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A Special Bicentennial Issue
Lavoisier and the Chemical Revolution

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Editor William B. Jensen
Assistant Editor James J. Bohning
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The Cover...

This issue shows a romanticized 19th century woodcut of Lavoisier taken from the 1865 edition of M. A. Mangin's *Les Savants Illustres de la France* and supposedly based on the famous painting by David.

DEADLINES

The deadline for the next issue (Spring 1990) is 15 March 1990. All materials should be sent to Dr. William B. Jensen, Department of Chemistry, University of Cincinnati, Cincinnati, OH 45221, (513) 556-9308.

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SYMPOSIUM INTRODUCTION

Jane A. Miller, *University of Missouri - St. Louis*

This special issue of the *Bulletin* is based on a symposium in honor of the "Bicentennial of the Chemical Revolution", sponsored by the Division of the History of Chemistry at the 197th National Meeting of the American Chemical Society, held in Dallas, Texas, on 9-14 April 1989.

The first paper, by Dr. William A. Smeaton of University College - London, presents us with an overview of Lavoisier's legacy and with an evaluation of how well his ideas have withstood the test of time. This is followed by two papers, by Arthur Donovan and J. Edmund White, which provide some general biographical background on Lavoisier, while focusing on his involvement in late 18th century politics. The next four papers, by Robert Siegfried, Frederic Holmes, Truman Schwartz and Ben Chastain, treat various aspects of Lavoisier's scientific work, whereas the final two papers, by Derek Davenport and Kathleen Ireland and by William Jensen, deal with post-revolutionary reactions to Lavoisier's new system of antiphlogistic chemistry. For the benefit of the nonspecialist, the original papers have been supplemented by the addition of a general bibliography and a timetable.

Though, as Ben Chastain reminds us in this issue, the Chemical Revolution was process which extended over many years and not a single datable event, there are several reasons for choosing 1989 to celebrate its bicentennial. 1789 was a year of revolutionary activities in France. The bicentennial of the French Revolution was celebrated on 14 July of this year, on the occasion of the fall of the Bastille. This event signaled the start of the political revolution in France. We, on the other hand, are in many ways celebrating the conclusion of the Chemical Revolution. Its beginnings can be traced back to 1772, which Henry Guerlac has identified "the crucial year" (1). By 1789 most of Lavoisier's experimental research was completed; the *Traité Élémentaire de Chimie*, the summary volume explaining the work, was published; the *Annales de Chimie*, the journal of the new chemistry, was inaugurated; and most French chemists, and an increasing number of foreign chemists, openly espoused the system.

There are among contemporary historians those who question whether Lavoisier's work was as revolutionary as he would have had his contemporaries believe or that his was indeed a revolution in chemistry. Evan Melhado suggests that Lavoisier's contribution was not primarily chemistry, but physics (2). Jerry Gough considers Lavoisier's work as a fulfillment of that of Stahl (3). Robert Siegfried rightly argues that, without the work of Dalton and the acceptance of the atomic theory, we could not have our modern understanding of chemistry (4). However, to those interested in chemistry in 1789, the ideas of Lavoisier, his intuitive insistence on accep-

tance of the conservation of mass, his emphasis on the recognition of the elemental nature of metals and oxygen, and his evidence of a simpler, more experimental chemistry than that practiced by the phlogistonists, provided a system which was both stimulating and useful to the men who gathered to learn and use this new chemistry. The debates and questions led to enthusiastic acceptance of Lavoisier's ideas and, as Arthur Donovan has recognized, Lavoisier succeeded in bringing chemistry into science (5). These young chemists, with great fervor, declared that they were called to cultivate chemistry, new elements were discovered (the catalog almost doubled between 1791 and 1825), analyses were carried out with confidence, a useful nomenclature was invented, and within 20 years, the phlogiston theory had been effectively displaced by Lavoisier's chemistry.

Although one may argue that there was a tradition of analysis, a recognition of the importance of conservation, and that Lavoisier's conclusions were often incorrect, one cannot argue that there was no change in the way chemistry was viewed and in the way it was practiced after Lavoisier.

In conclusion, I would like to extend my thanks to Bill Jensen for his efforts in editing this volume and to Jeff Sturchio and Jim Traynham for assistance in the proofing. I would also like to thank the Monsanto Company, E. I. du Pont de Nemours, Inc., the Petroleum Research Fund, H. B. Alsobrook, Jr., and Mallinckrodt, Inc. for their generous support of the original symposium.

References and Notes

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3. J. B. Gough, "Lavoisier and the Fulfillment of the Stahlian Revolution", *Osiris*, **1988**, 4, 15-33.
4. R. Siegfried, "The Chemical Revolution and the History of Chemistry", *Osiris*, **1988**, 4, 34-50.
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La chimie est une science française; elle fut constituée par Lavoisier, d'immortelle mémoire.

A. Wurtz, *Histoire des Doctrines Chimiques*

THE LEGACY OF LAVOISIER

William A. Smeaton, University College, London

When invited to talk about “the consequences of the Chemical Revolution”, I immediately thought of Sir Christopher Wren’s epitaph in St. Paul’s Cathedral, which he designed and lived to see completed: *Si monumentum requiris, circumspice* (if you would see his monument, look around). The consequences of Lavoisier’s work, it seemed to me, are also around us in the whole structure of modern chemistry. However, on reflection I realized that the analogy is false. Wren’s masterpiece still stands as he left it, having withstood the ravages of nearly three centuries of London weather and the onslaught of the Nazi bombers which ruined its surroundings, but chemistry has changed dramatically since Lavoisier’s time. Some parts of Lavoisier’s chemistry were altered or even abandoned within 20 years of his death; the caloric theory survived until about 1850; but his greatest achievements, the proof of the compositions of air and water and the definition and listing of the chemical elements still survive almost unaltered. This is similar to the fate of many fortunes made by men and women, bequeathed to their immediate descendants and handed down in a family, so I chose the title “The Legacy of Lavoisier” (1).

Before examining the fate of this legacy, I wish to consider briefly the question of the true ownership of the chemical system advanced by Lavoisier in his *Elements of Chemistry* (*Traité Élémentaire de Chimie*, Paris, 1789). In a memoir written in 1792 or early in 1793, Lavoisier stated categorically that the new chemical theory “is mine”, and not, as he had heard it called, “the theory of the French chemists” (2). The words to which he objected so strongly seem to have first appeared in print in J. F. Bonjour’s preface to his translation of T. O.

Bergman’s *Traité des Affinités Chimiques* (Paris, 1788). Bonjour may have used the phrase because the names of four authors - L. B. Guyton de Morveau, A. L. Lavoisier, C. L. Berthollet and A. F. de Fourcroy - appeared on the title-page of the recently published *Méthode de Nomenclature Chimique* (Paris, 1787), in which every substance was given a name that reflected its composition according to the antiphlogistic theory

(3). The new theory was also publicized in Madame Lavoisier’s translation of Richard Kirwan’s *Essay on Phlogiston* (London, 1787), which appeared in Paris in 1788 as *Essai sur le Phlogistique* with an antiphlogistic commentary by Lavoisier or one of his colleagues at the end of each chapter. Like the *Nomenclature Chimique*, the Kirwan translation has several names on the title-page and this may have reinforced the opinion that the theory was that “of the French chemists”. And when *Annales de Chimie*, the journal published by Lavoisier and his colleagues, first appeared in April 1789 its title-page carried the names of no fewer than eight editors.

Lavoisier probably read the words “the theory of the French chemists” in Bonjour’s preface, and he may have seen a similar expression, “the new doctrine of the French chemists”, in the preface to the 4th edition of Fourcroy’s *Éléments d’Histoire Naturelle et de Chimie* (Paris, 1791), but later in the book Fourcroy gave full credit to Lavoisier for discovering the compositions of air and water. Fourcroy did, how-

ever, refer in 1792 to “the theory of the French chemists” in volume 2 of his *Encyclopédie Méthodique, Chimie* (4) and this may have been read by Lavoisier. However, when indignantly asserting that the theory “is mine”, Lavoisier seems to have been over-reacting to an expression used in print only once by Bonjour and rarely by Fourcroy, though of course the offending words may have been spoken in his presence. The first public tribute to Lavoisier was made nearly 18 months after his death, on 22 October 1795, when E. J. B. Bouillon-Lagrange



A curious 19th century woodcut of Lavoisier from the 1874 edition of L. Figuiet's *Vies des Savants Illustres*. Note the erroneous implication that Lavoisier was beheaded with an axe.

described him as having "created a new chemistry" (5), and full credit for the Chemical Revolution was given to Lavoisier by Fourcroy in the long historical account of chemistry that filled most of volume 3 of his *Encyclopédie Méthodique, Chimie*, published in 1797. I think we can assume that the antiphlogistic theory was regarded as Lavoisier's legacy to chemistry, at least by his French contemporaries.

From his own experiments and his reinterpretation of the works of other chemists, Lavoisier was able by 1779 to assert that air is a mixture of oxygen and nitrogen, and by 1784 he was convinced that water is a compound of hydrogen and oxygen. His theory of the composition of water has remained unchallenged, and Lavoisier can hardly be blamed for overlooking Cavendish's observation that a very small proportion of nitrogen seemed to be different from the rest and did not combine with oxygen under the influence of an electric spark, for no one else followed this up until 1894, when Ramsay and Rayleigh detected the presence in the atmosphere of argon, soon to be followed by its companions. Even so, the discovery of the noble gases did not affect Lavoisier's proof that only one constituent of air, oxygen, supported life and combined with combustibles and metals and that phlogiston could be excluded from chemistry.

The other durable part of Lavoisier's chemical legacy was his definition of a chemical element and his list of those substances which possessed the necessary attributes, together with his recognition that the list would have to be modified as chemistry advanced. In his *Sceptical Chymist* (1661), Robert Boyle severely criticized the Aristotelian theory that matter was composed of fire, air, water and earth, but his criticism was destructive and, though he defined elements in language which can be paraphrased as "the simplest products of chemical analysis" he did not name even one substance that satisfied this definition. Several 18th century chemists, including Guyton de Morveau, referred to "chemical elements", which were the simplest substances taking part in chemical reactions, but still believed them to be ultimately composed of Aristotle's four "natural elements" (6). Lavoisier not only proved that air and water are not elements, but showed that there are several distinct earths. However, he retained the elementary fire in his own "caloric", to which I shall return.

Lavoisier's definition of an element was similar to Boyle's, but he drew up a list and, recognizing that it might have to be modified, he preferred to refer to elements as "simple substances not yet decomposed". So this very important part of his legacy to chemistry included the implicit statement that chemistry was a progressing subject and that as new techniques were developed some at least of the "simple substances" might lose that status. This is best shown by his inclusion of the five earths - lime, magnesia, baryta, alumina and silica - even though he demonstrated their resemblances to metallic oxides and surmised that they might indeed be decomposed at some future date. He would not, therefore, have been discontent if he had

TABLEAU DES SUBSTANCES SIMPLES.

	Noms nouveaux.	Noms anciens correspondans.
	Lumière.....	Lumière. Chaleur. Principe de la chaleur.
	Calorique.....	Fluide igné. Feu.
Substances simples qui appartiennent aux trois règnes & qu'on peut regarder comme les éléments des corps.	Oxygène.....	Matière du feu & de la chaleur. Air déphlogistiqué. Air empiréal. Air vital.
	Azote.....	Gaz phlogistiqué. Mofete. Bafe de la mofete.
	Hydrogène.....	Gaz inflammable. Bafe du gaz inflammable.
		Soufre..... Phosphore..... Carbone.....
Substances simples non métalliques oxidables & acidifiables.	Radical muriatique.	Inconnu.
	Radical fluorique.	Inconnu.
	Radical boracique.	Inconnu.
	Antimoine.....	Antimoine.
	Argent.....	Argent.
	Arfenic.....	Arfenic.
	Bismuth.....	Bismuth.
	Cobolt.....	Cobolt.
	Cuivre.....	Cuivre.
	Etain.....	Etain.
Substances simples métalliques oxidables & acidifiables.	Fer.....	Fer.
	Manganèse.....	Manganèse.
	Mercuré.....	Mercuré.
	Molybdène.....	Molybdène.
	Nickel.....	Nickel.
	Or.....	Or.
	Platine.....	Platine.
	Plomb.....	Plomb.
	Tungstène..... Zinc.....	
	Chaux.....	Terre calcaire, chaux.
	Magnésie.....	Magnésie, bafe du fel d'Epfein.
Substances simples solifiables terreuses.	Baryte.....	Baryte, terre pesante.
	Alumine.....	Argile, terre de l'alun, bafe de l'alun.
	Silice.....	Terre siliceuse, terre vitrifiable.

Lavoisier's 1789 table of simple substances.

lived to see the proof by Davy and Berzelius in 1808, using the new electrolytic method of decomposition, that lime, baryta and magnesia were indeed metallic oxides.

It may at first seem that Lavoisier was being inconsistent by including in his list the earths, which he suspected to be oxides, but not the alkalis - potash and soda - which he also thought might be compounds. However, his evidence for the compound nature of the alkalis was stronger, though it later proved to be erroneous. Potash was found in the ashes of ordinary vegetable matter and soda in the ashes of plants growing near the sea. Lavoisier believed that they did not exist in living plants but were formed during combustion. Berthollet had shown that the third alkali, ammonia, was a compound of nitrogen and hydrogen, so Lavoisier suspected that nitrogen was a constituent of all three (7). He would probably have been surprised to learn of Davy's discovery in 1807 that potash and soda were in fact metallic oxides.

Lavoisier made it clear that he believed matter to be composed of particles, an opinion that he shared with nearly all 18th century chemists. However, unlike many of his predecessors and contemporaries, he refused to speculate about their nature (8). For many years Robert Siegfried has been arguing, convincingly in my opinion, that the Chemical Revolution was

not complete until after 1803, when John Dalton first showed that Lavoisier's elements could be considered as composed of indivisible atoms, each element having an atom of characteristic weight (9). This important part of Lavoisier's legacy was therefore augmented nearly ten years after his death. The debate about the indivisibility or otherwise of Dalton's atoms started almost immediately, with Davy and William Prout, and continued until the discovery of the electron and other subatomic particles, but for many chemical purposes, Lavoisier's elements and Dalton's atoms are still adequate.

Twenty-three of Lavoisier's original 33 elements still survive in the modern list. Among those that have disappeared are the first two, caloric (heat) and light. An essential part of Lavoisier's system was his theory that heat was a weightless fluid present in all matter. Addition of heat to a solid, liquid or gas normally caused expansion, with a rise in temperature that could be measured with a thermometer, but when a change of state occurred, from solid to liquid or liquid to gas, heat entered into combination with the solid or liquid without a temperature change. Lavoisier thus accounted for thermal expansion and for the latent heats of fusion and evaporation that had been discovered by Joseph Black in Scotland and J. C. Wilcke in Sweden.

Many chemical phenomena could also be explained by the caloric theory. For example, when phosphorus, a solid, burnt in oxygen to form another solid, the oxide, much heat was evolved; this was the caloric originally contained in the oxygen gas. However, less heat was evolved when carbon burnt in oxygen, for the product of combustion was itself a gas which retained some of the caloric from the original oxygen. Some historians have argued that Lavoisier's caloric was, in its chemical manifestations, simply phlogiston transferred from the combustible to the oxygen, but this is not the case, for there was one great difference between the two: caloric could be measured by the use of a suitable calorimeter, but no one ever succeeded in measuring phlogiston liberated during combustion. The weightlessness of caloric was an acceptable concept, for 18th century scientists were familiar with other weightless fluids, notably electricity, but there were a few difficulties. Lavoisier's followers could not explain satisfactorily the exothermic reaction between sulphur and iron filings, for example, since these were both solids which should have contained very little caloric. But there were not many such cases, and chemists generally applied the caloric theory with success until the concept of free energy was developed after about 1850. This was an important part of Lavoisier's legacy.

Although he listed heat and light as separate elements, Lavoisier conceded that they might be modifications of a single element, and his immediate successors found it difficult to reach a decision. Fourcroy, for example, considered them to be distinct in 1795, in his *Philosophie Chimique* (2nd ed.), but by 1800, in his *Système des Connaissances Chimiques*, he presented and obviously favoured the opinion of Gaspard

Monge that heat and light were modifications of the same element, less dense and moving slowly in heat, more dense and moving rapidly in light. However, the problem ceased to interest chemists a few years later when T. Young and A. J. Fresnel found proof of the wave theory of light and it ceased to be considered as a material substance. Even so, "light" appeared in a list of reducing agents on one of the tables displayed in the chemistry laboratory of the British High School that I attended 50 years ago!

Not everyone agreed that heat was a substance. Since the time of Francis Bacon in the early 17th century, some scientists had preferred his theory that heat was simply a manifestation of the motion of particles of ordinary matter, and some experimental support for this was provided by Benjamin Thompson, Count Rumford, in 1798. While in charge of the arsenal at Munich, he used the cannon-making equipment to drill a metal gun-barrel with a deliberately blunted drill-bit and found that he could produce an apparently indefinite amount of heat by friction without in any way altering the chemical or physical properties of the metal. This led him to conclude that heat is motion, not a material substance, but he did not carry out the experiments necessary to establish a quantitative relationship between the heat produced and the mechanical work done in rotating the drill. Had he remained in Munich, he might have continued this research, but he abandoned it when he moved to London and later to Paris, where he married Lavoisier's widow in 1805. The marriage proved to be unhappy and they parted after four years, but the cause of this was not his failure to accept Lavoisier's caloric theory (10).

The theory continued to be useful to physicists as well as chemists, and early in the new century both Dalton and Gay-Lussac used it in their work on the thermal properties of gases. Sadi Carnot was another man who utilized the caloric theory with success. In his study of the efficiency of heat engines, published in 1826, which was of great importance in the development of thermodynamics, he regarded the motive power produced by heat falling from a high to a low temperature as analogous to that produced by water falling from a high to a low level, and treated heat as a fluid in his calculations. His notebooks show that before his early death in 1832 he was leaning towards a kinetic theory of heat, but this work was unpublished and the caloric theory remained generally acceptable until the quantitative research of J. P. Joule and others in the 1840's enabled the mechanical equivalent of heat to be measured and led to a satisfactory kinetic theory of heat and the law of the conservation of energy. This important part of Lavoisier's legacy therefore served science well for half a century (11).

Lavoisier's theory of the nature of acids was less durable. He believed that they owed their acidity to the presence of oxygen, and this was why he chose the name, from *oxus*, the Greek word for acid. He had a sound reason for this belief, for the common inorganic acids (or their anhydrides, as later

chemists called them) were formed when sulphur, carbon, phosphorus and nitrogen combined with oxygen, which was also known to be present in most organic acids, combined with a "radical" composed of carbon and hydrogen. A few metals (arsenic, molybdenum and tungsten) formed oxides with acidic properties and, as Siegfried has recently pointed out, Lavoisier believed that all metals would yield acids if combined with sufficient oxygen and in his table of elements he described metals as "oxidable and acidifiable" (12).

Even as early as 1789 the oxygen theory of acids had run into some difficulties. No oxygen had been found in muriatic (HCl), boracic (H_3BO_3) or fluoric (HF) acids, so Lavoisier assumed that they were compounds of oxygen with radicals that were yet to be discovered and included these radicals in his list of elements. In view of the large number of acids that do contain oxygen, I think he was justified in doing this, in the same way that Mendeleev was later justified in leaving blank spaces in his periodic table for undiscovered elements. However, Lavoisier's treatment of prussic acid (HCN) was unsatisfactory, for Berthollet had shown in 1787 that it consisted only of carbon, nitrogen and hydrogen. In his *Traité* Lavoisier failed to mention the hydrogen, stating only that Berthollet had found carbon and nitrogen, and, while admitting that the substance combined with alkalis, earths and metals like other acids, he added, with no details, that it possessed only some properties of acids and should not perhaps be included in that class (13). Clearly he was worried about this apparent anomaly in his acid theory, but it was not regarded as a serious problem by Fourcroy, who later pointed out that Berthollet had not performed a complete analysis and thus there was still a possibility that oxygen might be found (14).

Thomas Thomson, on the other hand, considered that Berthollet's analysis rendered the presence of oxygen "somewhat unlikely" and also pointed out that sulphuretted hydrogen (H_2S) possessed all the properties of an acid but contained no oxygen, and concluded by writing: "all that can be meant, then, when it is said that oxygen is the acidifying principle, is merely that it exists as a component part in the greater number of acids, and that many acids are formed by combustion, or by some equivalent process" (15). This was written in 1804, only ten years after Lavoisier's death. Already, an important part of his legacy was no longer intact.

When muriatic acid reacted with manganese dioxide it yielded a gas which dissolved in water to form an acid solution and, on exposure to light, the solution gave off oxygen. This was therefore known as oxymuriatic acid and was considered to be composed of the unknown muriatic radical combined with more oxygen than ordinary muriatic acid (16). However, although the solution of oxymuriatic acid apparently contained oxygen, the gas showed no signs of its presence. In 1808 two of the younger French chemists, J. L. Gay-Lussac and L. J. Thenard, tried without success to decompose oxymuriatic acid gas with heated carbon but, while admitting that this might

show that the gas was in fact an element, they preferred to continue to believe that it contained oxygen. They were probably affected by loyalty to the French founder of the oxygen theory of acids, but Humphry Davy had no such feelings and in an extensive series of experiments in 1810 he failed to remove oxygen by using several reagents and became convinced that oxymuriatic acid gas was an element, which he named chlorine, and, further, that muriatic acid itself was a compound of chlorine and hydrogen. Gay-Lussac accepted this theory in 1814 and, as Partington puts it, "Lavoisier's oxygen theory of acids had now gone the way of phlogiston" (17). Curiously, no one has ever suggested that the name "oxygen" should be changed, so that important part of Lavoisier's legacy is still with us even though, contrary to the laws of nomenclature which he helped to formulate in 1787, the name of one of the most common and important elements now bears no relation to its properties.

Lavoisier's original name for the gas was "eminently respirable air" which he changed to "vital air" on the recommendation of Condorcet, the Secretary of the Academy of Sciences. This name referred to its ability to support life, and Lavoisier connected this to his theory of combustion, believing that, as a result of respiration, oxygen was absorbed in the blood where it reacted with carbon and hydrogen, derived from the digestion of food, and liberated the caloric which kept the body at a higher temperature than its surroundings. It was many years before physiologists elucidated the exact mechanism of this process, but Lavoisier was correct in principle and his theory of animal heat was an important part of his legacy.

Fourcroy had qualified as a physician and was always interested in the applications of chemistry to medicine. In 1789



Claude Louis Berthollet



Antoine Francois Fourcroy

he studied the effect of administering oxygen to sufferers from tuberculosis and found that they became feverish, an effect which, applying Lavoisier's theory, he attributed to the increased liberation of caloric in the body. On the other hand, languorous patients, or those with a lower temperature than normal, benefited from breathing more oxygen. It was commonly thought that the composition of the atmosphere varied from place to place, and during his travels in the Alps, H. B. de Saussure had carried out eudiometric experiments at various altitudes, but, probably because he used a faulty method, he concluded that air was best for respiration between about 800 and 1200 feet above sea level (18). Fourcroy may have been aware of Saussure's work, for he recommended that feverish patients should be kept at ground level but those suffering from languor would benefit by being sent to a high altitude (19). This was a rational suggestion until eudiometric methods were improved and, more importantly, until Gay-Lussac in 1804 made two balloon ascents, accompanied on one of them by J. B. Biot and, among other scientific experiments, took a sample of air at 6,000 metres which was found to have the same composition as that in Paris.

