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The boundaries of Lavoisier's chemical revolution/Les limites de la révolution chimique de Lavoisier

M Frédéric L. Holmes

Résumé

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The boundaries of Lavoisier's chemical revolution

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MOTS-CLÉS. — Révolution; phlogistique; nomenclature; controverse.

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KEYWORDS. — Revolution; phlogiston; nomenclature; controversy.

If revolutions really occur in science, then the chemical revolution identified with Antoine Lavoisier is a classic example. According to I. Bernard Cohen : « It is evident that Lavoisier's Chemical

(*) Frederic L. Holmes, Section of the History of Medicine, School of Medicine, L130 SHM, PO Box 208015, New Haven, Connecticut 06520-8015 USA.

Revolution passes all the tests for a revolution in science. It has been recognized as a revolution by all historians and scientists, just as it was seen to be a revolution in its own time (1). » Bernadette Bensaude-Vincent has suggested that our very idea of a scientific revolution is modelled after Lavoisier's chemical revolution. « Lavoisier, she asserts, invented the modern concept of a revolution as a rupture (2). » Despite widespread acceptance that the chemical revolution was a paradigmatic scientific revolution, 200 years of retrospective descriptions of the event, and 100 years of active historical scholarship, have not yet achieved agreement on its nature, scope, and meaning. A few years ago Carleton Perrin, a leading Lavoisier scholar, wrote that « the historiography of the Chemical Revolution has reached a state not unlike the crises associated with revolutions in science. The traditional view is under attack, alternative interpretations have proliferated, practitioners talk at cross-purposes, and consensus seems out of reach (3) ».

The « traditional view » to which Perrin alluded was that the chemical revolution was about the replacement of Stahl's phlogiston theory of combustion by Lavoisier's theory that combustion consists of the absorption of oxygen and the release of its caloric as free heat. This view was still sufficiently dominant at the end of World War II to induce James B. Conant to entitle his well-known Harvard Case History on the chemical revolution *The Overthrow of the Phlogiston Theory* (4). That this was the central issue of the chemical revolution appears on the surface obvious from the fact that Lavoisier's most partisan writings were directed against the phlogiston theory, and that during the campaign he led against « the partisans of Stahl », he and his associates became known as the « antiphlogistonists ». (Several historians have noticed that the Lavoisians themselves coined no positive alter-

(1) I. Bernard Cohen, *Revolution in Science* (Cambridge, MA : Harvard Univ. Press, 1985), 236.

(2) Bernadette Bensaude-Vincent, *Lavoisier : Mémoires d'une révolution* (Paris : Flammarion, 1993), 423 : *« Il invente le concept moderne de révolution comme rupture. »*

(3) Carleton E. Perrin, Research Traditions, Lavoisier, and the Chemical Revolution, *Osiris*, 4 (1988), 79.

(4) J. B. Conant, The Overthrow of the Phlogiston Theory : The Chemical Revolution of 1775-1789, in *Harvard Case Histories in Experimental Science*, ed. by J. B. Conant, vol. 1 (Cambridge, MA : Harvard Univ. Press, 1957), 67-115.

native to this negative label fixed on them by their adversaries (5). The professional historians who began to investigate the chemical revolution in greater detail during the postwar period, however, soon began to express dissatisfaction with this definition of its content.

In 1961 Henry Guerlac wrote that the appraisal that the chemical revolution was tantamount to the overthrow of the phlogiston theory « says at once too much and too little ». It both exaggerated the break with the past, and overlooked the fact that « something more fundamental occurred ». In Guerlac's opinion, Lavoisier merged two distinct chemical traditions, the pharmaceutical and analytical chemistry of the continent, and the pneumatic chemistry pioneered in Britain. « Methodologically, the key to the Revolution » was the systematic application of the balance to gases, as well as to solids and liquids (6). Since then historians have found other purported keys to the chemical revolution. Without reviewing these interpretations in detail, I will mention here a few of those that have been proposed. J. B. Gough maintained in 1981 that « the problem central to the beginning as well as the whole of the subsequent revolution in chemistry was not combustion, but the nature of vapors and the air and their relation to one another, in other words the problem of the gaseous state (7) ». Robert Siegfried and Betty Jo Dobbs asserted in 1968 that the inversions that Lavoisier's new system of chemistry caused in previous views of composition « are of sufficient importance to require that the « chemical revolution » be redefined in terms of them, rather than with the overthrow of phlogiston and the triumph of the oxygen theory (8) ». A number of scholars maintain that the new nomenclature devised by Lavoisier and his associates between 1787 and 1789 was the defining « revolutionary » feature of the new chemistry (a viewpoint to which I shall return at the end of this essay) (9).

Historians do not universally assent even to the general propo-

(5) Bensaude-Vincent, *op. cit.* in n. 2, 255-256.

(6) Henry Guerlac, *Lavoisier — The Crucial Year* (Ithaca : Cornell Univ. Press, 1961), XVII-XVIII.

(7) J. B. Gough, The origins of Lavoisier's theory of the gaseous state, in *The Analytic Spirit : Essays in the History of Science in honor of Henry Guerlac*, ed. Harry Woolf (Ithaca : Cornell Univ. Press, 1981), 15.

(8) Robert Siegfried and Betty Jo Dobbs, Composition : A neglected aspect of the chemical revolution, *Annals of Science*, 24 (1968), 292.

(9) Marco Beretta, *The Enlightenment of Matter : The Definition of Chemistry from Agricola to Lavoisier* (Canton, MA : Science History Publications, 1993), 258.

sition that Lavoisier was the central figure in a revolution coextensive in time with his own scientific career and with the changes that he introduced into chemistry. Some have seen his work as only the culmination of a longer development beginning early in the 18th century. An extreme form of this interpretation is Gough's argument that Lavoisier fulfilled the « Stahlian Revolution ». According to Gough the solution of the problem of chemical composition that « Lavoisier and his companions » were able to reach was an application of « the basic principles of the Stahlian doctrine of the chemical molecule » (10). At the other interpretative extreme, Siegfried and Dobbs believe that Lavoisier's *Traité élémentaire,* commonly regarded as the culmination of a revolution, merely initiated one that ended two decades later with the appearance of John Dalton's atomic theory (11).

The problems that current historians of science find in « recomposing a coherent image of the chemical revolution » are less likely, according to Bensaude-Vincent, to be solved by the discovery of new documents than by « a critical examination of our historiographic heritage ». Through such an analysis, she shows that the meaning of the revolution has been shifting continuously, ever since Lavoisier predicted early in 1773 that the new line of investigation he was about to pursue might « cause a revolution in physics and in chemistry ». As his work developed, and as he confronted both his supporters and his adversaries, Lavoisier's own understanding of the revolution he aimed to lead also continued to evolve. Both friends and foes agreed by 1790 that a « great revolution » had occurred, but its meaning differed according to their various relationships toward it. After Lavoisier's death, many layers of interpretation of his revolution were added by those, in France and in its rival nations, who wished to invoke his memory for their own purposes. By the end of the nineteenth century, when historians began to return to primary documents to reconstruct narratives of the chemical revolution, the pictures they sought to bring into sharper focus were already heavily shaped by the images, heroic and otherwise, that had been conjured during the intervening era.

(10) J. B. Gough, Lavoisier and the fulfillment of the Stahlian revolution, *Osiris,* 4 (1988), 15-33.

(11) Siegfried and Dobbs, *op. cit.* in n. 8, 292. This view is further developed in Robert Siegfried, The chemical revolution in the history of chemistry, *Osiris,* 4 (1988), 34-50.

Is it still possible to escape the myths that have grown up around the chemical revolution, and to see it as it really took place? Bensaude-Vincent believes that the « variety of perspectives » from which the revolution has been viewed should not lead us to conclude « that the revolution has no objective reality. On the contrary, the revolution is a historical event » whose dimensions can be reconstructed, even without disposing of the divergent viewpoints from which it has been observed ever since its inception (12).

One of the major obstacles to a coherent reconstruction of the chemical revolution, I believe, is a tendency among recent and current historians of science to merge two different images of Lavoisier that had developed during the nineteenth century. One was that of the leader of a historic revolution in science, the second was of the founder of the modern science of chemistry. The latter image owes much to nationalistic motives, the most famous being embodied in Adolphe Wurtz's declaration in 1868 that « chemistry is a French science : it was constituted by Lavoisier, of immortal memory ». This claim carried with it the corollary that chemistry before Lavoisier was in a primitive state. Those who countered Wurtz's claim, therefore, tended not only to diminish the novelty of Lavoisier's achievements, but to regard pre-Lavoisierian chemistry as already a well-developed science (13). Whatever grounds there may be for or against the claim that Lavoisier founded modern chemistry, these claims should have been distinguished from characterizations of the historical event known as the chemical revolution. That the two have instead become confused is perhaps attributable to the first post-world war II generation of historians of science who portrayed a great scientific revolution out of which the modern sciences collectively emerged by breaking free from

(12) Bensaude-Vincent, *op. cit.* in n. 2, esp. 24, 25 and 419 : « ... *recomposer une image cohérente de la révolution chimique...* » (24); « ... *un regard critique sur notre héritage historiographique...* » (25); « *L'importance de l'objet m'a engagé à reprendre tout ce travail qui m'a paru fait pour occasionner une révolution en physique et en chimie* » (Lavoisier, 21 février 1773, registre de laboratoire I, Arch. Acad. des sciences); « *De la variété des perspectives sur la révolution chimique, il ne faudrait pas conclure que la révolution n'a aucune réalité objective [...]. Tout au contraire, la révolution est un événement historique* » (419).

(13) *Ibid.*, 393 : « *La chimie est une science française : elle fut constituée par Lavoisier, d'immortelle mémoire.* » See also, Alan J. Rocke, History and Science, History of Science : Adolphe Wurtz and the Renovation of the Academic Professions in France, *Ambix*, 41 (1994), 20-32.

the shackles of tradition. Within this overall scheme Lavoisier's revolution was taken to be the fulcrum around which chemistry made its transition to modernity, a view expressed most vividly in Herbert Butterfields influential *The Origins of Modern Science.* Butterfield began his account of « The Postponed Scientific Revolution in Chemistry » with the assertion that « it has often been a matter of surprise that the emergence of modern chemistry should come at so late a stage in the story of scientific progress ». After recounting the long path of chemical change from alchemy to Robert Boyle, to the « fashionable » phlogiston theory that « incapacitated » chemists from « realizing the implications of their own work », and recapitulating the canonical discoveries associated with Black, Cavendish, Priestley and Lavoisier, Butterfield ended his story with the comment that « the chemical revolution which he [Lavoisier] set out to achieve was incorporated in the new terminology as well as in a new treatise on chemistry. [...] Over a broad field, therefore, he made good his victory, so that he stands as the founder of the modern science » (14). Much as we may have refined our picture of the events surrounding Lavoisier during the decades since Butterfield wrote these lines, I believe that we have so far not been able to escape the consequences of the conceptual looseness that allowed him and others to treat victory in the chemical revolution as synonymous with the foundation of modern chemistry.

