhaps the advantage which Dr. Wollaston assigned for making the atom of oxygen unity will ultimately disappear: for there is no reason for believing that the other supporters of combustion are not capable of entering into as many compounds as oxygen. But, from the constitution of the atmosphere, it is obvious that the compounds into which oxygen enters will always be of more importance to us than any others; and in this point of view it may be attended with considerable convenience to have oxygen represented by 1. In the present state of the atomic theory there is another reason for making the atom of oxygen unity, which I think of considerable importance. Chemists are not yet agreed about the atom of hydrogen. Some consider water a compound of 1 atom of oxygen and 2 atoms of hydrogen; others, of 1 atom of oxygen and 1 atom of hydrogen. According to the first view, the atom of hydrogen is only 1-16th of the weight of an atom of oxygen; according to the second, it is 1-8th. If, therefore, we were to represent the atom of hydrogen by 1, the consequence would be, that two tables of atomic weights would be requisite—all the atoms in one being double the weight of the atoms in the other: whereas, if we make the atom of oxygen unity, it will be the atom of hydrogen only that will differ in the two tables. In the one table it will be 0·125, in the other it will be 0·0625: or, reckoning with Berzelius the atom of oxygen = 100, we have that of hydrogen = 12·5 or 6·25, according as we view water to be a compound of 1 atom of oxygen with 1 or 2 atoms of hydrogen.

In the year 1809 Gay-Lussac published in the second volume of the Mémoires d'Arcueil a paper on the union of the gaseous substances with each other. In this paper he shows that the proportions in which the gases unite with each other are of the simplest kind. One volume of one gas either combining with one volume of another, or with two volumes, or with half a volume. The atomic theory of Dalton had been opposed with considerable keenness by Berthollet in his Introduction to the French translation of my System of Chemistry. Nor was this opposition to be wondered at; because its admission would of course overturn all the opinions which Berthollet had laboured to establish in his Chemical Statics. The object of Gay-Lussac's paper was to confirm and establish the new atomic theory, by exhibiting it in a new point of view. Nothing can be more ingenious than his mode of treating the subject, or more complete than the proofs which he brings forward in support of it. It had been already established that water is formed by the union of one volume of oxygen and two volumes of hydrogen gas. Gay-Lussac found by experiment, that one volume of muriatic acid gas is just saturated by one volume of ammoniacal gas: the product is sal ammoniac. Fluoboric acid gas unites in two proportions with ammoniacal gas: the first compound consists of one volume of fluoboric gas, and one volume of ammoniacal; the second, of one volume of the acid gas, and two volumes of the alkaline. The first forms a neutral salt, the second an alkaline salt. He showed likewise, that carbonic acid and ammoniacal gas could combine also in two proportions; namely, one volume of the acid gas with one or two volumes of the alkaline gas.

M. Amédée Berthollet had proved that ammonia is a compound of one volume of azotic, and three volumes of hydrogen gas. Gay-Lussac himself had shown that sulphuric acid is composed of one volume sulphurous acid gas, and a half-volume of oxygen gas. He showed further, that the compounds of azote and oxygen were composed as follows:

	Azote.	Oxygen.
Protoxide of azote	1 volume	+ ½ volume
Deutoxide of azote	1 ,,	+ 1
Nitrous acid .	1 ,,	+ 2

He showed also, that when the two gases after combining remained in the gaseous state, the diminution of volume was either 0, or ⅓, or ½.

The constancy of these proportions left no doubt that the combinations of all gaseous bodies were definite. The theory of Dalton applied to them with great facility. We have only to consider a volume of gas to represent an atom, and then we see that in gases one atom of one gas combines either with one, two, or three atoms of another gas, and never with more. There is, indeed, a difficulty occasioned by the way in which we view the composition of water. If water be composed of one atom of oxygen and one atom of hydrogen, then it follows that a volume of oxygen contains twice as many atoms as a volume of hydrogen. Consequently, if a volume of hydrogen gas represent an atom, half a volume of oxygen gas must represent an atom.

Dr. Prout soon after showed that there is an intimate connexion between the atomic weight of a gas and its specific gravity. This indeed is obvious at once. I afterwards showed that the specific gravity of a gas is either equal to its atomic weight multiplied by $1{\cdot}111\dot{1}$ (the specific gravity of oxygen gas), or by $0{\cdot}555\dot{5}$ (half the specific gravity of oxygen gas), or by $0{\cdot}277\dot{7}$ (1-4th of the specific gravity of oxygen gas), these differences depending upon the relative condensation which the gases undergo when their elements unite. The following table exhibits the atoms and specific gravity of these three sets of gases:

I. Sp. Gr. = Atomic Weight × $1{\cdot}111\dot{1}$

	Atomic weight.	Sp. gravity.
Oxygen gas	1	$1{\cdot}111\dot{1}$
Fluosilicic acid	3·25	$3{\cdot}611\dot{1}$

