# LAVOISIER, THE TWO FRENCH REVOLUTIONS AND 'THE IMPERIAL DESPOTISM OF OXYGEN' *

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**Chemistry and the political revolution**

The more famous of the revolutions referred to in the title of this paper is, of course, the political and social revolution which is traditionally dated 1789 but which developed in succeeding years, reaching its climax in the Terror of 1793-94. There were also many other major changes, for example in education and in scientific institutions. But it is the other revolution, the 'chemical revolution' of the 1780s associated with the name of Lavoisier which is the more immediate concern of historians of science. People at the time were ready to draw a parallel between the great changes beginning to take place in society and the fundamental changes in chemistry[1] even if the phrase 'the chemical revolution' did not pass into common usage until a hundred years later.[2]

No-one would want to argue a cause and effect connection between the two revolutions but it might be reasonable to argue along the lines of a common context. What I hope to illustrate in this paper is that the worlds of chemistry and of politics were not insulated from each other and I shall be concerned mainly with two countries, France and Britain, the latter providing a classic example of reaction against the political revolution. When war between France and Britain was declared in 1793 there was an additional reason for British writers to

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* This paper was written in the summer of 1992 and consequently does not take into consideration the many publications on Lavoisier that have appeared since that time.

view developments in France with scepticism if not outright hostility. For a preliminary example of the links between science and politics we may take the case of the Unitarian clergyman Joseph Priestley. As a Dissenter Priestley reacted strongly against the special social and political privileges associated with the Church of England. But we are concerned not only with what Priestley said and did but what others thought he stood for. The most influential witness in this matter was Edmund Burke, parliamentarian and writer. Although Burke and Priestley had been largely in agreement on earlier movements, notably the rights of the American colonists to independence, when it came to the French Revolution Burke could only view Priestley's support with horror. In his prophetic *Reflections on the revolution in France* (1791) Burke saw the end of an age of order and legitimacy and the rise of a new tyranny[3]. Burke viewed with particular horror 'philosophers', writers like Voltaire and Rousseau, who had helped undermine the social fabric of the ancien régime. In England he was especially critical of the dissenting clergymen Price and Priestley, who had welcomed the political revolution in France. And Burke pointed to Priestley's special concern with science and with chemistry in particular. Late eighteenth-century chemistry for Burke was associated with gases and explosions. He could not resist a reference to the 'airy speculations' of the chemists[4]. Referring to the political revolution he said:

> The wild gas, the fixed air is plainly broken loose...[5]

As for explosions, Priestley had been rash enough in 1787 to claim to be laying a trail of gunpowder to blow up old institutions.[6] When his words were later quoted in Parliament, far from retracting, Priestley confirmed his hostility to the conservative clergy of the Church of England by continuing the metaphor and saying:

> 'If I be laying gunpowder, they are providing the match'[7]

a sentiment which confirmed in Burke's mind the danger of Priestley. Priestley was dangerous in politics and doubly dangerous because of his enthusiasm for experimental science:

> 'These philosophers consider men in their experiments no more than they do mice in an air pump or in a recipient of mephitic gas.'[8]

Here was a chilling indictment of the new brand of amoral philosophers[9] who would soon be described by the term 'scientist'. Burke may seem in some ways to represent an extreme but there is more than an echo of Burke in the less well-known comments made in about 1800
by Henry Redhead Yorke:[10]

> "...the French chymists have, almost universally, been the most enthusiastic agents in the revolution; perhaps they flattered themselves that mankind are capable of being composed and decomposed after the model of a chemical process. It is certainly not irrational to ascribe their revolutionary zeal to their habitual fondness for experiments."[11]

Unlike the writings of Burke, it is possible to read this in a semihumourous vein. Yet it provides confirmation that writers *at the time* saw a clear connection between experimental science and politics.

