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THE CHEMICAL REVOLUTION

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During the late eighteenth century, chemistry underwent a dramatic and profound transformation that tradition has labelled 'the chemical revolution'. Its drama lay in a conflict that split the chemical community into opposing factions but its significance is more complex. According to the most popular interpretation, the chemical revolution consisted in the overthrow of the phlogiston theory and its replacement by the oxygen theory of combustion. On a grander scale, the episode has been hailed as the founding era of modern chemistry and Antoine Laurent Lavoisier (1743–94), creator of the oxygen theory, as its 'father'. In recent decades, the conventional view has been challenged.[1] In the first place, combustion was not an overwhelming concern of eighteenth-century chemists, so it is not clear why a new combustion theory should provoke such strong reaction, nor be equated with the ascendance of modern chemistry. Historians now ask what the chemical revolution was really about. Secondly, Lavoisier's contributions have often been studied out of context, with the result that continuity of chemical inquiry has been overshadowed and the epistemic break with tradition possibly exaggerated. Current investigations aim to relate Lavoisier's achievements to the work of his predecessors and contemporaries. Because the chemical revolution is a major episode in the history of science, often cited by historians, philosophers and sociologists to illustrate their conceptions of scientific change, these problems have implications that transcend the episode itself.

Chemistry in 1800 was certainly different from what it had been a century earlier. Proponents of Lavoisian chemistry shared a strong conviction that its adoption represented a rupture with the past, as the words of a young Swiss convert, Marc-Auguste Pictet (1752–1825) illustrate.

> Chemistry, banished until now to a small circle of adepts whose language and ideas were equally obscure, has become the inseparable aide and companion of Physics: these sciences, united and guided by experiment alone, have proceeded at a rapid pace; Chemistry itself has undergone a great revolution, a frightful scaffolding has given way to a simple and illuminating theory, based upon the immediate consequences of experiment . . . Logic, without which experiments are mere isolated facts, has become more severe in its conduct and more certain in its results: in short, everything indicates that we are on the right path, and that it will lead daily to discoveries in the natural sciences.[2]

Pictet's view was from the vantage point of a triumphant campaign for the new theory. Chemistry, in the eyes of its promoters, had been created anew with a methodological soundness and a theoretical rigour which was lacking before. But to what extent were its advances due to the Lavoisian innovations rather than a century of evolutionary development?

For an appraisal of the status of chemistry prior to the upheaval, we may turn to Gabriel Venel's article 'Chymie', written at mid-century for Diderot's *Encyclopédie.* Venel (1723–75) found that chemistry in France suffered from a twofold sense of inferiority. On the one hand, it was not perceived by French intellectuals as a science on a par with mathematical physics. Its image was still tarnished by association with alchemical 'puffers'; it seemed to lack a powerful organising theory. Secondly, French chemistry had not enjoyed the encouragement accorded to its German counterpart. Although critical of the current state of chemistry, Venel was sanguine about its potential. To make chemistry respectable required a breakthrough to an overview which would give coherence to chemical doctrine. Venel called for a 'new Paracelsus' with the insight and the dramatic flair necessary to bring about a 'revolution' in the science.[3]

Developments in the first half of the century gave Venel grounds for optimism, for there had been notable advances in chemical practice and theory. Practical chemistry contributed improvements in mineral analysis and metallurgical techniques, as well as isolation of new materials, including phosphorus. Nor did chemistry lack for theory; several frameworks were available, including the venerable theory of the four elements, the doctrine of essential chemical principles and the notion of chemical affinities. Problems arose in the compatibility of diverse theories, or, more pressingly, in the relation of theory to laboratory practice. Venel's discontent with the chemical literature was twofold. On the one hand he found a plethora of petty chemical treatises competently treating matters of detail but supplying little interpretation. At the other extreme were speculative treatises, written by philosophers with little chemical knowledge and contributing little to advance the science. Venel espoused a 'philosophical chemistry' that would follow an intermediate path, guided by an intuitive method grounded in laboratory experience rather than extrinsic philosophical doctrine.