II. Sp. Gr. = Atomic Weight × 0·555$\dot{5}$.

	Atomic weight.	Sp. gravity.
Hydrogen	0·125	0·069$\dot{4}$
Azotic	1·75	0·072$\dot{2}$
Chlorine	4·5	2·5
Carbon vapour	0·75	0·416$\dot{6}$
Phosphorus vapour	2	1·111$\dot{1}$
Sulphur vapour	2	1·111$\dot{1}$
Tellurium vapour	4	2·222$\dot{2}$
Arsenic vapour	4 75	2·638$\dot{8}$
Selenium vapour	5	2·777$\dot{7}$
Bromine vapour	10	5·555$\dot{5}$
Iodine vapour	15·75	8·75
Steam	1·125	0·625
Carbonic oxide gas	1·75	0·972$\dot{2}$
Carbonic acid	2·75	1·527$\dot{7}$
Protoxide of azote	2·75	1·527$\dot{7}$
Nitric acid vapour	6·75	3·75
Sulphurous acid	4	2.222$\dot{2}$
Sulphuric acid vapour	5	2·777$\dot{7}$
Cyanogen	3·25	1·805$\dot{5}$
Fluoboric acid	4·25	2·361$\dot{1}$
Bisulphuret of carbon	4·75	2·638$\dot{8}$
Chloro-carbonic acid	6·25	3·472$\dot{2}$

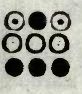
III. Sp. Gr. = Atomic Weight × 0·277$\dot{7}$.

	Atomic weight.	Sp. gravity.
Ammoniacal gas	2·125	0·5902$\dot{7}$
Hydrocyanic acid	3·375	0·9375
Deutoxide of azote	3·75	1·041$\dot{6}$
Muriatic acid	4·625	1·2847$\dot{2}$
Hydrobromic acid	10·125	2·8125
Hydriodic acid	15·875	4·40973

When Professor Berzelius, of Stockholm, thought of writing his Elementary Treatise on Chemistry, the first volume of which was published in the year 1808, he prepared himself for the task by reading several chemical works which do not commonly fall under the eye of those who compose elementary treatises. Among other books he read the Stochiometry of Richter, and was much struck with the explanations there given of the composition of salts, and the precipitation of metals by each other. It followed from the researches of Richter, that if we were in possession of good analyses of certain salts, we might by means of them calculate with accuracy the composition of all the rest. Berzelius formed immediately the project of analyzing a series of salts with the most minute attention to accuracy. While employed in putting this project in execution, Davy discovered the constituents of the alkalies and earths, Mr. Dalton gave to the world his notions respecting the atomic theory, and Gay-Lussac made known his theory of volumes. This greatly enlarged his views as he proceeded, and induced him to embrace a much wider field than he had originally contemplated. His first analyses were unsatisfactory; but by repeating them and varying the methods, he detected errors, improved his processes, and finally obtained results, which agreed exceedingly well with the theoretical calculations. These laborious investigations occupied him several years. The first outline of his experiments appeared in the 77th volume of the Annales de Chimie, in 1811, in a letter addressed by Berzelius to Berthollet. In this letter he gives an account of his methods of analyses together with the composition of forty-seven compound bodies. He shows that when a metallic protosulphuret is converted into a sulphate, the sulphate is neutral; that an atom of sulphur is twice as heavy as an atom of oxygen; and that when sulphite of barytes is converted into sulphate, the sulphate is neutral, there being no excess either of acid or base. From these and many other important facts he finally draws this conclusion: " In a compound formed by the union of two oxides, the one which (when decomposed by the galvanic battery) attaches itself to the positive pole (the *acid* for example) contains two, three, four, five, &c., times as much oxygen, as the one which attaches itself to the negative pole (the alkali, earth, or metallic oxide)." Berzelius's essay itself appeared in the third volume of the Afhandlingar, in 1810. It was almost immediately translated into German, and published by Gilbert in his Annalen der Physik. But no English translation has ever appeared, the editors of our periodical works being in general unacquainted with the German and other northern languages In 1815 Berzelius applied the atomic theory to the mineral kingdom, and showed with infinite ingenuity that minerals are chemical compounds in definite or atomic proportions, and by far the greater number of them combinations of acids and bases. He applied the theory also to the vegetable kingdom by analyzing several of the vegetable acids, and showing their atomic constitution. But here a difficulty occurs, which in the present state of our knowledge, we are unable to surmount. There are two acids, the *acetic* and *succinic*, that are composed of exactly the same number, and same kind of atoms, and whose atomic weight is 6·25. The constituents of these two acids are