Thirty years after the publication of Thomas Kuhn's *The Structure of Scientific Revolutions,* we can still hardly discuss the chemical revolution without reference to the role Kuhn gave to that event as an exemplar of the process of revolutionary change in science. Kuhn used the chemical revolution as one of his most prominent illustrations of the scheme in which a « normal science » enters a period of crisis as its dominant paradigm repeatedly fails to resolve the anomalies confronting it, in which a new candidate for a paradigm emerges, is successful at resolving the crisis, attracts converts, and replaces the old paradigm. At a superficial level Kuhn's scheme fitted the chemical revolution persuasively into a broader explanation of scientific change; but his substantive treat-

(14) Herbert Butterfield, *The Origins of Modern Science, 1300-1800,* 2nd ed. rev. (Toronto : Clarke, Irwin, 1957), 191-209.

ment of the chemical revolution itself was fragmented and disappointingly vague. Nowhere in *The Structure* did he provide a coherent description of what he took to be the pre-Lavoisian normal science of chemistry, or specify when and how he thought it had emerged. At one point he hinted that the works of Boyle and of Boerhaave ended the « fundamental disagreements » marking a pre-paradigmatic state. This pairing of two figures whose chemical publications were separated by nearly half a century allows far too much leeway to identify the assumed transition to a normal science. At another point Kuhn suggests that the mechanical philosophy, in the hands of Robert Boyle, directed « particular attention to reactions that could be viewed as transmutations ». This is a function that Kuhn generally ascribed to paradigms, but he did not make clear whether or not he regarded Boyle's use of the mechanical philosophy as the introduction of a paradigm into chemistry. In another place Kuhn described affinity theory as « an admirable chemical paradigm » in the eighteenth century, but did not connect this paradigm with Lavoisier's chemical revolution. Only when he discussed « the crisis that preceded Lavoisier's theory of combustion » did Kuhn refer to the phlogiston theory as « a paradigm of eighteenth century chemistry [that] was gradually losing its unique status » (15).

Did Kuhn view eighteenth century chemistry as a whole before Lavoisier as a « normal science », or did he see only pockets of paradigmatic order within an otherwise pre-paradigmatic science? In a list of the « classic » textbooks that he believed provided foundations for fields of normal science, the only chemical example he gave was « Lavoisier's *Chemistry* » (16), an indication perhaps that he did not think a normal science had existed before then. Kuhn's scattered allusions to pre-Lavoisian science do not, however, add up to a consistent picture.

Kuhn's descriptions of the revolutionary process itself were more coherent. His account of the discovery of oxygen made a persuasive case for his argument that discovery is not a single event in time but a complex, extended process. He appears, however, to have accepted with little critical analysis the traditional view that

(15) Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago : Univ. of Chicago Press, 1970), 15, 41, 69-72, 130.

(16) *Ibid.*, 10.

the revolution consisted of the replacement of the phlogiston theory by Lavoisier's oxygen theory of combustion. The fact that, for his description of the « crisis » that arose over the problem of weight-gain in combustion he relied on Guerlac's *Lavoisier-The Crucial Year* — the same book in which Guerlac declared the traditional view to be inadequate —, makes it curious that Kuhn adhered to that view without questioning it or justifying his choice (17).

In the preface to *The Structure,* Kuhn acknowledged that he had presented his views in an « extremely condensed and schematic form », and announced his intention to produce « a longer version » that would provide more « scope and depth » (18). Had he done so, perhaps he could have resolved these unanswered questions about his view of the chemical revolution. Three decades later it may seem superfluous to point out that the task has never been done; but that is justified, I think by the enduring influence that *The Structure of Scientific Revolutions* has exerted on our field. Even though few historians of science may adhere formally to the Kuhnian scheme, we continue to think of scientific revolutions in the language of paradigm replacement. Moreover, Kuhn's lack of clarity over whether the chemical revolution restructured chemistry as a whole or only replaced « a paradigm of chemistry » reflects the deeper-seated ambiguity in a long historiographic tradition that still blurs our discussions of the subject.

A more promising avenue today for furthering our understanding of the chemical revolution is to follow Bernard Cohen's invitation not to impose later definitions of a scientific revolution on this « case », but to clarify what the participants and contemporary observers thought was revolutionary about it. The chemical revolution is particularly favorable to such examination because of the regularity with which both friends and foes of Lavoisier's chemistry called its advent a revolution. Cohen cautions us that during this era the word « revolution » itself was still undergoing a transition from its older meaning as a cyclic return to a former state, toward the « modern » meaning of a radical, irreversible break with the past. Those who used it in the context of the che-

(17) *Ibid.,* 53-56, 69-72, 79, 85-86, 118, 148, 157.
(18) *Ibid.,* VIII-IX.

mical revolution were, however, almost certainly invoking the latter image, applied both to political and scientific change : that of a rupture that involved the overturn of an existing structure, conflict, and a competition to establish new forms of authority (19). When Lavoisier juxtaposed the revolution « that has transpired in chemistry » in a letter to Benjamin Franklin in 1790 with an account « of our political revolution », he clearly implied that the two revolutions were analogous in structure (20).

If we accept that Lavoisier and other contemporaries defined the revolutionary aspects of what had « transpired in chemistry » in such terms, then there can be little doubt that the rupture they had in mind was Lavoisier's break from the phlogiston theory, that the ensuing struggle was between « phlogistonists » and « antiphlogistonists », and that the « victory » that Lavoisier and his followers celebrated was the acceptance of the oxygen theory of combustion in place of the phlogiston theory. When Lavoisier informed Franklin that « the revolution has taken place in an important branch of human knowledge », that it was « well-advanced », and would be « complete » if Franklin were to « stand with us », the principal reason that he adduced, in addition to listing the prominent French scientists « on my side », was that « the scholars of London and of England are also gradually dropping the doctrine of Stahl ».

Lavoisier's own perspective on his nearly completed revolution does not exhaust the meaning that the historical event held for his contemporaries, but it is as critical a witness to the core of the issue as we have. It is also supported by the testimony of his most strategic convert, Guyton de Morveau. In the « Second

(19) Cohen, *op. cit.* in n. 1, IX-XVII, 21-25, 40-47, 213-228. Marco Beretta provides an extensive set of contemporary quotations that employ the word revolution to describe the events surrounding Lavoisier's chemistry. See Beretta, *op. cit.* in n. 9, 249-258.

(20) Here and in the following paragraph I follow the English translation of Lavoisier's letter published by Beretta, *ibid.*, 250-251. Original French text : « *Après vous avoir entretenu de ce qui se passe dans la chimie, ce seroit bien le cas de vous parler de notre révolution politique. Les sçavans français sont partagés dans le moment entre l'ancienne et la nouvelle doctrine. J'ay de mon côté M. de Morveau, M. Bertholet, M. de fourcroy, M. de la place, M. Monge et en général les phisiciens de l'académie. Les sçavans de Londres et de l'Angleterre abandonnent aussy insensiblement la doctrine de Stalh mais les chimistes allemands y tiennent beaucoup.* [...] *Voilà donc une révolution qui s'est faite depuis votre départ d'Europe dans une partie importante des connoissances humaines, je tiendrai cette révolution pour bien avancée et même pour complètement faite si vous vous rangez parmi nous.* » (Lettre du 2 février 1790, Arch. Acad. sciences, dation Chabrol.)

Advertisement », inserted in the middle of the chemical section of the *Encyclopédie Méthodique,* Guyton declared his acceptance of « the antiphlogistic doctrine » and explained why he could now state « with confidence » that the « revolution is consummated ». Looking backward, he recalled why the transformation of sulfur to an acid and of metals to their calces had made « a profound impression on the genius of Stahl », and he characterized Stahl's phlogiston theory as « the first system that had begun to establish some connections between a multitude of scattered facts and isolated observations ». The system had at first been more useful than harmful to chemistry, but the very investigations later undertaken to perfect it had « gradually prepared its ruin ». Mentioning the steps by which « the illustrious Lavoisier had been the first to interrupt the long cult » of able chemists « born in this religion », Guyton summarized succinctly but concretely the evidence that had finally overcome his doubts about the sufficiency of the « antiphlogistic doctrine ». All of his reasons consisted of specific combustion phenomena for which Lavoisier's theory provided a more satisfactory account than could the Stahlian hypothesis. Critical for Guyton was the discovery of the decomposition of water, which finally allowed the antiphlogistic doctrine to explain an observation — the release of inflammable gas in the dissolution of metals in dilute acids — that had previously provided critical support for the Stahlians. Thus for Guyton as for his new leader, the « consummation » of the revolution meant the choice of Lavoisier's theory of combustion in place of the Stahlian phlogiston « system » (21).

If we disentangle the chemical revolution as a historical event from the larger question of Lavoisier's role in the foundation of modern chemistry, then we are justified in reconsidering whether

(21) L.-B. Guyton de Morveau, Maret, and Duhamel, *Encyclopédie Méthodique. Chymie, Pharmacie et Métallurgie,* vol. 1, pt. 2 (Paris, 1789), 625-634 : « ... *la révolution est consommée : on me pardonnera sans doute la confiance avec laquelle je crois pouvoir l'anoncer...* » (626); « *Ces phénomènes firent une impression profonde sur le génie de Stahl...* » (626); « ... *je me bornerai en ce moment à faire remarquer que c'est le premier systême qui a commencé à établir quelque liaison entre une multitude de faits épars & d'observations isolées* » (626-627); « ...*les travaux entrepris dans la vue de perfectionner ce systême, en ont insensiblement préparé la ruine...* » (627); « *L'illustre Lavoisier a le premier interrompu ce long culte* » (627); « ...*& pour tout dire en un mot, ils [Margraff, Bergman, Macquer, Scheele, Black, Priestley, Kirwan] étoient nés dans cette religion* » (627).

the older view that the revolution was centered on the overthrow of the phlogiston theory may not have been more realistic than the revisionist interpretations that have recently been proposed. That is not to deny that this core confrontation also carried broader consequences. Lavoisier's combustion theory did entail a new understanding of the gaseous state, but by the time he had formulated it there were few left who would defend the old view that there was only one kind of air. The experimental operations on which he built his theory did merge continental analytical chemistry and British pneumatic chemistry, but a synthesis is not in itself a revolution. Lavoisier's revolution was eventually expressed in a reformed nomenclature and a *Traité* designed to introduce his chemical system insulated from the past, but these were fruits of his victory, they did not constitute the revolution itself. The consolidation of his system did invert orders of composition previously accepted, but these were corollaries of his theory of combustion, implied by, but not at the center of the revolutionary struggle.

If we return to a view of the chemical revolution centered on the contest by Lavoisier and the followers he acquired, against the « partisans of Stahl », then we must also agree with Bensaude-Vincent, that it was « integral to French culture ». The struggle was waged in Paris, « where a local conflict, stirred up by one individual, took on the dimensions of a scientific revolution » (22). Guyton was, I believe, correct in his assessment that the revolution was consummated by 1787. By that time the most important experimental and theoretical confrontations on which the issue hung were essentially over. What remained was to consolidate the position attained — by capturing for its linguistic expression the movement for nomenclature reform that Guyton had initiated within the older chemistry, by packaging it in textbook forms, by spreading its influence through a new journal created by its partisans, and by overcoming the long-entrenched attitudes of those who were, either geographically or professionally, further

(22) Bensaude-Vincent, *op. cit.* in n. 2, 283-284 : « *La révolution chimique apparaît d'abord comme un événement intégré à la culture française : c'est dans un milieu où se banalise l'expression de « révolution », où la chimie jouit d'un prestige social et d'un intérêt public, où la communauté scientifique est organisée autour de l'Académie de Paris, qu'un conflit local, suscité par un individu, a pris la dimension d'une révolution scientifique.* »

removed from the immediate arena within which the contest had been decided.