Most French chemists at mid-century accepted that natural bodies were composed ultimately of earth, water, air and fire. The conviction had been reinforced by Stephen Hales's discovery in the 1720s that air could be extracted from animal, vegetable or mineral substances and trapped (by displacement of water) in a 'pneumatic apparatus'. In principle any substance could be analysed into its elements, in practice the theory was of limited utility. When chemists decomposed a substance, the typical result was several products, none of which was a pure element; moreover, the products often varied depending upon the method of analysis. Chemists required a theoretical framework more directly related to reagents found on laboratory shelves. One such scheme was provided in the table of chemical affinities systematised by the French chemist Étienne-François Geoffroy (1672–1731) around 1718. The principle was a simple one: affinity tables were divided into several columns, each headed by a common reagent – a mineral acid, an alkali or a metal. Below each reagent were listed the principal substances that would react with it, in order of decreasing affinity. The tables encapsulated information about chemical reactions and facilitated prediction by analogy, but even more importantly, their use encouraged chemists to distinguish substances by systematic reaction with a series of reagents, rather than mere observation of their individual properties.

The brightest prospects for chemistry, in Venel's eyes, lay in the teachings of Johann Becher (1635–82) and Georg Stahl (*c.* 1660–1734), founders of the doctrine of essential chemical principles. In the eighteenth century, substances were classified into generic groups defined by a shared property, that is, according to whether they were earthy, metallic, combustible, acidic, caustic, and so forth. In essence the doctrine of Becher and Stahl embodied the defining properties in a set of hypothetical, quality-bearing material 'principles'. The notion had long roots reaching back to the Paracelsan *tria prima* and beyond to Aristotelian essences, but Stahl had cast the doctrine into a viable form by his dramatic success with the inflammable principle. He did not invent the concept, though he gave it a new name-*phlogiston*. Stahl's celebrated achievement was his taming of the elusive principle. Phlogiston could not be isolated or displayed, but Stahl showed that it was identical in the three realms of nature and could be passed from one combination to another at the chemist's whim. Phlogiston from charcoal could restore the principle that a metal lost upon calcination or could regenerate sulphur from its combustion product, sulphuric acid. It was the chemist's ability to manipulate phlogiston that made its existence plausible.

Lavoisier entered chemistry during the 1760s at a time when the efforts of the encyclopedists promoted a growing appreciation of its utility and philosophical interest. He did not arrive by the traditional routes of medicine, pharmacy or metallurgy. Instead he received a broad exposure to mathematics and the natural sciences through formal education at the Collège des Quatre Nations and the family connections that introduced him to leading Parisian men of science. The deepest influence upon the young Lavoisier was probably that of Jean-Étienne Guettard (1715–86), who engaged him as an assistant on excursions

to chart the mineralogical resources of France. It was a desire to extend this preparation for geological work that led Lavoisier to follow Guillaume Rouelle's popular chemistry lectures in the early 1760s. After his study with Rouelle, Lavoisier continued his education by reading extensively in mineralogy and chemistry throughout the middle years of the decade. There is a common impression that he was disenchanted from the start with current chemical concepts. Surviving documents do not support that contention: he defended the theory of the four elements and used the concepts of phlogiston and chemical affinity in the same routine way as his contemporaries. However, his approach to the chemical literature was exceptional, for he read with an unusually critical eye, alert to discrepancies among different authors or to 'singular' phenomena, that did not follow simply from accepted doctrine. Such loose ends appealed both to Lavoisier's intellect and to his ambition as opportunities for 'a beautiful course of experiments'.[4] What further set him apart from typical students of chemistry was the taste he had developed for the rigour of mathematical logic and the precision afforded by instruments in physical experiments. He found such rigour and precision were often lacking in chemical investigations, but they became the hallmark of his own work. Lavoisier's experiments did not yield new substances, such as the gases and organic acids isolated by his distinguished contemporaries Joseph Priestley (1733–1804) and Carl Wilhelm Scheele (1742–86). Throughout his career Lavoisier's efforts were focused upon *problems* and his discoveries were solutions to those problems.