			Atomic weight.
2	atoms	hydrogen	0·25
4	„	carbon	3
3	„	oxygen	3
			6·25

So that they consist of *nine* atoms. Now as these two acids are composed of the same number and the same kind of atoms, one would expect that their properties should be the same; but this is not the case: acetic acid has a strong and aromatic smell, succinic acid has no smell whatever. Acetic acid is so soluble in water that it is difficult to obtain it in crystals, and it cannot be procured in a separate state free from water; for the crystals of acetic acid are composed of one atom of acid and one atom of water united together; but succinic acid is not only easily obtained free from water, but it is not even very soluble in that liquid. The nature of the salts formed by these two acids is quite different; the action of heat upon each is quite different; the specific gravity of each differs. In short all their properties exhibit a striking contrast. Now how are we to account for this? Undoubtedly by the different ways in which the atoms are arranged in each. If the electro-chemical theory of combination be correct, we can only view atoms as combining two by two. A substance then, containing nine atoms, such as acetic acid, must be of a very complex nature. And it is obvious enough that these nine atoms might arrange themselves in a great variety of binary compounds, and the way in which these binary compounds unite may, and doubtless does, produce a considerable effect upon the nature of the compound formed. Thus, if we make use of Mr. Dalton's symbols to represent the atoms of hydrogen, carbon and oxygen, we may suppose the nine atoms constituting acetic and succinic acid to be arranged thus:



Or thus:

Now, undoubtedly these two arrangements would produce a great change in the nature of the compound.

There is something in the vegetable acids quite different from the acids of the inorganic kingdom, and which would lead to the suspicion that the electro-chemical theory will not apply to them as it does to the others. In the acids of carbon, sulphur, phosphorus, selenium, &c., we find one atom of a positive substance united to one, two, or three of a negative substance: we are not surprised, therefore, to find the acid formed negative also. But in acetic and succinic acids we find every atom of oxygen united with two electro-positive atoms: the wonder then is, that the acid should not only retain its electro-negative properties, but that it should possess considerable power as an acid. In benzoic acid, for every atom of oxygen, there are present no fewer than seven electro-positive atoms.

Berzelius has returned to these analytical experiments repeatedly, so that at last he has brought his results very near the truth indeed. It is to his labours chiefly that the great progress which the atomic theory has made is owing.

In the year 1814 there appeared in the Philosophical Transactions a description of a Synoptical Scale of Chemical Equivalents, by Dr. Wollaston. In this paper we have the equivalents or atomic weights of seventy-three different bodies, deduced chiefly from a sagacious comparison of the previous analytical experiments of others, and almost all of them very near the truth. These numbers are laid down upon a sliding rule, by means of a table of logarithms, and over against them the names of the substances. By means of this rule a great many important questions respecting the substances contained on the scale may be solved. Hence the scale is of great advantage to the practical chemist. It gives, by bare inspection, the constituents of all the salts contained on it, the quantity of any other ingredient necessary to decompose any salt, and the weights of the new constituents that will be formed. The contrivance of this scale, therefore, may be considered as an important addition to the atomic theory. It rendered that theory every where familiar to all those who employed it. To it chiefly we owe, I believe, the currency of that theory in Great Britain; and the prevalence of the mode which Dr. Wollaston introduced, namely, of representing the atom of oxygen by unity, or at least by ten, which comes nearly to the same thing.

Perhaps the reader will excuse me if to the preceding historical details I add a few words to make him acquainted with my own attempts to render the atomic theory more accurate by new and careful analyses. I shall not say any thing respecting the experiments which I undertook to determine the specific gravity of the gases; though they were performed with much care, and at a considerable expense, and though I believe the results obtained approached accuracy as nearly as the present state of chemical apparatus enables us to go. In the year 1819 I began a set of experiments to determine the exact composition of the salts containing the different elementary bodies by means of double decomposition, as was done by Wenzel, conceiving that in that way the results would be very near the truth, while the experiments would be more easily made. My mode was to dissolve, for example, a certain weight of muriate of barytes in distilled water, and then to ascertain by repeated trials what weight of sulphate of soda must be added to precipitate the whole of the barytes without leaving any surplus of sulphuric acid in the liquid. To determine this I put into a watch-glass a few drops of the filtered liquor consisting of the mixture of solutions of the two salts: to this I added a drop of solution of sulphate of soda. If the liquid remained clear it was a proof that it contained no sensible quantity of barytes. To another portion of the liquid, also in a watch-glass, I added a drop of muriate of barytes. If there was no precipitate it was a proof that the liquid contained no sensible quantity of sulphuric acid. If there was a precipitate, on the addition of either of these solutions, it showed that there was an excess of one or other of the salts. I then mixed the two salts in another proportion, and proceeded in this way till I had found two quantities which when mixed exhibited no evidence of the residual liquid containing any sulphuric acid or barytes. I considered these two weights of the salts as the equivalent weights of the salt, or as weights proportional to an integrant particle of each salt. I made no attempt to collect the two new formed salts and to weigh them separately.