To revive a traditional view of the central meaning of the chemical revolution is not to maintain that the dynamics of the revolutionary process have already been fully elucidated. Nineteenth century views of the event were often distorted by heroic images of Lavoisier and by reactions against these images. Twentieth century accounts often amounted to variations on a canonical tale. The historians who began to scrutinize the revolution in closer detail in the post-war period focused more on the origins of Lavoisier's ideas, or on the conceptual variations in the « later phlogiston theory », than on the underlying strata of experimental investigations on which both were constructed. The most recent interpretations of the contest between the phlogistonists and the antiphlogistonists have turned attention to rhetorical strategies, the emotional tones of the debate, contrasting values concerning the practice of science, cultural factors that conditioned opinions, on the campaigns orchestrated by Lavoisier and his followers to win converts, and on that of their foes to hold ranks (23). These studies have enlarged our understanding of the revolutionary arena, but they must be balanced by closer attention to the evolving structure of the theories, the arguments given in support of them, and the experimental investigations pertinent to them, on both sides of the divide. Lavoisier's relation to the phlogiston theory was more intimate, sometimes more ambivalent, and more vacillating than it is commonly portrayed. His confidence concerning his own theoretical structure was sometimes less robust than his eventual triumph allows us easily to recognize. The meaning of the phlogiston theory and its applications among the adherents of that theory were also shifting throughout the period of controversy. The story both of the pre-revolutionary context and the events that followed Lavoisier's public break with the Stahlian system in 1777 still requires reconstruction in finer detail, if we are ever to understand Lavoisier's revolution in its full richness and complexity. Here I can

(23) See especially C. E. Perrin, The Triumph of the Antiphlogistians, in Gough, *op. cit.* in n. 7, 40-63; Bensaude-Vincent, *op. cit.* in n. 2, 255-284; Arthur Donovan, *Antoine Lavoisier : Science, Administration and Revolution* (Oxford, UK-Cambridge, Mass. : Blackwell, 1993), 157-187; Jan Golinski, *Science as Public Culture : Chemistry and Enlightenment in Britain, 1760-1820* (Cambridge : Cambridge Univ. Press, 1992), 129-152.

only sketch a few highlights of some of the developments that should eventually be incorporated into a long narrative.

The sparse documentation concerning the development before 1770 of Lavoisier's knowledge and interests in chemistry has left much room for historical speculation about the beginnings of his chemical career (as well as about its relation to his multiple other scientific interests in mineralogy, geology, geometry, experimental physics, botany and technology). During the last few years, however, scholars have identified in the vast collection of surviving Lavoisier manuscripts several documents that provide strategic clues about his early chemical orientation. Bensaude-Vincent has published and analyzed a pedagogical discourse written by Lavoisier in 1792 that provides retrospective insights concerning his dissatisfactions with the chemistry courses he had followed in his youth (24). Marco Beretta has identified a manuscript, written in an unknown hand, but with corrections by Lavoisier, that may represent part of a course in chemistry that Lavoisier planned as early as 1764. If so, this document provides strategic evidence for some of Lavoisier's general views of the subject at that fledgling stage in his career (25). Beretta has also found that Lavoisier purchased an unpublished Latin manuscript of Stahl's treatise on sulfur, perhaps in 1766, and heavily annotated the sections on combustion (26).

According to Beretta, the latter document will establish a « new significance » for previous studies of the origins of Lavoisier's experiments of 1772 and 1773 on combustion. Assuming that « its pages constitute the main source of his early experiments on the calcination of metals », Beretta believes that Lavoisier's study of Stahl's text helped him « to decide the experimental priorities that eventually led to his new theory of calcination » (27). Beretta intends in a future work to publish the specific annotations that Lavoisier wrote on his copy of the manuscript, and to explicate its role in the origins of Lavoisier's study of combustion. This new document is obviously a strategic addition to our knowledge of Lavoi-

(24) Bensaude-Vincent, *op. cit.* in n. 2, 292; *Id.*, A view of the chemical revolution through contemporary textbooks : Lavoisier, Fourcroy, and Chaptal, *British Journal for the History of Science*, 23 (1990), 435-460.

(25) Marco Beretta, *A New Course in Chemistry : Lavoisier's First Chemical Paper* (Firenze : Leo S. Olschki, 1994).

(26) Beretta, *op. cit.* in n. 9, 161-168.

(27) *Ibid.*, 167-168.

sier's thought on the problem that shaped his career, and all scholars concerned with the nature of the chemical revolution will await its publication with interest. It is unlikely to provide a new « key » to the origin of the revolution, however, until it is situated more broadly within the context of the chemistry to which Lavoisier would already have been exposed when he procured it. Whether he derived his initial ideas concerning combustion mainly from the lectures of Guillaume-François Rouelle, from those of the less known, but no less lucid Charles-Louis de La Planche that he had previously attended, or by reading in such sources as the influential *Dictionnaire de chimie* published anonymously by Pierre-Joseph Macquer in 1766, is less crucial than the fact there was in this period among the French teachers of chemistry a strong consensus in favor of the great importance of Stahl's phlogiston theory.

Rouelle is usually credited with having popularized phlogiston in Paris through his widely attended lectures (28). The most coherent discussion of the integrative power of phlogiston to explain the relations between phosphorus, sulfur and a hypothetical « nitrous sulfur » and the respective acids their combustion produced, as well as those between the calcination of metals, the combustion of charcoal, the reduction of metals in the presence of charcoal, and a series of related chemical « operations », was that provided in Macquer's *Dictionnaire* (and also in somewhat less concentrated form in his earlier chemical textbooks). What most impressed Macquer about the phlogiston theory, in contrast to a family of similar earlier ideas about an « inflammable principle », was that Stahl had shown how chemists can « remove the inflammable principle from a compound to pass it into a new combination, without combustion, without dissipating it, thereby furnishing chemists with the means to make the most important observations on the effects it produces in an infinity of chemical operations ». Through the fact that phlogiston from any source combined with vitriolic acid produced the same sulfur, Stahl had demonstrated, in Macquer's words, that « there is in nature only one single inflammable principle, always identical to itself ». It was for such reasons that Macquer asserted that « the illustrious Stahl [...] has in a

(28) Guerlac, *op. cit.* in n. 6, 32. Guerlac relied for this interpretation on an unpublished MA thesis by Rhoda Rappaport, entitled « G. F. Rouelle, his *Cours de chimie* and their Significance for Eighteenth Century Chemistry », Cornell Univ., 1958.

certain sense created a new chemistry, and has entirely changed the face of that science » (29).

To appreciate the compelling case that Macquer could still make for phlogiston at the time he wrote his *Dictionnaire,* the modern historian must not only analyze textual discourse, but gain a feeling for the chemical operations carried out in contemporary laboratories. Mid-century chemists applied the Stahlian doctrine not only as a general explanation for the phenomena of combustion and calcination, but as a discriminating guide in innovative research. Within the domain of the qualitative techniques dominating the identification of substances and the analysis of their composition, the phlogiston theory functioned remarkably well (30). Lavoisier too undertook his first chemical investigations within this conceptual and operational framework.

The circumstances of Lavoisier's chemical education suggest that the manuscript of Stahl that he acquired and studied with care is less likely to have been the initial source of his interest in combustion than a sign of his interest in tracing to its original source the ideas on combustion that already saturated the milieu in which he entered the field. Moreover, Lavoisier was likely to have been perceptive enough to recognize Stahl's writings as more of historical than of current importance. Their author had died more than thirty years before, and the progress of chemistry since then was generally seen by contemporaries as on an accelerating course. Although it is risky to project backward Lavoisier's later attitudes, the fact that in his famous « Réflexions sur le phlogistique » Lavoisier chose in 1785 to summarize « the doctrine of Stahl », not in the form in which Stahl had originated it in an earlier « epoch », but « as it was conceived and presented by M. Macquer » in his *Dictionnaire de chimie* (31), is suggestive evidence that Lavoisier

(29) [P.-J. Macquer], *Dictionnaire de Chymie* (Paris, 1766), vol. 2, 199-220 : « *Quoi qu'il en soit, le pouvoir qu'ont les Chymistes d'enlever le principe inflammable d'un composé, & de le faire passer dans une nouvelle combinaison, sans combustion & sans qu'il se dissipe, leur a fourni les moyens de faire les observations les plus importantes sur les effets qu'il produit dans une infinité d'opérations chymiques...* » (203) ; « ... *il n'y a dans la nature qu'un seul principe inflammable, toujours le même, toujours exactement semblable à lui-même. [...] Nous devons la connoissance de ces vérités si importantes, aux Chymistes modernes & sur-tout à l'illustre Stahl, qui a créé par-là en quelque sorte une Chymie nouvelle, & fait entierement changer la face à cette science* » (200).

(30) See Jon Eklund, « Chemical Analysis and the Phlogiston Theory », Ph. D. Diss., Yale Univ., 1972.

(31) A.-L. Lavoisier, Réflexions sur le phlogistique, in *Œuvres de Lavoisier*, vol. 2

may well have understood Macquer's rendition of the phlogiston theory from the beginning to be the one most pertinent to the current state of the field.

Of the several recent efforts, beginning with Guerlac's *The Crucial Year,* to reconstruct the immediate events that led Lavoisier in the fall of 1772 to undertake the experiments on combustion and calcination that mark the beginning of his route toward a new general theory of combustion, the latest and most persuasive was published in three articles written by Carl Perrin just before his untimely death in 1988 (32). The revelations that the Stahl text found by Beretta might provide will probably require revision of Perrin's view that before 1772 Lavoisier took the phlogiston theory for granted without devoting special attention to it, but they are unlikely to undermine the dense account that Perrin has given us of Lavoisier's thoughts and actions on the threshold of, during, and following these crucial investigations. Perrin illuminates also in new ways the shifting attitudes of Lavoisier toward Stahl's phlogiston theory during this formative period.

In the famous « sealed-note » that he deposited at the Academy of sciences on November 1, 1772, containing his first brief description of his discovery that the weight gains of phosphorus and sulfur during their combustion « come from a prodigious quantity of air that is fixed », and that the reduction of lead calx to the metal « disengages a considerable quantity of air », Lavoisier made the widely-quoted comment (33) : « This discovery appeared to me one of the most interesting that has been made since Stahl. » Perrin argues plausibly that, at this point Lavoisier was not hinting at an eventual rejection of Stahl's theory, but implying that his continuing investigation would complement what Stahl had achieved.

(Paris : Académie des sciences, 1862), 624 : « *A l'époque où Stalh a écrit, les principaux phénomènes de la combustion étoient encore ignorés. [...] j'ai pensé [...] que je pourrois m'en tenir à la doctrine de Stalh, telle qu'elle a été conçue & présentée par M. Macquer.* »

(32) C. E. Perrin, Lavoisier's Thoughts on Calcination and Combustion, 1772-1773, *Isis,* 77 (1986), 647-666; *Id.,* Research Traditions, Lavoisier and the Chemical Revolution, *Osiris,* 4 (1988), 53-81 ; and *Id.,* Document, Text and Myth : Lavoisier's Crucial Year Revisited, *British Journal for the History of Science,* 22 (1989), 3-25.