One of the most consequential of the problems that captured Lavoisier's imagination during that youthful period was inspired by reading an essay on the elements by the German chemist, Johann Theodor Eller (1689–1760). Eller had revived van Helmont's attempt to reduce the number of *bona fide* elements, by claiming that water was transmutable into both earth and air. Lavoisier was intrigued by Eller's argument that air was 'factitious', that is, composed of water combined with fire which made it expansive. However, he rejected Eller's conclusion that air was the same thing as water vapour, suspecting instead that it might consist of an *unknown* substance rendered into vapour form by union with the matter of fire. This notion, conceived in 1766, was the seed that Lavoisier gradually articulated into his theory of the vapour state.[5]

At that time Hales's discovery that air could be elicited from many substances had spawned quite different traditions in Britain and France. In Britain it inspired pneumatic chemistry: the isolation and study of different kinds of air. Joseph Black (1728–99) was the first to characterise a species of air differing from atmospheric air; it would not support combustion but had the property of precipitating limewater. Because it entered concrete substances like chalk or magnesia alba, he called it 'fixed air' (carbon dioxide). In 1766, Henry Cavendish (1731–1810) similarly described a species of air that was inflammable. But Joseph Priestley, in the early 1770s, emerged as the most creative discoverer and leading authority in pneumatic chemistry, by his isolation of a series of airs and his tests to distinguish them. Meanwhile, in France, Lavoisier remained unaware of the post-Halesian work until the end of 1772. Disseminated in Rouelle's lectures, Hales's results gave rise not to a tradition of pneumatic researches but to a theoretical dispute. Was the air obtained from bodies truly combined in them, or was it merely physically trapped in their pores? Then again, might the air be factitious, that is, not actually present in the bodies at all, but formed during their decomposition by the union of some other substances? Lavoisier's hunch of 1766 offered a simple resolution to the fixation of air. Removal of the combined fire that made air expansible would reduce it back to a concrete substance capable of entering chemical combination in the ordinary way. This promising notion did not immediately lead to an experimental investigation, perhaps because of competing demands on Lavoisier's time, but perhaps also because a line of attack had not yet crystallised in his mind.

Only in the summer of 1772 did Lavoisier undertake a sustained programme of research to elucidate the chemical role of air. The origin of his pivotal experiments on combustion and calcination, executed in the autumn of that year, has been the subject of controversy. H. Guerlac placed those experiments in a new light by showing that during the previous summer Lavoisier drafted a theoretical paper on the chemical role of air and fire; moreover he specifically intended to extract air from a variety of minerals by means of a large burning lens. It remains a moot point how Lavoisier proceeded from those plans to his crucial discovery (deposited at the Academy of Sciences in a sealed note on 1 November 1772) that combustibles or metals, when burned, gain weight by absorption of air. One view (favoured by Guerlac) is that the mysterious weight gain of calcined metals aroused Lavoisier's curiosity; alternatively, it has been proposed that an interest in combustion or in the formation of acids actually provided his starting point. Although none of these topics was foreign to Lavoisier as a possible area of study, the evidence does not support an explicit intent on his part to investigate any one of them. A more likely scenario is that continuing themes of his research were channelled in new directions by a series of contingencies, leading to an unforeseen discovery.[6]

In the spring of 1772 Lavoisier's name had been put forward to a committee set up by the Academy to investigate the puzzling disappearance of diamond when heated in a potter's kiln. Members of the committee requested that a large burning lens belonging to the Academy be brought out of storage in order to extend the study of the diamond and other gems to higher temperatures. Lavoisier seized the opportunity as a new means to pursue an old interest in minerals. Hales's results on a few minerals convinced him that many contained air – which he hoped to disengage with the burning lens and capture in a pneumatic device. He suspected, too, that metals (known to effervesce with acids) might also yield 'fixed air'. These were the most original of the experiments he proposed to try with the burning lens; but because of the difficulty of finding vessels to withstand its intense heat, they were never completed.