I published the result of my numerous experiments in 1825, in a work entitled " An Attempt to establish the First Principles of Chemistry by Experiment." The most valuable part of this book is the account of the salts; about three hundred of which I subjected to actual analysis. Of these the worst executed are the phosphates; for with respect to them I was sometimes misled by my method of double decomposition. I was not aware at first, that, in certain cases, the proportion of acid in these salts varies, and the phosphate of soda which I employed gave me a wrong number for the atomic weight of phosphoric acid.

CHAPTER VII.

OF THE PRESENT STATE OF CHEMISTRY.

To finish this history it will be now proper to lay before the reader a kind of map of the present state of chemistry, that he may be able to judge how much of the science has been already explored, and how much still remains untrodden ground.

Leaving out of view light, heat, and electricity, respecting the nature of which only conjectures can be formed, we are at present acquainted with fifty-three simple bodies, which naturally divide themselves into three classes; namely, *supporters*, *acidifiable bases*, and *alkalifiable bases*.

The supporters are oxygen, chlorine, bromine, iodine, and fluorine. They are all in a state of negative electricity: for when compounds containing them are decomposed by the voltaic battery they all attach themselves to the positive pole. They have the property of uniting with every individual belonging to the other two classes. When they combine with the acidifiable bases in certain proportions they constitute *acids;* when with the alkalifiable bases, *alkalies.* In certain proportions they constitute *neutral* bodies, which possess neither the properties of acids nor alkalies.

The acidifiable bases are seventeen in number; namely, hydrogen, azote, carbon, boron, silicon, sulphur, selenium, tellurium, phosphorus, arsenic, antimony, chromium, uranium; molybdenum, tungsten, titanium, columbium. These bodies do not form acids with every supporter, or in every proportion; but they constitute the bases of all the known acids, which form a numerous set of bodies, many of which are still very imperfectly investigated. And indeed there are a good many of them that may be considered as unknown. These acidifiable bases are all electro-positive; but they differ, in this respect, considerably from each other; hydrogen and carbon being two of the most powerful, while titanium and columbium have the least energy. Sulphur and selenium, and probably some other bodies belonging to this class are occasional electro-negative bodies, as well as the supporters. Hence, when united to other acidifiable bases, they produce a new class of acids, analogous to those formed by the supporters. These have got the name of sulphur acids, selenium acids, &c. Sulphur forms acids with arsenic, antimony, molybdenum, and tungsten, and doubtless with several other bases. To distinguish such acids from alkaline bases, I have of late made an alteration in the termination of the old word *sulphuret*, employed to denote the combination of sulphur with a base. Thus *sulphide* of arsenic means an acid formed by the union of sulphur and arsenic; *sulphuret* of copper means an alkaline body formed by the union of sulphur and copper. The term *sulphide* implies an *acid*, the term *sulphuret* a *base*. This mode of naming has become necessary, as without it many of these new salts could not be described in an intelligible manner. The same mode will apply to the acid and alkaline compounds of silenium. Thus a *selenide* is an acid compound, and a *seleniet* an alkaline compound in which selenium acts the part of a supporter or electro-negative body. The same mode of naming might and doubtless will be extended to all the other similar compounds, as soon as it becomes necessary. In order to form a systematic momenclature it will speedily be requisite to new-model all the old names which denote acids and bases; because unless this is done the names will become too numerous to be remembered. At present we denote the alkaline bodies formed by the union of manganese and oxygen by the name of *oxides of manganese*, and the acid compound of oxygen and the same metal by the name of *manganesic acid*. The word *oxide* applies to every compound of a base and oxygen, whether neutral or alkaline; but when the compound has acid qualities this is denoted by adding the syllable *ic* to the name of the base. This mode of naming answered tolerably well as long as the acids and alkalies were all combinations of oxygen with a base; but now that we know the existence of eight or ten classes of acids and alkalies, consisting of as many supporters, or acidifiable bases united to bases, it is needless to remark how very defective it has become. But this is not the place to dwell longer upon such a subject.

The alkalifiable bases are thirty-one in number; namely, potassium, sodium, lithium, barium, strontium, calcium, magnesium, aluminum, glucinum, yttrium, cerium, zirconium, thorinum, iron, manganese, nickel, cobalt, zinc, cadmium, lead, tin, bismuth, copper, mercury, silver, gold, platinum, palladium, rhodium, iridium, osmium. The compounds which these bodies form with oxygen, and the other supporters, constitute all the alkaline bases or the substances capable of neutralizing the acids.