(33) From the text reprinted in Guerlac, *op. cit.* in n. 6, 227-228 : « ... *j'ai fait la réduction de la litharge dans des vaisseaux fermés, avec l'appareil de Hales, et j'ai observé qu'il se dégageait, au moment du passage de la chaux en métal, une quantité considérable d'air [...]. Cette découverte me paraissant une des plus intéressantes de celles qui aient été faites depuis Stahl, j'ai cru devoir m'en assurer la propriété...* »

Just as Stahl had been able to trace the passage of the inflammable matter from one combination to another, so Lavoisier thought he was now in a position to begin tracing the passage of air from one substance to another. In a draft memoir which Perrin has concluded Lavoisier wrote shortly afterward, however, Lavoisier was already thinking about altering, although not overturning, Stahl's theory. « It is evident, he wrote then, that Stahl's theory on the calcination and the reduction of metals is extremely imperfect, and that it requires modifications. He considers all calcinations as only a loss of phlogiston, whereas it is proved that there is at the same time the loss of phlogiston and absorption of air (34). »

That was Lavoisier's position at the end of 1772. During the first months of 1773, he not only extended his own experiments, but read widely in the literature related to airs and the processes that fix or release it. During this time, according to Perrin, Lavoisier grew privately more skeptical about the phlogiston theory. In an early draft of a memoir he planned to deliver at the public meeting of the Academy at Easter, he wrote : « I have even come to the point of doubting whether what Stahl calls phlogiston exists, at least in the sense he gives to the word. It seems that in all cases one can substitute for it the name of matter of fire, of light, and of heat. » In the final version, however, he said instead : « It would be appropriate to make some reflections here on the doctrine of Stahl concerning the principle he called phlogiston [...], but my experiments are not yet complete enough to dare enter into a contest with the celebrated chemist (35). »

(34) Perrin, Research Traditions..., *op. cit.* in n. 32, 73-74; *Id.*, Lavoisier's Thoughts, *op. cit.* in n. 32, 664. I have slightly modified Perrin's translation of this passage, using his transcription of the memoir « Sur la cause de l'augmentation de pesanteur qu'acquièrent les métaux et quelques autres substances par la calcination » (Arch. Acad. sci., Lavoisier, 1303) : « *Il est évident que la théorie de Stalh sur la calcination et la réduction des métaux est extremement imparfaite et quelle demande des modiffications. Il n'a regardé toute calcination que comme une perte de phlogistique tandis qu'il est prouvé qu'il y a à la fois perte de phlogistique et absorbtion d'air.* »

(35) Perrin, Research Traditions..., *op. cit.* in n. 32, 75, according to the memoir « Sur une nouvelle théorie de la calcination et de la réduction des substances metaliques, sur la cause de l'augmentation de poids qu'elles acquièrent au feu par la calcination et sur quelques autres phenomenes qui appartiennent à l'air fixe » (Arch. Acad. sci., Lavoisier, 1303) : « *Je suis même parvenu au point de douter si ce que Sthal appelle phlosgistique existe, du moins dans le sens qu'il donne à ce mot. Il me semble que dans tous les cas on peut lui substituer le nom de matière du feu, de la lumière et de la chaleur.* »

René Fric, Contribution à l'étude de l'évolution des idées de Lavoisier sur la nature

Perrin did not live to continue his account of Lavoisier's progress past the spring of 1773. His treatment suggests that Perrin accepted the conventional historical view that from this time forward Lavoisier privately rejected phlogiston. According to this view, Lavoisier delayed attacking what he had privately abandoned, until he could develop a strong enough case to challenge successfully the prevailing orthodoxy. That is an impression that Lavoisier himself fostered by commenting, several years later when he did make his public break, that when he had first laid « the foundations » of his own theory, he was « too little confident of [his] own enlightenment » to dare to express an opinion « directly contrary to the theory of Stahl and to that of the many celebrated men who followed him » (36). Detailed study of Lavoisier's long odyssey through the intervening years suggests, however, that at the end of it, when all seemed clear to him, he no longer recalled the deep ambivalence in his earlier views, even in those he held privately.

In another memoir that Lavoisier wrote in April, 1773, he had expressed an unsteady compromise between his belief in late 1772 that there may be two distinct processes — the release of phlogiston and the absorption of air — involved in combustion and calcination, and the opinion he entertained in the draft of his Easter memoir, that « in all cases » a « matter of fire » could be substituted for phlogiston. « Every time in which we produce a fixation of air, he wrote, there must be a disengagement of phlogiston, or matter of fire », and every time we release the air which has been fixed, « we can only attain it [as air] by rendering to it a sufficient quantity of fire, or of phlogiston, to constitute it in a vapor state ». Further on he wrote : « If the air is, as I think, composed of [...] a matter which forms its base and of phlogiston,

de l'air et sur la calcination des métaux, *Archives internationales d'histoire des sciences*, 12 (1959), 137-168, here 162 : « *Ce seroit Sans doute icy le lieu de faire quelques refflexions Sur la doctrine de Sthalh sur le principe quil appelloit Plogiston [...], mais mes experiences ne Sont point encore assez Complettes pour oser entrer en lice avec Ce Celebre chimiste.* »

(36) A.-L. Lavoisier, Mémoire sur la combustion en général, *Mémoires de l'Académie des Sciences pour 1777*, 593 : « *J'ai déjà jeté les premiers fondemens de cette hypothèse*, pages 279 & 280 du premier Tome de mes Opuscules physiques & chimiques, *mais j'avoue que peu confiant dans mes propres lumières, je n'osai pas alors mettre en avant une opinion qui pouvoit paroître singulière, & qui étoit directement contraire à la théorie de Sthal, & à celle de plusieurs Hommes célèbres qui l'ont suivi.* »

and in every calcination and combustion there is a fixation of air, one must conclude that [...] there is always a disengagement of phlogiston in these operations, otherwise what becomes of the phlogiston of the air, which, becoming free produces the heat and the flame (37) [?] » Here Lavoisier appears to have covered his indecision about whether to modify or to replace phlogiston, by juxtaposing the terms « phlogiston » and « matter of fire » and treating them as interchangeable. By the end of 1773, in his *Opuscules Physiques et Chymiques,* he had temporarily stabilized a different form of the same compromise, by placing his unanswered questions within the range of modified roles for phlogiston. « Reflecting » there on the experiments on the reduction of lead with charcoal that he had just described, he asked whether the charcoal served « as the disciples of Stahl think, to render to the metal the phlogiston that it has lost? Or rather do these materials enter into the composition of the elastic fluid? » Acknowledging that « the present state of our knowledge » does not yet permit a pronouncement on the subject, Lavoisier nevertheless ventured the « conjecture » that the inflammable principle was fixed in the air released, and it was on that « which depends its state of elasticity ». Although the idea « may appear remote from that of Stahl, it is perhaps not incompatible with it », Lavoisier added, because it was possible that the charcoal both rendered to the metal its inflammable principle and to the air « the principle that constitutes its elasticity » (38).

(37) Fric, *op. cit.* in n. 35, 149, 151 : « ... *toutes les fois que nous opererons une fixation dair il doit y avoir degagement de phlogistique ou de matierre du feu de meme lorsque lair aura ete une fois fixé et que nous voudrons le reviviffier, nous ne pourrons y parvenir quen lui rendant la quantité de matierre du feu ou de phlogistique necessaire pour le Constituer dans letat de fluide en vapeurs* » (149) ; « *Si lair est en effet Comme je le pretens composé de deux Substances dune matierre qui forme Sa base et de phlogistique de ce que dans toute calcination et dans toute inflammation il y a fixation dair il en faut Conclure que dans chacune de ces operation il doit toujours y avoir degagement de phlogistique autrement que deviendroit le phlogistique de lair or cest le* phlogistique meme *de lair qui devenant* libre *forme la chaleur et la flamme* » (151).

(38) A.-L. Lavoisier, *Opuscules Physiques et Chymiques* (Paris, 1774), 279-280 : « *Serventelles, comme le pensent les disciples de M. Stalh, à rendre au métal le phlogistique qu'il a perdu ? c'est sur quoi il me semble que l'état actuel de nos connoissances ne nous permet pas encore de prononcer* » (279) ; « *S'il étoit permis de se livrer aux conjectures, je dirois que quelques Expériences [...] me portent à croire que tout fluide élastique résulte de la combinaison d'un corps quelconque solide ou fluide, avec un principe inflammable, ou peut être même avec la matiere du feu, & que c'est de cette combinaison que dépend l'élasticité* » (279-280) ; « *Ce sentiment, quelqu'éloigné qu'il paroisse à celui de M. Stalh, n'est peut-être pas incompatible avec lui* » (280) ; « ... le principe qui constitue son élasticité » (280).

Passages such as these, in which a « matter of fire » representing both the heat released in combustions and the principle of « elasticity » sometimes merges with, sometimes is distinguished from, and sometimes replaces phlogiston, would seem to support the interpretation some historians have long maintained, that what Lavoisier eventually named caloric was only a modified version of phlogiston. After he had made his break from the phlogiston theory, according to this view, he severed their connections and obscured the derivation of one from the other. One could also regard his early vacillation over whether to discard phlogiston or change it to fit his own developing views of combustion and the elastic state of matter as a question of political and rhetorical strategies. Would he be better off to placate the celebrated men « born to the cult » of phlogiston, or challenge them to abandon a central feature of their chemical heritage? Lavoisier's course over the next few years, however, fits less easily the image of a wily tactician than of a man sincerely groping to clarify his own somewhat incoherent views and to connect what he was discovering to the existing structure of the field. Having moved rather quickly to the point of doubting the existence of phlogiston, he seems afterward to have doubted his own doubts. For subsequent periods in Lavoisier's scientific life I have found that it is often possible to connect his shifting views, his inconsistencies and ambiguities, with the immediate vicissitudes in his ongoing experimental pathway. His surviving laboratory notebooks commence with the beginning of the systematic program of investigations he set out for himself early in 1773, and provide a rich record of its formative stage. My preliminary examination of the record for this period suggests that at first his experiments often did not work as he intended them to, and that the experimental style for which he afterward became famous was in part forged by his efforts to cope with his early difficulties. A close reconstruction of his experimental progress during this year would, I believe, illuminate as much as external circumstances do, the vagaries in his theoretical position highlighted in the passages cited above. In any case, Lavoisier did not necessarily defer a more open challenge because he feared to offend an entrenched establishment, but rather because he genuinely felt that « only time and experience will allow us to fix our opinions » (39).

(39) *Ibid.*, 281 : « *C'est au temps seul & à l'expérience, qu'il appartiendra de fixer nos opinions* » (281).

If Lavoisier were really as unsure of his attitude toward the phlogiston theory through 1773 as I maintain, how can we explain the self-confidence with which he predicted early in that year that the investigative agenda he had by then set for himself appeared important enough « to cause a revolution in physics and chemistry (40) »? Historians have perhaps assumed too readily from this precocious remark that Lavoisier was so prescient as to have from the beginning an intuitive grasp of all that he would later achieve. Perrin and Bensaude-Vincent have shown that, within the brief period between his first experiments in the fall of 1772 and the memoirs he wrote in the spring of 1773, the meaning of the « revolution » he foresaw took on several divergent profiles. In the earliest of his revolutionary allusions, the second version of the manuscript he wrote in the fall of 1772 on his recent discovery of the weight gain in calcinations and combustions, he classified his experiments as the « kind that overturn accredited systems, that open new directions of experiment and reason, and in a word, appear suited to cause a revolution in the science ». Perrin believed that at this time Lavoisier did not know what system his nascent experimental program would overturn. He simply expected that it would undermine some of the conventional assumptions about composition (41). The phrase about a revolution in physics and chemistry, quoted at the beginning of this paragraph, Lavoisier put down in his laboratory notebook in February, 1773. It preceded an assertion that previous authors had presented « separated portions of a great chain [...] but there remains a vast chain of experiments to provide a continuity (42) ». Here therefore, Lavoisier seemed to mean by revolution, not a rupture and overthrow of an existing system, but a synthesis of fragmentary previous work into a new whole. When he used the word again in his Easter memoir, however, he returned to its more disruptive meaning. « I believe, he wrote, that the present theory of chemistry is defective in many parts, and that this science is approaching the epoch of a nearly com-

(40) From the text reprinted in Guerlac, *op. cit.* in n. 6, 230 (see n. 2).