Gay-Lussac and Biot were members of the Société d'Arcueil, founded by Berthollet and Laplace, the great mathematician, in 1807, though the informal meetings that led to its formation began several years earlier. They owned neighboring houses in the village of Arcueil, near Paris, and it was there that about 15 members met regularly to perform experiments and discuss their scientific ideas in a less formal atmosphere than that of the Institut, to which most of them had been elected. Berthollet and Laplace were among the earliest converts to Lavoisier's new chemistry who had met regularly in his laboratory, and Maurice Crosland has argued persuasively that at Arcueil they

were continuing Lavoisier's research program. He has called the Arcueil group "the heirs of Lavoisier" and, while I do not disagree with him, I should like to point out that they were not Lavoisier's only heirs (20). Part of Lavoisier's legacy was his pioneering work on organic analysis, and this was continued by Fourcroy and his former student, L. N. Vauquelin (1763-1829), who worked at the Muséum d'Histoire Naturelle and were not associated with the Arcueil group.

When Lavoisier and his colleagues were replying in 1788 to Kirwan's *Essay on Phlogiston*, it was Fourcroy who was chosen to comment on the chapter on the acid of sugar (oxalic acid), for he had specialized most on the chemistry of vegetable and animal substances. Lavoisier had earlier shown that organic matter consisted mainly of carbon, hydrogen, oxygen and nitrogen, and he attempted to determine their proportions in a variety of substances, such as sugar, alcohol, olive oil and various gums. He used several methods. Fermentation (in the case of sugar) yielded carbonic acid (carbon dioxide), which could be collected and measured and thus provided a measure of the carbon content of the sugar. In other cases he obtained the carbon dioxide from his sample by combustion in oxygen or heating with certain metallic oxides, particularly mercuric oxide, which readily lost its oxygen. Much of this work was done in 1787 and 1788, but remained unpublished, and his laboratory notes show that he obtained very inconsistent results because of his doubts about the purity of the mercuric oxide and his uncertainty about the percentage of carbon in carbonic acid (21).

Another reason for Lavoisier's difficulties with organic analysis may have been that he was working with impure



Louis Nicolas Vauquelin

natural substances of varying composition. He was skilled in most branches of practical chemistry, but had little experience of the techniques required for extracting pure compounds from crude vegetable and animal matter. This was a field in which Fourcroy excelled, even before 1790 when he began to enjoy the collaboration of Vauquelin. They developed a procedure for extracting pure compounds by systematic extraction with water, alcohol, ether and dilute acids and alkalis and worked on many vegetable and animal substances until Fourcroy's death in 1809 (22).

Before joining Fourcroy at the Muséum d'Histoire Naturelle, Vauquelin had taught at the Collège de France and the École Polytechnique (where he was also Fourcroy's colleague) and in a private laboratory which he owned jointly with Fourcroy and in which they manufactured fine chemicals. Vauquelin employed several young men as assistants and trained them well in practical chemistry. Two later achieved great distinction, namely L. J. Thenard (1777 - 1857) and M. E. Chevreul (1786 - 1889), who began to work with Vauquelin about 1797 and 1803 respectively. They both became skilled at the isolation of pure organic compounds from natural substances.

After his early experience with Vauquelin, Thenard was accepted into the Arcueil group and, with Gay-Lussac, he devised a method for the elementary analysis of organic compounds by combustion with potassium chlorate. Gay-Lussac later improved it by using cupric oxide as the oxidizing agent. Thenard and Gay-Lussac both did research on other branches of chemistry, but Chevreul, who remained with Vauquelin at the Muséum after Fourcroy's death and did not belong to the Arcueil group, devoted himself to organic chemistry. From about 1811 to 1820 he carried out many fundamental experiments on animal fats, proving that they were compounds of glycerol with various organic acids, which he isolated and purified using the techniques learned from Vauquelin, and he characterized them by their melting points, a method introduced by Fourcroy as early as 1786, but rarely used since then. He also showed that, during the saponification of a fat with alkali, it was broken into its components, the glycerol being liberated and the alkali combining with the acid to form a soap. At every stage he determined the carbon, hydrogen and oxygen in his products, using a modification of Gay-Lussac's copper oxide method, and his quantitative analyses supported his qualitative conclusions (23).

Before his death, Lavoisier claimed that he had the first ideas concerning the composition of plant and animal substances (24). I think that Fourcroy, Vauquelin and Chevreul deserve much credit for preserving this part of his legacy.

Lavoisier did not use potassium chlorate, which had been discovered by Berthollet, for organic analysis, but his laboratory notebooks show that on 22 and 24 October 1788 he examined its reaction with carbon. This research was done in connection with his work for the Régie des Poudres, the royal gunpowder manufacturing monopoly of which he was one of



Michel Eugène Chevreul

the directors. A few days later, on 27 October 1788, Lavoisier and his wife, with Berthollet and some other officials and visitors, were present at the gunpowder factory at Essonnes, about 40 kilometres south of Paris, when an attempt was made to manufacture a batch of powder with potassium chlorate instead of the usual nitrate. An explosion killed two people, and the use of chlorate was abandoned for military gunpowder, though it was later used in pyrotechnics (25).

His scientific and administrative skills made Lavoisier a valuable member of the Régie des Poudres, but he was not involved in the day-to-day operation of the factory at Essonnes. However, in 1787 he was able to secure an appointment there for Eleuthère Irénée du Pont (1771-1834), the younger son of his friend Pierre Samuel du Pont (1739-1817), the economist. The young man worked there until 1791, and learned enough to set up his own gunpowder mill at Wilmington, Delaware, after the du Pont family emigrated to America in 1799. He originally called it the Eleutherian Mills, incorporating his own first name, but on 12 June 1803 he wrote to his father that he had "definitely decided on Lavoisier Mill which is suitable and which shows my gratitude to one whose goodness to me was the first cause of my enterprise". His brother Victor, writing from New York on 13 July 1803, addressed his letter to "E. I. du Pont de Nemours, Esquire, Lavoisier's Mills, near Wilmington, Delaware", but, for an unknown reason the name was not retained, and on 2 May 1804 Victor suggested "E. I. du Pont de Nemours Gun Powder Manufactory" (26).

E. I. du Pont de Nemours had no idea that his powder mill would grow into one of the world's greatest industrial companies. It is, perhaps, a pity that he did not keep his patron's name in its title, but even so it remains as part of Lavoisier's legacy to chemistry and industry.

References and Notes

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Dr. William A. Smeaton is member of the Department of History and Philosophy of Science at University College London, London WC1E 6BT, England. Best known for his biographical study of A. F. Fourcroy, he is interested in 18th century French chemistry and is currently studying the life and work of L. B. Guyton de Morveau, one of the first converts to Lavoisier's system.

LAVOISIER'S POLITICS

Arthur L. Donovan, U.S. Merchant Marine Academy

Antoine Lavoisier, one of history's greatest scientists, was also a prominent public administrator during the final decades of the Old Regime. Several of his involvements in public affairs are fairly well known. For years he served on commissions and committees of the Academy of Sciences, as a Director of the National Gunpowder Administration, and as a tax farmer

and financial advisor to the crown. His contributions to these activities all deserve further study and we must hope that in time administrative historians will undertake them. My goal in this brief paper is biographical, however. My purpose is to provide a sketch of what we might call Lavoisier's ethic of public service. How did he view his responsibilities as a citizen, a term that was widely used before the outbreak of political revolution, and to what extent did his personal ethics of public service reflect the political culture of his time? My subject, then, is Lavoisier the public administrator, not 18th century chemistry as such, nor the nature of his contributions to the revolution in which modern chemistry was forged.

Antoine Lavoisier was born into a well-to-do professional family. His father, while a young man, had come to Paris to study law, and in 1741 he had the good fortune to inherit an uncle's estate, which included a house and a position as barrister at the Parlement of Paris. A year later he married the well-dowered daughter of an attorney at the Paris law courts and she bore them two children before her own untimely death in 1748. Antoine, born in 1743, and his younger sister, who died while in her teens, were then reared by their father, at all times a devoted and attentive parent, and by a maternal aunt who lavished adoring care on the children.

Lavoisier's sense of his place in the world and of the opportunities open to him naturally reflected the circumstances of his youth. While many of the most notable writers and philosophers of the Enlightenment were bright provincial lads who had come to Paris to make their marks, Lavoisier was born and educated in the capital. His family, while well-off, had neither the landed wealth nor the taste for luxury displayed by the leaders of aristocratic society. And Antoine, as the scion of a prosperous but relatively new family in Paris, acquired and was guided by a powerful sense of responsibility, purpose and ambition. His vision of himself and his duties was therefore shaped far more by what he aspired to achieve than by the advantages he enjoyed. More specifically, the Lavoisiers belonged to the social group that historians now call the bourgeoisie of the Old Regime, a group made up primarily of lawyers, judicial officials, administrators, and technicians serving in royal or provincial administration. This service bourgeoisie was everywhere integrated into established society and, along with the service elite of the nobility of the sword, they formed the dynamic core of French public life (1). Service to the state, and the expectation of personal advancement through such service, were as much a part of Lavoisier's heritage as were his family name and his religion. His later commitment to science would have been unthinkable had it not provided an additional way of fulfilling this compelling vision of his responsibilities to family and nation.

What strategies did Lavoisier follow in seeking to fulfill his responsibilities and satisfy his ambitions? Today we think of him primarily as a chemist, but if we are to render his involvements in public affairs intelligible, we need to take a more



A late 19th century pen drawing of Lavoisier from the 1904 edition of A. Rebière's *La Vie et les Travaux des Savants Modernes*. Supposedly based on a painting by David.

comprehensive view of his interests. I therefore suggest that we not cast Lavoisier as a scientist, a term that in any case was not coined until the early 19th century, but rather as an 18th century philosophe who was especially interested in and adept at scientific investigation. He was, in other words, one of the independent-minded, politically-engaged men of letters committed to the program of cultural reform known then and now as the Enlightenment. Indeed, I would argue that Lavoisier should be thought of as one of France's foremost philosophes in the last generation to reach maturity before the collapse of the Old Regime.

The Enlightenment itself should be thought of as a program of reform that adapted to changing circumstances as it developed through time. When Lavoisier set out to make a place for himself in Paris, the Enlightenment had already achieved a high level of visibility and acceptance. Voltaire's *Philosophical Letters*, written from exile in England and designed to demonstrate the advantages of English liberalism and Newtonianism, was published ten years before Lavoisier's birth, and the first volume of Diderot's and D'Alembert's great *Encyclopédie* was issued when he was eight years old. Thus by the time Lavoisier arrived at center stage, the high Enlightenment's program of naturalism in science, literature, art and manners was a well-developed philosophical movement, not a radical perspective awaiting articulation. If Lavoisier was to succeed in making a name for himself, he had to do more than announce that he preferred scientific naturalism to religious authority and the unadorned beauty of nature to the artificiality of courtly society. Such attitudes had become

commonplace by the 1760's and were beginning to look slightly archaic in their abstractness. To carry the program of reform forward, the younger men had to develop strategies capable of making the principles of the Enlightenment part of the political, social and cultural fabric of the nation. Lavoisier and the philosophes of his generation faced a challenge of engagement, a challenge that called for doing more than opposing authority with assertions of universal rights. Their task was to put advanced ideas into action.

Lavoisier was enrolled as a day student at the Collège Mazarin in 1754, shortly after his 11th birthday. The imposing buildings of the Collège stand opposite the Louvre, where the Academy of Sciences held its meetings, and today house the constituent societies of the Institut of France. In the 18th century the Collège had many prominent scientists on its faculty and offered a first-class education in the sciences and the humanities. The normal course of study lasted nine years. Lavoisier was a highly successful student and after seven years he transferred to the faculty of law. Two years later, at age 20, he received his law degree and the following year he was admitted to the bar at the Parlement of Paris. Yet long before qualifying to follow his father's career, he had decided to devote a large part of his energy and time to science. While reading law he had continued to work with the Abbé de Lacaille, who had introduced him to mathematics and astronomy as a student. He also attended the private lectures of such prominent teachers as the chemist G. F. Rouelle and the experimental physicist the Abbé de Nollet, and he geologized on field trips with the Academician and family friend J. E. Guettard. Although Lavoisier studied chemistry in the early 1760's, nearly another decade passed before he formulated the program of research that led to the oxygen theory of combustion. Thus while in the 1760's Lavoisier made a serious commitment to physical science, he had not, so far as we know, set himself the task of effecting a revolution in chemistry.

From 1764 to 1768 Lavoisier campaigned vigorously for a place in the national Academy of Sciences. He sought to advance his candidacy by presenting three memoirs: an essay on how best to illuminate city streets, a two-part paper on the mineral gypsum, and a three-part paper on mineral waters. These papers reveal a good deal about Lavoisier's views on science and its practical applications, yet they hardly constitute first steps toward a revolution in chemistry. They served their intended purpose admirably, however, and in 1768, shortly before his 25th birthday, Lavoisier was installed as an assistant chemist in the Academy of Sciences.

Let us take a look at the events that occupied Lavoisier during the next few years before considering the significance of his election to the Academy of Sciences. Lavoisier's mother had left him a considerable legacy and, upon reaching his majority, he was eager to invest in a venture capable of providing the additional income he needed to pursue a career in science. He therefore followed the advice of a family friend

and in 1768 purchased a share in the Tax Farm, a private corporation that collected certain taxes for the government. This was not a passive investment, however, and during much of 1769 and 1770 Lavoisier was obliged to travel widely outside Paris while inspecting the collection of duties and the manufacture and sale of tobacco at locations under his jurisdiction. His immediate supervisor in these matters was the senior tax farmer Jacques Paulze. In 1771 Lavoisier and Paulze's daughter were married. This justly famous union occurred a year before Lavoisier began his epochal experiments in pneumatic chemistry. Following the wedding, Lavoisier, who was then 28, moved out of the house in which he had been reared and, together with his 13-year-old bride, established a home of his own. Twenty years later he was to recall that at that time "I was young; I had just entered on my career in science; [and] I was hungry for glory" (2). Financially independent, happily married, and intellectually vigorous, Lavoisier was searching for opportunities to employ his talents on the stages provided by the Academy of Sciences and in the King's service. He was eager to put advanced ideas into action and he was exceptionally well prepared to do so.

In 1771 Lavoisier began drafting an eulogy of Jean Baptiste Colbert, the great 17th century minister of finance. He planned to submit the essay to a prize competition announced by the Academie Française, but he never completed it. A manuscript copy of his draft survived, however, and was published in his collected works. It is a most revealing essay, for like many of Lavoisier's drafts, it is more forthright than the works he sent to press. One passage from the final section particularly deserves attention. Lavoisier is assessing the role of royal academies in the cultural life of the nation. It may be, of course, that the view he expresses is nothing more than an attempt by a junior member of the Academy of Sciences to flatter the immortals of the Academie Française, but I am inclined to think that it accurately represents Lavoisier's true feelings about the function of the academies. He begins by crediting Colbert with founding the Academy of Sciences and, more generally, for linking the patronage of high culture to the glorification of the King. The academies, he continues, function like little republics that perpetuate their power from age to age (3):

These institutions, the greatest gift that mankind has ever received, are above all monuments erected against ignorance and barbarism. These bodies, rich with vitality, not only conserve from age to age the initial impulse that a great minister gave them, but their vitality overwhelms the resistance that ignorance, superstition and barbarism present to them. I am happy to compare these bodies to the immense bodies that revolve overhead and to which the creator imparted the initial motion that they have conserved since the beginning of the universe.

Three of Lavoisier's assertions about academies are especially noteworthy: 1. They are "little republics." I take this to

mean that in the republic of science, as in the republic of letters, one establishes one's standing by producing notable work and, having obtained recognition as a member of the republic, one is treated as an equal. 2. They are bastions against ignorance, superstition and barbarism, which rather nicely captures Lavoisier's unexceptional views on the beliefs of the unenlightened. 3. They succeed admirably as institutions. The academies serve their purposes generation after generation without requiring the intervention of enlightened ministers or other patrons. Note also that the academies he is praising are *royal* institutions. Lavoisier, like most other philosophes, evidently believed that only institutions under the control of the monarchy were capable of putting enlightened ideas into action.

Lavoisier's views on the social structure and function of academies are revealing for several reasons. As a scientist he saw the Academy of Sciences as the arena in which he could distinguish himself by presenting his theories and experiments before his peers and by serving on numerous highly visible commissions and committees. As an administrator, Lavoisier was especially interested in the organization and internal governance of the Academy. Even before he entered the Academy he was drafting proposals for its reorganization, and he struggled mightily to save the Academy of Sciences before it was abolished, along with the other academies, in 1793. The academies also served as representatives of the more general program of reform championed by Lavoisier and others, most notably A. R. Turgot and the Marquis de Condorcet. This program was simultaneously royalist, rational and republican. While quite radical in the political context of the Old Regime, this program came to appear centrist as the revolution became increasingly radical, and it was ultimately cut down in the cross fire of left and right extremism.

What were the distinctive features of this comprehensive program of reform? Historians of the French Revolution still consider the answer provided by Alexis de Tocqueville in his classic *The Old Regime and the French Revolution* the best starting point for any exploration of this complex question (4). In the Old Regime, de Tocqueville argues, France was undergoing a wrenching administrative centralization. This transfer of power threatened to transform a national polity organized around local and regional judicial courts into a centralized absolutism in which the functions previously performed by these courts would be brought under royal administrative control. Traditionalists viewed their ancient privileges, which came to be condemned as feudal, as the legitimate rights and responsibilities of local corporations, the most notable of which were the regional courts of law called Parlements. Those who, with Tom Paine, think of the French Revolution as a great liberation from the dead hands of feudalism and absolutism consider this aristocratic defense of pre-revolutionary French politics reactionary. But those who, like Edmund Burke, are troubled by the revolution's legitimization of un-

bounded national autonomy consider the particularism of pre-revolutionary politics essential to the defense of individual liberty. For in fact, prior to the mobilization of the nation in defense of the revolution, the power of the French state was quite limited. The Bourbon kings, despite their claims to divine sanction and absolute authority, could do little more than hold together a nation that in its daily life was profoundly divided by diverse social customs and dispersed political authority. The King's political power was largely symbolic, for it rested on his claim that he alone represented the nation as a whole. His will was public will, but he could only work his will if he could obtain the support and cooperation of regional authorities.

The central drama of pre-revolutionary politics revolved around the contest between the King's ministers, who sought to subordinate political particularism to administrative centralization, and the local aristocrats, who sought to defend what they deemed their ancient liberties. The issue was clearly drawn in the crisis of 1771, the same year in which Lavoisier drafted his eulogy of Colbert. Chancellor Maupeou, in an attempt to break the power of the Parlements that were opposing reforms proposed by the King, "abolished venality of parliamentary office, reorganized parliamentary jurisdictions, limited the judicial right of remonstrance, and staffed his remodeled "parlements" with men willing to exercise their functions on condition of removability subject to the royal will" (5). The parliamentary judges thrown out of office objected strenuously to this "reform," which they considered a despotic usurpation of power. They responded by leading the first of the "aristocratic" revolts that, by rendering France ungovernable, finally forced the calling of the States General in 1788 and all that followed.

Familiarity with this axis of political tension helps render intelligible the suspicion with which Lavoisier's efforts to apply his scientific knowledge to the needs of the nation were sometimes greeted (6). For Lavoisier was above all else one of the King's men, an administrator ready to serve the nation by exercising authority in the King's name. Consider, for instance, his long and successful service in the Gunpowder Administration, to which Turgot appointed him in 1775. Previously the production of gunpowder had been farmed out, like the collection of taxes, and France had produced less than half of the powder she required. Lavoisier centralized the collection of raw materials and the production of powder, and his reforms were so successful that in 1776 and 1777 France was able to supply the American patriots with powder for their war of independence. By 1788 France had achieved self-sufficiency in powder production and her powder was considered the best in Europe (7). It is therefore only just to note that the success of France's revolutionary armies owed a good deal to Lavoisier's reforms. It would be simpleminded, however, to think that politically alert Frenchmen judged Lavoisier's efforts in strictly national/utilitarian terms. The central

question at issue was not how much gunpowder was being produced, but which locus of authority was being strengthened. In fact, the more Lavoisier rationalized gunpowder production, the more suspicious those opposed to the King became. In other words, his efforts on behalf of the nation were judged in terms of the political vision they served rather than for their technical efficacy.

Lavoisier's commitment to centralized and rationalized administration put him at odds with the defenders of a political order based on the particularist liberties of local corporations. At the same time, his allegiance to the King was equally offensive to those on the political left. The issue here is essentially constitutional, the central question being: How did the legitimation of power at the national level come to be separated from the person of the King and vested in the collective will of the people as expressed through their representatives? Contemporary historians of the French Revolution describe this shift as the emerging hegemony of public opinion, a subject that is still of disturbing centrality in modern democracies. Of course the political concept of a General Will had been spelled out by J. J. Rousseau well before the Revolution put it into practice, and the role of public opinion and the problem of representation were much discussed in the decades before 1789. The issue was a difficult one for centralizing ministers like Turgot, for while they considered respect for the authority of the King fundamental to national cohesion, they were hardly advocates of what they condemned as "oriental despotism". The trick was to provide all citizens with meaningful forms of political participation while maintaining the political stability and administrative integrity of the state as a whole.

This problem had an analogy in the world of science and there Lavoisier opted for the republican resolution. According to this view, the authority vested in the Academy of Sciences rests with those who by virtue of talent and achievement have been assigned responsibility for determining what is and what is not good science. Within this community of peers, only reason and evidence are considered persuasive. In its relations with others, the community of science speaks with the authority granted to it by the nation. But this assignment and exercise of authority was resented by those who, like the radical and would-be scientist Jean-Paul Marat, had been denied a place in the Academy. Therefore, when the opportunity presented itself, they were quick to condemn as the abuse of privilege what Lavoisier saw as the proper use of authority grounded in reason and merit. To the radical Jacobins, only voluntary associations, as opposed to those invested with royal privileges, were legitimate, and it was this view that sealed the fate of the royal academies. Utilitarian arguments proved to be without force in this contest between the meritocratic principles that informed the republican image of the world of learning and the democratic interpretation of how one should go about pursuing and applying scientific knowledge in a

society devoted to the principles of liberty, equality, and fraternity.

Lavoisier for years operated with notable success in a highly stressed and rapidly changing political culture. The tensions within that culture were not simply ideological expressions of the antagonism between two opposed social classes - that view, central to Marxist historiography, emerged later from a social analysis of the effects of industrialization and a political analysis of the French revolution. Nor was the revolutionary transformation of French politics simply a progressive liberation achieved through the destruction of the old order. Lavoisier and his colleagues in reform were neither reactionary absolutists nor revolutionary democrats. Rather, they sought to alter radically the way the French state functioned while preserving its unity and promoting its prosperity. Although in the short run their program failed dramatically, and at terrible personal cost, in the long run their vision of the relations between scientific knowledge and political power came to prevail. But it did so only after the people of France, speaking first through their revolutionary leaders and then either through their Emperor, or through plebiscites, or through their legislative representatives, had replaced the King as the embodiment of the nation.

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Dr. Arthur L. Donovan teaches in the Department of Humanities of the United States Merchant Marine Academy, King's Point, New York 11024-1699. He is author of "Philosophical Chemistry in the Scottish Enlightenment" and editor of the special edition of "Osiris" on the Chemical Revolution. He is currently editing a selection of Lavoisier's chemical writings for use in college courses and is in the early stages of writing a biography of Lavoisier.

SCIENTIFIC REVOLUTIONARIES CAUGHT IN POLITICAL REVOLUTION: PRIESTLEY AND LAVOISIER

*J. Edmund White, Southern Illinois University
at Edwardsville*

This symposium rightfully focuses on Antoine Lavoisier's contributions to the establishment of modern chemistry, but other aspects of his career also are worthy of our attention. Dr. Donovan already has described many of Lavoisier's activities as an administrator (1). It is informative and interesting to compare Lavoisier's career to that of his English contemporary and sometime chemical adversary, Joseph Priestley. Priestley's discovery of oxygen not only helped Lavoisier toward a new explanation of chemistry but, combined with his other work on gases, helped to earn him a reputation as a chemist. These two were considered by their contemporaries to be the outstanding scientists of their countries, if not the world. Their activities, interests, and honors were similar in many respects, although Lavoisier worked in business, political, and social environments, whereas Priestley's were religious and academic.