Some of the acidifiable bases, when united to a certain portion of oxygen, constitute, not acids, but *bases* or *alkalies*. Thus the *green oxides of chro-*

mium and uranium are alkalies; while, on the other hand, there is a compound of oxygen and manganese which possesses acid properties. In such cases it is always the compound containing the least oxygen which is an alkali, and that containing the most oxygen that is an acid.

The opinion at present universally adopted by chemists is, that the ultimate particles of bodies consist of *atoms*, incapable of further division; and these atoms are of a size almost infinitely small. It can be demonstrated that the size of an atom of *lead* does not amount to so much as $\frac{1}{888,492,000,000,000}$ of a cubic inch.

But, notwithstanding this extreme minuteness, each of these atoms possesses a peculiar weight and a peculiar bulk, which distinguish it from the atoms of every other body. We cannot determine the absolute weight of any of them, but merely the relative weights; and this is done by ascertaining the relative proportions in which they unite. When two bodies unite in only one proportion, it is reasonable to conclude that the compound consists of 1 atom of the one body, united to 1 atom of the other. Thus oxide of bismuth is a compound of 1 oxygen and 9 bismuth; and, as the bodies unite in no other proportion, we conclude that an atom of bismuth is nine times as heavy as an atom of oxygen. It is in this way that the atomic weights of the simple bodies have been attempted to be determined. The following table exhibits these weights referred to oxygen as unity, and deduced from the best data at present in our possession:

	Atomic weight.		Atomic weight.
Oxygen	1	Calcium	2·5
Fluorine	2·25	Magnesium	1·5
Chlorine	4·5	Aluminum	1·25
Bromine	10	Glucinum	2·25

	Atomic weight.		Atomic weight.
Iodine	15·75	Yttrium	4·25
Hydrogen	0·125	Zirconium	5
Azote	1·75	Thorinum	7·5
Carbon	0·75	Iron	3·5
Boron	1	Manganese	3·5
Silicon	1	Nickel	3·25
Phosphorus	2	Cobalt	3·25
Sulphur	2	Cerium	6·25
Selenium	5	Zinc	4·25
Tellurium	4	Cadmium	7
Arsenic	4·75	Lead	13
Antimony	8	Tin	7·25
Chromium	4	Bismuth	9
Uranium	26	Copper	4
Molybdenum	6	Mercury	12·5
Tungsten	12·5	Silver	13·75
Titanium	3·25	Gold	12·5
Columbium	22·75	Platinum	12
Potassium	5	Palladium	6·75
Sodium	3	Rhodium	6·75
Lithium	0·75	Iridium	12·25
Barium	8·5	Osmium	12·5
Strontium	5·5		

The atomic weights of these bodies, divided by their specific gravity, ought to give us the comparative size of the atoms. The following table, constructed in this way, exhibits the relative bulks of these atoms which belong to bodies whose specific gravity is known:

	Volume.		Volume.
Carbon	1	Platinum	2·6
Nickel	1·75	Palladium	
Cobalt		Zinc	2·75
Manganese	2	Rhodium	3
Copper		Tellurium	
Iron		Chromium	

	Volume.		Volume.
Molybdenum	3·25	Gold	6
Silica	3·5	Silver	
Titanium		Osmium	
Cadmium	3·75	Oxygen	9·33
Arsenic	4	Hydrogen	
Phosphorus		Azote	
Antimony		Chlorine	
Tungsten	4·25	Uranium	13·5
Bismuth		Columbium	14
Mercury		Sodium	
Tin	4·66	Bromine	15·75
Sulphur		Iodine	24
Selenium	5·4	Potassium	27
Lead			

We have no data to enable us to determine the shape of these atoms. The most generally received opinion is, that they are spheres or spheroids; though there are difficulties in the way of admitting such an opinion, in the present state of our knowledge, nearly insurmountable.

The probability is, that all the supporters have the property of uniting with all the bases, in at least three proportions. But by far the greater number of these compounds still remain unknown. The greatest progress has been made in our knowledge of the compounds of oxygen; but even there much remains to be investigated; owing, in a great measure, to the scarcity of several of the bases which prevent chemists from subjecting them to the requisite number of experiments. The compounds of chlorine have also been a good deal investigated; but bromine and iodine have been known for so short a time, that chemists have not yet had leisure to contrive the requisite processes for causing them to unite with bases.