Lavoisier's participation in the burning lens investigations brought him into frequent contact with Pierre Mitouard, a Parisian pharmacist who had recently obtained a good sample of white phosphorous from Germany. Several authors had noted that phosphoric acid obtained by spontaneous combustion was heavier than the original phosphorus; some attributed the gain to absorption of air by the acid, others to attraction of atmospheric moisture. Mitouard probably mentioned these opinions, which were not, however, a focus of his own research. Lavoisier, at the time preoccupied with the chemical role of air, purchased an ounce of phosphorus from Mitouard to try his hand at resolving the question. Initially, the solution evaded Lavoisier, for he had to expose his phosphorus to the atmosphere in order to burn it; the resulting acid absorbed moisture by deliquescence as it formed, gradually dissolving itself. It remained unclear how much of the increased weight was due to moisture and how much, if any, might be due to air. Never one to waste his efforts, Lavoisier saw that repeated combustions could yield a concentrated sample of the acid from which he could derive its little-known salts. Over the following weeks he laboriously collected his sample and began a paper on the formation of the acid and its salts. But as he proceeded, a resolution to his original problem dawned upon him. He placed his acid solution in a flask, marked its level and weighed it; he then filled the flask to the mark with distilled water and weighed it again. The difference in weight gave the amount of phosphoric acid in solution, which turned out to be markedly greater than the weight of phosphorus consumed – an increase, Lavoisier concluded, that could only be due to absorbed air.

The discovery so excited Lavoisier that he dropped his study of phosphoric acid salts and deposited his unfinished paper at the Academy (on 20 October 1772) to retain priority. Having demonstrated the absorption of air by burning phosphorus, he wanted to see if the same might occur with sulphur. A positive result with sulphur then led him to speculate whether what was true of phosphorus and sulphur might apply to *any* combustible that gained weight by burning. In particular, he suspected that the weight acquired by metals during calcination (in Stahl's theory merely a slow combustion) might involve absorption of air. Within a week he confirmed his conjecture by the reverse process of reducing litharge (a calx of lead), from which he obtained a volume of air nearly a thousand times that of the calx.

The result with litharge completed a striking reversal of Lavoisier's expectations of the previous August. At that time he had hoped to extract air from metals by intense calcination; he now saw that on the contrary, under such circumstances they *absorb* air. His understanding of the relation of metal to calx (or sulphur to its acid) was inverted! Success in monitoring the entry of air into combination, and his resulting insights into the composition of familiar materials, inspired Lavoisier to predict a 'revolution' in chemistry. It is often assumed from hindsight that he meant the overthrow of phlogiston. However, the evidence does not support that claim, because he believed initially that air entered a combustible *as phlogiston left*. Subsequently, his doubts about the role of phlogiston grew until, in the early months of 1773, he came to the verge of total rejection of the concept. Therefore, the dismissal of phlogiston was a consequence rather than a cause of his revolutionary programme (and did not become final for several years).

The scope of Lavoisier's programme is more fully set out in a research memorandum he drafted on 20 February 1773. In the interval he had read other discoveries relating to 'fixed' air (including the British work) which he now saw as isolated links of a great chain – one he personally hoped to join together. The key to his anticipated synthesis was to concentrate upon the processes which absorbed air; in that way he hoped to learn about the *origin* of air in substances and to follow its progress from one combination to another. As promising starting-points, he singled out the processes of vegetation, animal respiration, combustion, calcination and certain chemical combinations. From the start Lavoisier had a larger goal than a new combustion theory. His vision embraced a new system of chemistry in which atmospheric air, or some component of the air, entered by traceable chemical paths into operations of the animal, vegetable and mineral kingdoms.