(41) Perrin, Research Traditions..., *op. cit.* in n. 32, 73-74; *Id.*, Lavoisier's Thoughts..., *op. cit.* in n. 32, 665.

(42) Guerlac, *op. cit.* in n. 6, 230 : « ... *des portions séparées d'une grande chaîne [...]. Mais il reste une suite d'expériences immense à faire pour former une continuité.* »

plete revolution (43). » Placed just after the passage, previously discussed, in which he said that he was not ready to enter a contest with Stahl, this statement strongly suggests that the phlogiston theory constituted one of the prominent « defects » that the coming revolution would correct; but it seems evident that he expected it would also be a much broader upheaval, in ways that he could not yet specify. There is no reason to conclude, however, that from this day on Lavoisier fixed his sights on becoming the leader of such a great chemical revolution (44).

It took Lavoisier nearly four more years to « fix his opinions » concerning phlogiston and the alternative theories he could devise for the processes thought to involve it. In that long interval he continued sometimes to merge his matter of fire with phlogiston, sometimes to separate their roles, and sometimes to replace one with the other. He eliminated phlogiston from the explanation of some processes even while retaining it in other cases. At least through 1775, and probably longer, he exhibited nearly as much ambiguity and incoherence on these questions as during the first year that he had grappled with them. I have described Lavoisier's extended struggle to resolve these issues elsewhere, and space does not permit me to summarize them here (45). One point that I did not then recognize needs to be emphasized, however, for it helps us to understand the persistence of his perplexity. Beginning in 1774, Lavoisier had to contend with a different, more current, and more expansive phlogiston theory than the one he had confronted in the fall of 1772.

When he began his first experiments on combustion and calcination, the phlogiston « system » with which he had to deal was that of Stahl, probably as interpreted by Rouelle and Macquer, that he had known since his youth. From 1774 onward, Lavoisier was following in the wake of Joseph Priestley's epoch-making experiments on airs. Priestley's interpretation that the newly identified airs differed mainly in their content of phlogiston forced Lavoisier

(43) Fric, *op. cit.* in n. 35, 162 : « *Je Crois Cependant en avoir asses dit pour faire sentir que la theorie actuelle des chimistes est deffectueuse dans bien des points et que Cette Science approche de l'époque d'une revolution presque Complette.* »

(44) For a more comprehensive treatment of the displacements in meaning of « revolution » for Lavoisier, see Bensaude-Vincent, *op. cit.* in n. 2, 117-138.

(45) Frederic Lawrence Holmes, *Lavoisier and the Chemistry of Life* (Madison-London : Univ. of Wisconsin Press, 1985), esp. 26-40.

either to accept these definitions or to arrive at a better account of their relationships. For an extended period he could do neither. Priestley's poweful discovery of « dephlogisticated air » impinged so strongly on Lavoisier's own efforts to identify the portions of the atmosphere fixed and released in calcinations and combustions that Lavoisier found it difficult to escape thinking about that substance within the framework set by the name its discoverer had given it. Even as he strove for a conceptual framework of his own, Lavoisier had moments in which he contemplated falling back into line with the theoretical leadership of the English scientist whose brilliant discoveries had inspired some of his own critical experimental moves (46).

Late in the summer of 1777, Lavoisier crossed his personal Rubicon. Having finally resolved the vexing questions about the composition of the various airs that had obstructed his views on combustion and calcination, he had become confident enough about a new general theory of combustion to present it publicly as a competitor to existing theories (47). In the draft of a manuscript he was preparing on the combustion of candles, he wrote :

> « Besides, since I am at the point of attacking the entire doctrine of Stahl concerning phlogiston, and of undertaking to prove that it is erroneous in every respect, if my opinions are well-founded, M. Priestley's phlogisticated air will find itself entangled in the ruins of the edifice (48). »

Lavoisier deferred his attack until December, and even then, by comparison with this militant statement, he muted his intentions. Beginning mildly, he offered his « new theory of combustion [...], or, rather, to speak with the reserve I impose on myself, a hypothesis with the aid of which one explains very satisfactorily all of the phenomena of combustion, calcination, and even a part of those which accompany respiration ». He acknowledged that the same phenomena « are explained in a very satisfactory manner in the

(46) *Ibid.*, 96-99.

(47) For a detailed description of the problems Lavoisier faced, and the way in which he resolved them, see *ibid.*, 96-120.

(48) Lavoisier, « De la combustion des chandelles dans l'air atmosphérique et dans l'air pur », Archives of the Académie des sciences, Lavoisier 1311 (3), p. [14-15] : « *Au reste comme je suis au moment d'attaquer par une suitte d'experiences la doctrine de Sthal sur le phlogistique et d'entreprendre de prouver qu'elle est erronée dans tous ses points, l'air phlogistiqué de M. Priestley si mes opinions sont fondées se trouvera enveloppé dans la ruine de l'édiffice.* »

hypothesis of Stahl », but the supposition that it required — that phlogiston is fixed in all combustible bodies — amounted to the vicious circle of « explaining combustion by combustion ». Phlogiston was only a hypothesis. Therefore, if he could « show that the same phenomena could be explained equally naturally by the opposite hypothesis, that is, without supposing that matter of fire or phlogiston exists in the bodies called *combustible,* the system of Stahl would find itself shaken to its foundations » (49).

This last phrase was a ringing declaration, but it stopped well short of claiming to prove that the entire doctrine of Stahl was erroneous. His purpose, he stated, « was not to substitute a rigorously demonstrated theory, but only a hypothesis which appears to me more probable, more conformable to the laws of nature », and to entail fewer « forced explanations and contradictions » (50). That is, he did not try to force his audience to abandon their allegiances to the phlogiston theory, but invited them to consider whether his theory might provide a better alternative.

The presentation of Lavoisier's memoir on combustion, which was reported to have caused a sensation at the Academy, must be regarded as the beginning of the chemical revolution, if we view the revolution as an abrupt rupture with the existing structure. This was the point at which he publicly and overtly « attacked the doctrine of Stahl (51) » for the first time. It was, however, not a violent beginning. Lavoisier did not storm the fortress. Rather, by his tactful approach to his peers, he seemed to wish to initiate a peaceful revolution.

(49) Lavoisier, *op. cit.* in n. 36, 592-593, 595 : « ... *je hasarde de proposer aujourd'hui à l'Académie, une théorie nouvelle de la combustion : ou plutôt, pour parler avec la réserve dont je me suis imposé la loi, une hypothèse, à l'aide de laquelle on explique d'une manière très-satisfaisante, tous les phénomènes de la combustion, de la calcination, & même en partie ceux qui accompagnent la respiration des animaux* » (592-593) ; « *Ces différens phénomènes de la calcination des métaux & de la combustion, s'expliquent d'une manière très-heureuse dans l'hypothèse de Sthal; mais* [...] *il est aisé de voir qu'en dernière analyse, c'est expliquer la combustion par la combustion* » (594-595) ; « ... *mais si je fais voir que ces mêmes phénomènes peuvent s'expliquer d'une manière toute aussi naturelle dans l'hypothèse opposée, c'est-à-dire, sans supposer qu'il existe de matière du feu, ni de phlogistique dans les matières appelées* combustibles, *le système de Sthal se trouvera ébranlé jusque dans ses fondemens* » (595).

(50) *Ibid.*, 600 : « ... *en attaquant ici la doctrine de Sthal, je n'ai pas pour objet d'y substituer une théorie rigoureusement démontrée, mais seulement une hypothèse qui me semble plus probable, plus conforme aux lois de la Nature, qui me paroît renfermer des explications moins forcées & moins de contradictions.* »

(51) *Ibid.*

In January, 1778, Macquer confessed in a letter to Guyton de Morveau that until he heard Lavoisier's presentation he had feared that Lavoisier had made « a great discovery » with which he would « overturn the phlogiston theory from top to bottom ». Afterward Macquer was greatly relieved. Lavoisier had claimed, as Macquer put it, that « there is no inflammable matter in combustible bodies, it is one of the constituent parts of the air ». In combustions the fire matter is disengaged from the air, leaving « only that which he calls *the base of the air,* which he admits is entirely unknown to him. You can judge whether I should have been so afraid ». In his memoir Lavoisier had described « pure air, that which Mr. Priestley calls *dephlogisticated air* », as a combination of the matter of fire « and another substance which enters as a base ». It was his inability to identify further that other substance that allowed Macquer to believe he had perceived a fatal flaw in Lavoisier's theory which disarmed it as a threat to « our old chemistry ». Lavoisier's confidence that the substance existed was based on the weight gains associated with combustions and on the overall coherence of his theoretical structure. It is not surprising that for the more traditional Macquer, attuned neither to the new views nor to Lavoisier's quantitative criteria for determining composition, this « base » of pure air appeared to be an ill-defined, hypothetical entity (52).

Neither Guyton nor Macquer was at this time shaken from his prior theoretical commitments. Historians have regarded Lavoisier as isolated in the wake of his failure in 1777 to persuade the French chemists to consider favorably his new theory of combustion. The only immediate adherent he acquired was Jean-Baptiste Bucquet, a chemist three years younger than Lavoisier himself. Generally overlooked, however, is how strategic Bucquet's conversion was. A rising star, Bucquet had become known, since the death of Rouelle in 1770, as the most brilliant teacher of chemistry in Paris. In charge of the public chemistry course of the

(52) *Ibid.*, 596. D. I. Duveen and H. S. Klickstein, A letter from Guyton de Morveau to Macquart relating to Lavoisier's attack against the *phlogiston* theory, *Osiris*, 12 (1957), 347 : « *M. Lavoisier m'effrayoit depuis long-temps par une grande découverte qu'il réservoit* in petto, *& qui n'alloit pas moins qu'à renverser de fond en comble toute la théorie du phlogistique ou feu combiné.* [...] *Suivant M. Lavoisier, il n'y a point de matière du feu dans les corps combustibles; elle n'est qu'une des parties constituantes de l'air.* [...] *Il ne reste plus que ce qu'il nomme* la base de l'air, *substance qu'il avoue lui être entièrement inconnu. Jugez si j'avois sujet d'avoir une si grande peur.* »

Faculty of Medicine, Bucquet was well placed to shape chemical education in the capital of French science. In 1778 he and Lavoisier began an investigative partnership that promised to expand and solidify the new theoretical structure that Lavoisier hoped to embed into the teaching and practice of chemistry. That matters did not turn out this way was due to Bucquet's unfortunate illness and death, in 1780, at the early age of 33. This loss, which Lavoisier felt very deeply, probably also disrupted more seriously than is usually recognized, the strategy he seems to have adopted, with Bucquet's collaboration, to bring about a general reform of chemical knowledge (53).