Lavoisier was on the inside. Wealthy and with social position, he served on committees of the Academy of Sciences and of the government and was appointed to increasingly important positions. Rapidly, however, the situation in France reversed, and he was executed. Priestley, on the other hand, was always on the outside, always opposing the "Establishment," whether church or government, in his preaching and in his writing. Eventually, his situation in England became so unpleasant that he voluntarily took himself into exile.

Today neither Lavoisier nor Priestley is considered by historians to be a major figure of this period; they were not involved in significant events that would get their names in the typical histories. Their contributions to society are not the kind made by kings, generals, and statesmen. At the time, however, each was widely known and respected for his scientific reputation and his considerable contributions in other areas. Al-

though neither was directly involved in revolutionary activities, both were brought down by the political upheaval in France and the attendant mob actions.

A summary of the areas of interest of these versatile men (Table 1) brings out the similarities and the differences. Both were well educated, had a consuming interest in chemistry, made innovative proposals for improving the educational systems of their countries, and were active members of their national scientific societies. Lavoisier earned his major income from the Tax Farm, was concerned about economic matters, including scientific farming, and was a public servant in the French tradition at that time. Priestley worked as preacher and teacher, wrote textbooks on electricity, grammar, history, and perspective in drawing (several going through

many editions), contributed as critic and theoretician of government, and wrote theological treatises.

A chronological list of the major events in their lives (Table 2) is useful for making comparisons. We see, for example, that, although Priestley was ten years older than Lavoisier, they were admitted to their respective scientific societies at about the same time and started chemical investigations in the same year. Reference to Table 2

will help to keep straight the sequence of events to be described, which will cover the period of almost 21 years from 1774 to 1794.

Three events make 1774 significant: on 9 May, Louis XV died; on 1 August, Priestley discovered oxygen, and in October he visited Lavoisier and described his results. France was on the brink of disaster. She was nearly bankrupt, and there was a great gulf between the privileged and the common people. The nobility and the church were not prepared to give up any of their perquisites, especially their freedom from taxation. The death of the King marked the beginning of change. Louis XVI was only 20 years old and was not strong enough to stand up to the entrenched supporters of the status quo. He tried to achieve reform by appointing Turgot to the post of Controller General, but Turgot's changes were popular only with the common people. In two years, he was ousted and his reforms were erased. This was the start of inevitable deterioration in France.



The trial of Lavoisier and the Tax Farm, a 19th century reconstruction from the 1874 edition of L. Fuguier's *Vies des Savants Illustres*.

Table 1. A Comparison of the Careers of Lavoisier and Priestley.

<i>Lavoisier</i>	<i>Priestley</i>
Lawyer	Minister
Businessman	Teacher
Chemist	Chemist
Economist	Grammarians
Educational Theorist	Educational Theorist
Experimental Farmer	Historian
Public Servant	Political Theorist

For our two heroes, however, 1774 was the start of a 15-year period of successes. Both rose in position and in public recognition during this time. Lavoisier was appointed head of the Powder Commission and moved to the Arsenal, where he had space to set up the well-equipped laboratory described by Dr. Schwartz (2). He became Director of the Academy of Sciences, receiving intellectual stimulation there as well as from visitors to his famous laboratory. It is remarkable that nearly everything he did was creative, well-thought-out, and useful. Some examples are:

* As Farmer-General, he had many assignments over the years. Some achievements were abolishment in his district of a tax on Jews, proposal of a wall around Paris to stop smuggling (it was constructed), and preparation of an instruction manual for officials of the organization. He thought the system was wasteful, and he tried to achieve equity and to reduce the burden on the lower classes.

* As Powder Commissioner, he improved the quality of the gun powder and the system of manufacturing it. France changed from an importer to an exporter of powder and, therefore, was able to supply the American colonies during their conflict. The greater range of French guns is given credit for the defeat of the Prussians and for Napoleon's success.

* As member and Director of the Academy of Sciences, he served on many committees which produced over 200 reports (usually written by him) on topics assigned by the government. Significant ones are those on prison reform, hot-air balloon flights, and Mesmerism (this committee included Benjamin Franklin and Joseph Guillotin).

* As farmer, he tried new methods of cultivation and fertilization, kept careful records for ten years, and helped found the Royal Society of Agriculture.

* As elected official, he represented the Third Estate at the Provisional Assembly of Orléanais, 6 September 1787. According to L. de Lavergne, "It was Lavoisier who did everything, who inspired everything, who was everywhere" (3). As usual, he wrote most of the committee reports. His innovative proposals concerning tax reform, forced labor on roads, agriculture, social security, and a Savings Bank were passed, but

nothing ever came of them.

Priestley became librarian for the Earl of Shelburne, moved to Calne, and had time to do his work on gases at Bowood. In a few years, he left, probably because he was becoming a political liability to Shelburne, but, in Birmingham and the Lunar Society, he found intellectual stimulation and wrote his best theological works.

Probably this was the happiest and most satisfying time of both their lives, but, on 14 July 1789, came the fall of the Bastille. This was the beginning of the end for both men, because each had become quite involved in public affairs. Lavoisier was on the inside, active on many committees set up by the government to report on assigned problems. Priestley, on the outside, had supported the American colonies in their revolt and had gained notoriety from his polemical writings on matters of church and state and his role as a leader of the Dissenters.

The situation grew worse in both France and England with

Table 2. Comparison of Significant Dates in the Lives of Lavoisier and Priestley.

<i>Lavoisier</i>	Year	<i>Priestley</i>
	1733	Birth, 13 March
Birth, 26 August	1743	
	1761	Warrington Academy
	1766	Royal Society
Academy of Sciences; Tax Farm	1768	
Diamonds	1772	Soda Water
	1773	Copley Medal; Lord Shelburne
Ascension of Louis XVI; Entertained Priestley	1774	Oxygen; Visited Paris
Powder Commission; Arsenal	1775	
	1780	Move to Birmingham; Lunar Society
Director, Academy of Sciences	1785	
Nomenclature Report	1787	"Honest Heretic"
<i>Traité Élémentaire</i> ; Bastille, 14 July	1789	
Weights and Measures	1790	Response to Burke
National Treasury	1791	Birmingham Riot; Pastor at Hackney
Public Instruction	1792	
Arrest of Members of the Tax Farm	1793	
Death, 8 May	1794	Emigration to U.S.
	1804	Death, 6 February

the fall of the Bastille, only a few months after the event celebrated by this symposium: the publication of Lavoisier's book describing the new system of chemistry. Soon, each man was being attacked and abused in the press. For Priestley, the critical episode was his response to a speech by Edmund Burke in opposition to the French Revolution. Burke had supported the American Revolution, so he seemed to Priestley to be a turncoat. Characteristically, Priestley fired off a vigorous attack, his "Letters to Burke." This and other writings made him the target of cartoonists, such as Gillray, whose "Birmingham Toast" shows Priestley calling for the King's head on a platter. "Dr. Phlogiston" shows Priestley as a firebrand, setting the flames of revolution. In 1791, a mob burned his church and house during the Birmingham riot. Lavoisier and others sent expressions of sympathy, and Priestley was made an honorary citizen of France, which did not help him at home. He wrote from his new home in Hackney that "On the 14th of July, 1792, it was taken for granted by many of the neighbors, that my house was to come down, just as at Birmingham the year before" (4). He received many threatening letters and was burned in effigy many times. Also, he was "much restricted with respect to my philosophical acquaintance; most of the members of the Royal Society shunning me" (5).

For Lavoisier, the critical factor was the shift in power; anything from the Old Regime came under attack, often not fairly. He was criticized for the tax wall as early as 1784, "... M. Lavoisier ... is the beneficent patriot to whom we owe this ingenious and solitary imprisonment of the capital of France" (6). In 1790, Lavoisier still had hope that a constitutional monarchy might result; he wrote optimistically to Franklin, "We regard the Revolution as finished, irrevocably ... There is a weak royalist party ... the constitutional party ... is numerous, including among its numbers the intelligent and enlightened citizens" (7). The attacks became more vicious, "I denounce to you the coryphaeus of charlatans, the sieur Lavoisier, son of a land-grabber, pupil of the Geneva stock-jobber, Farmer-General, controller of gunpowder and saltpeter, governor of the Discount Bank, secretary to the King, member of the Academy of Sciences ..." (8). This came in 1791 from Marat, who hated Lavoisier because Lavoisier had ridiculed publicly a paper submitted by Marat and had prevented his admission to the Academy of Sciences. Apparently Lavoisier lost the hope expressed to Franklin; he began to resign from positions and to refuse new appointments, trying to separate himself from the Old Regime. In 1792, he moved from the Arsenal. He did hold on to the directorship of the Academy, trying to save it as an institution. When funds were withheld, he used his own. Late in 1793, all the Farmers-General were arrested, and, after several months in prison, they were given a farcical trial and were executed on 8 May 1794.

Earlier in the same year, Priestley stated, "But I see no occasion to expose myself to danger without any prospect of doing good, or to continue any longer in a country in which I



"Dr. Phlogiston, The Priestley Politician or the Political Priest".
Commentary on Priestley's political and religious activities.

am so unjustly become the object of general dislike ..." (9). He decided to join his sons in Pennsylvania and was at sea when Lavoisier died. In New York and Philadelphia, he was welcomed royally, even meeting with President Washington. The lovely home he built in Northampton, PA (now closely associated with the founding of the American Chemical Society), was isolated from the cities but even more so from England. Letters could not replace the give-and-take of the intellectual discussions he loved so much. He did do some chemistry, discovering carbon monoxide. His painting by Rembrandt Peale appears on a postage stamp of the United States, but Great Britain has not honored him in this way. On the other hand, there are several statues of him in England.

In France, the situation changed so rapidly that, only two years after his death, Lavoisier was commemorated by a ceremony and the issuance of a medal. His head from the portrait by David appears on a stamp of France.

We have looked at the non-scientific aspects of the lives and careers of two men who were active contributors in many

areas of society but are remembered primarily for their scientific contributions. Many parallels appeared, which might be expected for two well-educated and intelligent men of that time. Differences were due largely to the chance of birth: Lavoisier wealthy and "in" socially, Priestley poor and outside the establishment. In summary:

- * Both names became household words.
- * Both men were visionaries, making proposals that were ahead of their time in the areas of education, economics, government, human rights, and religion.
- * Both were scientific revolutionaries, earning recognition as outstanding scientists and making lasting contributions to science.
- * Both men were destroyed by political revolution but for opposite reasons: Priestley because he supported it, and Lavoisier because he was seen as a representative of the system against which it was directed.

References and Notes

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J. Edmund White is Professor of Chemistry at Southern Illinois University at Edwardsville, Edwardsville, IL 62026, where he teaches a course in the history of chemistry.

LAVOISIER AND THE CONSERVATION OF WEIGHT PRINCIPLE

Robert Siegfried, University of Wisconsin

It is generally agreed today that when Antoine Laurent Lavoisier overthrew the phlogiston conception of combustion, he achieved a revolution in chemistry. In its simplest outline the story goes like this. In the phlogistic view that widely prevailed when Lavoisier began his chemical work in the 1760's, sub-

stances owed their combustibility to the presence of phlogiston in their make-up. When the body was actually burned, the phlogiston departed, leaving behind the other components - the acid in the cases of sulfur and phosphorus, and the calx in the case of metals. In this view both combustion and calcination were decomposition processes. In this regard, the phlogistic view was an 18th century sophisticated version of a centuries-old tradition of fire analysis, that the application of great heat reduced any body to its simpler components, if not necessarily to its true elements. Thus, in the cases illustrated, the acid was simpler than the sulfur and the calx simpler than the metal.

Lavoisier was able to force the inversion of this compositional relationship by keeping a balance-sheet account of the weights of all the participants in the reactions. For example, when he heated a weighed quantity of mercury in a closed container, he was able to show that the weight gained by the metal in becoming a calx was equal to the weight lost by the air in which the reaction took place. Lavoisier carried out similarly monitored experiments on the combustion of sulfur and phosphorus and again was able to account for the weights of all the participating materials. From these kinds of experiments he was able successfully to argue that combustion was a process whereby something in the air (later named oxygen) combined with the combustible, rather than something leaving it. In Lavoisier's view, appropriately named the "anti-phlogistic chemistry," a metal was simpler than its calx, and sulfur and phosphorus were simpler than their acids. Until Lavoisier made weight a primary criterion for the recording of chemical change, the phlogistic view had been a useful way of organizing a large number of important chemical relationships in a qualitative way. But Lavoisier's persistent application of balance-sheet accounting made the older view untenable, and phlogiston rather quickly disappeared from the chemical scene.

Lavoisier was very conscious of his method in these events and stated the principle quite explicitly in his famous *Traité Élémentaire de Chimie* of 1789, whose bicentennial we are celebrating this year (1):

We may lay it down as an incontestable axiom, that, in all the operations of art and nature, nothing is created; an equal quantity of matter exists both before and after the experiment ...

With weight as his measure of the "quantity of matter," his persistent and imaginative application established the conservation of weight as the standard principle in chemical investigations, for which he has very properly received full credit.

I have no intention either of challenging that judgment or of further illustrating Lavoisier's systematic use of that principle. Rather, I wish to address the question of why the conservation of weight had not been more vigorously used in chemistry before Lavoisier's time, for the idea of conservation is as old as Western philosophy. As early as the 5th century B.C. Anaxagoras laid it out that, "nothing comes into being or is

destroyed; but all is an aggregation or secretion of pre-existing things; so that all becoming might more correctly be called becoming mixed, and all corruptions, becoming separate.”

Other examples from antiquity are readily found in the writings of Democritus, Lucretius, and any number of other philosophers. Marcelin Berthelot, the great pioneer of synthetic organic chemistry and historian of early chemistry, pointed out that “even the alchemists never did pretend to create gold or metals, but only to transmute the fundamental and pre-existing metal.”

Closer to Lavoisier’s own time, Francis Bacon offered a more operational version of this principle in the early 17th century, clothing it metaphorically in the language of the bookkeeper (2):

Men should frequently call upon nature to render her account; that is, when they promise that a body which was before manifest to the sense has escaped and disappeared, they should not admit or liquidate the account before it has been shown to them where the body has gone to, and into what it has been received.

There are two major reasons why the chemical application of this principle was so long delayed. The first and more immediately significant was the general ignorance of the existence of gases as chemical agents. The other was the more fundamental question of how the quantity of matter is to be measured. The two problems are closely linked, but we can treat them most easily by dealing first with the problem of “airs,” and then seeing how the philosophical problem permeated the practical one without being directly addressed.

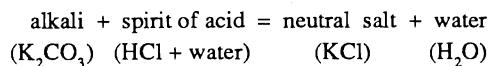
Pneumatic chemistry hardly existed when Lavoisier began his work, and from the beginning his chemical interests were focused on what was just then beginning to be the “hot topic” of the fixing and liberation of “air”, as all gases were then called. Without a knowledge of the role of gases in chemical change, many familiar reactions defied a meaningful application of the principle of conservation. Descriptions of some early 18th century experiments will illustrate the difficulties of applying the conservation principle when unaware of the role of gases.

Wilhelm Homberg, the most vigorous experimental chemist of the Paris Academy in the late 17th century, was curious to know how much “solid acid salt” was in the acid solutions available at that time. Such solutions were prepared by heating an appropriate salt and collecting the “spirit of salt” in water. Thus the heating of marine salt (NaCl) with a vitriol, or more commonly with a clay, yielded the “spirit of marine salt” (HCl) which was captured in an aqueous solution. Homberg, in order to determine the “quantity of solid acid salt” contained in the solution, carried out a very simple experiment based implicitly on the concept of conservation of weight. He added a solution of spirit of marine salt to a weighed quantity of dry alkali until the cessation of effervescence indicated the alkali to be satu-



Stephen Hales

rated. The resulting neutral salt was then dried and weighed. Since all the water had been driven off, Homberg reasonably assumed that the weight of the neutral salt was equal to the sum of the weights of the alkali and the solid acid salt whose quantity he sought (3):



By the end of the 17th century, “air” as one of the traditional Aristotelian elements had virtually dropped out of the chemists’ considerations, though great progress had been made in characterizing its physical properties. Hence, Homberg was totally unaware of the loss of gaseous carbon dioxide in his reaction, so his results were meaningless. As Joseph Black pointed out a half century later, Homberg’s “estimate was not accurate; because the alkali loses weight, as well as gains it” (4).

The irony of this experiment is that Homberg was staring right at the escaping gas all the time he was neutralizing the alkali. The effervescence that he monitored to determine the saturation point of the alkali was seen not as liberation of air, but as evidence of the “intestine motion” of the particles of acid and alkali in their vigorous strife of mutual destruction. Although vegetable indicators had been used by Robert Boyle before this time, they did not come into common practice until about the middle of the 18th century.

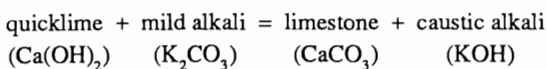
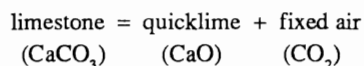
Stephen Hales first clearly established that “air” had to be taken into chemical account and, in doing so, implicitly utilized the conservation of weight principle as part of his argument. In his *Vegetable Staticks*, published in 1727, Hales reported at length on a series of experiments in which he

strongly heated a wide variety of substances from which he collected various quantities of air in an apparatus devised for the purpose. The loss in weight of the original body, which he attributed to the air emitted, was sometimes as much as half the weight of the original solid body. After finding air produced from such a wide variety of materials from all the kingdoms of nature, and often in such large quantities, he felt justified in summarizing his conclusions as follows (5):

[M]ay we not with good reason adopt this now fixt, now volatile *Proteus* among the chymical principles ... notwithstanding it has hitherto been overlooked and rejected by Chymists ...?

In spite of Hales' abundant evidence of the presence of air in a wide variety of substances, he saw it all as simply "air" with no chemical features to distinguish one from another. Hales, a great admirer of Isaac Newton and Robert Boyle, followed them in their view that air was simply air, that any differences that might be noticed in their properties were attributable to various impurities physically present. Nor was the chemical community much impressed by Hales' evidence, for air continued to be "overlooked and rejected by Chymists" (6).

Before pneumatic chemistry could be said to exist, some distinctions between "airs" had to be made. This was first accomplished by Joseph Black shortly after the middle of the century. In his study on *magnesia alba*, a naturally occurring hydrated magnesium carbonate, Black noted first its similarity to lime and conducted parallel experiments on both. He was able to show that when limestone was strongly heated, the resulting calx (quicklime) weighed only a little more than half as much as the initial limestone. As had Hales before him, Black tacitly assumed the conservation of weight and concluded that the loss in weight was owing to the loss of the air, a conclusion strongly supported by the fact that the quicklime did not effervesce with acid as had the original limestone. The original weight of the limestone could be recovered from the quicklime by reacting its solution with the mild alkali, that is, potassium carbonate. Black was able to characterize this "fixed air" as distinct from common atmospheric air:



From these he could conclude that mild alkali was equivalent to caustic alkali plus fixed air.

Here for the first time it had been demonstrated that the presence of a particular air was explicitly related to the presence or absence of particular properties. Although Black never weighed the emitted air directly, all his arguments are based on the assumption that the weight of the emitted air would be

equal to the loss in weight from the original limestone. By assuming that weight is conserved, he was able to demonstrate the consistency of the cycle of chemical changes involved (7).

But even Black's work did not immediately cause chemists to turn their attention to the chemistry of gases. In the years between 1735, when the *Vegetable Staticks* was translated into French, and about 1770, the *Mémoires* of the Paris Academy show no significant attention to the role of air in chemistry. Nor does Diderot's *Encyclopédie* reflect a more perceptive response. The ten-page article "Air" in the first volume (1751) is devoted almost entirely to the physical properties of air, with a brief paragraph merely announcing that Hales had shown air to be obtainable from a wide variety of substances. Two examples are offered without suggesting that the work is of chemical significance.

What I have offered up to this point is evidence that, long before Lavoisier, the principle of the conservation of weight in chemical change was sufficiently well known that at least three workers could apply it without feeling obligated either to justify it or even mention it as a distinct principle. But in the absence of a general awareness that gases enter into many common chemical processes, it is not surprising that no one saw the systematic application of the principle as a promising chemical procedure. Even Black's work, as significant as it was historically, did not utilize the principle as a challenging principle against which *all* chemical changes must be measured. This vision was put into practice first by Lavoisier. Although he first stated it as a fundamental principle only in 1789, his experimental practice had from the beginning been designed on that principle. In the famous *Traité*, he wrote (8):

As the usefulness and accuracy of chemistry depends entirely upon the determination of the weights of the ingredients and products both before and after experiments, too much precision cannot be employed in this part of the subject; and, for this purpose, we must be provided with good instruments ...

In this manner of operating, we have always a very material proof of the accuracy of the analysis, as the whole weights of the products taken together, after the process is finished, must be exactly equal to the weight of the original substance submitted to distillation.

Needless to say, Lavoisier himself did not always follow his own precepts (that's a story for another time and place). But the conservation of weight principle had entered the toolbox of deliberate and conscious chemical argumentation and the science would never be without it again.

Recall that Lavoisier in the main body of the *Traité* stated the conservation principle in the most general terms; that "an equal quantity of matter exists both before and after the experiment" (9). For us today it is a perfectly obvious procedure to balance the quantitative books with weight as the proper measure of the quantity of chemical matter. In the 18th century, matter without weight was not a self-evident contra-



Isaac Newton

diction as it would seem to us today.

The fundamental justification for identifying the quantity of matter with its mass (or on the earth's surface, its weight) came with Newton's law of universal gravitation. The weight of a body is the force of attraction between it and the earth. Hence all massive bodies on the earth should experience that attraction and be ponderable. But Newton himself gave ambiguous exception to this seemingly restrictive meaning of matter when he added the General Scholium to the second edition of the *Principia* in 1713 (10):

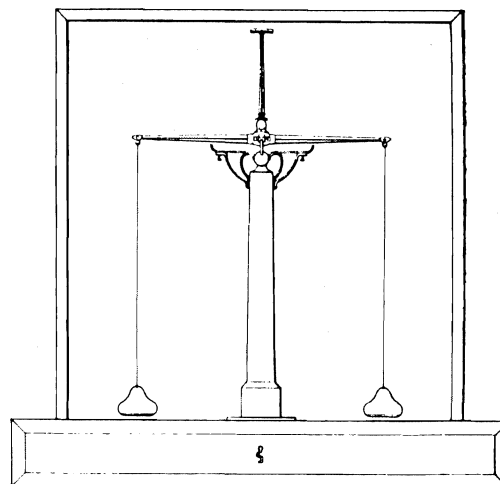
And now we might add something concerning a certain most subtle Spirit, which pervades and lies hid in all gross bodies; by the force and action of which Spirit, the particles of bodies mutually attract ... and electric bodies operate ... as well repelling as attracting the neighbouring corpuscles, and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the members of animal bodies move at the command of the will, namely, by the vibrations of this spirit, mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles. But these are things that cannot be explained in a few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the law by which this electric and elastic spirit operates.

Newton's "most subtle spirit" is itself ambiguous on this point. The word spirit in Newton's time derived from the earlier usage as anything volatile, an ethereal spirit. It was associated with the Paracelsian mercury, symbolic representa-

tive of the volatility of all such, the very essence of ethereal spirits. Was it material, or was it spiritual in the theological, non-material sense of the word? Also Newton clearly allows repulsive as well as attractive forces, from which some bodies might show absolute levity by being repelled from the earth.