From one perspective, the remainder of Lavoisier's career can be viewed as the fulfillment of that programme. His early publications aimed at demonstrating the role of air in calcination; his emphasis shifted in the mid-1770s to combustion and the constitution of acids, then in the 1780s to fermentation, vegetation and respiration. This interpretation stresses the continuity and momentum inherent in his programme. From another point of view, however, Lavoisier's early conceptions were far from the system that he eventually published in 1789 as his *Traité Elémentaire de Chimie*. To that point his departure from convention consisted in (1) an emphasis upon air and an embryonic theory of its fixation, (2) an inversion of familiar assumptions about composition, and (3) a recognition of the incompleteness of Stahl's combustion theory. On the other hand, he had not yet distinguished 'fixed' air from ordinary air, still regarded air and water as elements, had not definitively rejected phlogiston and he still comfortably worked within the framework of chemical principles. Indeed, Lavoisier's programme of 1772–3 might be characterised as an attempt to establish air as a chemical principle, much as Stahl had established phlogiston. The further transformation of his views resulted partly from articulation of his programme and partly from its collision with discoveries and ideas advanced by his contemporaries.

Response to Lavoisier's innovations occurred in three phases. The first public disclosure of his work came in a paper on calcination read at the Academy's Easter public meeting in 1773. There he hinted at differences with Stahl and ventured that deeper study of fixed air would lead to a period of 'almost complete revolution'. That brash forecast drew an admonition from his academic colleague, Antoine Baumé (1728–1804), who complained in print of certain physicists who wished to substitute fixed air for phlogiston. It may have been reproof from his colleagues, as much as the conceptual difficulty arising from the elimination of phlogiston, that led Lavoisier to avoid confrontation over Stahlian theory when he published an account of his research, *Opuscules Physiques et Chymiques* early in 1774.

Outside Paris, Lavoisier's early publications attracted attention primarily where his conclusions about calcination and combustion clashed with other views. The most prominent of the early encounters was with Joseph Priestley, whose concurrent investigations led to ideas on the role of air in combustion diametrically opposed to Lavoiser's. Like Lavoisier, Priestley was indebted to Hales for many of his experiments – which were conducted, however, from a distinctive point of view. Priestley set out not to reform chemical notions, but to explore the role of air in larger economies of nature. His experiments aimed at elucidating the constitution of atmospheric air, the nature of other kinds of air and their effects upon such processes as the respiration of mice and the growth of plants. Aware that certain processes, including combustion and respiration, 'spoilt' the air, he sought ways to restore its 'goodness', measuring the 'goodness' by diminution of a sample when mixed with a predetermined amount of nitrous air (nitric oxide). Priestley's work found enthusiastic emulators on the continent; Lavoisier himself first showed an interest in respiration after reading Priestley's early paper on gases and learned to distinguish gases qualitatively by repeating Priestley's experiments.

In his *Opuscules*, Lavoisier criticised Priestley's view that ordinary air was diminished during combustion by phlogistication and implied that the British philosopher had failed to note the *absorption* of a portion of air by the burning substance. Priestley was quick to deny any suggestion that phlogiston diminished the elasticity of air, pointing out that it had the effect of precipitating 'fixed' air from the atmosphere – hence the shrinkage. Differences between Priestley and Lavoisier were exacerbated in 1775 when Lavoisier published his famous paper showing that the red calx of mercury (upon reduction without charcoal) yielded not Black's 'fixed' air, but an air capable of supporting combustion. Priestley himself had obtained such an air from the red calx in August 1774 and had mentioned it in Lavoisier's presence during a Paris visit in October. In the second volume of his *Experiments and Observations on Different Kinds of Air* Priestley not only chided Lavoisier for failing to observe that the air was a species much better than ordinary air (Priestley proposed to call it dephlogisticated air), but also implied that Lavoisier ought to have acknowledged his own priority. Theoretical differences between Priestley and Lavoisier were therefore compounded by an apparent breach of 'philosophical' etiquette – plus perhaps, a rivalry for authority in the popular new field of pneumatic chemistry.