In the second edition of his *Dictionnaire de Chymie,* published in 1778, Macquer attached to the article on phlogiston retained from the first edition, a long addition in which he altered the conception of the inflammable principle still maintained in the first half of the entry. He now identified phlogiston with light, rather than with elementary fire, and he described combustion and calcination as processes in which « there is a diminution and absorption of the air which has supported the combustion ». This air displaces and disengages the phlogiston contained in the combustible body (54). Historians have long regarded Macquer's « modified » phlogiston theory as an attempted compromise between the old phlogiston theory and Lavoisier's theory of combustion. (Perrin has likened it to Tycho Brahe's planetary theory seen as a compromise between the Ptolemaic and the Copernican systems (55).) A close study of Macquer's text shows, however, that this was not what Macquer had in mind when he formulated his modifications. As J. R. Partington noted thirty years ago in his discussion of « the later phlogiston theory (56) », Macquer was defending the phlogiston theory, not against Lavoisier's first attack on it, but against a critique of the « chemist's » phlogiston made by Georges-Louis de Buffon in 1774.

In a treatise entitled « De la Lumière, de la Chaleur et du Feu »,

(53) See Holmes, *op. cit.* in n. 45, 130-138, 145-146.

(54) P.-J. Macquer, *Dictionnaire de Chymie,* 2nd ed. rev. (Paris, 1778), vol. 3, 99-144, here 133 : *« Il y a diminution et absorbtion de l'air qui a concouru à cette combustion. »*

(55) See, for example, Max Speter, *Lavoisier and seine Vorläufer* (Stuttgart : Ferdinand Enke, 1910), 42; Henry Guerlac, *Antoine-Laurent Lavoisier : Chemist and Revolutionary* (New York : Charles Scribner's Sons, 1975), 105; Perrin, Research Traditions..., *op. cit.* in n. 32, 43.

(56) J. R. Partington, *A History of Chemistry,* vol. 3 (London : MacMillan, 1962), 616.

published in the first supplementary volume of his *Histoire Naturelle,* Buffon attempted « to deduce the principal operations of nature » from the first « principles of rational mechanics ». His fundamental principle was that « there is only one common matter », which is « always ready to attract or to repel itself, depending on the circumstances ». Heat, light, and fire were, according to Buffon, modified forms of this common matter. Among the many phenomena that he explained by his principles were combustion and calcination. The differences between those cases in which weight was gained or lost he attributed to whether attraction or repulsion prevailed in the products. Along the way, Buffon remarked that « the famous phlogiston of the chemists (a creature of their method more than of nature) is not a simple and identical principle, as they present it. It is a compound, a product of mixture, the result of the combination of two elements, air and fire, fixed in bodies ». We cannot here enter into Buffon's argument for his view, except to say that he based it on his broad overview as a naturalist, rather than on detailed chemical evidence. « I sense », Buffon wrote, that his assertions « may be rejected, especially by those who have studied nature only by way of chemistry; but I ask them to consider that their method is not that of nature » (57).

Macquer seemed deeply stung by Buffon's criticism, less because of its pertinence than because of the celebrated author who had launched it. Buffon, he complained, was tarnishing « all physical scientists who occupy themselves with chemistry ». The general public, who knew chemistry « only as a name » and had not studied it seriously, was apt to accept the illustrious Buffon's ideas uncritically. To forestall that eventuality, Macquer defended with passion

(57) G.-L. Buffon, *Histoire Naturelle, Générale et Particulière,* suppl. vol. 1 (Paris, 1774), 1-78, quotes 18, 44, 47, 77 : « *Et de ces grands principes qui tous sont fondés sur la mécanique rationnelle, j'ai essayé de déduire les principales opérations de la Nature...* » (77); « ... *il n'existe en un mot qu'une seule force & une seule matière toujours prête à s'attirer ou à se repousser suivant les circonstances* » (18); « *Le fameux Phlogistique des Chimistes (être de leur méthode plutôt que de la Nature), n'est pas un principe simple & identique, comme ils nous le présentent : c'est un composé, un produit de l'alliage, un résultat de la combinaison des deux élémens, de l'air & du feu fixés dans les corps* » (44); « *Je sens que cette dernière assertion ne sera pas admise, & pourra même être rejetée, sur-tout par ceux qui n'ont étudié la Nature que par la voie de la chimie : mais je les prie de considérer que leur méthode n'est pas celle de la Nature, qu'elle ne pourra le devenir ou même s'en approcher qu'autant qu'elle s'accordera avec la saine physique, autant qu'on en bannira, non-seulement les expressions obscures & techniques, mais sur-tout les principes précaires, les êtres fictifs auxquels on fait jouer le plus grand rôle, sans néanmoins les connoître* » (47).

the belief, already stated in the first edition of his *Dictionnaire,* that phlogiston, from whatever source, was always identical with itself. His new identification of phlogiston with light did not require major changes in his previous concept of the inflammable matter, because Macquer had now concluded that « pure fire » itself was identical with light. He believed, however, that this identification removed some of the obscurity surrounding the loose association of fire with heat in his previous treatment. Heat he now viewed as the motion of ordinary matter, whereas fire, or light, was a particular form of matter (58).

The identification of phlogiston with light did enable Macquer to explain one phenomenon, unknown at the time of his first edition, which had become an embarrassment to the old « system of Stahl » : that is, the reduction of mercury calx without charcoal, the presumptive source of the inflammable matter in previously known reductions. « Mercury [calx] is only reduced to flowing mercury in these experiments, he wrote, because the matter of light, which can pass through the vessels, especially when they are red-hot, combines in sufficient quantity, and intimately enough, with the mercury calx, to become its phlogiston, and to reestablish its metallic state (59). »

Macquer did not specify the source of the experiments which « demonstrated » that air is absorbed in combustions, but he most likely had in mind those of Lavoisier. Far from a difficulty for Macquer, this « fact » provided him with welcome support for his argument against Buffon's idea that phlogiston is a mixture of fire and air. « Far from the matter of fire requiring a combination with air to be fixed in bodies and to become the phlogiston of nature, he countered, these two elements have, on the contrary, a kind of incompatibility, as they reciprocally drive one another out, and one cannot be fixed in a body without excluding the other (60). »

(58) Macquer, *op. cit.* in n. 54, vol. 3, 121-132 : « *Voilà un arrêt qui, de la part dont il vient, seroit certainement une flétrissure éclatante pour tous les Physiciens qui se sont occupés de la Chymie, depuis le renouvellement des Sciences, s'il étoit mérité, & qu'il eût été prononcé en connoissance de cause* » (127).

(59) *Ibid.*, 141 : « *[Les chaux] du mercure ne se réduisent en mercure coulant dans les expériences dont il s'agit, que, parceque la matiere de la lumiere, qui peut passer à travers les vaisseaux, sur-tout lorsqu'ils sont rouges, se recombine en quantité suffisante & assez intimement avec la chaux de mercure, pour devenir son phlogistique, & la rétablir par-là dans son état métallique.* »

(60) *Ibid.*, 134 : « ... *bien loin que la matiere du feu ait besoin du concours & de l'alliage de l'air pour se fixer dans les corps, & devenir le phlogistique de la nature, ces deux élémens ont au contraire une espece d'incompatibilité, puisqu'ils se chassent réciproquement, & que l'un ne peut se fixer dans un corps, sans donner l'exclusion à l'autre.* »

Macquer's article on phlogiston in the second edition of his *Dictionnaire* gives evidence of having been a rather hasty response to Buffon's critique. Rather than revising the whole entry, he reprinted his original article with minor changes and tacked his new views onto the end of it. The two sections are in some respects redundant, in other respects incoherent. The lack of any explicit reference to Lavoisier's experiments, and even of any implicit allusion to his new theory of combustion, suggests that the article, which appeared only a few months after Lavoisier presented his theory to the Academy, may have been written before Macquer knew about the latter. At any event, there is no hint in his « modified » phlogiston theory of the fear for its future that Macquer expressed in his letter to Guyton.

Later on, Macquer did come to see his revised treatment of phlogiston as an intermediate position between the old system and that of Lavoisier. In a review of Antoine Fourcroy's new chemistry text in 1782, Macquer expressed satisfaction that Fourcroy had « not taken the violent expedient of rejecting phlogiston altogether ». It was necessary, however, to add « to the sublime theory of Stahl that in all combustions pure air alone can disengage the matter of fire from its bonds of combination ». In the restoration of sulfur from vitriolic acid, « it is wiser to connect the two theories, and to admit the separation of phlogiston from the charcoal at the same time as the separation of the air from the acid ». Macquer understood, however, that his compromise was not a permanent resolution of the question. « The future, he added, will make known which of these opinions will ultimately survive the others (61). »

By 1783 the shape of « the future » was becoming clearer to Macquer. Summarizing in *Le Journal des Sçavans* a letter he had received from the Count Morozzo describing Morozzo's experiments on combustion and calcination in several of the recently

(61) Quoted in Guyton, Second Avertissement, in *op. cit.* in n. 21, 628-629 : « *[Macquer] n'oublie pas d'avertir que [Fourcroy]* n'a pas pris le parti violent de rejeter entièrement le phlogistique; *mais il reconnait déjà qu'il faut ajouter à* la sublime théorie *de Stahl* que, dans toute combustion, l'air pur peut seul dégager la matière du feu des liens de la combinaison; *il avoue, par rapport à la restitution de l'acide vitriolique à l'état de soufre,* qu'il est plus sage de lier les deux théories & d'admettre la séparation du phlogistique du charbon, en même temps que la séparation de l'air de l'acide » (628); « La suite fera connoître laquelle de ces opinions survivra enfin à toutes les autres » (629).

identified gases, Macquer commented that « M. Lavoisier [...], who has already carried out capital investigations on these subjects, and who pursues them with utmost zeal, holds, concerning that topic, a completely new idea, to which he has given much probability with a great number of very beautiful experiments. [...] Many of the observations of the Count Morozzo are more favorable than contrary to the system of Lavoisier (62) ». This was Macquer's last statement on the subject : three months later he died. Although falling short of an endorsement of Lavoisier's system, these remarks suggest that Macquer was preparing himself for the prospect that Lavoisier's theory would be the one to survive what he himself had previously supported. Considering the depth of Macquer's attachment to the Stahlian system, and the dread that the possibility that Lavoisier could overturn phlogiston had once aroused in him, the openness to this denouement that he had reached by the end of his life is testimony to the fairness and flexibility of this last survivor from the generation of French chemists that had once been inspired to see in Stahl's theory the foundation for a new chemistry.

William Smeaton has written that these passages indicate that Macquer « was very near to accepting the antiphlogistic theory just before his death (63). » Whatever we surmise Macquer might have done had he lived longer, the attitude he maintained at the end should prompt us to reconsider the tones in which we have depicted the « struggle » between phlogistonists and antiphlogistonists. Perrin stressed in his interpretation of the campaign of the antiphlogistonists that familiar accounts of the reception of Lavoisier's views have exaggerated Lavoisier's « degree of isolation in the period prior to 1785 », and portrayed « the early conversions [after that time] as more sudden and straightforward than they were » (64). Beretta rejects Perrin's view on the grounds that no

(62) P.-J. Macquer, Lettre de M. le Comte Morozzo à M. Macquer, *Le Journal des Sçavans* (1783), 867 : « *M. Lavoisier, de l'Académie des Sciences, qui a déja fait sur ces objets de grands travaux, & qui les suit avec le plus grand zèle, a sur cela une idée absolument neuve, à laquelle il a déja donné beaucoup de vraisemblance par un grand nombre de très-belles expériences. Nous nous contenterons de faire remarquer que plusieurs des faits & des observations de M. le Comte Morozzo sont plutôt favorables que contraires au systême de M. Lavoisier.* »

(63) William Smeaton, MACQUER, Pierre Joseph, in *Dictionary of Scientific Biography*, ed. C. C. Gillispie (New York : Charles Scribner's Sons, 1970-1980), vol. 8, 620.