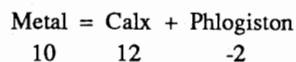
18th century philosophers and scientists found in this passage justification for dealing with imponderable matter generally. Treating heat, electricity, magnetism, and light as imponderable fluids provided the 18th century with at least a vocabulary for discussing phenomena which then still lacked the energy concepts of the 19th century. For the chemist, heat or the matter of fire was the most important of the imponderable fluids, taking on a material or instrumental quality while retaining something of the elusive nature of the ancient element of fire - nonisolable and known only by its effects. Of similar conceptual existence was the equally imponderable, but more elusive, phlogiston. In this guise the tradition of imponderable matter was to be directly confronted by the newly emphasized ponderable aspect in the work of Lavoisier.

That some metals gained weight when calcined in the air had been known for a long time. This had not been perceived as a problem early in the 18th century, for as we have already seen, the conservation of weight had not yet become a consciously applied axiom. But from about the middle of the century sufficient attention began to be given to the particular fact of weight gain in calcination that various explanations began to appear. These often showed much confusion between specific and absolute weights. As the question became more focused and the phlogistic view of composition became more widely held, serious proposals were made to assign that elusive material a negative weight or an absolute levity, thereby explaining a gain in weight in spite of a loss of material (11).



Small balance constructed for Lavoisier by Mégnié after a design by Truchot. Sensitivity: 0.1 g. Original now in the the Musée des Techniques, Conservatoire National des Arts et Métiers in Paris (18).

However heroically perverse negative weight might seem to us today, its introduction derived from the implied application of the conservation of weight principle to the calcination of metals. Putting arbitrary figures on the reactants, the implied argument becomes clear:



In the absence of the knowledge of the role of oxygen, there is no other way to balance the weights.

When Lavoisier first encountered the problem of the gain in weight by metals in calcination, he had already created a theory of the gaseous state and recognized the possibility that the gain was due to the fixing of air (12). Because Lavoisier's approach was new to the problem, he was not trapped into futile attempts to tinker with new concepts of gravity, but focused on the air which, unlike phlogiston, was already known to be ponderable and thus conformed with the positive gravity or weight in the ordinary sense of usage. When he was able to show that the weight gained by a calcined metal was equal to the loss of weight of the ambient air, even the phlogistonists soon accepted the evidence and admitted that oxygen (or dephlogisticated air as they called it) did indeed combine with the metal. They too were inheritors of the conservation principle and were not about to argue with it when used in such convincing fashion. Some still held that, since Lavoisier had not demonstrated the non-existence of phlogiston, it combined with the oxygen and in that combination remained a part of the calx or the acid. Thus, they were able to maintain the faith of their early training.

Lavoisier recognized his inability to prove the negative and, in his most thorough-going attack on the phlogistic doctrine, argued chiefly that phlogiston was inconsistent and unnecessary. Following a summary of his own demonstration of the role of oxygen in calcination, he wrote (13):

... if everything in chemistry is explained in a satisfactory manner without the help of phlogiston, it is by that reason alone infinitely probable that the principle does not exist; that it is a hypothetical body, a gratuitous supposition; indeed, it is in the principles of good logic, not to multiply bodies without necessity.

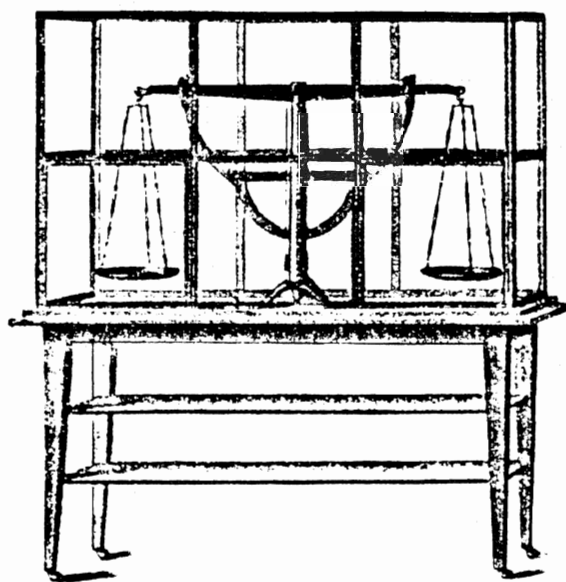
But if Lavoisier found the imponderable phlogiston hypothetical and useless, he found good use for his own equally imponderable caloric or the matter of heat. The idea that all gases were combinations of the matter of heat with different chemical substances was the earliest and most persistent of Lavoisier's scientific concepts. Caloric was for him the principle of the gaseous state even as phlogiston was for Priestley the principle of inflammability. In Lavoisier's list of elements or simple bodies, the imponderable bodies, caloric and light are listed in the first group along with the ponderable

oxygen, hydrogen, and nitrogen (14).

Indeed, caloric was probably more materially real for Lavoisier than phlogiston was for Priestley, for he had established its quantitative existence by carefully measuring not its weight, but the amount of ice it could melt. In a long series of calorimetric experiments carried out in 1783, Lavoisier and his colleague, Pierre-Simon de Laplace, determined heats of combustion and specific and latent heats, all measured on the assumption that the quantity of heat or caloric is conserved (15). When we recall Lavoisier's later statement that "an equal quantity of *matter* exists both before and after the experiment ..." [my emphasis], can we not reasonably infer that he had imponderable caloric in mind as well as the ponderable elements? The utility, not to say the necessity, of the imponderable fluids remained in science well into the 19th century, when energy concepts were able to make them unnecessary.

In summary, we have seen that, though the principle of the conservation of matter was well known long before Lavoisier introduced it as a decisive tool in chemical investigation, the ignorance of the chemical role of gases made its application meaningless. Noting also the deep philosophic and procedural uncertainties concerning the nature of matter, we can appreciate even more deeply Lavoisier's greatness in establishing *weight* as the operating criterion by which the quantity of *matter* is to be measured and conserved in chemistry.

Postscript - The history of science can teach fundamental lessons about the nature of scientific thought itself. One such lesson from this particular story has not always been under-



Large balance built for Lavoisier by Fortin in 1788 after a design by Truchot. Capacity: 10 kg, sensitivity: 25 mg. Original in the Musée des Techniques, Conservatoire National des Arts et Métiers (18).

stood by those charged with teaching the science to new generations of students. Witness the following quotation from a contemporary general chemistry text (16):

Antoine Lavoisier (1743-1794), a French chemist, insisted on the use of the balance in chemical research. His experiments demonstrated the law of conservation of mass, *a principle that states that mass remains constant during a chemical change (chemical reaction)*. A flash bulb gives a convenient illustration of this law.

The author completes his point by indicating that the flash bulb weighs the same before and after it is ignited.

But what will the student learn from this passage? First, that Lavoisier *demonstrated* the law of conservation of mass or weight, presumably in a manner like that utilized in the flashbulb experiment. As we have seen, Lavoisier did no such thing, but took the principle as "an incontestable axiom" incapable of direct experimental demonstration.

But the important point here is not the author's misrepresentation of Lavoisier's work (though that is lamentable enough), but that in so doing he misrepresents the manner by which such broad general principles are established in science. By implying that Lavoisier arrived at this principle by generalization from a large number of cases, presumably some 18th century equivalent of flash bulbs, the author is promoting the Baconian or inductive method, a view long recognized as inadequate and misleading. As we have seen, Lavoisier assumed an axiomatic validity for the principle from which he drew the conclusion that "the whole weights of the products taken together, after the process is finished, must be exactly equal to the weight of the original substance submitted to distillation" (17). Since all his measurements were consistent with the conclusion derived from his axiomatic principle, Lavoisier could feel confident of its validity and justified in its continued use. This procedure has since become familiar as the hypothetico-deductive method, a practice that might lead to the falsification of the assumption, but not to its direct demonstration. It is unfortunate that a modern textbook writer would promote a scientific methodology a century and a half out of date.

References and Notes

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2. All these examples are given by I. Freund in *The Study of Chemical Composition: An Account of its Method and Historical Development*, New York, 1904; Dover reprint edition, 1968, pp. 58 and 60.
3. W. Homberg, "Observations sur la Quantité Exacte des Sels Volatiles Acides Contenus dans les Differens Esprits Acides," *Mém. Acad. R. Sci. (Paris)*, **1699**, 44-51.
4. J. Black, *Experiments on Magnesia Alba*, Alembic Club Reprints, No.1, Edinburgh, 1893, p. 18. Also in H. M. Leicester and H. S. Klickstein, Eds., *A Source Book in Chemistry, 1400-1900*, Harvard, Cambridge, 1963, p. 83.
5. S. Hales, *Vegetable Staticks*, London, 1727. Quotation from the Oldbourne reprint edition, London, 1961, pp. 179-180.
6. The neglect of Hales' demonstration of the role of air might be attributed to the fact that it was contained in a book most obviously dedicated to botanical matters. It was undoubtedly his interest in natural history that led le Conte de Buffon to read the *Vegetable Staticks*, but it was Hales' long chapter on the "Analysis of Air" that caught his particular attention and which he emphasized in translating the book into French in 1735. In giving the French title as *La Statique des Végétaux et l'Analyse de l'Air*, Buffon not only shortened Hales' page-long title, but also promoted the analysis of air to equal status with the primary botanical subject. In the translator's preface (p. vii), he praises Hales for avoiding all "spirit of system" and relying on experimentation, "The initiation of the analysis of air is the best part of his book, ... everything is new in this part of his work; it is a fruitful idea from which follows an infinity of discoveries on the nature of different bodies which he submits to a new kind of test: there are surprising facts that he has hardly deigned to announce. Had it been imagined that air could become a solid body? Had it been believed that its springiness could be removed and restored? Had it been thought possible that certain bodies, such as the bladder-stone and tartar are more than two thirds air, solid and transformed?" Buffon's perception of the significance of Hales' work on air was not apparently shared by the French scientific community generally, for there is little evidence of serious attention to the chemistry of gases before 1766.
7. Reference 4.
8. Reference 1, pp. 297 and 393.
9. *Ibid.*, p. 130.
10. I. Newton, *Mathematical Principles of Natural Philosophy*, Andrew Motte's translation of 1729, revised by Florian Cajori, 2 Vols, University of California, Berkeley, 1966, p. 547.
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13. A. L. Lavoisier, "Reflexions sur le Phlogistique", *Mém. Acad. R. Sci.*, **1783**, (**1786**), 505-538. Quotation from *Oeuvres de Lavoisier*, Vol. II, Paris, 1862-1893, p. 623.
14. For a discussion of Lavoisier's views on the elements, see R.

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Dr. Robert Siegfried is Professor in the History of Science Department, University of Wisconsin, Madison, WI 53706. Well-known for his work on the Chemical Revolution, he has taken early retirement in order to work on a book on 18th century chemistry.

LAVOISIER THE EXPERIMENTALIST

Frederic L. Holmes, Yale University

Historians have paid more attention to Lavoisier the theorist than to Lavoisier the experimentalist. His conceptions of heat, the gaseous state and the composition of the atmosphere, his theories of combustion and of oxygen as the acidifying principle, his definition of an element and the reordering of chemical composition, his attacks on the phlogiston theory and his reform of the nomenclature of chemistry have all been thoroughly analyzed. Much scholarship has been devoted to the origins of his interest in these subjects, to the genesis of his ideas concerning them, and to the influences of other thinkers on his views. In part because the Chemical Revolution is treated as the construction of a new conceptual foundation for that science, Lavoisier has been viewed predominantly as a great theorist. It is frequently pointed out that at critical points he borrowed the experimental findings of others - especially those of the experimentally brilliant Joseph Priestley - and reinterpreted their results to fit his emerging theoretical framework. Some have even maintained that Lavoisier himself did not make major experimental discoveries.

Lavoisier is also known as the author of the fundamental principle of the conservation of mass. In the *Traité Élémentaire*, whose bicentennial we are celebrating this year, he wrote (1):

... nothing is created, either in the operations of art, or in those of nature, and one can state as a principle that in every operation there is an equal quantity of material before and after the operation.

It is recognized that this statement was the operating principle on which Lavoisier based his "balance sheet" method of experimentation; but the priority given to Lavoisier as a theoretician has prompted historians to wonder why he located the statement of so general a principle in a detailed discussion of fermentation rather than in a broader context. If one follows closely Lavoisier's prolonged investigation of fermentation, however, a very reasonable explanation for this connection becomes apparent. The fermentation reaction he viewed as a difficult, almost climactic test of his experimental method. As he put it in an earlier paper on fermentation that he did not publish, for a simple case there is no difficulty following a chain of reasoning in which the equation between the materials and the products of a chemical change is implicit. It is in handling a complicated case like fermentation that it is most important to keep this principle firmly in mind (2). That example alone should suggest that it would be fruitful to place more emphasis than is commonly done on the details of Lavoisier's experimental practice.

Historians here frequently noted that Lavoisier practiced quantitative "balance sheet" methods long before he stated the general principle on which they are based. His first notable experiments on the transmutation of water in 1768-70 relied on that method, and it pervaded all of his experimental investigations through the next two decades. There has been, however, an implicit assumption that Lavoisier's most significant experimental achievement was simply to adopt this criterion and the quantitative methods necessary to implement it. Making them actually work has not been viewed as a major problem once the "airs" in which Lavoisier was interested had been incorporated into the balances. When we follow Lavoisier's investigative pathway, however - in particular when we reconstruct his experimental ventures at the intimate level recoverable from his laboratory notebooks - we find that he did not have a global method for ensuring that his balance sheets would balance out; that they frequently did not; that he encountered myriad errors, the sources of which he could not always identify with certainty; that he often had to calculate indirectly what he could not measure directly; that he exerted great ingenuity in the management of his data so as to make flawed experiments support his interpretations; and that he devoted much care and effort to the design of experiments so as to obviate such difficulties, but that he often settled for results he knew to be inaccurate, using his faith in the conservation principle to complete or correct the measured quantities. Much of his scientific success, I would claim, is rooted in the resourcefulness with which Lavoisier confronted the many pitfalls that lay along the quantitative investigative pathway he had chosen. He was, in fact, the most innovative experimental chemist of his age. He invented a whole new way to perform chemical experiments, and it required all of his considerable technical skill and critical judgment to make it succeed.

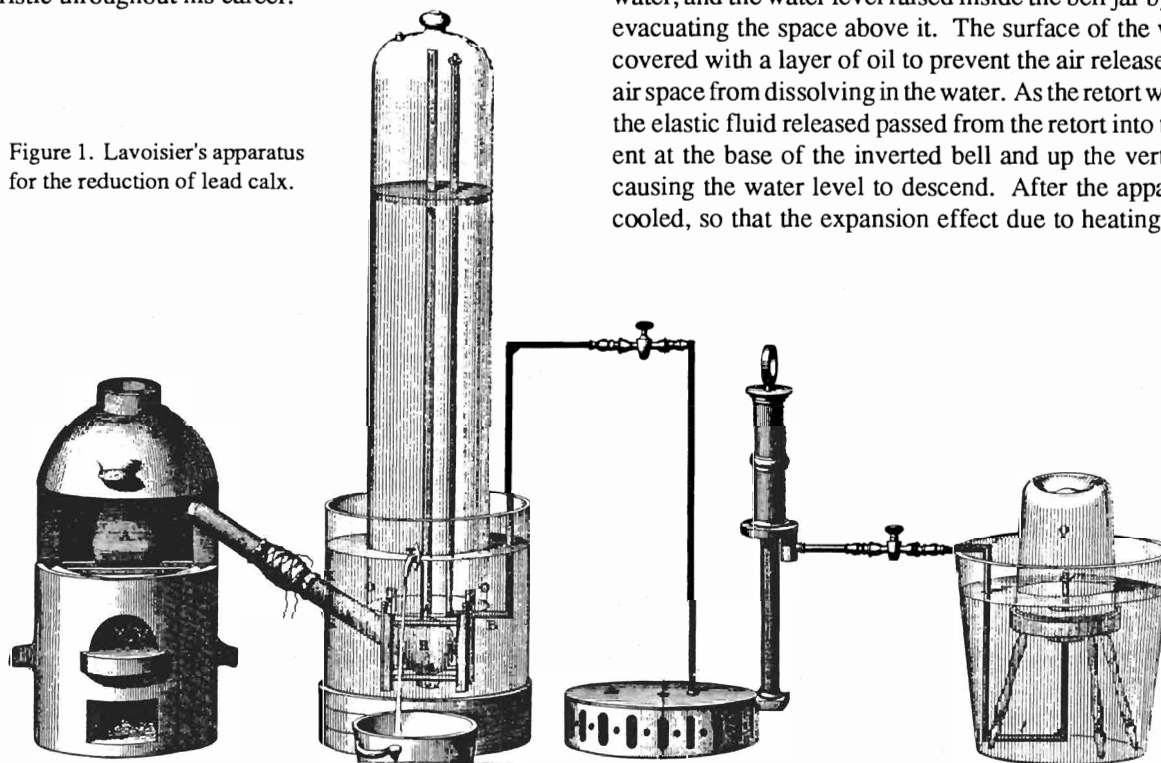
Recently there has been some discussion over the question

of whether the innovations that Lavoisier introduced into experimental chemistry were modeled on experimental physics. It is argued that, in breaking with the qualitative methods of earlier chemistry, he emulated a quantitative approach already manifested in the physics of his time (3). This point of view has some interest; but unless we can identify specific aspects of Lavoisier's experimental apparatus and methods that he derived from experiments carried out in the domains that were regarded as parts of physics, it will not carry us far toward an understanding of Lavoisier's investigative approach. It is, moreover, a question which I do not believe would have concerned Lavoisier as much as it does current historians. Lavoisier's scientific interests and background were multifaceted. He had studied physics, mathematics, chemistry, mineralogy, geology, and other subjects, and he exploited ideas or methods drawn from any of them that he found pertinent to the problems in which he engaged himself. The most realistic portrait of Lavoisier's scientific orientation, I think, is that given by the late Carl Perrin in a beautiful article entitled "Research Traditions, Lavoisier, and the Chemical Revolution." From the mid-1760's, when he committed himself to a career in science, Perrin points out, Lavoisier was "continually on the lookout for what he called 'une belle carrière d'expériences à faire', a fine course of experiments to run" (4). He sought, in other words, problems that would open up lines of investigation that promised to lead him to novel insights. He was ready to pursue such opportunities into whatever disciplinary areas they might lead. That pragmatic quality remained characteristic throughout his career.

I would now like to illustrate Lavoisier's experimental approach by discussing in some detail several concrete examples. From the time he engaged himself in 1773 in a broad investigative program to study the processes that fix or release "elastic fluids" from other bodies, until the end of his career 20 years later, there was a remarkable continuity in the experimental problems he pursued, the methods he applied, and the apparatus he used. During those two decades, the problems, methods, and apparatus evolved together from simple beginnings toward ever greater complexity and refinement. In his early experimental set-ups we can readily discern improvised adaptations of equipment that had long been in use. Later he increasingly resorted to equipment designed and constructed especially for each type of investigation; but the descent of his elegantly crafted later apparatus from the crude early ones is self-evident.

My first example is from a series of experiments that Lavoisier published early in 1774, that were performed during the course of the preceding year, on the reduction of lead ore. His purpose was to measure the quantity of elastic fluid given off in the reduction by a given quantity of lead calx. Prior experiments showing that lead calx did release an air had constituted one of the prime discoveries that prompted him to initiate his long research program on the fixation and release of airs. The apparatus is shown in figure 1. On the left is a furnace containing a retort fabricated from four pieces of iron soldered together. In it Lavoisier placed the lead ore mixed with charcoal. The bell jar in the middle was inverted in a basin of water, and the water level raised inside the bell jar by partially evacuating the space above it. The surface of the water was covered with a layer of oil to prevent the air released into the air space from dissolving in the water. As the retort was heated, the elastic fluid released passed from the retort into the recipient at the base of the inverted bell and up the vertical tube, causing the water level to descend. After the apparatus had cooled, so that the expansion effect due to heating had been

Figure 1. Lavoisier's apparatus for the reduction of lead calx.



eliminated, Lavoisier marked the change in the level of the water and from that he calculated the volume of the elastic fluid released from the lead calx (5).

Did Lavoisier model the design of this experiment on experimental physics or did he derive it from existing chemical practice? If we examine the apparatus closely, we see that it includes components from three distinct sources. On the right is a piston-operated vacuum pump. Such vacuum pumps descended from the pump invented by Robert Boyle in the 17th century. Boyle was both a chemist and a natural philosopher, but we associate his experiments using the pump mainly with physics, and it was part of the repertoire of 18th century physics as well. The vacuum pump served here, however, only the subsidiary purpose of raising the water level in the bell jar.

The furnace and retort on the left derive from the most traditional equipment of the chemical laboratory. Lavoisier had, in fact, begun with an ordinary glass retort, but had found it unusable because the lead ore attacked the glass. He then tried ordinary clay retorts, but they were porous enough to leak a little air. Requiring an absolutely air-tight system, he was, after a number of failed attempts, driven to have a special retort fabricated in iron. This modification is, I believe, typical of the pragmatic moves through which Lavoisier began early in his career to adapt standard chemical apparatus to the new demands of his methods.

The inverted bell jar central to the experiment is, of course, a modification of the pneumatic flask invented nearly a half century earlier by Stephen Hales. Lavoisier himself wrote that "the idea" for the apparatus "came originally from Hales" (6). Figures 2 and 3, showing two of Hales' experimental arrangements, confirm visually that they were the source for Lavoisier's apparatus (7). Can we say that Hales' experiments constituted a part of physics or of chemistry? Readers of Hales' *Vegetable Statics* will know that the inspiration for his measurements, whether of the blood pressure in a horse, the height to which sap can rise in plants, or the quantities of air that can be "fixed" in solid bodies, was Isaac Newton; and therefore that it is traceable to one of the greatest achievements in physics (8). Hales' interest in the fixed airs derived more immediately, however, from his study of plants and was incorporated into a book about his experiments on plants. We might, therefore, just as well derive the pneumatic apparatus from botany as from physics or chemistry. Subsequently, Guillaume-François Rouelle, the popular teacher of chemistry in France, incorporated Hales' pneumatic experiments into his chemical lectures, where Lavoisier undoubtedly first encountered them (9). Moreover, a plate in the well-known *Encyclopedia*, published in 1777, depicting a typical chemical laboratory, shows a pneumatic apparatus among the more traditional equipment of the chemistry laboratory (10). On balance, therefore, the methods that Lavoisier adapted to this crucial experimental problem seem to be associated more directly with experimental chemistry than with experimental physics. Nevertheless, the composite sources

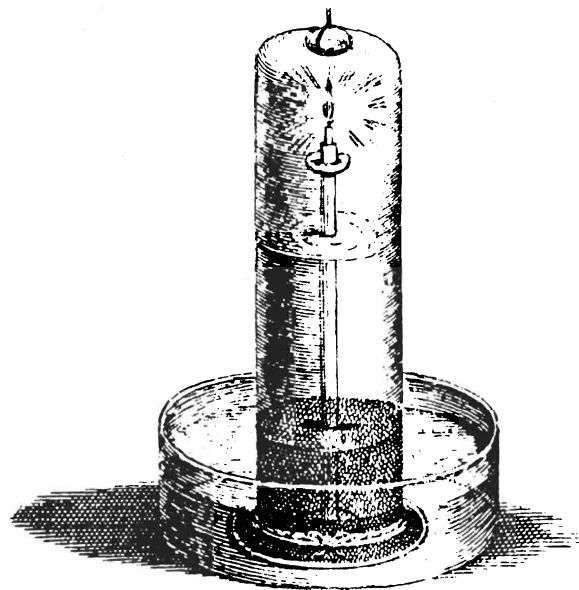


Figure 2. Apparatus used by Hales to collect and study airs.

for his overall design, as well as the experimental problem itself, reinforce my earlier suggestion that Lavoisier's approach is not easy to classify primarily as physical or chemical. He was practical enough to find his models wherever they could help him.