In its early manifestations, Lavoisier's programme was not perceived as a threat to the foundations of chemistry because its revolutionary implications were concealed and the validity of its limited claims disputed. A second phase in its reception was launched with his reading of a paper 'On Combustion in General' in the autumn of 1777. On that occasion Lavoisier made clear his intention to account for combustion without employing the concept of phlogiston; accompanying thermal effects were instead attributed to the matter of fire released as air relinquished the vapour state. For several years, Lavoisier refrained from direct confrontation with colleagues over the existence of phlogiston, electing instead to develop the positive implications of his theory. He proceeded with analyses of acids (showing that they contained dephlogisticated air, which he renamed *oxygen*, the acid-former), and elaborated his theory of heat in collaboration with Laplace. Around 1781–2 he sketched an outline of a systematic presentation of his views, in which for the first time he articulated his new understanding of the elements, defined pragmatically as simple substances that were the end result of chemical analysis. His list included the metals, sulphur and phosphorus as well as the more traditional earths and water. Despite the growing power and coherence of his system, he was confronted by rival theories and disconcerting counter-evidence. His colleague, Pierre Macquer, had advanced a compromise theory that granted the absorption of air in combustion (and hence could explain weight relations precisely as Lavoisier did), but retained phlogiston as the pure matter of sunlight (allowing him to account for reduction without charcoal). The theory attracted a following in France as a way of incorporating Lavoisier's compelling results within a conventional theoretical framework. Another serious competitor was the theory-favoured by Jean-Claude de Lamétherie (1743–1817) in France and R. Kirwan (*c.* 1733–1812) in England – that inflammable air was pure phlogiston. Metals were known to release inflammable air when they dissolved in acid; even more compelling was Priestley's observation in 1782 that a calx of lead was successfully reduced by heating it in inflammable air (which was consumed as the metal formed). How could such phenomena be explained without the assumption of an inflammable principle in the metal? The second phase closed with a startling discovery that turned these phenomena to Lavoisier's advantage. Repetition of Cavendish's ignition of a mixture of inflammable and dephlogisticated airs, yielding water as a product, led Lavoisier to recognise that water was not an element but a compound of the two gases. So inflammable air evolved 'from metals' could be attributed instead to decomposition of water. This insight was supported by an analysis of water, by passing steam over heated iron filings. The water was decomposed, its oxygen combined with the iron and its inflammable air (which Lavoisier now renamed *hydrogen*, the generator of water) was set free in considerable quantity.

A third phase in the reception of Lavoisier's views began with public response to the alleged composition of water and to his provocative 'Reflections on Phlogiston' read to the Academy in the summer of 1785. In it he argued that Stahl's theory was based upon circular reasoning, that variations of phlogiston theory were incompatible with Stahl's and with one another, and that the principal phenomena of chemistry could be simply explained without phlogiston – a strong indication of its non-existence. Although Stahl's theory might have benefited chemistry initially, its retention had become an obstacle to further progress. The 'Reflections' produced an uproar of protest, at first within the Academy then later more widely with the paper's publication in 1786. The tacit truce on fundamental issues was abandoned. The existence of phlogiston was perceived as the central issue and the analysis of water as a crucial experiment. Stahl's followers objected that the formation of water from the two gases indicated that it was a constituent of the gases, rather than the other way around; in the alleged analysis of water, the abundant inflammable air came, they said, from the iron filings, not the water. The experiments were difficult to repeat and both the results and the interpretation remained contentious for several years.

As the deeper implications of Lavoisier's programme spread and especially as a few prominent members of the French community declared their support, its threat could no longer be ignored. A schism began to develop within the French community. A couple of the Academy's younger chemists, Claude Louis Berthollet (1748–1822) and Antoine François de Fourcroy (1755–1809) – and many of its mathematicians and physicists – rallied to Lavoisier's side; from Montpellier Jean Antoine Chaptal (1756–1832) declared his support. Promotion of the new chemistry increasingly took on the aspect of a collaborative campaign; its advocates were dubbed 'antiphlogistians' and their activities were described in political metaphor. Active opposition to the new chemistry grew in proportion, finding its voice in the *Journal de physique*, under de Lamétherie's editorship. Criticism was initially reasonable, stressing counter-evidence and theoretical objections, but as the strength of the antiphlogistic party grew, it sometimes turned acrimonious and personal.