But if Priestley represents a particularly good example of the overlap of religion, politics and science in late eighteenth-century Britain, Lavoisier may appear much closer to the modern idea of the man of science who works in his laboratory and on government committees as a technical expert but keeps his distance from politics. Only with the Revolution did Lavoisier express moderate reformist opinions. Lavoisier had devoted much of his time in the 1770s and 1780s to studying chemical reactions. When these exhibited anomalies according to the current theory of phlogiston Lavoisier first attempted to reconcile the theory with the new knowledge of gases before eventually overthrowing it. This story has often been told in terms of experiments and memoirs presented to the Academy. These were obviously of crucial importance in gaining a hearing for his new ideas. But Lavoisier was extremely ambitious. To obtain a hearing was not enough. Nothing less than total conversion of the science and its practitioners was his goal. I shall therefore say something about Lavoisier's consolidation of his theory before considering reactions to his approach.

## The consolidation of Lavoisier's theory and his many claims to authority

One of the methods used by Lavoisier to implant his theory more strongly in the mainstream of science was to write a textbook. Often textbooks are the work of teachers who draw on their lecture notes. Lavoisier never held a teaching post. His *Traité élémentaire de chimie* (1789) therefore holds a more important position. It was not written as a work representing an overflow from teaching nor as an additional source of income but purely as a work intended to persuade readers of the logic of his new system of chemistry. Since the *Traité* has often been discussed I will pass on immediately to mention a second publication which has received much less attention. The *Annales de chimie* is much more difficult to summarise because it consisted of so many volumes by so many different authors.[12] At the heart of it, however, was an editorial board which for the first three years was dominated by Lavoisier. It was intended to propagate the new chemistry and was all the more necessary since the only other general French journal of science at the time, the *Observations sur la physique*, was under the control of La Métherie, an inveterate opponent of the new chemistry. An important feature of the *Annales*, in which it differed from most other scientific journals, is that the leading members of the editorial board: Guyton de Morveau, Lavoisier, Berthollet and Fourcroy were also the main regular contributors to the journal.[13] Other prominent chemists including Vauquelin, Chaptal, Gay-Lussac and Thenard later joined the editorial board, thus consolidating the journal as the organ of the new chemistry.

We are concerned with the whole range of methods by which the new chemistry was propagated. We also have a special interest in reactions to that propagation. In particular did Lavoisier claim any special authority or did he employ unusual methods? It seems that Lavoisier did claim special authority and the types of strategies he used may warrant some brief consideration. I have been particularly struck by a general British hostility to Lavoisier's assumption of special claims to authority. I propose to consider Lavoisier's (mainly positive) claims under ten headings and deal briefly with each in turn. Instead of focussing on one aspect in great detail, I prefer a wider and more general approach which I hope will raise a number of issues that others may wish to follow up. The points I am going to make cover a range of different arguments, strategies and situations. It is, however, simpler to consider them all together. We will review in turn the authority of:

(i) logic, (ii) Nature, (iii) rejection of imagination, (iv) rejection of predecessors, (v) collaboration with colleagues, (vi) experiment, (vii) quantity, (viii) language, (ix) institutions, (x) nationality.

(i) It seems to me that much chemical theory before Lavoisier was based on little more than analogies and probabilities. Ideas of affinity between atoms, for example, were based on analogy with attraction in the solar system and theories of phlogiston were presented as probable rather than certain. Lavoisier, drawing on Condillac, moved chemistry onto another plane by claiming logical and mathematical certainty.[14] This was not an open-ended question like literary criticism. In the new chemistry shades of grey were replaced by featureg in black and white. Priestley, accustomed to theological and political discussion on questions which might seem open-ended, resented the finality of Lavoisier's presentation of the new chemistry. The French chemists seemed not to treat their opponents as equals but rather as misguided or even stupid colleagues who failed to see the significance of the new evidence. Of course Lavoisier was not unique in the history of science in claiming objectivity but he was unusually explicit in claiming that logic was on his side.