The acids at present known amount to a very great number. The oxygen acids have been most investigated. They consist of two sets: those consisting of oxygen united to a single base, and those in which it is united to two or more bases. The last set are derived from the animal and vegetable kingdoms: it does not seem likely that the electro-chemical theory of Davy applies to them. They must derive their acid qualities from some electric principle not yet adverted to; for, from Davy's experiments, there can be little doubt that they are electro-negative, as well as the other acids. The acid compounds of oxygen and a single base are about thirty-two in number. Their names are

Hyponitrous acid
Nitrous acid ?
Nitric acid
Carbonic acid
Oxalic acid
Boracic acid
Silicic acid
Hypophosphorous acid
Phosphorous acid
Phosphoric acid
Hyposulphurous acid
Subsulphurous acid
Sulphurous acid
Sulphuric acid
Hyposulphuric acid
Selenious acid
Selenic acid
Arsenious acid
Arsenic acid
Antimonious acid
Antimonic acid
Oxide of tellurium
Chromic acid
Uranic acid
Molybdic acid
Tungstic acid
Titanic acid
Columbic acid
Manganesic acid
Chloric acid
Bromic acid
Iodic acid.

The acids from the vegetable and animal kingdoms (not reckoning a considerable number which consist of combinations of sulphuric acid with a vegetable or animal body), amount to about forty-three: so that at present we are acquainted with very nearly eighty acids which contain oxygen as an essential constituent.

The other classes of acids have been but imperfectly investigated. Hydrogen enters into combination and forms powerful acids with all the supporters except oxygen. These have been called hydracids. They are

Muriatic acid, or hydrochloric acid
Hydrobromic acid
Hydriodic acid
Hydrofluoric acid, or fluoric acid
Hydrosulphuric acid
Hydroselenic acid
Hydrotelluric acid

These constitute (such of them as can be procured) some of the most useful and most powerful chemical reagents in use. There is also another compound body, *cyanogen*, similar in its characters to a supporter: it also forms various acids, by uniting to hydrogen, chlorine, oxygen, sulphur, &c. Thus we have

Hydrocyanic acid
Chlorocyanic acid
Cyanic acid
Sulphocyanic acid, &c.

We know, also, fluosilicic acid and fluoboric acids. If to these we add fulminic acid, and the various sulphur acids already investigated, we may state, without risk of any excess, that the number of acids at present known to chemists, and capable of uniting to bases, exceeds a hundred.

The number of alkaline bases is not, perhaps, so great; but it must even at present exceed seventy; and it will certainly be much augmented when chemists turn their attention to the subject. Now every base is capable of uniting with almost every acid,* in all probability in at least three different

---

* Acids and bases of the same class all unite. Thus sulphur acids unite with sulphur bases; oxygen acids with oxygen bases, &c.

proportions: so that the number of *salts* which they are capable of forming cannot be fewer than 21,000. Now scarcely 1000 of these are at present known, or have been investigated with tolerable precision. What a prodigious field of investigation remains to be traversed must be obvious to the most careless reader. In such a number of salts, how many remain unknown that might be applied to useful purposes, either in medicine, or as mordants, or dyes, &c. How much, in all probability, will be added to the resources of mankind by such investigations need not be observed.

The animal and vegetable kingdoms present a still more tempting field of investigation. Animal and vegetable substances may be arranged under three classes, acids, alkalies, and neutrals. The class of acids presents many substances of great utility, either in the arts, or for seasoning food. The alkalies contain almost all the powerful medicines that are drawn from the vegetable kingdom. The neutral bodies are important as articles of food, and are applied, too, to many other purposes of firstrate utility. All these bodies are composed (chiefly, at least) of hydrogen, carbon, oxygen, and azote; substances easily procured abundantly at a cheap rate. Should chemists, in consequence of the knowledge acquired by future investigations, ever arrive at the knowledge of the mode of forming these principles from their elements at a cheap rate, the prodigious change which such a discovery would make upon the state of society must be at once evident. Mankind would be, in some measure, independent of climate and situation; every thing could be produced at pleasure in every part of the earth; and the inhabitants of the warmer regions would no longer be the exclusive possessors of comforts and conveniences to which those in less favoured regions of the earth are strangers. Let the science advance for another century with the same rapidity that it has done during the last fifty years, and it will produce effects upon society of which the present race can form no adequate idea. Even already some of these effects are beginning to develop themselves;—our streets are now illuminated with gas drawn from the bowels of the earth; and the failure of the Greenland fishery during an unfortunate season like the last, no longer fills us with dismay. What a change has been produced in the country by the introduction of steam-boats! and what a still greater improvement is at present in progress, when steam-carriages and railroads are gradually taking the place of horses and common roads. Distances will soon be reduced to one-half of what they are at present; while the diminished force and increased rate of conveyance will contribute essentially to lower the rest of our manufactures, and enable us to enter into a successful competition with other nations.

I must say a few words upon the application of chemistry to physiology before concluding this imperfect sketch of the present state of the science. The only functions of the living body upon which chemistry is calculated to throw light, are the processes of digestion, assimilation, and secretion. The nervous system is regulated by laws seemingly quite unconnected with chemistry and mechanics, and, in the present state of our knowledge, perfectly inscrutable. Even in the processes of digestion, assimilation, and secretion, the nervous influence is important and essential. Hence even of these functions our notions are necessarily very imperfect; but the application of chemistry supplies us with some data at least, which are too important to be altogether neglected.