Early in 1787, Louis Bernard Guyton de Morveau (1737–1816) travelled to Paris to consult leading chemists on the reform of chemical nomenclature that he had been urging for several years. Long a respected defender of phlogiston, Guyton had gradually modified his position, in the light of new evidence, until little separated it from Lavoisier's. In Paris he was finally won over. In collaboration with the antiphlogistians, he then drew up a new method of chemical nomenclature grounded on Lavoisian principles. The language reflected the new view of chemical composition. It was founded upon a set of operationallydefined 'simple' substances', each given a simple name. Compounds received binomial names revealing their elemental constitution and assigning them a genus and species. So a lead calx, for example, was allotted to the genus oxide, species lead. It is the familiar naming system for simple inorganic substances such as sulfides, nitrites, nitrates and so forth, still in use today. Promoters of the new nomenclature saw it as a significant advance. Not only would chemistry become easier for beginners to learn, but the systematic and analytical nature of the language would also improve reasoning in chemistry. To opponents, on the other hand, the new terms had a barbarous ring. The system was objectionable because, from a practical point of view, experienced chemists would not understand the foreign terms; philosophically, the whole system was founded upon hypotheses which might collapse; politically, it was seen as a blatant attempt to impose the new theory by forcing beginners to learn the new language.

Despite vocal opposition, the trickle of converts to the new chemistry turned into a stream during the late 1780s. Its early promoters included several of France's most successful chemistry teachers: each year hundreds of students assimilated the new view through the able lectures of Fourcroy in Paris, de Morveau in Dijon and Chaptal in Montpellier. However, the transition was not simply a matter of an older generation dying off to be replaced by a younger and more flexible one. The community as a whole shifted to the new system, though not all at once, nor with uniform enthusiasm. Conversion proved more difficult for older chemists, who were possessed of deeper attachment to the old theory and acute awareness of the limitations of the new. Yet many of them managed partial adoption of the antiphlogistic system as its experimental claims were confirmed and its theory clarified. The campaign was significantly advanced by publication of several antiphlogistic treatises – especially Lavoisier's *Traité* – providing a coherent overview of the new system to combat the fragmentary and often distorted perceptions of it in circulation.

The struggle for the new chemistry in France was effectively accomplished by 1789–90, although some prominent figures persisted in their opposition. During the early 1790s many sceptics quietly began to employ the new concepts. By the middle of the decade, even that inveterate opponent, de Lamétherie, found himself obliged to use the new nomenclature in order to be understood by readers of the *Journal de physique*. Elsewhere, dissemination of the new theory proceeded at different rates, according to the commitments of local communities and the quality of their communications with France. In Britain, where mutual interests and excellent lines of communication facilitated awareness, the course of debate closely paralleled that in France; antiphlogistic publications were rapidly translated into English. By the early 1790s the new chemistry was well established in Edinburgh and Glasgow where it was endorsed by Black and his students. Prominent English philosophers were reluctant to concede the superiority of the Lavoisian system: Cavendish appears to have abandoned the defence of phlogiston (without championing oxygen) by about 1787; James Watt adopted the new ideas with reservations, while Richard Kirwan capitulated to the antiphlogistians in 1791 but never became a wholehearted supporter. Of course, Priestley and his friend James Keir remained against it to the very end. The stubborn German opposition to the antiphlogistic chemistry is well known. Ironically, it owed much to the self-sufficiency of the German chemical community (reinforced by emergent cultural nationalism), which did not encourage chemists to pay attention to foreign developments. More generally, the implications of the Lavoisian system tended to be ignored until they were championed locally by a few individuals. Typically, those individuals were young; many had travelled and come into contact with antiphlogistians. Once the issue had been raised, arguments followed similar patterns to those in France and Britain, but with local variations; the existence of phlogiston always remained at the heart of the debate. By the close of the century the new chemistry had penetrated even the remote corners of Europe and America. Paris emerged as the leading European centre for chemical instruction; students flocked there to hear lecturers by Lavoisier's supporters who, since the political revolution, had dominated all of the Parisian scientific institutions.