(ii) It should never be forgotten that Lavoisier was a child of the Enlightenment, which consistently appealed to the authority of Nature. He said:

> We trust to nothing but facts: These are presented to us by Nature and cannot deceive.[15]

Lavoisier undertook a new arrangement of chemistry, 'more consonant to the order of Nature'.[16] His clarification and division of substances into genera and species was a distinction 'established by Nature'.[17] Lavoisier was not alone in trying to build a new science on the order of nature. The science of measurement was to be reformed by replacing the old arbitrary units by a measurement of length, the meter, which was a known fraction of the dimensions of the Earth, the common home of the human race.[18]

(iii) In this list of different kinds of authority relevant to Lavoisier's claims I am including two kinds explicitly *rejected* by him. One can understand a person's position much better if one considers not only his use of certain strategies but also his refusal to make use of others. Lavoisier explicitly rejected imagination 'which is ever wandering beyond the bounds of truth'.[19] The rejection of imagination can be interpreted as a positive step to establish greater certainty. Some earlier thinkers were said to have been over confident and had ended up by deceiving both themselves and others. Lavoisier, therefore, in true Enlightenment style, attacked *prejudice*, and quoted Condillac rebuking previous thinkers:

> Instead of applying observation to the things we [i.e. they] wished to know, we have chosen rather to imagine them. Advancing from one ill-founded supposition to another, we have at last bewildered ourselves amidst a multitude of errors. These errors...[become]...prejudices...[20]

(iv) Like many reformers Lavoisier rejected the authority of the ancients. Thinking particularly of the four elements of Aristotle, he wrote:

> The authority of these fathers of human philosophy still carry great weight and there is reason to fear that it will bear hard upon generations yet to come.[21]

But Lavoisier was also notorious for doing less than justice to his immediate predecessors, especially foreign chemists.[22] In his Traité he was prepared to acknowledge this deficiency[23] but here he had an excuse:

> It is not to the history of the science of the human mind that we are to attend in an elementary treatise...[24]

In this respect he differed significantly from his colleague Fourcroy. We may accept, however, that a historical section in Lavoisier's textbook would have been a diversion from his strictly logical approach. In his memoirs, however, Lavoisier was guilty of insensitivity. It was not unexpected that the British pneumatic chemists, who had supplied basic data, should consider themselves slighted.[25] A traditional way of building up an argument had been to quote from previous writers, each providing some authority. Lavoisier was explicitly rejecting the authority of the ancients but he was also putting to one side the work of his immediate predecessors. He refused formally to acknowledge their work as the foundation for his own.

(v) In contrast to Lavoisier's playing down the role of people like Black and Priestley in providing the foundation of his own work, he was happy to acknowledge that he had benefited from the advice of a number of French colleagues who had worked with him in Paris:

> If at any time I have adopted, without acknowledgement the experiments or the opinions of M. Berthollet, M. Fourcroy, M. de la Place, M. Monge, or, in general, of any of those whose principles are the same with my own, it is owing to this circumstance, that frequent intercourse, and the habit of communicating our ideas, our observations, and our way of thinking to each other, has established between us a sort of community of opinions, in which it is often difficult for everyone to know his own.[26]

This statement can be interpreted on at least two different levels. On the one hand it seems a charmingly frank account of communal activity in which Lavoisier was uncharacteristically modest. But it is also a reminder that Lavoisier was not alone in his new theory. He was proud to acknowledge the support of several leading Academicians including both chemists and mathematicians. In proposing the new nomenclature Lavoisier collaborated with three other leading chemists and the *Méthode de nomenclature chimique* was published under their joint authority and that of the Academy of Sciences. When he launched the *Annales de chimie* as the journal of the new chemistry in 1789 it was with the collective responsibility of a whole editorial board: Guyton de Morveau, Lavoisier, Monge, Berthollet, Fourcroy, Dietrich, Hassenfratz and Adet. Each issue of the journal carried the names of all the members of the editorial board, who took joint responsibility for the contents.