The food of man consists of solids and liquids, and the quantity of each taken by different individuals is so various, that no general average can be struck. I think that the drink will, in most cases, exceed the solid food in nearly the proportion of 4 to 3; but the solid food itself contains not less than 7-10ths of its weight of water. In reality, then, the quantity of liquid taken into the stomach is to that of solid matter as 10 to 1. The food is introduced into the mouth, comminuted by the teeth, and mixed up with the saliva into a kind of pulp.

The saliva is a liquid expressly secreted for this purpose, and the quantity certainly does not fall short of ten ounces in the twenty-four hours: indeed I believe it exceeds that amount: it is a liquid almost as colourless as water, slightly viscid, and without taste or smell: it contains about $\frac{3}{1000}$ of its weight of a peculiar matter, which is transparent and soluble in water: it has suspended in it about $\frac{1.4}{1000}$ of its weight of mucus; and in solution, about $\frac{2.8}{1000}$ of common salt and soda: the rest is water.

From the mouth the food passes into the stomach, where it is changed to a kind of pap called *chyme*. The nature of the food can readily be distinguished after mastication; but when converted into chyme, it loses its characteristic properties. This conversion is produced by the action of the eighth pair of nerves, which are partly distributed on the stomach; for when they are cut, the process is stopped: but if a current of electricity, by means of a small voltaic battery, be made to pass through the stomach, the process goes on as usual. Hence the process is obviously connected with the action of electricity. A current of electricity, by means of the nerves, seems to pass through the food in the stomach, and to decompose the common salt which is always mixed with the food. The muriatic acid is set at liberty, and dissolves the food; for *chyme* seems to be simply a solution of the food in muriatic acid.

The chyme passes through the pyloric orifice of the stomach into the duodenum, the first of the small intestines, where it is mixed with two liquids, the bile, secreted by the liver, and the pancreatic juice, secreted by the pancreas, and both discharged into the duodenum to assist in the further digestion of the food. The chyme is always acid; but after it has been mixed with the bile, the acidity disappears. The characteristic constituent of the bile is a bitter-tasted substance called *picromel*, which has the property of combining with muriatic acid, and forming with it an insoluble compound. The pancreatic juice also contains a peculiar matter, to which chlorine communicates a red colour. The use of the pancreatic juice is not understood.

During the passage of the chyme through the small intestines it is gradually separated into two substances; the *chyle*, which is absorbed by the lacteals, and the excrementitious matter, which is gradually protruded along the great intestines, and at last evacuated. The chyle, in animals that live on vegetable food, is semitransparent, colourless, and without smell; but in those that use animal food it is white, slightly similar to milk, with a tint of pink. When left exposed to the air it coagulates as blood does. The coagulum is *fibrin*. The liquid portion contains *albumen*, and the usual salts that exist in the blood. Thus the chyle contains two of the constituents of blood; namely, *albumen*, which perhaps may be formed in the stomach, and *fibrin*, which is formed in the small intestines. It still wants the third constituent of blood, namely, the *red* globules.

From the lacteals the chyle passes into the thoracic duct; thence into the left subclavian vein, by which it is conveyed to the heart. From the heart it passes into the lungs, during its circulation through which the *red globules* are supposed to be formed, though of this we have no direct evidence.

The lungs are the organs of *breathing*, a function so necessary to hot-blooded animals, that it cannot be suspended, even for a few minutes, without occasioning death. In general, about twenty inspirations, and as many expirations, are made in a minute. The quantity of air which the lungs of an ordinary sized man can contain, when fully distended, is about 300 cubic inches. But the quantity actually drawn in and thrown out, during ordinary inspirations and expirations, amounts to about sixteen cubic inches each time.

In ordinary cases the volume of air is not sensibly altered by respiration; but it undergoes two remarkable changes. A portion of its oxygen is converted into carbonic acid gas, and the air expired is saturated with humidity at the temperature of 98°. The moisture thus given out amounts to about seven ounces troy, or very little short of half an avoirdupois pound. The quantity of carbonic acid formed varies much in different individuals, and also at different times in the day; being a maximum at twelve o'clock at noon, and a minimum at midnight. Perhaps four of carbonic acid, in every 100 cubic inches of air breathed, may be a tolerable approach to the truth; that is to say, that every six respirations produce four cubic inches of carbonic acid. This would amount to 19,200 cubic inches in twenty-four hours. Now the weight of 19,200 cubic inches of carbonic acid gas is 18·98 troy ounces, which contain rather more than five troy ounces of carbon.