In the execution of this experiment Lavoisier encountered obstacles typical of many of his efforts to balance the matter existing before and after an operation. Ideally he would have shown that the difference between the lead ore he had placed in the retort and the lead he collected from it afterward equaled the weight of the elastic fluid disengaged; for his aim was to confirm that the lead ore was composed of the lead and the elastic fluid. There were, however, complications. The difference between the ore and the lead was 6 gros 6 grains. He had measured, however, the volume rather than the weight of the elastic fluid. Since he was not certain of the nature of that fluid, he could not be certain of its density either. If it were ordinary air, the volume of 560 cubic inches would have weighed only 3 gros 41 grains. If it were the same elastic fluid (fixed air) released in the reaction of an acid with lime, the density would be somewhat greater - from the results of such an experiment he had estimated a density of 575/1000 grain per cubic inch. Even then the weight would be only 4 gros 34 grains, "and there still remains a *deficit* by weight of 1 gros 44 grains" - that is, of about one-fourth (11).

Seeking to account for this discrepancy, Lavoisier suspected, since a few drops of water had accumulated in the small receiver, that perhaps the lead ore had contained "a portion of water." To check that possibility he reduced the same quantity of lead in an ordinary retort with a large recipient in which he hoped more of the water might collect. He obtained, however,

only 24 grains of water, far too little to cover his deficit. His only remaining recourse was to assume that additional water vapor had been carried away by the current of the elastic fluid produced in the reduction (12). Thus in this, as in so many other cases, Lavoisier's balance sheet did not balance. There were factors he could not control. His faith in the principle was not an outcome of his experimental experience, but an axiom without which he could not conduct his experiments at all. Lavoisier did not need proof of its validity - he simply could not imagine any rational alternative to the view that the weight of the matter present before an operation is equal to that resulting from it.

In this experiment Lavoisier had to contend also with the possibility that the elastic fluid had been disengaged not from the lead but from the charcoal essential to its reduction. Through additional experiments he was able to show that the charcoal consumed was not sufficient to supply all of the elastic fluid, so that some of it, at least, must have come from the lead ore. At the time he published these experiments, however, he still did not know how much that was (13). That he was willing to make public an investigation leaving so much to be desired from a quantitative standpoint may be viewed in part as a mark of his youthful ambition, in part because he regarded his results as a progress report on ongoing investigation; but a more basic reason is that the result sufficed for his present purpose, which was only to confirm that the calx of a metal was a combination of the metal with an elastic fluid. A more complete balance sheet would have been nice, but was

Figure 3. Apparatus used by Hales to collect and study airs.

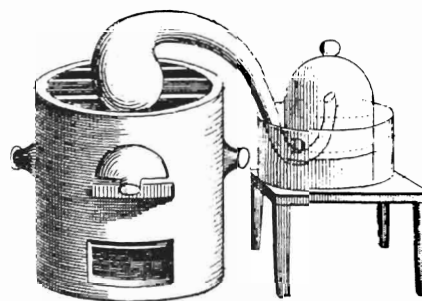
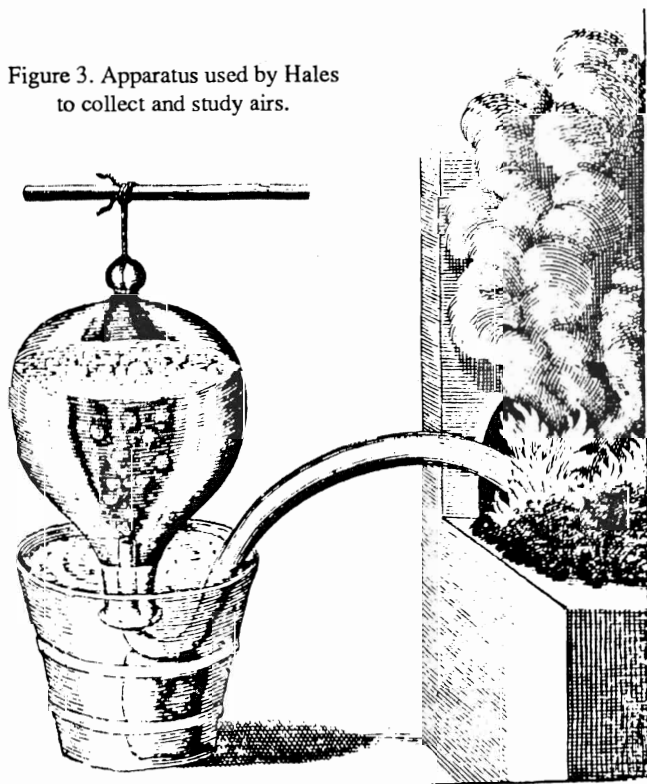


Figure 4. Lavoisier's apparatus for the oxidation of mercury and the reduction of mercury calx.

not necessary for the pragmatic argument he wished to make.

During those formative years that led Lavoisier by the Fall of 1777 to a general theory of combustion and his first full break with the phlogiston theory, he relied upon experimentation and reasoning that was essentially quantitative, but that did not require precise quantitative results. Experiments combining conventional chemical processes and apparatus with the pneumatic bell jar and the identification of elastic fluids continued to play the central role. The most decisive and brilliant experiment during these years, Lavoisier's famous analysis and synthesis of the air by calcining mercury and reducing the resultant calx without charcoal, typifies his approach. Figure 4, taken from the *Traité*, depicts the apparatus first used in April 1776 for that experiment. The physical resemblance to Stephen Hales' original experimental arrangements is obvious. An ordinary chemical retort has its neck curved so that it will connect with the interior of a pneumatic flask rather than with a conventional receiver. In the experiment Lavoisier showed that "about 1/6" of the air in the flask is removed during the calcination. Replacing that portion with about the same amount of "dephlogisticated air" - as he still called the air obtained by reducing mercury calx without charcoal - he restored the original atmospheric air. The tests by which he confirmed that he had reconstituted ordinary air were partly qualitative, but included also Priestley's semi-quantitative nitrous air test. Quantitative measurements were thus basic to the conclusions Lavoisier reached. In the first step the mercury gained weight while the air lost volume; in the second step the same volume of an air produced in an operation in which mercury calx loses weight was added to the air remaining after the first step. For Lavoisier's purpose, however, quantitative accuracy was not essential (14).

It was during the years 1781-1785, when Lavoisier extended the conceptual structure and methods that he had established during the previous decade to more complex situations and during which he encountered technically more difficult problems than those in the early years, that he was pressed to strive not merely for quantitative results, but results reliable enough and precise enough to use as foundations for further calculations. He was probably also influenced during

this period, through his collaboration with two able mathematicians, Pierre-Simon Laplace and Jean-Baptiste Meusnier, to seek greater vigor both in his experimental measurements and the calculations he made with his data.

A key transition point in Lavoisier's movement from rough quantification toward a drive for accuracy was the calorimetric experiments that he performed with Laplace during the Winter and Spring of 1783 to measure specific heats and the heat released in combustions and respiration. Their aim in the design of the ice calorimeter, they wrote afterward, was to find "a method appropriate to determine those quantities with precision." Under certain restricted conditions, most notably that they were able to operate only on those rare days when the ambient temperature was within two degrees of the freezing point of water, they were able to achieve remarkably good results, given that these were the very first measurements of their kind. Their investigative goals propelled them, however, to also seek more accurate results in other types of experiments (15-16).

A prime objective of the calorimeter experiments was to confirm Lavoisier's theory that respiration consisted of the combustion of carbon. They wished to show that respiration released the same quantity of heat in producing a given quantity of fixed air that the combustion of charcoal yielded. To do so they required four separate measured quantities: calorimetric measurements of the heat released by a guinea pig over a given time period; of the heat released in burning a given quantity of charcoal in oxygen; the amount of fixed air produced by the animal in the given time; and the fixed air released by burning a given quantity of charcoal. Attaining accurate measurements of the latter two quantities proved to be more difficult than the calorimetric measurements (17).

I wish to concentrate for now on the measurement involving charcoal. Although Lavoisier had concluded in 1777 that fixed air is composed of carbon and oxygen, this was primarily a deduction from the overall theoretical framework he had by then constructed. Having earlier shown that phosphorus and sulfur absorb oxygen to form acids, he reasoned analogously that charcoal absorbs oxygen to form the fixed air which Torbern Bergman had shown also to have acidic properties. To prove this relationship experimentally was, however, more difficult than for the other two cases, because the product was also an "air;" and there is no evidence that Lavoisier had done so at that time. Now, in 1783, he had to tackle the problem not only of establishing that relationship empirically, but of determining quantitatively the proportions between the charcoal consumed and the fixed air formed (18).

Lavoisier carried out the operations for the combustion of charcoal in a pneumatic apparatus similar to the one shown in figure 5. Although it appears simple in comparison to some of the apparatus that he had by this time employed, much like the pneumatic troughs that Priestley and other predecessors had used, this apparatus too bears refinements reflecting Lavoisier's

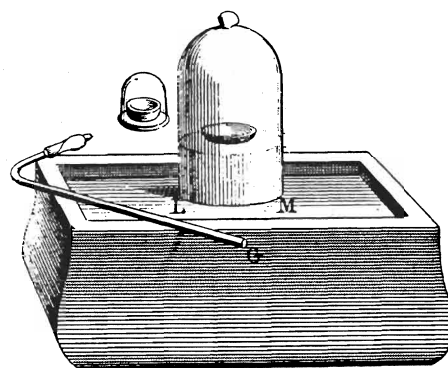


Figure 5. Simple pneumatic trough used by Lavoisier.

imaginative experimental craftsmanship.

In the experiment, Lavoisier filled the bell jar with oxygen, placed a weighed quantity of calcined charcoal in the dish, marked the level of the mercury on the side of the bell jar, ignited the charcoal, and after the apparatus had cooled, marked the level to which the mercury had risen. He then inserted caustic alkali in another dish to absorb the fixed air formed, marking the further rise of the mercury. Disassembling the apparatus, he afterward weighed the charcoal again to estimate the quantity consumed. In principle his method allowed him to determine the quantities of oxygen and carbon used and of the product, fixed air, thus permitting a total measured balance sheet of the chemical operation. In practice he encountered serious anomalies. The total decrease in volume would be expected to represent the oxygen consumed, whereas the decrease due to the caustic alkali would represent the fixed air evolved. By now he had better figures for the densities of both airs and, after correcting for the barometric pressure and temperature, he ought to be able to calculate reliable values for the weights of the two airs. There was, however, an unresolved ambiguity, because in respiration experiments there appeared to be no decrease in volume during the conversion of oxygen to fixed air, whereas in this experiment, supposed to represent the same process, there was a substantial diminution. Still more awkward for Lavoisier was that the total weight of the oxygen and charcoal consumed was greater than that of the fixed air produced by an amount equal to nearly one-third of the charcoal consumed. Lavoisier reported in his laboratory notebook rather dryly that "there seems to be a portion of the weight lost." When he wrote up the experiment for the memoir on heat that he and Laplace presented shortly afterward to the Academy of Sciences, Lavoisier glossed over these discrepancies. For his immediate purpose he did not require the complete balance sheet, and he did not include one in the paper. He merely calculated the quantity of fixed air formed in the combustion of one ounce of charcoal, as a basis for the further calculation of the equivalence of combustion and respiration. He was circumspect enough to state that "we can ... not be entirely sure of its

precision until we have repeated" the experiment several times. Here again the practical Lavoisier was willing to present publicly quantitative results he knew to be in some respects unsatisfactory, so long as they were adequate to his short-term objectives (19).

A year later such a result was no longer adequate to Lavoisier's needs, and he had moreover a new conceptual basis available to interpret in a more complex manner what was involved in the combustion of charcoal. Even as he and Laplace were making public their studies on heat in June 1783, they were participating in the momentous discovery that water can be synthesized by burning the very light air known then as "inflammable air" in oxygen in a closed container. The discovery that water is decomposable not only claimed much of Lavoisier's investigative attention during the following year, but cast new light on many of the experiments that he had previously carried out on the age-old assumption that water is one of the elements. One of the early reappraisals that Lavoisier made was of the experiment he had carried out ten years before on the reduction of lead that I described earlier. The small amount of water that had appeared in the receiver then, he could now explain in a very different way than he had done; the charcoal must have contained, in addition to carbon, a portion of inflammable air that combined with some of the oxygen in the lead ore to form the water (20).

During the spring of 1784 it became an urgent matter to determine accurately the proportions of carbon and oxygen in fixed air, because Lavoisier and his collaborator Meusnier needed that data in order to interpret an elaborate but flawed experiment they had conducted on the decomposition of water. They had passed water through an inclined gun barrel containing powdered charcoal and heated to incandescence. The carbon in the charcoal combined with the oxygen of the water to form fixed air, releasing inflammable air. The inflammable air and the fixed air collected in a pneumatic flask, but the experimental arrangement did not permit them to separate these airs, and they had moreover collected them over water, resulting in an unmeasurable loss of some of the fixed air into the water. In an effort to calculate indirectly the quantities they could not measure directly, Meusnier submitted the results to an enormously complicated analysis that required him to know the exact proportions of the carbon and oxygen in fixed air. Returning to the record of Lavoisier's single experiment on the combustion of charcoal of the previous spring, Meusnier treated the deficit that Lavoisier had then left unexplained as due to the combination of a small quantity of inflammable air contained in the charcoal with oxygen to form water. He was then able to calculate both the ratio of carbon to oxygen in fixed air and the composition of the charcoal as a combination of carbon and inflammable air (21).

Convinced by now that it was vital to determine the proportions both of inflammable air and oxygen in water and of the carbon and oxygen in fixed air "with rigor", by "comparing

together at one time the results of numerous experiments," Lavoisier set out in May, 1784, to multiply his experiments on the combustion of carbon. He tried several approaches. One was to burn charcoal that was so highly purified that he could regard it as containing no inflammable air. Another was to burn wax and to calculate all of the quantities, as Meusnier had done for his earlier experiments with charcoal. None of the several experiments that Lavoisier performed during the next month, or those that he retrieved from his earlier work, was unproblematic. Some of them yielded proportions of carbon to oxygen that diverged too far from his expectations for him to accept. In others there remained substantial deficits. The best result, he thought, was that obtained by burning wax; but since the calculation depended upon the theory of the composition of water that he thought some chemists were not ready to accept, he wished to base his result also on experiments that did not rely on that theory. Calculating and recalculating the proportions, he added "corrections" for such factors as suspected losses of the air, incomplete coolings and possible changes in the weight of the residual charcoal due to the absorption of moisture. By so astutely managing his data, he was able to make the results of each of his individually flawed experiments converge upon the ratio of 72 parts carbon to 28 parts oxygen, an outcome that happens to be remarkably close to the accepted modern value (22).

We could follow Lavoisier similarly through the even more challenging experimental problems that he encountered during the next four years when he took up the analysis of plant substances, such as wax, oils, alcohol and sugar, and as he then marshalled all of his accumulated experience to bring fermentation within the compass of his balance sheets and arrived in the process at the crucial concept of the chemical equation. To do so, however, would be to crowd too much into a short presentation, and I would like to pause instead for a few general reflections.

In his published papers Lavoisier habitually claimed to have carried out numerous experiments of whatever type he was describing, giving the impression that the few he reported in detail were selected from a much larger number. My experience comparing his publications with his laboratory notebooks has, however, persuaded me that this was rhetorical exaggeration, that he actually performed relatively few experiments that he did not in one way or another incorporate into his publications. Why then, if he were the consummate experimentalist that I believe he was, did he so regularly settle for one or several imperfect experiments on a given problem? Why did he go to such great lengths to salvage the data from the few he had performed rather than to repeat them until he had reduced or removed the sources of error? There is no definitive answer to these questions, but I am persuaded that the most likely reason is that these experiments were far more difficult to prepare and to perform, more time-consuming, and more expensive than they appear to us as we look back on them from

the distance of two centuries. It is easy to overlook the effort that it took to assemble apparatus that had to be luted together, the frequency with which pieces cracked, or leaks ruined the results, the difficulty of maintaining a steady temperature for several days or more by means of a charcoal fire, and numerous other obstacles.

Lavoisier was wealthy enough to spend a great deal on the apparatus he had constructed for his experiments and on the supplies required to sustain them, but his resources were not unlimited and the time he had available for laboratory work was even more restricted. Under the circumstances, the way in which Lavoisier proceeded was probably the most effective allocation he could make of his time and money. Had he persevered with each of the many experimental problems he took up until he had reached the best results he could hope to attain, he would never have been able to explore the broad scope of the investigative enterprise he had outlined for himself in 1773.

The second general point I wish to make is to reemphasize the pragmatic character of Lavoisier's investigative pathway. Although he glimpsed very early the potentially revolutionary nature of his initial discoveries in 1772 concerning the fixation and release of airs, and wrote out for himself the elements of a research program based on them which he pursued faithfully for 20 years, he could not foresee in detail where that program would lead him, nor define the general principles that he would eventually extract from the work he had done. His quantitative experimental methods, like his concepts, evolved as he went, became more tightly structured, more effective, and broader in their reach. He did not set for himself ideal goals of quantitative precision, but achieved at each stage sufficient accuracy to support the current state of his conceptual structure. When the problems he took up began to exceed the standards of his prior experimental practices, he did his best to improve his methods as far as he needed to in order to meet his more stringent requirements. Eventually he met problems so complicated that he was unable completely to resolve his experimental difficulties, but even then his efforts yielded insights of lasting value.

In the early stages of his prolonged investigative odyssey it was sufficient for Lavoisier to show that metals or combustible bodies combined with or released an air by demonstrating approximate correspondences between gains and losses of weights and increases or decreases in the volume of air in a pneumatic flask. By 1783, as the examples I have described indicate, he had reached the point at which it became important to determine accurately the combining proportions of the components of substances such as fixed air and water, and in the next years he extended this concern to plant materials. He did so not because he had derived a law of definite proportions from fundamental considerations, but because his immediate experimental problems required him to know these proportions. He made no effort to justify his implicit assumption that substances actually combine in definite proportions; he simply

set out to determine them.

To those who wish mainly to know whether Lavoisier was a reformer or a revolutionary; whether the essence of his revolution was the overthrow of phlogiston, the oxygen theory of acids, a new conception of the gaseous state or the reversal of accepted orders of comparison; whether he supplanted an existing chemistry with a new science or created a science where none had existed; whether he perceived himself as a physicist or a chemist; and to those who view the highlight of the Chemical Revolution as the new chemical language that linked Lavoisier with the broader currents of the French Enlightenment, to such people tracing the details of his experimental procedures as I have sought to do here may seem a narrow enterprise. It is, however, in my view, the foundation on which all else we can say about Lavoisier as a scientist must rest. Without the ongoing movement of the investigative enterprise that he pursued day-by-day in his laboratory and at his writing desk as he interpreted the results of his completed experiments or planned future ones, none of the great events surrounding him that we celebrate this year could have taken place.

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Dr. Frederic L. Holmes is a professor in the Section of the History of Medicine at Yale University, New Haven, CT, 06510 and is the author of "Claude Bernard and Animal Chemistry" and "Lavoisier and the Chemistry of Life".

INSTRUMENTS OF THE REVOLUTION: LAVOISIER'S APPARATUS

A. Truman Schwartz, *Macalester College*

The development of the new chemistry required the design and use of new apparatus. In this respect, Lavoisier's experimental *modus operandi* marks another departure from the procedures of his predecessors and contemporaries. In contrast, Joseph Priestley seems to have performed many of his experiments with conventional equipment - retorts, receivers, furnaces, and burning glasses. Indeed, the drawings of Priestley's equipment show something that looks like a common basin used as a pneumatic trough and ordinary wine glasses and jars used as his glassware. But Lavoisier's laboratory at the Arsenal of Paris was equipped with the products of some of Europe's finest instrument makers, much of it designed to the scientist's exacting specifications and constructed for a specific investigation (1).

A study of the instruments of Lavoisier's revolution is facilitated by the superb engravings that illustrate the *Traité Élémentaire de Chimie* and his other works. For the most part, they were based upon drawings made by Antoine's wife, Marie Paulze Lavoisier. This gifted woman's formal convent-based education had concluded shortly before her marriage, at the age of 13. Nevertheless, she played a major role in her husband's busy life - especially his scientific researches. She studied English and translated into French a number of important chemical works, including Kirwan's *Essay on Phlogiston* (1788). Following Antoine's death, she edited, published, and privately distributed his *Mémoires de Chimie* (1805) (2).

Mme. Lavoisier's natural talent for drawing, enhanced by her studies with David, are evident from her illustrations. Almost all the original sketches, drawings, and proofs have survived, so one can trace her method. She began with water-



Madame Lavoisier (Marie Anne Pierette Paulze) as a young girl.

color sketches and then copied these, in pencil, on squared paper corresponding in size to the desired plates. The pencil drawings were, in turn, transferred by stylus to the copper engraving plates. Like her husband, Mme. Lavoisier appears to have been a demanding perfectionist. Denis Duveen and Herbert Klickstein, in their bibliography of Lavoisier's works (3), report that a number of revisions were sometimes required before the proof warranted her stamp of approval - the word "Bonne" followed by her initials. It is also worth noting that Marie Lavoisier painted a portrait of Benjamin Franklin that greatly pleased the subject. Unfortunately, it is lost.

If nothing more than the plates to Lavoisier's works had survived, one could probably reconstruct his apparatus without much difficulty. But, somewhat surprisingly given the circumstances of his death, much of his equipment has actually been preserved. The Musée des Techniques of the Conservatoire National des Arts et Métiers has an extensive collection. Indeed, that rather dusty and sleepy institution is something of a sacred shrine for chemists. The museum, which includes a deconsecrated church, is an eclectic mixture of early airplanes and automobiles, clocks and watches, Jacquard looms, and a preliminary model of the Statue of Liberty. Its centerpiece is the Lavoisier exhibit.

In his biography of Lavoisier, Douglas McKie calls the laboratory at the Arsenal "remarkable." "Up to that time," he writes (4):

... there had been nothing to compare with it; and many years were to pass before such a collection of instruments, especially of precision instruments and chemical apparatus, would be put together again as the working tools of a laboratory - probably not until the rise of the

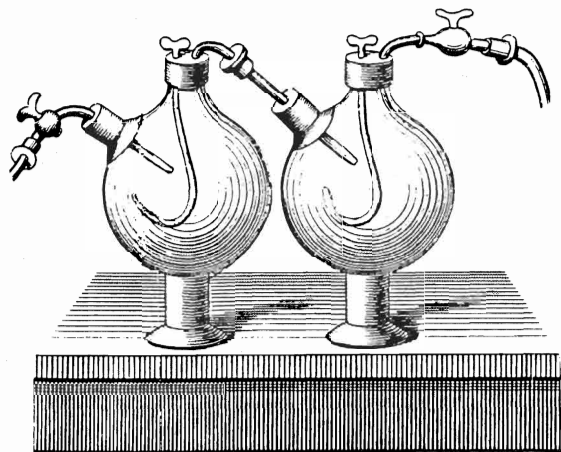
modern research institutions.

Maurice Daumas, in his studies of 18th century scientific apparatus, frequently comments on the state-of-the-art equipment made for and used by Lavoisier (5).