(vi) Since the seventeenth century science had made much of the importance of experimental evidence. In principle experiments would be reported in detail so that they could be repeated by others. In many cases insufficient detail was given so that, with the best will in the world, it was difficult for an experimenter, relying only on a written report, to recreate the original conditions.[27] Lavoisier himself admitted that he had not always supplied sufficient detail of his experiments.[28] But there was a second barrier to the repetition of Lavoisier's experiments which relates to his apparatus, specially constructed by skilled artisans at great cost. One thinks of the ice calorimeter or his apparatus for the synthesis of water by burning hydrogen in an atmosphere of oxygen. Of the *gazometer* used to supply a continuous stream of oxygen in some experiments Lavoisier wrote:

> without [it] ...it is hardly possible to perform most of the very exact experiments.[29]

The complex apparatus, eight feet long, is illustrated in a large folding plate in his Traité,[30] perhaps intended to impress as much as to inform. His complex apparatus for organic analysis took up even more space. The purchase of such apparatus was quite out of the question for most chemists. In the case of Priestley I have previously even argued that it was the *low cost* of the basic apparatus of pneumatic chemistry which had been a major factor in attracting him into this field of science.[32]

(vii) Not unrelated to his special apparatus was Lavoisier's insistence on quantitative precision, in which he sometimes went to extremes. In the last part of his *Traité* he wrote:

> As the usefulness and accuracy of chemistry depends entirely upon the determination of the weights of the ingredients and products before and after experiments, too much precision cannot be employed in this part of the subject; and for this purpose we must be provided with good instruments.[33]

He then referred to several leading French instrument makers, including Fortin, whom he had commissioned to make a range of different balances and other instruments and added that he considered his balances unique in their precision.

Lavoisier did not feel that he had to justify this quantitative approach. For him it was axiomatic. He had probably learned the importance of the balance from Black. One could detect the addition or escape of gases by weighing, as in the conversion of chalk (calcium carbonate) to quicklime (calcium oxide). If one relied purely on observation one would see only a white powder both before and after the experiment, the carbon dioxide evolved being, of course, invisible. This major change in chemistry made by the balance could be compared to the difference made to astronomy by Galileo's telescope in a science which had previously depended entirely on naked-eye observation.

This was a problem confronting Priestley, who, like many adherents of the phlogiston theory, thought of chemistry as a *qualitative* science. There was so much that one could do in chemistry in purely qualitative terms and perhaps Priestley's greatest contribution to chemistry lay in the preparation of a number of new gases or 'airs', an achievement which depended on the study of qualitative differences.[34] Of course, Priestley occasionally made measurements, as when testing the 'goodness' of different airs, but unlike Lavoisier, this was not at the heart of his science

(viii) One of the steps taken by Lavoisier to consolidate his system that provoked the greatest hostility was his reform of chemical nomenclature. In a period before the organisation of international conferences it was left to ambitious practitioners in a particular science to propose a new nomenclature. Such was the case with Linnaeus in botany and with Lavoisier in chemistry - except that Lavoisier was wise enough to obtain the collaboration of other leading French chemists who had recently been converted to his theory. Especially important was Guyton de Morveau, who already in 1782 had suggested a rationalisation of chemical nomenclature. The addition of Berthollet and Fourcroy gave the collaborative *Méthode de nomenclature chimique* of 1787 additional authority.

Yet the French chemists were not content simply to propose a new nomenclature, to be used, for example, at some future date, a procedure which is normal for much legislation. Instead they themselves immediately began using the new terms in their memoirs and books. This helped to give currency to the new language and, since the authors were leaders in their field, all other chemists wishing to keep abreast of current research were obliged to make themselves familiar with the new terms.

There were four principal objections to the new nomenclature. The first was simply to challenge the authority of any group of chemists to change the whole language of a science. Secondly the nomenclature was attacked because it was very obviously theory-laden. To speak of *oxide of mercury*, *hydrogen*, and *sulphuric acid*, for example, presupposed the truth of the new theory. The very word *oxygen*, for those with a knowledge of Greek, presupposed Lavoisier's (erroneous) theory of acidity. Of course, Priestley's term *dephlogisticated air* presupposed the truth of the old theory but there were many old terms, like *fixed air* and *calx*, which were independent of any theory. Thirdly there were the linguistic purists who objected to certain inconsistencies in the new nomenclature which drew on both Greek and Latin. Part of this inconsistency was due to the fact that the reformers had not wished to change more chemical terms than they considered strictly necessary. Finally, for those, like Robison, who saw the new French chemistry as a conspiracy, the new nomenclature seemed not only a device to inculcate the new ideas but even more a means of obliterating the old ones.[35]