These alterations in the air are doubtless connected with corresponding alterations in the blood, though with respect to the specific nature of these alterations we are ignorant. But there are two purposes which respiration answers, the nature of which we can understand, and which seem to afford a reason why it cannot be interrupted without death. It serves to develop the *animal heat*, which is so essential to the continuance of life; and it gives the blood the property of stimulating the heart; without which it would cease to contract, and put an end to the circulation of the blood. This stimulating property is connected with the scarlet colour which the blood acquires during respiration; for when the scarlet colour disappears the blood ceases to stimulate the heart.

The temperature of the human body in a state of health is about 98° in this country; but in the torrid zone it is a little higher. Now as we are almost always surrounded by a medium colder than 98°, it is obvious that the human body is constantly giving out heat; so that if it did not possess the power of generating heat, it is clear that its temperature would soon sink as low as that of the surrounding atmosphere.

It is now generally understood that common combustion is nothing else than the union of oxygen gas with the burning body. The substances commonly employed as combustibles are composed chiefly of carbon and hydrogen. The heat evolved is proportional to the oxygen gas which unites with these bodies. And it has been ascertained that every 3½ cubic inches of oxygen which combine with carbon or hydrogen occasion the evolution of 1° of heat.

There are reasons for believing that not only carbon but also hydrogen unite with oxygen in the lungs, and that therefore both carbonic acid and water are formed in that organ. And from the late experiments of M. Dupretz it is clear that the heat evolved in a given time, by a hot-blooded animal, is very little short of the heat that would be evolved by the combustion of the same weight of carbon and hydrogen consumed during that time in the lungs. Hence it follows that the heat evolved in the lungs is the consequence of the union of the oxygen of the air with the carbon and hydrogen of the blood, and that the process is perfectly analogous to combustion.

The specific heat of arterial blood is somewhat greater than that of venous blood. Hence the reason why the temperature of the lungs does not become higher by breathing, and why the temperature of the other parts of the body are kept up by the circulation.

The blood seems to be completed in the kidneys. It consists essentially of albumen, fibrin, and the red globules, with a considerable quantity of water, holding in solution certain salts which are found equally in all the animal fluids. It is employed during the circulation in supplying the waste of the system, and in being manufactured into all the different secretions necessary for the various functions of the living body. By these different applications of it we cannot doubt that its nature undergoes very great changes, and that it would soon become unfit for the purposes of the living body were there not an organ expressly destined to withdraw the redundant and useless portions of that liquid, and to restore it to the same state that it was in when it left the lungs. These organs are the *kidneys;* through which all the blood passes, and during its circulation through which the urine is separated from it and withdrawn altogether from the body. These organs are as necessary for the continuance of life as the lungs themselves; accordingly, when they are diseased or destroyed, death very speedily ensues.

The quantity of urine voided daily is very various; though, doubtless, it bears a close relation to that of the drink. It is nearly but not quite equal to the amount of the drink; and is seldom, in persons who enjoy health, less than 2lbs. avoirdupois in twenty-four hours. Urine is one of the most complex substances in the animal kingdom, containing a much greater number of ingredients than are to be found in the blood from which it is secreted.

The water in urine voided daily amounts to about 1·866lbs. The blood contains no acid except a little muriatic. But in urine we find sulphuric, phosphoric, and uric acids, and sometimes oxalic and nitric acids, and perhaps also some others. The quantity of sulphuric acid may be about forty-eight grains daily, containing nineteen grains of sulphur. The phosphoric acid about thirty-three grains, containing about fourteen grains of phosphorus. The uric acid may amount to fourteen grains. These acids are in combination with potash, or soda, or ammonia, and also with a very little lime and magnesia. The common salt evacuated daily in the urine amounts to about sixty-two grains. The urea, a peculiar substance found only in the urine, amounts perhaps to as much as 420 grains.

It would appear from these facts that the kidneys possess the property of converting the sulphur and phosphorus, which are known to exist in the blood, into acids, and likewise of forming other acids and urea.

The quantity of water thrown out of the system by the urine and lungs is scarcely equal to the amount of liquid daily consumed along with the food. But there is another organ which has been ascertained to throw out likewise a considerable quantity of moisture, this organ is the skin; and the process is called *perspiration*. From the experiments of Lavoisier and Seguin it appears that the quantity of moisture given out daily by the skin amounts to 54 89 ounces: this added to the quantity evolved from the lungs and the urine considerably exceeds the weight of liquid taken with the food, and leaves no doubt that water as well as carbonic acid must be formed in the lungs during respiration.

Such is an imperfect sketch of the present state of that department of physiology which is most intimately connected with Chemistry. It is amply sufficient, short as it is, to satisfy the most careless observer how little progress has hitherto been made in these investigations; and what an extensive field remains yet to be traversed by future observers.

THE END.

C. WHITING, BEAUFORT HOUSE, STRAND.