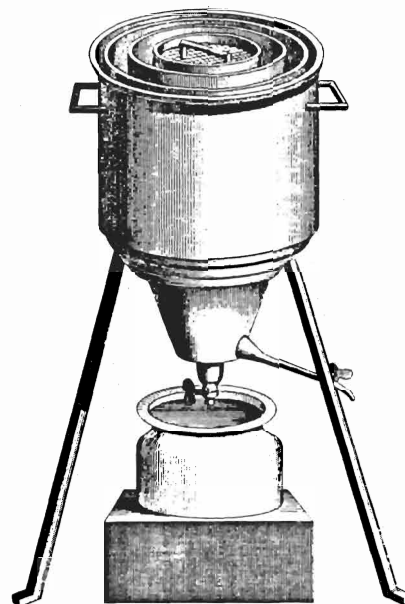
The fact that Lavoisier's inherited wealth and his income from the Ferme-Général and the Gunpowder Commission made him well-to-do is obvious from the care and craftsmanship devoted to the design and construction of his instruments. Many pieces were designed specifically for certain experiments, for example his calorimeters, gasometers, combustion bulbs and trains, and fermentation equipment (see figures in this article and on p. 25 and pp. 27-28). In his description of the gasometer, Lavoisier acknowledges the high costs associated with doing chemical research (6):

In the present advanced state of chemistry, very expensive and complicated instruments are become indispensably necessary for ascertaining the analysis and synthesis of bodies with the requisite precision as to quantity and proportion; it is certainly proper to endeavor to simplify these, and to render them less costly; but this ought by no means to be attempted at the expense of their conveniency of application, and much less of their accuracy.

The reliance that Lavoisier placed on physical measurements and the apparatus necessary to make them supports Arthur Donovan's thesis that the great French scientist was attempting to establish a new, more scientific chemistry based upon the model of experimental physics (7). Quantitative experimentation was the means, but theoretical knowledge was the end. As John McEvoy and others have pointed out, Lavoisier worked and thought algebraically (8). His experiments and his logic were intellectually, if not always symboli-



Gas absorption bulbs belonging to a special apparatus for the combustion analysis of oils constructed for Lavoisier by Fortin in 1788. The bulbs are now in the Musée des Techniques, Conservatoire National des Arts et Métre in Paris.



The ice calorimeter used by Lavoisier and Laplace as pictured in the *Traité*.

cally, based on equations. Hence the importance of reliable, accurate, and precise instruments.

Lavoisier's favorite instrument-makers were Nicolas Fortin and Pierre Mégnié (Mégnié the Younger), but McKie's 1952 inventory of 418 items lists apparatus made by 28 others, including Adams, Beringer, Chevalier, Ciceri, Delabarre, Le Maire, Ramsden, and Richer.

After Lavoisier's execution, his home at 243 Boulevard de la Madeleine de la Ville l' Evêque was visited by large numbers of representatives of the various commissions and committees on which Lavoisier had served. They were all eager to recover property belonging to these groups. A number of inventories were drawn up, and the Lavoisier bicentennial exhibit at the Palais de la Découverte included four. Two chemists, Nicholas Leblanc and Claude Louis Berthollet, began their inventory of the chemical apparatus on 15 June, but broke off shortly after Mme. Lavoisier was arrested on that day. They resumed their work in September, following her release in August. Altogether they listed over 13,000 items of chemical apparatus and specimens, valued at more than 7,000 livres. Included were 170 pounds of mercury and 60 pounds of mercuric oxide, valued at 2,030 livres. Jacques Charles (of hydrogen balloon fame), Fortin (the instrument maker), and Lenoir prepared an inventory of about 250 instruments and pieces of physical apparatus, including three balances (see figures on pp. 21-22), which were valued at 3,500 livres. An interesting footnote is that one of the inventories was attested to by Dr. Joseph Guillotin who, in 1784, had served with Lavoisier on the commission investigating mesmerism.

Lavoisier's apparatus, chemicals, books, papers, and furni-

ture, including his roll-top desk, were confiscated and divided among the École Centrale, the Museum of Natural History, and the Bureau of Mines. The fact that they were returned within less than a year probably saved them from being widely scattered and lost. Again, the records seem to have been remarkably thorough and the restoration essentially complete. For example, of the books taken from Lavoisier's country estate in Fréchines, only three volumes were missing. It is unfortunate that the republican government of France did not show comparable concern for preserving human resources, but, as Coffenhal supposedly said in delivering his verdict, "La République n'a pas besoin des savants."

The trail of Lavoisier's instruments breaks, at least for me, on their return to his widow. She, of course, survived her brief and rocky marriage to Count Rumford and lived until 1836. Since she and Antoine had no children, and since his only sibling died without issue at 15, Lavoisier's direct line came to an end. However, Lavoisier's effects were apparently passed on to members of Marie's family. The apparatus surfaces again in the 20th century in the possession of Madame la Comtesse de Chazelle of Champaissant in the department of Sarthe. She is identified as a descendant of Mme. Lavoisier and the owner of most of the books, papers, and apparatus exhibited in 1943 at the Palais de la Découverte to commemorate the bicentennial of Lavoisier's birth. This was, of course, during the German occupation of France, but the catalogue suggests a carefully planned and complete exposition.

Many of the same pieces again appear on an inventory of 418 items belonging to Madame de Chazelles prepared in 1952 by Douglas McKie at the request of Pierre Samuel du Pont. This list, kindly provided to me by the Hagley Museum and Library of Wilmington, Delaware, includes, in addition to many of the major items shown earlier, many thermometers, barometers, weights, and pieces of electrical equipment. McKie

concludes that several of the items, among them a plaster seal of the Royal Institution of Great Britain and two freemason's aprons, must have belonged to Rumford.

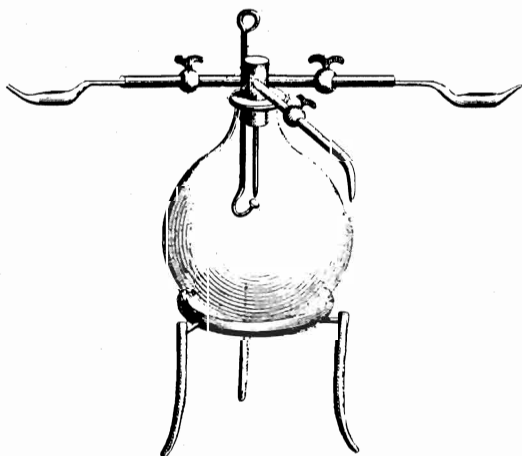
The inventory was undertaken preliminary to the purchase of the collection by du Pont for presentation to the Conservatoire National des Arts et Métiers. A letter from du Pont to McKie, dated 15 July 1952, reports that the final payment to Madame de Chazelle had just been sent. "This settlement," du Pont continues, "will, I believe, restore Madame's confidence and good humor." Alas, there is no elaboration of this intriguing morsel. The benefactor proves modest in his final paragraph (9):

In some of the correspondence in regard to the collection, it has been intimated that it was proposed to confer upon me some recognition by the Legion of Honor or other society. This I feel is quite unnecessary, as the satisfaction in having the collection preserved in a safe place is quite sufficient award for me.

There is a fitting symmetry in the involvement of the du Pont family in seeing Lavoisier's apparatus delivered to the people of France. An ancestor of the benefactor, who also bore the name Pierre Samuel, was a close friend of Lavoisier. A widower, he unsuccessfully proposed marriage to Marie Lavoisier before he emigrated to America in 1800. With him went his two sons, Victor and Eleuthère Irénée. The latter had spent five years with Lavoisier at the Arsenal, helping with the research and learning the art and science of gunpowder manufacture. The success of the gunpowder factory E. I. du Pont founded on the banks of the Brandywine River in Wilmington, Delaware, suggests that he learned well. The revolution launched by Lavoisier's intelligence, insight, and instrumentation had come to the United States with the du Ponts.

References and Notes

1. The author wishes to thank Ms. Maureen O'Brien Quimby, Curator of Collections, and her colleagues at the Hagley Museum, Wilmington, Delaware, for supplying important documents, and Professor Mary Lou Wolsey, Department of French, College of St. Thomas, St. Paul, Minnesota, for assistance in the translation and interpretation of certain passages.
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A combustion bulb with electrodes for studying the combination of hydrogen and oxygen under a constant flow of the reacting gases.

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Dr. A. Truman Schwartz is Professor of Chemistry at Macalester College, 1600 Grand Avenue, Saint Paul, MN 55105. His original talk was accompanied by a set of beautiful color slides of Lavoisier's apparatus taken at the Musée des Techniques, Conservatoire National des Arts et Métre in Paris, which we are unfortunately unable to reproduce within the limits of the Bulletin's current format.

BOOKS OF THE CHEMICAL REVOLUTION

Part III: *Traité Élémentaire de Chimie*

Ben B. Chastain, Samford University

The revolution of modern chemistry was a process, not an event. There is no Bastille Day to point to as the seminal occurrence from which it grew. The fact that 1989 has been the year chosen for its bicentennial celebration is in no small part due to the publication in March 1789, in Paris, of the *Traité Élémentaire de Chimie* by Antoine Lavoisier. It is certainly the most widely known "book of the revolution". Professor Douglas McKie, among others, has claimed that Lavoisier's *Traité* did for chemistry what Newton's *Principia* had done for physics a century before (1).

Earlier papers in this series presented the *Méthode de Nomenclature Chimique* of 1787 as the lexicon of the revolution; and the third edition of Fourcroy's *Éléments d'Histoire Naturelle et de Chimie*, published in December 1788, as its first textbook (2). By the spring of 1789 the *Méthode* had already been translated into English and Spanish, and at least summarized in Italian; the earlier editions of the *Éléments* had also been widely disseminated, and the new material of the third edition had already appeared in English as well as French. These two books, then, were at work spreading the revolution when the *Traité* appeared on the scene. This paper will briefly discuss this third book, and its relationship to the previous two.

We should recall that Lavoisier's contribution to the 1787 *Méthode* consisted of the text of a paper which he had presented in April of that year to a public meeting of the French

T R A I T É É L É M E N T A I R E D E C H I M I E,

PRÉSENTÉ DANS UN ORDRE NOUVEAU

ET D'APRÈS LES DÉCOUVERTES MODERNES;

Avec Figures :

Par M. LAVOISIER, de l'Académie des Sciences, de la Société Royale de Médecine, des Sociétés d'Agriculture de Paris & d'Orléans, de la Société Royale de Londres, de l'Institut de Bologne, de la Société Helvétique de Basle, de celles de Philadelphie, Harlem, Manchester, l'Indoue, &c.



A P A R I S,

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M. D C C. L X X X I X.

Sous le Privilège de l'Académie des Sciences & de la Société Royale de Médecine.

Academy of Sciences. In it, he gave the background of the suggested reforms, and credited those in the past who had worked on nomenclature, including Macquer in France for his 1766 *Dictionnaire de Chimie*, Bergman in Sweden for his 1784 scheme for classifying and naming minerals, and especially Guyton de Morveau, whose 1782 paper formed the basis for the new system. Lavoisier praised Guyton for his willingness to sacrifice his own ideas and previous work to the present collaboration. He described the conferences of the four authors, conferences which ranged over the whole of chemistry as well as the metaphysics of language, as being quite free of personal considerations. The rest of the paper dealt mainly with the ideas of the Abbé Bonnot de Condillac on the importance of language, with quotes such as "We only reason well or reason badly in so far as our language is well or badly constructed ..." and "The progress of the sciences depends entirely on the progress of their languages".

According to the preface to the *Traité*, it was Lavoisier's intention only to "extend and explain" this paper on nomenclature when he began the work which grew into the *Traité Élémentaire de Chimie, présente dans un ordre nouveau et d'après les découvertes modernes* (3). His extension and explanation became a book which might be described as both a manifesto for the Chemical Revolution and a manual for new revolutionaries.

A manifesto is a public declaration, made by a person or

group claiming important status or taking important actions, which sets forth the reasons, motives, or objects of their claim. Indeed, Lavoisier had written in his notebooks in 1773 that his researches seemed "destined to bring about a revolution in physics and in chemistry", and perhaps the *Traité* can be seen as his public declaration that the revolution was taking place and the new chemistry was overthrowing the old. Especially in the Preface and in the first section of the book, he sets forth the motivation and approach which led to the revolution and details the steps taken to bring it about.

In the Preface we find this commentary on the "study and practice of the sciences" (pp. xvii-xviii):

Imagination, which is ever wandering beyond the bounds of truth, joined to self-love and that self-confidence we are so apt to indulge, prompt us to draw conclusions which are not immediately derived from facts; so that we become in some measure interested in deceiving ourselves. Hence it is by no means to be wondered that, in the science of physics in general, men have often made suppositions, instead of forming conclusions. These suppositions, handed down from one age to another, acquire additional weight from the authorities by which they are supported, till at last they are received, even by men of genius, as fundamental truths. The only method of preventing such errors taking place, and of correcting them when formed, is to restrain and simplify our reasoning as much as possible ... We must trust to nothing but facts; these are presented to us by Nature, and cannot deceive ... I have imposed upon myself, as a law, never to advance but from what is known to what is unknown; never to form any conclusion which is not an immediate consequence necessarily flowing from observation and experiment ...

Here, then, is a basic principle of the approach to be taken in



Antoine Laurent Lavoisier

modern chemistry and a manifesto-like statement.

Parenthetically, let us note that in this same section he says:

It ought to be considered that very little of chemistry can be learned in a first course, which is hardly sufficient to make the language of the science familiar to the ears, or the apparatus familiar to the eyes. It is almost impossible to become a chemist in less than three or four years of constant application.

And that's 200 years ago, with no remedial work or general liberal arts curriculum!

Another important principle of the new chemistry is also stated in the preface (p. xxiv):

All that can be said upon the number and nature of elements is, in my opinion, confined to discussions entirely of a metaphysical nature ... If, by the term elements, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable we know nothing at all about them; but, if we apply the term elements, or principles of bodies, to express our idea of the last point which analysis is capable of reaching, we must admit as elements all the substances into which we are capable, by any means, to reduce bodies by decomposition.

It is the application of this principle which leads to the "Table of Simple Substances" found at the beginning of the second part of the book (p. 175), frequently called the first list of the elements, and cited by Douglas McKie in his biography of Lavoisier as "the most revolutionary feature of the *Traité*."

One more quote from the preface, dealing with part one (pp. xxxiii-xxxiv):

... chemists will easily perceive that, in the first part of my work, I make very little use of any experiments but those which were made by myself: If at any time I have adopted, without acknowledgement, the experiments or the opinions of M. Berthollet, M. Fourcroy, M. de la Place, M. Monge, or, in general, of any of those whose principles are the same with my own, it is owing to this circumstance, that frequent intercourse, and the habit of communicating our ideas, our observations, and our way of thinking to each other, has established between us a sort of community of opinions, in which it is often difficult for every one to know his own.

This may have some bearing on the later insistence of Lavoisier that the new theory was his alone, and not that of "the French chemists" as was said by some (4).

The first part of the *Traité*, entitled "Of the Formation and Decomposition of Aeriform Fluids, - of the Combustion of Simple Bodies, and the Formation of Acids" is in effect a summary of the researches of Lavoisier (and perhaps some others) over the period 1773-1788, arranged in such an order "as shall render it most easy for beginners in the study of chemistry thoroughly to understand them" (p. xviii).

It describes in detail many experiments which reveal the composition of the atmosphere, of water, and of acids and bases. In a chapter on vinous fermentation we find the first application of the law of conservation of matter to chemical changes (p. 130):

We may lay it down as an incontestible axiom that, in all the operations of art and Nature, nothing is created; an equal quantity of matter exists both before and after the experiment; the quality and quantity of the elements remain precisely the same; and nothing takes place beyond changes and modifications in the combination of these elements. Upon this principle the whole art of performing chemical experiments depends ...

Later in the same chapter he points to a consequence of this principle, the chemical equation (p. 140):

We may consider the substances submitted to fermentation, and the products resulting from that operation, as forming an algebraic equation; and, by successively supposing each of the elements in this equation unknown, we can calculate their values in succession, and thus verify our experiments by calculation, and our calculation by experiments reciprocally.

This first part, then, comprises the essentials of the new chemistry, and, taken with the preface, might be considered the manifesto of the revolution, in the words of the premier revolutionary.

The middle section of the *Traité*, entitled "Of the Combinations of Acids with Salifiable Bases, and of the Formation of Neutral Salts", contains little that is new, and in Lavoisier's own words "nothing which I can call my own". It is chiefly tables of the new nomenclature for salts, and the acids and bases from which they are made. Its most noted feature is the aforementioned "Table of Simple Substances" (p. 175). It is shorter than the corresponding table in the 1787 *Méthode*, lacking the list of organic radicals (which Lavoisier had decided were made of carbon and hydrogen.) It contains 33 items, 23 of which we still consider elements - 17 metals, oxygen, hydrogen, azote (nitrogen), sulphur, phosphorus, and charcoal (carbon). Also listed are light and caloric (heat), which Lavoisier still felt to be substances; three acid radicals - muriatic, fluoric, and boracic (derived from chlorine, fluorine, boron); and five "earthy substances" - lime, magnesia, barytes, argill (alumina), and silex. Once again the author cautions the reader (p. 177):

... these things we at present suppose simple may soon be found quite otherwise. All we dare venture to affirm of any substance is, that it must be considered as simple in the present state of our knowledge ... We may even presume that the earths must soon cease to be considered as simple bodies; they are the only bodies of the salifiable class which have no tendency to unite with oxygen; and I am much inclined

to believe that this proceeds from their being already saturated with that element. If so, they will fall to be considered as compounds consisting of simple substances, perhaps metallic, oxydated to a certain degree.

He also notes that "the fixed alkalies, potash and soda, are omitted in the foregoing Table, because they are evidently compound substances, though we are ignorant as yet what are the elements they are composed of" (p. 178). This table, with its caveats, perhaps deserves to be regarded as the first list of the chemical elements.

The third section of the *Traité* is what I have chosen to call the manual of the revolution, instructions for do-it-yourself "new chemistry". Its title is "Description of the Instruments and Operations of Chemistry"; its purpose is explained in the Preface (p. xxxv):

The method of performing experiments, and particularly those of modern chemistry, is not so generally known as it ought to be; and had I, in the different memoirs which I have presented to the Academy, been more particular in the details of the manipulations of my experiments, it is probable I should have made myself better understood, and the science might have made a more rapid progress.

He adds: "I need hardly mention that this part could not be borrowed from any other work, and that, in the principal articles it contains, I could not derive assistance from anything but the experiments which I have made myself."

The plates, originally drawn by Mme. Lavoisier, are beautifully detailed; comparison with some of the actual pieces of apparatus (which can still be seen at the Musée des Techniques in Paris) attest to their accuracy. One interested in doing so should have been able to construct such apparatus and reproduce the results given in the book. As aids to this end, appendices give various unit conversions, densities of several gases, and specific gravities for a large number of substances.

As has been pointed out by several writers, Lavoisier's *Traité* is really not a textbook for beginners in chemistry (4); its limited scope and research approach make it less valuable in that regard than Fourcroy's *Éléments* or others that came along - Chaptal's, for instance (5). Nevertheless, it was reprinted a number of times in France, and translated into many other languages. Its place, and its author's, in the history of chemistry are quite secure. It has been for these 200 years emblematic of the revolution of modern chemistry. But in promoting the spread of that revolution, it shares credit with at least two other influential volumes, and joins them on my "Revolutionary Bookshelf". There we find the lexicon of the revolution, its first textbook, and its manifesto/manual - worthy to be remembered and celebrated by chemists in this bicentennial year or, for that matter, in any other year.

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Dr. Ben Chastain is Chairman of the Department of Chemistry of Samford University, Birmingham, AL 35229 and is interested in the great books of chemistry. This article completes his three-part series on the "Books of the Chemical Revolution."

THE INGENIOUS, LIVELY AND CELEBRATED MRS. FULHAME AND THE DYER'S HAND

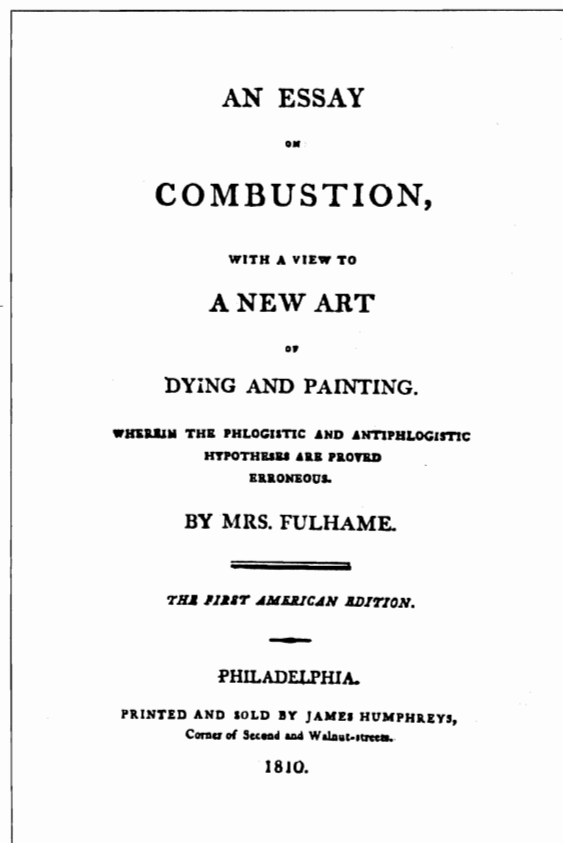
Derek A. Davenport and Kathleen M. Ireland, Purdue University

Recent revisionist scholarship still allows that 1794 was a crucial year for both Antoine Lavoisier and Joseph Priestley (1). It was also a crucial year for the less often celebrated Mrs. Fulhame - the intermittent labors of close to 14 years culminated in the publication of her *Essay on Combustion with a view to a New Art of Dying and Painting wherein the Phlogistic and Antiphlogistic Hypotheses are Proved Erroneous* (2). The *Essay* was to prove Mrs. Fulhame's only publication and what little we know of her must be inferred from the idiosyncratic preface and from the few personal references in the body of the book. As with Shakespeare's sonnets and their elusive lady:

*My nature is subdued
To what it works in, like the dyer's hand;
Pity me then and wish I were renewed.*

Not that the book was to pass unnoticed. The normally ungenerous Count Rumford conceded (3):

This agrees perfectly with the results of similar experiments by the ingenious and lively Mrs. Fulhame. It was on reading her book that



First American edition of Mrs. Fulhame's book.

I was induced to engage in these investigations; and it was by her experiments that most of the foregoing experiments were suggested.

while the normally charitable Priestley grumped (4):

... her theory is fanciful, and fabulous, as the story of the phenix itself.

a quotation to which J. R. Partington added the even less charitable and quite gratuitous footnote (5):

The phoenix, it may be noted, was a fabulous bird regarded as sexless.

The *Essay* received several reviews in French journals, one, by Coindet, running to 27 pages with detailed chapter-by-chapter summaries (6). A German translation appeared in 1798 and, as we shall see, an American edition in 1810.

The genesis of the book is described in the preface:

The possibility of making cloths of gold, silver, and other metals by chymical processes, occurred to me in the year 1780; the project being mentioned to Doctor Fulhame and some friends, was deemed improbable. However, after some time, I had the satisfaction of realizing the idea in some degree by experiment.

Though I was, after some considerable time, able to make small bits of cloth of gold and silver, yet I did not think them worthy of public attention; but by persevering, I at length succeeded in making pieces of gold cloth, as large as my finances would admit.

Some time after this period, I found the invention was applicable to painting, and would also contribute to facilitate the study of geography; for I have applied it to some maps, the rivers of which I represented in silver, and the cities in gold. The rivers appearing as it were in silver streams, have a most pleasing effect on the sight, and relieve the eye of that painful search for the course and origin of rivers...

So far it looks a suitably domestic hobby for the wife of a doctor of modest means, particularly one who describes herself as "averse from indolence, and having much leisure."

But this particular author was no ordinary housewife. Halfway through the preface the tone abruptly changes from insecurity to near bellicosity:

As to patrons, I have heard of such beings on the record of fame, but never saw one; on the contrary, it has been my lot to know of many whose malignant breath, as far as its deadly influence can extend, never ceases to blast the unsheltered blossoms of science. And as for a patent, had I even the means I should perhaps never attempt it; for if we may judge of the future by the past, I can safely affirm, that such an application would be vain.