(64) Perrin, *op. cit.* in n. 23, 41-43.

one before 1785 had « dared to say [...] officially [that] phlogiston was a word devoid of scientific meaning » (65). In adjudicating such divergent historical claims, we should cautiously acknowledge that the documentary evidence available to assess the degree of support or lack of support that Lavoisier experienced in Paris in this period is very impoverished. Most of the scientists engaged in the issue were in frequent personal contact. The many conversations that would reveal the nuances in their positions relative to Lavoisier's theory are lost to us. Nevertheless, Perrin's interpretation seems inherently reasonable. Beretta's resistance to it is symptomatic, I believe, of a strong tendency in the historiography of the chemical revolution to intensify its drama by highlighting crisis, conflict, campaigns for conversion and resistance, and contests for power and authority. These biases have reached such a point that one of the qualities we most admire in a scientist in principle — the capacity to weigh evidence with an open mind, to postpone commitment to one or another of two competing theories until further investigation should decide the issue — goes virtually unnoticed in the participants in this great debate. We may no longer believe that any scientist can be totally objective, but we should not overlook the degree to which an old « partisan of Stahl », such as Macquer, strove to overcome his own subjectivity.

Our fixation on the « battle-lines » between phlogistonists and antiphlogistonists has been reinforced, I believe, by a general acquiescence, whether deliberate or subliminal, in Thomas Kuhn's account of the unbridgeable gaps between adherents of an old and a new paradigm in scientific revolutions. The two groups « talk past one another », « live in different worlds », speak in part in different languages. Because the two paradigms must be incommensurable, there is no logical intermediate position. Moving from one to another is, therefore, a conversion experience, which may happen to different individuals at different times and for different reasons, but must be a holistic process, an indivisible leap from one paradigm to the other (66). Within such a structure of revolutionary change there is little room for a compromise such as the one Macquer tried to maintain in 1782. His position cannot be seen as reasonable, because there is no logical space for it. He

(65) Beretta, *op. cit.* in n. 9, 183.
(66) Kuhn, *op. cit.* in n. 15, 92-159.

can be only weak, indecisive, or incoherent. Many scientists, however, including Lavoisier himself, have had to endure incoherence in their ideas for prolonged periods, while they searched for ways to mend the fissures in their conceptual landscapes (67).

In the 1970s Larry Laudan offered a number of criticisms of Kuhn's structure of scientific revolutions, some of which were cogent. Among the useful distinctions Laudan made is that scientists often entertain theories, and even pursue them vigorously, without necessarily accepting them as true. A scientific revolution occurs, according to Laudan, when a candidate for a new paradigm (or as Laudan preferred to call it, « a research tradition hitherto [...] ignored by scientists in a given field ») « reaches a point of development where scientists in the field feel obliged to consider it seriously as a contender for [their] allegiance » (68). Were we to adopt Laudan's view, rather than the more influential Kuhnian one that a revolution is completed only when a sufficient number of scientists have been *converted* to the new paradigm to control the further development of the field, then Macquer's views of 1783 could be taken as a signal that the chemical revolution had by that time taken place. To untertain this idea is not to accept it as true. My purpose here is not to replace Kuhn's view of the chemical revolution with one based on Laudan's account, but to illustrate the ways in which normative definitions can shape — and sometimes distort — our perceptions of concrete historical events.

Nowhere have the political, martial, and even apocalyptic overtones that historians have imparted to their descriptions of the chemical revolution been more conspicuous than in their characterizations of Lavoisier's « Réflexions sur le phlogistique » in which, after eight years of silence on the subject, he sought to persuade his audience in 1785 that « it is infinitely probable » that phlogiston « does not exist » (69). In 1910 Max Speter called the memoir « The judgment of damnation on the phlogiston theory » (70).

(67) See, for example, Holmes, *op. cit.* in n. 45, 28-62.

(68) Larry Laudan, *Progress and its Problems : Towards a Theory of Scientific Growth* (Berkeley : Univ. of California Press, 1977), 108-110, 138.

(69) Lavoisier, *op. cit.* in n. 31, 623 : « *Mais si tout s'explique en Chimie d'une manière satisfaisante, sans le secours du phlogistique, il est par cela seul infiniment probable que ce principe n'existe pas.* »

(70) Speter, *op. cit.* in n. 55, 43.

According to Douglas McKie, Lavoisier aimed in it « to rout » the defenders of phlogiston « with a destructive fire of chemical facts » (71). For Perrin the memoir marked Lavoisier's recognition « that the time was ripe for an open assault » on phlogiston (72). Arthur Donovan labels the « Réflexions » a « rhetorical tour de force » of « ferocious brilliance », and a « withering criticism at the reformulations of the phlogiston theory proposed » by his academic colleagues. Donovan sees Lavoisier in it mocking and taunting his colleagues, and making a « grab for power »; but unfortunately, Lavoisier « wildly overreached himself », and was forced afterward to resort to other strategies to win chemists over to his new theory (73).

Although historians have repeatedly summarized the contents of the memoir, most emphasis has been placed on Lavoisier's general declarations : on the passage in which he asked his readers « to strip themselves as far as possible of all prejudice » by « transporting themselves to a time before Stahl »; in which he characterized phlogiston as « a veritable Proteus which changes its form at each instant »; and in which he placed his faith in the « young, who begin to study science without prejudice » (74). Although the memoir has also been described as a « brilliant dialectical performance » and « closely reasoned » refutation of phlogiston (75), much less attention has been devoted to its logical structure than to its rhetoric; much less to the evidence he brought to bear on his conclusions than to the exclamatory forms with which he punctuated them.

In view of the sharp attack metaphors that historians habitually apply to his « Réflexions sur le phlogistique », one is rather surprised on reading it through to notice the *civility* with which Lavoisier treated those chemists whose opinions he rejected. He began with tributes to Stahl, to whom one « owes two important discoveries independent of all systems and hypotheses, which are

(71) Douglas McKie, *Antoine Lavoisier : The Father of Modern Chemistry* (Philadelphia : J. B. Lippincott, 1936), 220.

(72) Perrin, *op. cit.* in n. 23, 44.

(73) Donovan, *op. cit.* in n. 23, 158, 167-173.

(74) Lavoisier, *op. cit.* in n. 31, 624, 640, 655 : « *Je prie mes lecteurs* [...] *de se dépouiller, autant qu'il leur sera possible, de tout préjugé;* [...] *de se transporter aux temps antérieurs à Stahl...* » (624); « *C'est un véritable Protée qui change de forme à tout instant* » (640); « ... *les jeunes gens qui commencent à étudier la Science sans préjugé* » (655).

(75) Guerlac, *op. cit.* in n. 6, 105.

eternal truths ». The first was that « metals are combustible bodies, that calcination is a true combustion ». The second « which is still more important, is that the property of burning, of being inflammable, can be transmitted from one body to another » (76). Not only were his comments generous to the « patriarch » of chemists whose system Lavoisier was in the act of overturning, but they constitute as succinct and accurate a statement as could be made, even today, of the permanent contribution of Stahl to the structure of modern chemistry.

Nor did Lavoisier merely spare Stahl in order to concentrate his fire on the later « disciples of Stahl », among whom he singled out for extended discussion Antoine Baumé and Macquer. In his treatment of both of them his text displays more fairness than ferocity. Before exposing the contradictions inherent in their theories, he credited each of them with resourceful efforts to overcome the objections that had confronted the old phlogiston theory (which Lavoisier took to be Macquer's representation of the Stahlian doctrine in the first edition of his *Dictionnaire*). Baumé had sensed that the weight gain in metals in calcination « demanded a reform of the system of Stahl, and he had the courage to undertake it »; but his attempted solution entailed a large weight for « pure fire », a requirement « contrary to all the facts » (77).

Macquer too had sensed, according to Lavoisier, that « new facts » — in particular Lavoisier's own demonstration of the augmentation of weight in metallic calcinations and combustions — « disrupted the system of Stahl [...], but he believed at the same time that it would not be impossible to reconcile the modern experiments with the doctrine of phlogiston ». Macquer's modified phlogiston, identified with light, « explained a great number of the objections completely insoluble in the hypothesis of Stahl, in a simple and natural manner », but it was, in Lavoisier's

(76) Lavoisier, *op. cit.* in n. 31, 624-625 : « ... *on doit à Stahl deux découvertes importantes, indépendantes de tout système, de toute hypothèse, qui seront des vérités éternelles : premièrement, c'est que les métaux sont des corps combustibles; que la calcination est une véritable combustion* [...]. *La seconde* [...] *qui est plus importante encore, c'est que la propriété de brûler, d'être inflammable, peut se transmettre d'un corps à un autre.* »

(77) *Ibid.*, 626-627 : « *M. Baumé* [...] *a bien senti qu'une contradiction aussi formelle entre la théorie et les faits exigeait une réforme dans le système de Stahl, et il a eu le courage de l'entreprendre* » (626); « ... *il faudrait donc que le feu pur eût occasionné* [...] *toute cette augmentation* [...] : *or cette supposition de la grande pesanteur du feu, est contraire à tous les faits* » (627).

view « a completely new theory, which conserved the name of phlogiston without the substance ». Macquer's phlogiston was required to pass through vessel walls, a property which was in contradiction to the system of Stahl adopted « without modification by M. Priestley », in which phlogiston « is a body incapable of penetrating through vessels » (78).

Lavoisier drew his description of Macquer's modifications of the phlogiston theory from the « learned exposition » in the second edition of Macquer's *Dictionnaire de Chymie.* In two respects he represented it differently from the way Macquer had actually presented it there. Macquer had, as we have seen, devised his revisions in response to Buffon's criticism, not Lavoisier's experiments, and he had believed that his identification of phlogiston with light clarified and sustained Stahl's system, whereas Lavoisier insisted it constituted a different theory. These were, however, natural differences of perspective between the author writing in 1778 and a critic re-examining the author's writings in 1785. On the whole, Lavoisier gave an accurate summary of the structure of Macquer's theory. The contradictions which he showed that the theory encountered in the explanation of particular processes were serious inferences drawn from Macquer's theory, not rhetorical traps set for an adversary. Lavoisier portrayed Baumé and Macquer as unsuccessful mainly because the task they had set for themselves was impossible.

The image that historians have constructed of Lavoisier's « Réflexions sur le phlogistique » as an assault on his opponents has been magnified by the testimony of the Dutch chemist Martinus Van Marum, who was present at the session of the Academy at which Lavoisier delivered the paper. Van Marum reported that

(78) *Ibid.*, 629-634 : « *Ces nouveaux faits déconcertaient et le système de Stahl et celui de M. Baumé. Macquer le sentit; mais il crut en même temps qu'il n'étoit pas impossible de concilier les expériences modernes avec la doctrine du phlogistique* » (629); « *Il est certain qu'un grand nombre d'objections qui étaient complètement insolubles dans l'hypothèse de Stahl s'expliquent d'une manière naturelle et simple, avec les modifications qui y ont été apportées par M. Macquer* » (631); « *On est étonné d'y voir M. Macquer, tout en paraissant défendre la doctrine de Stahl, en conservant la dénomination de phlogistique, présenter une théorie toute nouvelle, et qui n'est point celle de Stahl;* [...] *M. Macquer a conservé le mot sans conserver la chose...* » (629-630); « *Le système de Stahl, admis sans modification, et tel qu'il a été adopté par M. Priestley, ne pouvait satisfaire à l'explication des phénomènes de cette expérience; car, puisque, dans ce système, le phlogistique est un corps incapable de pénétrer à travers les vaisseaux...* » (632).

violent objections were made against it, and that the reader was interrupted so continually that he had difficulty being heard (79). We do not know, however, who objected, and to what they objected. Whatever it was may have related to something Lavoisier said extemporaneously, or to something he read that he eliminated from the final text. The published memoir is a relentless argument, but the « mocking », « taunting », « militant » tones that historians have detected in it do not dominate its language. Much of it appears, in fact, conciliatory toward those whom Lavoisier felt, with good reason, to be on the losing side of a scientific revolution.