The new chemistry was established amidst dissension; today its interpretation continues in controversy. There is consensus that the overthrow of phlogiston is too narrow a focus, but no suitable synthesis has taken its place. Novel (and sometimes incompatible) interpretations have been proposed. The Lavoisian achievement has been portrayed as the reaffirmation of material principles over British attempts at mechanisation, or alternatively, as the subordination of chemistry to physics.[7] Several authors shift the emphasis away from phlogiston or combustion to the nature of the vapour state, or the theory of acidity, or the new view of composition. But insistence upon any one of these aspects would be just as restrictive.

A more promising approach would be to treat the transformation as a complex process with several layers of meaning – perceived differently according to time and place. Lavoisier first spoke of 'revolution' when he saw a way to elucidate the nature of air; ultimately he succeeded in his goal of establishing air (or rather oxygen) as a key chemical entity. His path led through detailed studies of calcination, combustion, acids, fermentation, organic analysis and respiration; it was his specific claims about these processes that constituted the manifest implications of his work. But his programme had deeper consequences (some unforeseen in the autumn of 1772), notably the elimination of phlogiston and the overturning of the traditional hierarchy of composition. Close colleagues had an early glimmer of these implications, but the community as a whole remained unconcerned until the direct attacks upon phlogiston and the publication of the nomenclature. On a disciplinary level, Lavoisier employed physical methods, received support from physicists and brought chemistry much closer to its sister science – an intrusion that was resisted. Even more imperceptibly, Lavoisier's synthesis undermined the entire doctrine of chemical principles, substituting a combinatorial view in which the properties of simple substances bore no necessary relation to those of their compounds. These changes were considerable. Nineteenth-century historians, comparing the concepts, language and methods of Lavoisier's chemistry with its predecessors, saw in them the beginnings of modern chemistry.

The Chemical Revolution is a classic instance of conceptual change in science – one of the first to be foretold. Venel had called for a breakthrough that would exploit the distinctive methods and concepts of chemistry to establish it as the independent peer of physics. Ironically, Lavoisier found that breakthrough in the chemical role of air, but pursued it by the methods of experimental physics. His innovations transformed the structure and language of chemistry, generating a crisis that split the community. Chemistry emerged from the conflict as a more mature discipline with the public recognition Venel had desired. A new historiographic synthesis is called for, one that neither imposes another straitjacket nor trivialises the episode, but incorporates the full richness of the transformation.

NOTES

1. The conventional view is clearly articulated in H. Butterfield, *The origins of modern science, 1300–1800* (rev. ed., New York, 1965), chap. 11; for recent reviews see M. Crosland, 'Chemistry and the chemical revolution', in *The ferment of knowledge*, (eds.), G. S. Rousseau and R. Porter (Cambridge, 1980), pp. 389–416; J. R. R. Christie and J. V. Golinski, 'The spreading of the word: new directions in the historiography of chemistry 1600–1800', *History of science*, 20 (1982), 235–66.
2. M. A. Pictet, 'Lettre de M. le professeur Pictet aux rédacteurs du Journal', *Journal de Genève*, 28 November 1789.
3. G. F. Venel, 'Chymie', *Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers*, vol. 3 (Paris, 1753), pp. 409–10.
4. Lavoisier's reading notes from the period have been preserved, Archives de l'Académie des Sciences, Lavoisier Papers, Dossier 251.
5. The significance of the notes on Eller was first brought out by J. Gough, 'Lavoisier's early career in science: an examination of some new evidence', *British journal for the history of science*, 4 (1968), 52–7.
6. The reconstruction given here relies upon a recently discovered manuscript; see C. E. Perrin, 'Lavoisier's thoughts on calcination and combustion, 1772–1773', *Isis* (forthcoming).
7. R. Schofield, 'The counter-reformation in eighteenth-century science – last phase', in *Perspectives in the history of science and technology*, ed. D. H. D. Roller (Norman, 1971), pp. 39–54; E. M. Melhado, 'Chemistry, physics, and the chemical revolution', *Isis*, 76 (1985), 195–211.

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