Thus circumstanced, I published this essay in its present imperfect state, in order to prevent the furious attempts of the prowling plagiarist, and the insidious pretender to chymistry, from arrogating to themselves and assuming my invention in plundering silence; for there are those, who if they cannot by chymical, never fail by stratagem and mechanical means, to deprive industry of the fruits and fame of her labours.

Quickly the preface modulates to a rousing feminist diatribe:

It may appear presuming to *some*, that I should engage in pursuits of this nature; but averse from indolence, and having much leisure, my mind led me to this mode of amusement, which I found entertaining and will I hope be thought inoffensive by the liberal and the learned. But censure is perhaps inevitable; for some are so ignorant, that they grow sullen and silent, and are chilled with horror at the sight of any thing that bears the semblance of learning, in whatever shape it may appear; and should the *spectre* appear in the shape of *woman*, the pangs which they suffer are truly dismal.

There are others who suffer the same torture in a still higher degree; but by virtue of an *old inspiring tripod*, on which ignorance, servility, or chance, has placed them, assume a dictatorship in science, and fancying their rights and prerogatives invaded, swell with rage and are suddenly seized with a violent and irresistible desire of revenge, manifesting itself by innuendos, nods, whispers, sneers, grins, grimace, satanic smiles, and witticisms uttered sometimes in the acute, and sometimes in the nasal obtuse twang, with an affected hauteur, and

contempt of the *spectre*; shrugs, and a variety of other contortions attending.

Sometimes the goblin, which thus agitates them lurks latent, and nothing is perceived but hollow murmurs, portending storms: sometimes the lurking fiend darts with sidelong fury at the devoted object, which, if unarmed, falls a victim to the grisly monster.

But happily for human kind, the *magic tripod* drags none into its dizzy vortex, but those who are radically stupid and malicious, who are the beasts of prey destined to hunt down unprotected genius, to stain the page of biography, or to rot unnoted in the grave of oblivion.

But happen what may, I hope I shall never experience such desertion of mind, as not to hold the helm with becoming fortitude against the storm raised by ignorance, petulant arrogance, and privileged dulness.

With the marvellous phrase "ignorance, petulant arrogance, and privileged dulness" the storm subsides as suddenly as it began. To this point the Preface might seem to belong more to the history of feminism than to the history of chemistry. It is no doubt significant that the book was sold by Joseph Johnson, the famous liberal publisher of Thomas Paine, Richard Price, Joseph Priestley and Mary Wollstonecraft. Johnson's literary luncheons were famous, and it is tempting to imagine Mrs. Fulham meeting the author of *A Vindication of the Rights of Woman* at one of these. And it is surely to Priestley that she refers in the passage:

But the British empire should not forget, that she owes her power and greatness to commerce; that she is, as it were, the hive of the arts, and should not, by the sulphureous vapour of oppression and neglect, compel her bees to swarm for protection to foreign climes, but rather permit them to roam in their native soil, and allow them, in the winter of life, to sip a little of the honey of their own industry.

It is, however, the last paragraph of the Preface that assures Mrs. Fulham's *Essay* its modest place in the history of chemistry:

Finding, the experiments could not be explained on any theory hitherto advanced, I was led to form an opinion different from that of M. Lavoisier, and other great names. Persuaded that we are not to be deterred from the investigation of truth by any authority however great, and that every opinion must stand or fall by its own merits, I venture with diffidence to offer mine to the world, willing to relinquish it, as soon as a more rational appears.

November 5th, 1794

With diffidence? November 5th is, of course, Guy Fawkes' Day and in Mrs. Fulham's skeptical view both M. Lavoisier and "Gunpowder Joe" Priestley were due for their comeuppance.

Whatever had been the modest promptings for her experiments, by the time she came to publish the *Essay on Combustion*

Mrs. Fulhame had become at least as much interested in chemical theory as she was in the production of "cloths of gold". The introduction, with its summaries of Beccker (sic), Stahl, Lavoisier, Macquer, Scheele, and Kirwan reveals a well-versed author, though the only source cited is the second edition of Kirwan's *Essay on Phlogiston*. There is no mention of Kerr's translation of Lavoisier's *Traité* that had appeared several years earlier.

While she leans to the views of Lavoisier and the anti-phlogistonists she by no means follows the party line:

... the antiphlogistic account of calcination and reduction is no less complex, erroneous, and repugnant to the simplicity of nature: for when we consider the various sources, whence they derive the oxygen, which oxygenates bodies; and the long list of metallic reducers, which they suppose; it must be allowed, that if simplicity be a recommendation, the hypothesis is destitute of advantage.

The principal basis for her criticism lay in her firmly held belief that "the hydrogen of water is the only substance, that restores oxygenated bodies to their combustible state; and that water is the only source of the oxygen, which oxygenates combustible bodies." These principles she felt she had established by experiment.

The main part of the *Essay* describes these numerous, meticulous, and numbingly tedious experiments. We can vouch for their tediousness since we have repeated quite a number of them with results approximating hers. Where differences occur they may be attributed to the indeterminate purity of many of Mrs. Fulhame's reagents rather than to her (or our) lack of skill and scrupulosity. The nature of the experiments is well described in the teutonic title of the German translation of her book published in Göttingen in 1798: *Versuche über die Wiederherstellung der Metalle durch Wasserstoffgas, Phosphor, Schwefel, Schwefelleber, geschwefeltes Wasserstoffgas, gephosphortes Wasserstoffgas, Kohle, Licht und Sauren* (7). In general she exposed metallic salts in aqueous solution, in the dry state, and occasionally in ether or alcohol solution to the action of the various reducing agents. Her apparatus was generally of Priestleyan simplicity, though occasional recourse was made to the apparatus of Nooth and others.

It would be an act both of supererogation and of penance to summarize all her experimental findings. Rather we will state and comment on her own "Conclusion(s)" as listed in the final chapter of the book:

1. Neither the Phlogistians, nor Antiphlogistians, account in a satisfactory manner for the increase of weight, which bodies acquire during combustion.

There are no quantitative data in the *Essay* so Mrs. Fulhame's objections are qualitative. They are hard to fathom and would

seem largely prejudicial. One has already been mentioned: the putative complexity of Lavoisier's explanations. A second objection is based on Priestley's and Kirwan's conviction that "since the dryest oxygen gas contains a large proportion of water, ... and since the whole of the gas, except the caloric, and light, is absorbed, it necessarily follows, that the increase in weight which bodies acquire during combustion, depends not only on the oxygen, but also on the water, contained in vital air".

2. Their account of the formation of water, acids, and oxids, is erroneous; for it has been shown that the oxygen of water alone oxygenates combustible bodies.

Mrs. Fulhame showed experimentally that water was essential to most of the reductions she studied. Little reaction occurred in the dry state and even when the "ultimate particles" were separated in ethereal and alcoholic solution reaction remained slow or negligible as compared with the reaction in aqueous solution. She attributed any marginal activity in ether and alcohol to the difficulty of getting the solvents water-free. From the seeming necessity of the presence of water to the singular and necessary intermediacy of water was, for her, a short and logical step. We shall return to this in conclusion 5.

3. Combustible bodies, as hydrogen, phosphorus, sulphur, charcoal, light, etc. are capable of reducing the metals in the ordinary temperature of the atmosphere; and indeed I might add, at a much lower temperature, as I frequently experienced.

This was by far the most important of Mrs. Fulhame's generalizations as was pointed out by J. W. Mellor in one of the very few subsequent commentaries on her work (8). Previously, such reductions had tended to be carried out at high temperatures, often in a furnace. By means of several hundred qualitative experiments she showed that many metal ions in aqueous solution could be reduced at or near room temperature. Not surprisingly most of the metal salts reduced were those of metals with either positive or small negative reduction potentials. It is highly unlikely that zinc or even iron could be reduced by hydrogen at neutral pH and we must ascribe Mrs. Fulhame's observations to impurities in the metals, the hydrogen, or the silk. In a number of cases she made the acute observation that several of the reduced metals disappeared on exposure to air - one of the earliest examples of the dynamic nature of oxidation and reduction. On another occasion she encountered and explains correctly the passivation of iron. Not surprisingly, she was fascinated by the sequences of colors observed in the reductions and ascribed them to decreasing degrees of oxidation. She even speculated that these intermediate degrees of oxidation might be related to the colours metal ions give to glass.

4. Combustible bodies do not reduce the metals by giving them phlogiston, as the Phlogistians suppose; nor by uniting with, and separating their oxygen, as the Antiphlogistians maintain.

This would appear to be a general dissent while that in conclusion 1 was restricted to matters of weight.

5. Water is essential both to the reduction and oxygenation of bodies, and is always decomposed in these operations.

Many late 18th century chemists seemed reluctant to relinquish the idea that air, earth, fire, and water were, in some fundamental way, elemental. Even after Cavendish, Watt and Lavoisier had established the compound nature of water around 1783, many of them were loath to admit water as a mere chemical compound like any other. Or, to speak anachronistically, that oxygen pulled on its hydrogen atoms one bond at a time. We have seen how Priestley and Kirwan had claimed that even completely dry dephlogisticated air still contained water. Others "saw" water lurking in all sorts of strange and unverifiable places. Mrs. Fulhame is virtually obsessed with the idea that water played the central role in her oxidations and reductions and, by extension, in calcination and reduction in general. Throughout the book she insists on the mechanistic intermediacy of water: the metal [ion] gave its "oxygen" to the hydrogen



A Nooth apparatus used by Mrs Fulhame in her experiments. Originally made to carbonate water, gases chemically generated in the lower chamber are bubbled through the liquid in the upper chamber, where they can react with the liquid or with various solutes.

contained in the water while at the same time the hydrogen gas was combining with its incipiently deprived oxygen. She describes this process as being due to the operation of a double, as opposed to the more common single, affinity. At one point she seems to claim that two quantities of water appear where only one existed before, but the logic of her arithmetic escapes the modern eye. Furthermore she herself seems to implicitly recant this bizarre notion in conclusion 8 below. Her basic concept is clearly a mechanistic/catalytic one and, as J. W. Mellor has observed, well ahead of its time (8).

6. Water does not contribute to metallic reduction merely by dissolving and minutely dividing the particles of metallic salts, and thus removing the impediment opposed to chymical attraction by the attraction of cohesion: for were this the case, metallic solutions in ether and alcohol, in which that impediment is equally removed, should be as readily and effectually reduced, as metallic solutions in water are.

This conclusion is self-explanatory.

7. When one body is oxygenated, another, at least, is restored at the same time to its combustible state; and v.v. when one body is restored to its combustible state, another at least is at the same time oxygenated.

The language and the conclusion, taken in isolation, are pure anti-phlogistonism.

8. Quantities of air, and water, equal to those decomposed in the different species of combustion, are constantly forming. Thus nature, by maintaining this balance of power between combustible and oxygenated bodies, prevents the return of original chaos.

When Joseph Priestley first stumbled on the ability of a sprig of mint to revitalize "injured air" in 1772, he was quick to grasp its importance in maintaining Nature's balance. His subsequent work tended, not untypically, to cloud the issue (perhaps literally) and it was not until 1780 when he and Jan Ingenhousz shared "the same summer and the same sun" that the essential chemical component of the photosynthetic cycle was put on a firm footing. Mrs. Fulhame frequently comments on the reversibility of her reductions and emphasizes their complementarity. In the closing paragraph of her *Essay* she uses the word equilibrium in a surprisingly modern way and ends with the fine, if maligned, image of the phoenix rising from her ashes:

This view of combustion may serve to show how nature is always the same, and maintains her equilibrium by preserving the same quantities of air and water on the surface of the globe; for as fast as these are consumed in the various processes of combustion, equal quantities are formed, and rise regenerated like the Phenix from her ashes.

An American edition of Mrs. Fulhame's Essay was brought out in Philadelphia in 1810 (9). Its chief additional interest lies in a curious "Advertisement" by the American Editor:

The interesting contents of the subsequent pages, by the very ingenious Mrs. Fulhame, are assuredly deserving of more attention, than they have hitherto received; for although published so far back as the year 1794, little notice has been paid to the numerous experiments, by which she has opposed the doctrines of combustion, &c. advanced by the respective advocates of the phlogistic and antiphlogistic theories. How successfully she has executed this, must be left to the candid inquirer after truth, who, provided the end be attained, does not stop to consider from what source it is derived.

Whether it be that the pride of science, revolted at the idea of being taught by a female, I know not; but assuredly, the accomplished author of this essay, has sufficiently evinced the adequacy of her requirements, in the promulgation of opinions subversive of a part of the highly esteemed edifice, raised by the efforts of Lavoisier and others.

The anonymous Editor goes on to lament the near oblivion into which Mrs. Fulhame's book had fallen in England and hoped for a better fate for it "in this favoured land, where freedom of inquiry is so sedulously cherished". He was to prove a poor prophet. The advertisement concludes:

I cannot doubt the justice of the opinions deduced by Mrs. Fulhame from her numerous and well conducted experiments: and although it may be grating to many, to suppose a female capable of successfully opposing the opinions of some of our fathers in science; yet reflection will serve to satisfy the mind devoted to truth, that she has certainly thrown a stumbling block of no small magnitude, in the way of sentiments we have been taught to consider as sacred.

PHILADELPHIA,
February 14th, 1810.

As with the earlier Guy Fawkes' Day dateline, one wonders if February 14th were a mere coincidence.

Who was this anonymous, informed, opinionated, articulate, non-male-chauvinistic American Editor? James Woodhouse, who spoke approvingly of the celebrated Mrs. Fulhame, is one possibility. A more likely one is Thomas Cooper (10, 11). He was certainly opinionated, articulate and widely, even extravagantly, informed. Cooper had once run a bleaching and dyeing works in Manchester, and he was to compile and edit *A Practical Treatise on Dyeing and Callicoe Printing* published in Philadelphia in 1815. This was only one of several semi-hack publications on topics such as cookery, gas lights, lunatic asylums, etc., that Cooper published in the period 1808-1820. More substantial were his editions of Jane Marcet's *Conversations on Chemistry* and Thomas Thomson's *System of Chemistry* and his editorship of the *Emporium of Arts and Sciences*. Furthermore, Cooper had been living in Joseph Priestley's house in Northumberland at about the time Pries-

tley was writing the first edition of his *The Doctrine of Phlogiston Established*, with its somewhat condescending opinion of Mrs. Fulhame's theories. Most importantly, in 1792 Thomas Cooper had also been published by Joseph Johnson shortly before he was to bring out Mrs. Fulhame's *Essay*. Cooper's *A Reply to Mr. Burke's Invective* is informed, opinionated, fearfully articulate, and at times explicitly non-male-chauvinistic (12):

... I have repeatedly considered the subject of the *Rights of Women*, and I am perfectly unable to suggest any Argument in support of the political Superiority so generally arrogated to the Male Sex, which will not equally apply to any system of Despotism of Man over Man ... The fact is, that we behave to the female sex, much in the same Manner as we behave to the Poor. We first keep their Minds, and then their Persons in Subjection

... I have read the Writings of Mrs. Wollstonecroft, of Mrs. Barbaud, of Mrs. Montague, etc., in England ... I have conversed with Madame Condorcet, Madame Robert, Madame Lavoisier, etc., in Paris. What these Women are, other Women might become. I have often felt my own Inferiority, and often lamented the present iniquitous and most absurd notions on the Subject of the disparity of Sexes ... Let the Defenders of male Despotism answer (if they can) "THE RIGHTS OF WOMAN" by Miss Wollstonecroft.

On these, as on so many other matters, Thomas Cooper was, as he lamented late in life, "a man 50 years ahead of his time" - closer to 150 years perhaps in some things. In chemistry, however, Cooper tended to run behind the times, perhaps out of loyalty to his beloved Priestley, and in his 1811 Inaugural Lecture as Professor of Chemistry at Dickinson College he was still reluctant to admit the virtually total ascendancy of the antiphlogistonists though he did, rather grudgingly, render Lavoisier his due.

Thomas Cooper had another 30 years of turbulent and well-documented life ahead of him (13). Mrs. Fulhame drops from the pages of history. Her *Essay on Combustion* survives as a lively chronicle and idiosyncratic abstract of those paradigmatically fluxional times:

*Nor shall this peace sleep with her, but as when
The bird of wonder dies, the maiden phoenix,
Her ashes new-create another heir
As great in admiration as herself.*

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Dr. Derek A. Davenport is Professor of Chemistry at Purdue University, West Lafayette, IN 47907, where he teaches a course on the history of chemistry. Kathleen M. Ireland is currently completing an M.S. in Chemical Education at Purdue and plans to teach high school after graduating.

THOMAS DUCHÉ MITCHELL AND THE CHEMISTRY OF PRINCIPLES

William B. Jensen, University of Cincinnati

That Lavoisier's work constitutes, in some fundamental sense, a true chemical revolution has never been doubted by chemists, whether his contemporaries or those later generations blessed with the gift of historical, albeit whiggish, hindsight. Historians of science, on the other hand, have been less certain and a small, but vocal, literature has evolved debating the exact revolutionary content of Lavoisier's work, whether it was indeed a true revolution, and even the question of whether scientific revolutions exist in the first place (1). At times, and with more than a little exaggeration, one is tempted to compare this state of perpetual historical uncertainty with David Donald's evaluation of the state of American Civil War history - namely



Thomas Duché Mitchell

that "there must be more historians of the American Civil War than there were generals fighting it and, of the two groups, the historians are the more belligerent" (2).

It has been said that the art of revolution is really the art of making explicit the implicit and, on my better days, I delude myself that this simple aphorism is able to account for both the elements of continuity and discontinuity present in all such conceptual upheavals. If this characterization is even approximately acceptable, then there is one very fundamental aspect of the older chemistry which Lavoisier's work failed to transform immediately and that is the question of how to theoretically rationalize the specific or intrinsic properties of matter. For though, as I will argue later, Lavoisier implicitly provided the techniques which would lead to the modern viewpoint, he did not himself explicitly confront this issue, let alone revolutionize it.

As even a superficial glance at 19th century chemistry textbooks (and some of the better 20th century textbooks) will show, this problem lies at the very core of chemistry's identity as an independent science (3). Whereas classical physics deals with the general properties common to all matter, such as mass, inertia, the laws of motion, etc., it is chemistry which deals with the individuality of different kinds of matter; with their specific properties; with why they possess the colors, textures, odors, and flavors they have; and with why they can be interconverted into certain kinds of materials with equally mysterious arrays of specific properties, but not into others.

The modern interpretation of this problem is based on the atomic-molecular theory and the hypothesis that these properties are in some manner the emergent result of the number, kind, and arrangement of a substance's component atoms or, in more reductionist terms, of its ultimate electronic composition and structure. But from the time of the Greeks until the end of

the 18th century, the accepted rationale was quite different and was based instead on the concept of property-bearing principles or elements - the idea that the most important or salient characteristics of a substance were in some fashion the additive reflection of the properties of its components (much as a reddish color in a paint mixture automatically implies the presence of a red pigment). Instead of being discontinuous and emergent, specific properties were thought to be continuous and variable like those of physical mixtures (to make the paint redder one simply adds more red pigment). Though the identification of both the most important properties and their material approximations would vary from the earth, air, water, and fire of the Greeks, through the sulfur, salt, and mercury of the iatrochemist, to the various hybrid four and five element theories of the 17th century, and finally to the phlogiston of the 18th century, the underlying assumptions remained the same. Indeed, this concept was analyzed in some detail by David Oldroyd in 1970, who found that it had an almost bewildering array of philosophical precedents, the most important of which was the Neoplatonic concept of ideal forms - the view that there existed ideal essences corresponding to each of the characteristic properties which, though nonisolable, were transferable from substance to substance and of which the isolable material analogs of earth, air, sulfur, etc. were but imperfect reflections (4).

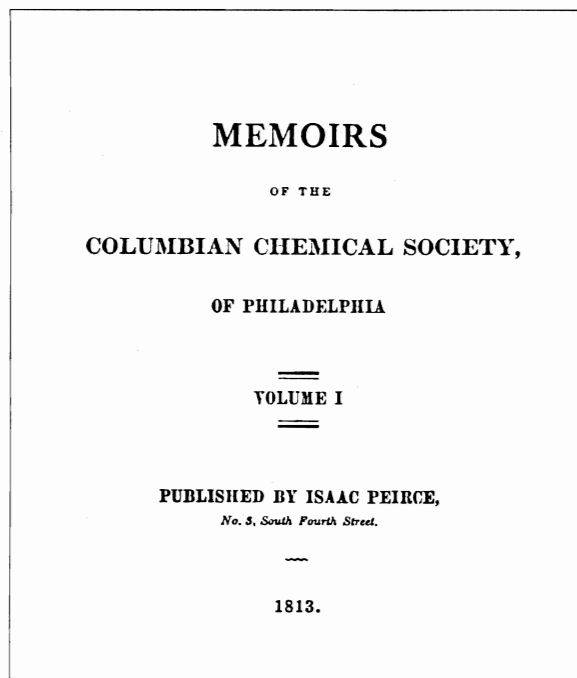
The point of this rather lengthy philosophical digression is, of course, that not only did Lavoisier fail to affect an explicit revolution in this time-honored mode of explanation, he actively carried it over into his new chemistry, albeit in a slightly modified form, and, in actual fact, it died a slow and very obscure death in the early decades of the 19th century.

The simplest evidence for this contention is Lavoisier's well-known use of oxygen as a principle of acidity. However, an even more pervasive argument was made by Carleton Perrin in 1973 as a result of analyzing the table of simple substances which appeared in Lavoisier's 1789 *Traité* (5). As Perrin noted, the first five entries in the table: light, caloric, oxygen, nitrogen (azote) and hydrogen are singled out and, in contrast to the other entries, which are always referred to as "simple substances", are alone in being given the name of "elements" - or to be more specific, are labelled as "simple substances belonging to all the kingdoms of nature, which may be considered the elements of bodies"(6). Since caloric is further described as being the principle of heat and Lavoisier considered oxygen to be the principle of acidity, nitrogen as a possible principle of alkalinity, and hydrogen as the principle of water, Perrin contended that the use of the word element in this context was intended in its older property-bearing sense and was a residual reflection of the older theory of property-bearing principles.

There are, however, some problems with this interpretation. In contrast to the older principles, which were both non-material and nonisolable, oxygen, nitrogen and hydrogen are

both material and isolable, though it might be argued that this materialization was the end result of the increasing tendency of later variants of the phlogiston theory to identify phlogiston with hydrogen. Likewise, unlike the older principles, which were literally the personification of the properties which they conferred on material substances, hydrogen, oxygen and nitrogen possess none of the properties of water, acidity or alkalinity which they supposedly imparted to their compounds. In passing, it should be noted that Robert Siegfried has also recently challenged Perrin's interpretation, arguing instead that this class of five elements is really a collection of odds and ends which Lavoisier was unable to group using his theory of salt formation, which forms the basis for classifying the other elements in the table - in short, that it is a kind of taxonomic miscellany (7).

Whatever the final consensus on this larger issue, there is no doubt that Lavoisier did use oxygen as a generic property-bearing principle and, if he failed explicitly to challenge the basic assumptions of the theory of principles, who did and at what point did chemists switch to the modern view? Alas, with the single exception which I will discuss in a moment, 19th century chemists - at least as far as I can tell - never did explicitly challenge this concept and, for that matter, never explicitly recognized that there was a fundamental difference between their premises and those of Lavoisier and his predecessors. Rather the change to the modern view seems to have occurred so slowly, or perhaps I should say, so obscurely, as to



Title page of the only published volume of the *Memoirs of the Columbian Chemical Society* in which Mitchell's initial essays appeared.

have been almost undetectable. Indeed, if we are to believe Oldroyd, historians of science have not done much better than the chemists with this subject, as he claimed in his 1970 article that they did not gain an explicit recognition and appreciation of the historical importance of the chemistry of principles until the 1960's.