The most frequently quoted passage in *Reflections on Phlogiston* is the summation :

> « Chemists have made of phlogiston a vague principle which is not rigorously defined, and which consequently can be adapted to every explanation into which one wishes to insert it. Sometimes the principle has weight, sometimes it has none. Sometimes it is free fire, sometimes it is combined with the terrestrial element. Sometimes it passes through the pores in vessels, sometimes these are impenetrable to it (80). »

After enumerating several more of its contradictory qualities, Lavoisier ended the paragraph with his ringing declaration that phlogiston was « a veritable Proteus ». Before reaching this rhetorical climax, however, Lavoisier had analyzed example after example of chemical processes central to the debate over theories of combustion, in which he described in detail how the applications of the phlogiston theory led necessarily to the contradictions to which the summary passage alluded (81). The rhetoric so much admired by historians is, therefore, a flourish on a serious, sustained scientific argument, incorporating both theoretical and experimental reasoning, which invites closer historical scrutiny than it has yet received. How consistent were Lavoisier's own arguments, and how faithfully did he represent those of the other side? To what extent

(79) See Perrin, *op. cit.* in n. 23, 46; Donovan, *op. cit.* in n. 23, 173.

(80) Lavoisier, *op. cit.* in n. 31, 640 : « ... *les chimistes ont fait du phlogistique un principe vague qui n'est point rigoureusement défini, et qui, par conséquent, s'adapte à toutes les explications dans lesquelles on veut le faire entrer : tantôt ce principe est pesant, et tantôt il ne l'est pas; tantôt il est le feu libre, tantôt il est le feu combiné avec l'élément terreux; tantôt il passe à travers les pores des vaisseaux, tantôt ils sont impénétrables pour lui...* »

(81) *Ibid.*, 632-640.

did he expose contradictions *within* the conceptual framework of the phlogistonists, and to what extent did he impose arguments derived from his own theoretical framework which they would not have accepted? To what extent did he rely on discrepancies within the views of individuals such as Macquer or Baumé, and to what extent did he play off differences between variant phlogiston schemes? How compelling (in contemporary terms) was the experimental evidence he adduced in support of his position, and to what extent did he rely on methodological criteria that the phlogistonists would not have acknowledged? Lavoisier's memoir was both a powerful rhetorical performance and a closely reasoned scientific refutation of the phlogiston theory. We should neglect neither dimension of this centerpiece of the chemical revolution (82).

Recent accounts of the conversion of other chemists to Lavoisier's theory have centered on the campaigns that Lavoisier and his initial followers organized to bring them into the fold. Chemists from outside Paris who visited there were subjected to rounds of personal contacts and hospitality that functioned, as Perrin put it, « as an almost irresistible instrument of conversion to the new theory ». The most prominent case in point is Guyton de Morveau. Guyton came to Paris early in 1787, presumably still as a committed phlogistonist, to enlist support for his reform of the nomenclature, was « finally persuaded through contact with Lavoisier and his associates to break definitely with phlogiston » (83), and became one of the four collaborators who transformed the nomenclature reform into a weapon for antiphlogistic chemistry. Donovan comments : « It evidently did not take Lavoisier and his friends long to bring Guyton around to their way of thinking (84). »

Lavoisier and his friends undoubtedly did cajole Guyton to change his standpoint; but to reduce Guyton's conversion to pressure tactics reduces Guyton to a pawn in a political game, hardly flattering to a person who was, in his own right, a chemist of inter-

(82) Several readers have criticized the distinction made here between argument and rhetoric on the grounds that, since rhetoric constitutes the art of using language to persuade, it includes closely reasoned argument. I acknowledge the problem, but have not found other appropriate terms to substitute. I use the word rhetoric here in one of its recognized meanings in common usage — that is, as language calculated to impress by its elegance, extravagance, or flourishes.

(83) Perrin, *op. cit.* in n. 23, 51-52.

(84) Donovan, *op. cit.* in n. 23, 167.

national standing. Here, too, we should balance the historians' attraction to the drama of political action with greater attention to the cognitive aspects of conversion. There is no reason not to take seriously Guyton's own assertion that he had been prepared for some time to reconsider his commitment to the phlogiston doctrine in the light of the continuing « progress » of the antiphlogiston doctrine, and that « the moment to fulfil that engagement » arrived when he became convinced that the cumulative evidence had become more favorable to it than to its rival doctrine. The arguments that he gave in the *Encyclopédie Méthodique,* while similar overall to Lavoisier's own arguments for his theory, are not mere echoes of Lavoisier's views. They give evidence that Guyton had not merely adopted a new party line, but thought the question through for himself. In a long summary paragraph Guyton enumerated some of the views that he no longer had to defend. Some, but not all, were similar to the contradictions in the phlogiston doctrine described in Lavoisier's « Réflexions sur le phlogistique ». Guyton wrote with evident personal feeling when he added :

> « We are no longer reduced to take for an explanation the fiction which identifies bodies as diverse as *vital air,* the *toxic* air of nitrous gas, *inflammable gas,* and *fixed* air, and which makes the enormous differences between them depend on no more than a little more or less phlogiston. As soon as one abandons the hypothesis of a unique combustible body, one sees everything return to an order conforming to the nature of things : each combustible is its own genre, each gives an essentially distinct product, [and] each has its own degree of specific affinity with the base of vital air (85). »

Perhaps during those months in Paris Guyton did undergo a « paradigm shift » of the type that Kuhn describes (86), and perhaps Lavoisier and his friends also imparted to the experience, as Donovan suggests, a quasi-mystical aspect, akin to entry into the

(85) Guyton, Second Avertissement, in *op. cit.* in n. 61, 629-634 : « *Le moment est venu de remplir cet engagement* » (630); « ... *nous ne serons plus réduits à prendre pour explication la fiction qui identifioit des corps aussi divers que l'air vital, l'air nuisible, le gas nitreux, le gas inflammable & l'air fixe, & qui ne faisoit dépendre des différences si énormes, que d'un peu plus ou d'un peu moins de phlogistique. Dès qu'on a abandonné l'hypothèse d'un combustible unique, on voit tout rentrer dans l'ordre conforme à la nature des choses : chaque combustible est un être de son genre, qui laisse un produit essentiellement distinct, qui a sa mesure propre d'affinité avec la base de l'air vital* » (633-634).

(86) Kuhn, *op. cit.* in n. 15, 151-159.

temple of a new religion (87). But we must balance against these vivid images Howard Gruber's well-founded assertion that a deep change in one's point of view is a long, slow process (88). Guyton's « conversion » was clearly a culmination of the longer intellectual transition of a rational man, who changed sides because his reason told him that the antiphlogistic doctrine was now both theoretically more consistent and experimentally better supported than that to which he had up until then maintained his allegiance.

In a group of lectures on eighteenth century chemistry, published in 1989, I maintained that « Lavoisier reconstructed not the whole of chemistry, but only a crucial domain within a larger science (89) ». That my position has occasionally been misunderstood as a denial that Lavoisier caused a chemical revolution is (90), I believe, another indicator of the confusion between a chemical revolution and the founding of modern chemistry. My argument that a well-structured science of chemistry existed before Lavoisier is in no way incompatible with the view that Lavoisier led a major scientific revolution, provided we do not assume that the boundaries of his revolution were co-extensive with the boundaries of chemistry itself. As I argued in those lectures, Lavoisier himself recognized the distinction when he noted in the preliminary discourse of his *Traité* that all of his own doctrine was contained in part one, that part two was drawn largely from previously existing sources (91). We should also notice that what we call the « chemical revolution », Lavoisier referred to as « a revolution in chemistry » (or « in physics and chemistry », or « in an important branch of human knowledge »). That he could attribute to Stahl « the glory of [...] having made a revolution in the science » (92),

(87) Donovan, *op. cit.* in n. 23, 160.

(88) Howard Gruber, *Darwin on Man*, 2nd ed. (Chicago : Univ. of Chicago Press, 1981), 4-6, 10. For an account of Guyton's « conversion » as the final step in a gradual transformation of his views about phlogiston, see William A. Smeaton, Guyton de Morveau and the Phlogiston Theory, in *L'Aventure de la science : Mélanges Alexandre Koyré* (Paris : Hermann, 1961), vol. 1, 522-540.

(89) Frederic Lawrence Holmes, *Eighteenth-Century Chemistry as an Investigative Enterprise* (Berkeley : Office for History of Science and Technology, Univ. of California, 1989).

(90) See, for example, Beretta, *op. cit.* in n. 9, 247, note 5.

(91) Holmes, *op. cit.* in n. 89, 108-109.

(92) Lavoisier, *op. cit.* in n. 31, 624 : « ... *la gloire de devenir un des patriarches de la chimie, et de faire une sorte de révolution dans la science...* »

at the very time he was, in his own revolution, overthrowing the most prominent manifestation of Stahl's chemical doctrine, is a strong indication that Lavoisier viewed revolutions as events that could occur repeatedly within the same science. He never believed, as many historians do, that his revolution either reconstituted, or constituted for the first time, the whole science of chemistry.

It has recently been argued that the new nomenclature that Lavoisier and his colleagues promulgated widened Lavoisier's revolution to a total chemical revolution, because it cut future chemists off from the chemistry of the past. According to Beretta, for example, the chemical revolution was « a total break with the past », and this revolution « was in fact provoked by a language that no longer reflected the history of chemistry and that forced all chemists [...] to become students of an entirely new chemical grammar » (93). But chemists are not primarily grammarians. The things they name in their texts are normally things they have recognized in their laboratories through more tangible signs. Even if the new nomenclature did make the access of post-Lavoisian chemists to the pre-Lavoisian literature more difficult, that does not mean that they no longer had access to pre-Lavoisian chemical knowledge. That knowledge was merely translated, where necessary, into the reformed language. Lavoisier himself initiated the process. Part 2 of this treatise, he explained, « contains nothing which belongs to me. It presents only a very concise summary of results extracted from different works (94) ». That part 2 contained nothing from Lavoisier himself was an unduly modest disclaimer. That it began the transmission of a vast body of earlier chemical knowledge, both empirical and conceptual, into the chemistry of the future, was, however, an accurate assessment.

(93) Beretta, *op. cit.* in n. 9, 255, 258. Bensaude-Vincent maintains a similar, but more moderate position : The reform of the nomenclature was, she writes, « the masterpiece of the process of chemical revolution to the extent it imposed a clear rupture between the before and the after which concerned all chemists in their daily lives » *(la pièce maîtresse du processus de révolution chimique dans la mesure où elle installe une rupture franche entre l'avant et l'après qui concerne tous les chimistes dans leur vie quotidienne).* (Bensaude-Vincent, *op. cit.* in n. 2, 254.)

(94) A.-L. Lavoisier, *Traité Élémentaire de Chimie* (Paris : Cuchet, 1789), vol. I, XXIX : « ... *cette seconde partie ne contient rien qui me soit propre; elle ne présente qu'un abrégé très-concis de résultats extraits de différens ouvrages.* »