The sole exception to this litany of ignorance is an obscure 19th century American chemist by the name of Thomas Duché Mitchell and, given the importance of this issue, as just outlined, in defining both the nature of Lavoisier's revolution and the course of its post-revolutionary consolidation, the relevance of briefly discussing Mitchell's critique of this concept, as well as its suitability as a concluding footnote to this symposium, are both self-evident.

Mitchell was born in Philadelphia in 1791 and studied medicine at the University of Pennsylvania, receiving his M.D. degree in 1812 (8). While still a student, he was drawn into the activities of the Philadelphia chemical-medical community, having spent a year in the drug store and chemical laboratory of Dr. Adam Seybert, a prominent member of the community who specialized in the chemical analysis of minerals, and his apprenticeship under Dr. Joseph Parrish, who had been active as a chemical lecturer in the Philadelphia area. Mitchell began to publish articles on chemistry and medicine while still a student, his first known chemical contribution, "On Nitric and Nitrous Acids", appearing in the *Medical Museum* in 1809. This was followed two years later by his organization of a student chemical society in collaboration with a fellow medical student named George Lehman. Known as the Columbian Chemical Society, it was the third such society organized in the Philadelphia area (9). The first, which had been organized in 1789 by John Pennington, had lasted only a year, whereas the second - The Chemical Society of Philadelphia, organized by John Redman Coxe in 1792 - had lasted for about 15 years (10). Though the Columbian Chemical Society would last for only three years (1811 - 1814), it was unique in that it succeeded, unlike its two predecessors, in actually publishing a volume of memoirs in 1813.

This 221-page volume contained 26 papers by members of the Society, nine of which were written by Mitchell, who had, incidentally, also served as the Society's first president in 1811. It is in these papers that Mitchell's initial critique of the chemistry of principles first appeared, largely in connection with his defense of Lavoisier's system against Davy's work on the elemental nature of chlorine and against a revival of the phlogiston theory proposed by John Redman Coxe, who was at the time Professor of Chemistry at the University of Pennsylvania.

Attacking Coxe's identification of hydrogen as a principle of inflammability on general philosophical grounds, Mitchell readily admitted that his objections applied with equal force to Lavoisier's use of oxygen as a principle of acidity. Mitchell wrote (11):

And in the first place, I would observe, that while the doctrine of an inflammable principle appears to me incorrect, it seems likewise unphilosophical. And while I say this, I am as willing to admit the error so often made in the use of the term, principle of acidity. Both alike are improper. It would be just as philosophical, when speaking of a neutral salt, to assert that the acid constituted the principle of neutrality as that it resided, exclusively, in the alkali. How absurd does this appear! Shall we then be excused, when we say that such a body is combustible, if we assert that the principle of inflammability belongs to one agent or, speaking of an acid, shall we be allowed to call one of its constituents the acidifying principle? ... it is an abuse of terms, a misapplication of words, to say that this or that is the principle of inflammability or of acidity. Inflammation and acidity are effects resulting from the action of relative causes and are not attributable to a single agent or principle.

In other words, Mitchell is contending that chemical and physical properties are not the inherent qualities of isolated substances, as implied by the chemistry of principles, but rather the relative, system-dependent result of the interaction of several substances, one of which could correspond to the organs of sensation in the human being. Thus, in another essay, Mitchell wrote (12):

Where was philosophy and reason when inflammability, or the power of burning, was consigned to one solitary agent? I challenge the whole host of opponents to the antiphlogistic system to adduce one single instance in all nature in which any body separately possesses an absolute principle or quality. When we speak of the properties of bodies, as taste, smell, etc., we do not mean that any of them possesses a positive quality. They are merely sensations or effects resulting from the actions of those bodies on our organs of taste, smell, etc. Inflammation, like odors, is the result of relative circumstances and not the product of a single agent.

Though other chemists would criticize Lavoisier's theory of acidity on purely chemical grounds, that is, in terms of whether or not all acids really did contain oxygen, it is important to realize that Mitchell is attacking on general philosophical grounds the premises behind the use of principles in general, be they of acidity or inflammability. And he is doing so on the basis of a critique of how we perceive and detect the properties of matter in general - namely, that all properties are due to purely relative effects in which all of the interacting agents play an equal role, for (13):

... what is a neutral salt, but the result of the mutual interaction of an acid and alkali, and what is combustion, but the effect of the mutual operation of oxygen gas, in some shape or other, and a combustible?

It is interesting to note that Mitchell's critique is virtually identical to that given a century later by the German philosopher, Ernst Cassirer, who, in discussing the physics of Anaxogoro-

ras and Aristotle, referred to their "hypostatization of sensuous qualities" or the "procedure of converting the relative properties of sensations into the absolute properties of things" (31).

In 1831 Mitchell, after practicing medicine for nine years in Frankford, Pennsylvania, just outside of Philadelphia, moved to Cincinnati to accept the position of Professor of Chemistry and Pharmacy at the Medical College of Ohio. The next year he published a 553-page textbook based on his lectures at the College, entitled *Elements of Chemical Philosophy*, in which he again reiterated his relativist position on the origins and nature of specific properties in chemistry (14):

On the subject of an acidifying principle, I have given my views at length, some years ago. It may not be amiss, however, to state, in this place, that the advances which chemical science is constantly making, have confirmed my early opinions on this point. I repeat, that the term acidifying principle is utterly unphilosophical, not only as applied to oxygen, but to hydrogen, and to every agent which may be supposed to exert an influence in developing acid properties. Every result in nature or produced by art, is a relative effect, and every item concerned, remotely or directly, in the accomplishment of the end, is essential to that end. Hence, I insist, that if an acid be discovered, which shall contain 50 component parts, all which are requisite in the formation of the compound, the only characteristic of which is acidity, I may affirm with equal propriety of any one, as of the other, of its constituents, that this or that is the acidifying principle. Abstract from the compound either of its parts, and you destroy the peculiar, distinctive character of the acid.

By way of biographical completeness, Mitchell left Cincinnati in 1837 to become Professor of Chemistry at the Medical School of Transylvania University in Lexington, Kentucky. In 1839 he gave up teaching chemistry, concentrating instead on therapeutics and materia medica. After serving as Dean of the Transylvania Medical School for several years, Mitchell returned to Philadelphia, where he died in 1865 at the age of 74.

Was Mitchell justified in his critique of the concept of property-bearing principles or was he merely an isolated American amateur flogging an already long-dead horse? In terms of his immediate environment, at least, the answer is that the attitudes of many of Mitchell's fellow chemists did indeed justify such a critique in 1813. As already mentioned, most of Mitchell's original comments appeared in his analysis of John Redman Coxe's recently published theory of combustion, which made controversial use of Humphry Davy's apparent discovery of hydrogen in such combustibles as sulfur, phosphorus and carbon, to support what was, in essence, a revival of a form of the phlogiston theory similar to the late 18th century variants proposed by the French chemists, Macquer and Guyton de Mourveau, and by the American chemist-physician, Samuel L. Mitchill (15). Essentially, the theory suggested that combustion involved not only the combination of the inflammable substance with oxygen, but the simultane-

ELEMENTS
OF
CHEMICAL PHILOSOPHY,
ON THE BASIS OF REID,
COMPRISING
THE RUDIMENTS OF THAT SCIENCE
AND
THE REQUISITE EXPERIMENTAL ILLUSTRATIONS,
WITH PLATES AND DIAGRAMS.

BY THOMAS D. MITCHELL, M. D.
Professor of Chemistry and Pharmacy in the Medical College of Ohio; President
of the Ohio Medical Lyceum; Honorary member of the Philadelphia
Medical and Columbian Chemical Societies, &c.

« Plusieurs sciences, arts et métiers reposent presque uniquement sur la chimie, qui, de
toutes les sciences, est celle dont on fait le plus d'applications aux besoins de la vie. »
Berzelius.

CINCINNATI:
PUBLISHED BY COREY & FAIREBANK.
NEW YORK: JONATHAN LEAVITT.
1832.

Title page of Mitchell's textbook in which he reiterated his criticisms of the chemistry of principles.

ous release and combustion of phlogiston (now identified as hydrogen) from the inflammable, the hydrogen having imparted the property of inflammability to the combustible in the first place. In addition, Franklin Bache, a fellow member of the Columbian Chemical Society, actually published a paper in the Society's memoirs entitled, "An Enquiry into What Circumstances Will Warrant Us Justly to Reckon Any Substance a Principle of a Common Property of Any Set of Bodies", in which he attempted to outline criteria for the development of a consistent chemistry of principles, albeit all now thoroughly materialized (16).

All of these cases indicate that not only chemists of Lavoisier's generation, such as Samuel L. Mitchill, but those of the next generation as well, such as Coxe and Bache, though apparently accepting the results of Lavoisier's system, continued to regress into explanations and modes of thought based on the concept of property-bearing principles - in short, that Lavoisier's work did not explicitly revolutionize this concept and that it lingered on well into the first decades of the 19th

century (17).

Even as late as 1832, Mitchell still had some justification for his remarks in his textbook. In 1828, just three years before Mitchell came to Cincinnati, Elijah Slack, his predecessor as Professor of Chemistry at the Medical College of Ohio, had published a small pamphlet on chemical nomenclature for the students at the college (18-19). This was the last of a long line of short chemical nomenclatures published in the United States, the first being Samuel L. Mitchill's *Nomenclature of the New Chemistry*, which appeared in 1794 (20). In this pamphlet, Slack continued to adopt an organization similar to that used by Lavoisier in his *Traité*, the sole difference being that the class of acidifying principles or supporters of combustion had now been generalized, via the suggestions of Davy, Gay-Lussac, Thomson and others, to include not only oxygen, but other highly electronegative elements, such as the halogens and sulfur. Thus one had not only an oxygen family of acids, bases and salts, but a chlorine family, a sulfur family, etc. (21).

Finally, we need to ask the question of where Mitchell got his ideas on the origins of specific properties. Given his emphasis on the emergent, relativistic nature of these properties and their interaction with our organs of sensation, the most obvious source which suggests itself is the writings of the 17th century British philosopher, John Locke, and his famous distinction between primary and secondary qualities (22). Unfortunately, we do not know enough about the details of Mitchell's education to show an explicit influence. In addition, as the recent book by Peter Alexander has shown, most of Locke's ideas on this subject are in reality a popularization of concepts found in the writings of Robert Boyle and are a logical consequence of accepting a mechanical atomic theory of matter - which Mitchell most certainly did (23).

Actually, there are some precedents in the chemical literature of the period which are more likely candidates and which Mitchell would have almost certainly encountered in the course of his chemical training. The most important of these is found in the multi-volume text, *A General System of Chemical Knowledge*, written by Lavoisier's friend and collaborator, Antoine Fourcroy, which had been translated into English by William Nicholson in 1804. In the first volume of this work, Fourcroy offered a list of established laws governing chemical change, the sixth of which reads (24):

Compounds formed by chemical attraction, possess new properties different from those of their component parts.

In commenting on this law, Fourcroy further noted that:

... chemists have long believed the contrary took place ... They thought, in fact, that the compounds possessed properties intermediate between those of their component parts; so that two bodies, very coloured, very sapid or insipid, soluble or insoluble, fusible or infusible, fixed or volatile, assumed, in chemical combination, a

shade of colour, or taste, solubility or volatility intermediate between, and in some sort composed of, the same properties which were considered in their principles. *This is an illusion or error which modern chemistry is highly interested to overthrow* (italics added).

Likewise, the Scottish chemist, John Murray, gave a detailed discussion of the same law in his 1806 text, a volume also easily accessible to Mitchell, and even cited quantitative data on densities to demonstrate its truth (25). Though the acceptance of this law is tantamount to rejecting the concept of property-bearing principles, it is interesting to note that neither Fourcroy nor Murray carried through with it in the later descriptive sections of their books, where both continued to employ Lavoisier's acidity principle, and it is only Mitchell, as far as I know, who explicitly spelled out its logical consequences for Lavoisier's original system.

While the apparently unique approach of Mitchell to this problem certainly merits the attention of historians, we are still left with our original question of when and why the 19th century chemist abandoned the chemistry of principles, for one cannot seriously maintain that Mitchell had a major impact on his fellow chemists. Though he published, in addition to several textbooks, more than 20 papers on chemical subjects, virtually all of them appeared in obscure medical journals with limited circulations which, like the *Memoirs of the Columbian Chemical Society*, became defunct after only one or two volumes. Likewise, his impact as a teacher of chemistry and as a textbook author was limited to audiences of beginning medical students in the newly-founded medical schools of Ohio and Kentucky, which failed to produce any future generations of chemists to carry on his particular point of view.

A tentative answer to our question can be obtained, however, by briefly considering why the proposition that:

$$\text{properties} = f(\text{composition}) \quad (1)$$

was so important to the 18th century chemist. Read from right to left, this statement can be charitably interpreted as one of the theoretical underpinnings of alchemy. However, long after chemists had ceased to seriously pursue the dreams of the alchemist, this proposition continued to be of importance as read from left to right. For virtually the only way 17th and 18th century chemists could operationally implement the claim that chemistry was the study of the composition of materials was via the premise that the composition of a substance could be directly inferred from its properties. Thus the presence of a sharp taste automatically implied the presence of "salt", insolubility and refractory behavior the presence of "earth", volatility the presence of "air" or "mercury", inflammability the presence of "sulfur", etc.

By changing the meaning of the word "composition" in relation 1 from "nonmaterial, nonisolable principles" to "material, isolable simple substances", and by supplying, via

the law of conservation of mass, a tool which allowed chemists to accurately distinguish between decomposition reactions, on the one hand, and addition and displacement reactions, on the other, Lavoisier supplied the means to circumvent this relation. Composition could now be inferred, not from properties, but via the use of chemical reactions to separate and isolate a species' material components. As the century progressed, the essence of a substance's identity came to be viewed less and less in terms of such properties as inflammability, metallicity or acidity and more and more in terms of mapping its position in the reactivity matrix - that is, in terms of what it could be made from and what it could be converted into. Indeed, though the terms as principle, acid and base, or combustible and supporter, continued to be used into the third and fourth decades of the century, they gradually ceased to imply the necessary existence of certain properties in the substances so labeled (such as sour taste in the case of acids) and became instead indicators of their taxonomic positions in the reactivity matrix. Thus the debates over the nature of acids in the 1830's had little or nothing to do (as so often misrepresented) with the question of whether oxygen or hydrogen was the true acidifying principle; rather they dealt with competing theories of salt formation, which is to say, with competing views of the taxonomic relationships between these substances in the reactivity matrix (e.g., do acid and bases form salts via addition or via displacement reactions ?) (26).

Consequently, as suggested earlier, there was no explicit confrontation with the chemistry of principles. It simply faded away, along with the importance of relation 1 as an approach to determining composition. Though Lavoisier failed to deal directly with the issue of principles and properties, he nonetheless provided chemists with the tools that would eventually direct their energies into a more fruitful approach to the problem of composition. By the time the property-composition problem was revived in the second half of the 19th century and correctly reformulated as:

$$\text{properties} = f(\text{composition and structure}) \quad (2)$$

the newer generation of structural organic chemists was no longer aware of the details of the earlier program and, with the accumulated fruits of Lavoisier's approach to composition to build upon, it seemed almost inconceivable that their program was anything other than the completion of his own (27).

In passing, it is of interest to note that there is evidence that it took chemists several decades to learn how to make full use of the implications of Lavoisier's approach to composition and, even to this day, it is misrepresented in both chemistry texts and in the history of science literature. Both sources generally imply that what is involved in analysis is the simple separation of a substance into its component elements. However, as anyone who has practiced classical chemical analysis knows, this is seldom the case. What one actually does is to

synthesize from an unknown substance one or more known compounds, from whose established composition one can infer the composition of the unknown (thus, in classical organic analysis, the unknown is converted into carbon dioxide and water) - a point which was emphasized by Fourcroy as early as 1804 (28). Even the known compounds produced in the analysis may never have been directly decomposed into their elements, but rather have, in turn, compositions inferred in a similar manner via their interconversions into yet other known compounds - the final end set of compounds actually having been directly decomposed into their elements being quite small.

In other words, classical chemical analysis depends on more than the definition of simple substances and the law of conservation of mass provided by Lavoisier. It also requires an empirical knowledge of the interconversions of different materials and an extensive mapping of the reactivity matrix, much of which, as the study of late 18th century analytical chemistry has shown, was done prior to the work of Lavoisier, whose contributions can in many ways be viewed as a set of rules for guiding the chemist through its manifold pathways (29). Indeed, the history of the discovery of new elements clearly shows that use of the reactivity matrix can even allow chemists to infer the existence of new elements through the behavior of their compounds (usually the oxides) without having actually isolated the element itself (30).

Lastly, it is of interest to note that, with the advent of the electronic theory of matter and the instrumentation revolution, modern chemistry has again returned to a left to right reading of relation 2 and direct physical methods of analysis are rapidly replacing the methods of classical chemical analysis. On the other hand, the reading of relation 2 from right to left, which may be interpreted as a form of "molecular engineering" and which is, in some sense, a modern equivalent of the alchemist's dream, still remains very much an open problem for 20th century chemistry.

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Dr. William B. Jensen holds the Oesper Position in History of Chemistry and Chemical Education at the University of Cincinnati, Cincinnati, OH 45221. He is interested in the history of inorganic and physical chemistry and in the development of the 19th century Cincinnati chemical community.

A BIOGRAPHICAL CHECKLIST

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A REVOLUTIONARY TIMETABLE

Year	<i>Lavoisier's Life</i>	<i>Concurrent Political and Chemical Events</i>
1743	* Born 26 August in Paris.	* Franklin founds the American Philosophical Society, Birth of Jefferson and Klaproth.
1745		* Lead chamber process, birth of Gahn.
1748	* Death of mother.	
1751		* Publication of Diderot's <i>Encyclopédie</i> and Macquer's <i>Eléments de Chymie</i> , discovery of nickel (Cronstedt).
1754	* Enters the Collège Mazarin.	
1755		* Birth of Fourcroy.
1756		* Start of Seven-Years War, publication of Black's <i>Experiments upon Magnesia, Quicklime and Other Alkaline Substances</i> .
1758		* Publication of Cronstedt's <i>Essay on the New Mineralogy</i> .
1760	* Death of sister.	
1761		* Death of Stephen Hales.
1762	* Attends Rouelle's chemical lectures.	
1763	* Obtains law degree, begins geological studies with Guettard.	* Peace of Paris, birth of Vauquelin.
1764	* Admitted to the bar.	* Publication of Macbride's <i>Experimental Essays</i> .
1765	* Reads first memoir (on gypsum) to the Académie.	* Death of Lomonosov, discovery of prussic acid (Scheele).
1766	* Receives medal for essay on lighting.	* Birth of Dalton and Wollaston, Cavendish reports the discovery of hydrogen, publication of Macquer's <i>Dictionnaire de Chymie</i> .
1767	* Geological tour with Guettard.	
1768	* Election to the Académie, purchase of interest in the Ferme Générale.	
1770	* Memoirs on transmutation of water and earth.	* Discovery of tartaric acid (Scheele), death of Rouelle.
1771	* Marriage to Marie Anne Paulze.	
1772	* Formulates program for study of airs, first experiments on combustion, (sealed note), combustion of diamond, associate at the Académie.	* Publication of Guyton's <i>Digressions Académique</i> and Priestley's first paper in his series on <i>Observations on Different Kinds of Air</i> , discovery of nitrogen (Rutherford).
1774	* Publication of <i>Opuscules Physique et Chimique</i> ,	* Ascension of Louis XVI, discovery of manganese (Gahn),

- 1775 memoir on calcination of tin and lead.
* Appointed Commissioner of the Régie des Poudre (Gunpowder Commission), takes up residence at the Arsenal, memoir on pure air, death of father.
- 1776 * Appointed Director of the Caisse d'Escomptes (Discount Bank), memoir on nitrous acid.
- 1777 * Memoirs on combustion, phosphorus, theory of gases, respiration, and acids.
- 1778 * Becomes a pensionnaire of the Académie, purchases estate at Fréchaines and begins agricultural experiments.
- 1779
- 1780 * Full member of the Ferme, report on prisons.
- 1781 * Memoir on carbon dioxide
- 1782 * Memoirs on the dissolution of metals and on fermentation.
- 1783 * Member of Royal Agricultural Society, memoirs on phlogiston, the composition of water, the caloric theory of gases and heat (with Laplace).
- 1784
- 1785 * Director of the Académie.
- 1786 * Secretary to the Committee of Agriculture.
- 1787 * Representative to the Assembly of Orléanais, publication of the *Méthode de Nomenclature*.
- 1788 * Annotated French edition of Kirwan's *Essay*, escapes explosion of chlorate-based gunpowder.
- 1789 * Report to the National Assembly, mob incident at the Arsenal, publication of the *Traité* and the first volume of the *Annales de Chimie*, memoirs on respiration.
- 1790 * Letters to Franklin and Black
- 1791 * Commissioner to the Treasury, report on the state of French finances, metric system committee, attacks by Marat, abolition of the Ferme, Treasurer of the Académie.
- 1792 * Moves from the Arsenal.
- 1793 * Dissolution of the Académie, arrest of the Ferme.
- 1794 * Execution on 8 May.
- chlorine (Scheele), and oxygen (Priestley).
* Turgot appointed Minister, Latin edition of Bergman's *Dissertation on Elective Affinities*.
- * Turgot dismissed, start of the American War of Independence.
- * Publication of Scheele's *Chemische Abhandlung von der Luft und Feuer*, Wenzel studies reaction rates, birth of Thenard.
- * Death of Rousseau, Voltaire and Linné, France enters American War, publication of Bergman's *De analysi aquarium*, first volume of Crell's journal, birth of Gay-Lussac and Davy.
- * Birth of Berzelius.
- * Reform of the Ferme.
- * Discovery of molybdenum (Hjelm).
- * Publication of Bergman's *Sciagraphia regni mineralis*.
- * Peace of Versailles, discovery of tungsten (D'Elhuyar) and tellurium (Müller), prize awarded for the Leblanc process.
- * Publication of Kirwan's *Essay on Phlogiston*, death of Bergman and Macquer, Cavendish announces the synthesis of water.
- * Introduction of *eau de Javel* (Berthollet).
- * Death of Scheele.
- * Wall built around Paris at Lavoisier's suggestion, publication of Fourcroy's *Principes de Chimie* which uses Lavoisier's system, analyses of ammonia, prussic acid and hydrogen sulfide (Berthollet).
- * 3rd edition of Fourcroy's *Éléments* adopts Lavoisier's system, French translation of Bergman's *Elective Affinities*, English and Spanish translations of the *Méthode*.
- * États généraux, abolition of Privileges, National Assembly, storming of the Bastille, publication of English edition of annotated version of Kirwan's *Essay* and Higgins' *Comparative View of the Phlogistic and Anti-phlogistic Theories*, discovery of uranium (Klaproth).
- * Festival on Champs de Mars, English translation of the *Traité*, Italian translation of the *Méthode*.
- * Flight of the King, salvo on Champs de Mars, Italian translation of the *Traité* and American edition of table from the *Méthode*, publication of Girtanner's *Neue Chemische Nomenclatur*, discovery of titanium (Gregor), Richter studies stoichiometry, Kirwan announces conversion to Lavoisier's system.
- * Outbreak of war, storming of Tuileries, Massacre of Paris, Battle of Valmy, proclamation of the Republic, German translation of the *Traité*, founding of the Chemical Society of Philadelphia.
- * Reign of Terror, execution of Louis XVI, Jacobins assume power, murder of Marat, execution of Queen, introduction of metric system, German translation of the *Méthode*.
- * Execution of Hébertists and Danton, abolition of royal rank, victory at Fleurus, downfall of Robespierre, publication of Mrs. Fulhame's *Essay* and Mitchill's *Nomenclature of the New Chemistry*, discovery of yttria (Gadolin).

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