(ix) We might now consider the institutional dimension. Lavoisier and his colleagues had to obtain the permission of the Academy for publication of the *Méthode de nomenclature chimique*. The commission appointed contained several people hostile to the new ideas[36] so one can hardly expect an enthusiastic report. Indeed they warned against 'the prestige of something new' and even claimed that the old nomenclature could be recommended for its 'wonderful clarity'![37] It was not, they said, 'a matter of a day to reform and obliterate a language which is already understood, already widespread and well known over the whole of Europe'. Nevertheless they reluctantly passed the book, saying that it would be left in the future for the Academy to decide whether it should 'legalise' the new terms. In fact the Academy was to play very little part in deciding the future of the nomenclature. We may conclude, therefore, that the Royal Academy of Sciences was in no way the patron of the reform. On the other hand the secretary Condorcet became increasingly involved in politics after 1789. Lavoisier now came to play an even more important part in the affairs of the Academy and it would have been easy for outsiders by 1792-93 to associate Lavoisier (and incidentally his work) with the Academy.

After suppression of the Royal Academy of Sciences along with all the other royal academies in August 1793 there was an interval of approximately eighteen months before another official body of science, the First Class of the National Institute, came into being. By this time Lavoisier was dead but his supporters were well represented in the chemistry section of the First Class. In 1800 the six members representing chemistry were Guyton, Berthollet, Fourcroy, Vauquelin, Deyeux and Chaptal, nearly all outspoken advocates of the new chemistry.[38] All became members of the editorial board of the *Annales de chimie*. The pharmacist Baumé (1728-1804), a supporter of the phlogiston theory, was never allowed to become a full member of the First Class although before the Revolution he had been a senior member of the Royal Academy.

The period after the Terror was one when new educational institutions were founded, notably the Ecole Polytechique. Chemistry was given special prominence in the curriculum and it may be a matter of no surprise that Guyton, Berthollet and Fourcroy were the principal teachers of chemistry. Berthollet also taught at the short-lived Ecole Normale and Fourcroy was professor of chemistry at the Muséum d'Histoire Naturelle. All these were state institutions and thus appointments carried civil authority as well as academic prestige. If we recall that Fourcroy was also the author of several influential chemistry textbooks in the period 1795-1805,[39] there can be no doubt that in this leading European centre for scientific education the chemistry taught was that of the Lavoisier school. Also in 1802 the First Class was given the responsibility of making recommendations for appointments in higher education.[40] This tended to result in more Academicians being recommended for professorships in Paris. If they already had one such position they might soon find themselves with two or even three such chairs, the process known as cumul. Thus the Academy system had the effect of reinforcing orthodoxy and the chemistry of the official body of science was chemistry in the Lavoisier tradition.

(x) Finally we come to the political dimension, which may also be considered as relating to the growth of national consciousness in the period after the French Revolution and particularly to the war. Partly because of the collaboration over nomenclature Lavoisier's theory was often described in nationalistic terms as 'French chemistry', Lavoisier himself argued that France had a special role in the propagation of science partly because of its geographical position as a central state in Europe and partly because of the role of the French language in the late eighteenth century as the international language of diplomacy.[41] Because of the existence in France of an official body of science it was very easy for chemists in other countries to see Lavoisier's chemistry as having the official approval of the French government. The fact that Napoleon Bonaparte was later elected as a member of the First Class, the fact that he saw himself after seizing power as a patron of science (among many other roles), the fact that he proposed that the First Class should award a major prize for research on electricity, all this served to reinforce in the mind of European intellectuals in the early 1800s the idea of close links between science and government. Was not Berthollet a close friend of Napoleon? Had not Chaptal been appointed Minister of the Interior under Napoleon (a post he held successfully for four years)? Did not Fourcroy play a major role in the organisation of science and medical education and was not Guyton at the head of the Ecole Polytechnique?
After this review we may be in a slightly better position to appreciate some objections to the new chemistry from Britain.

Some objections from Priestley

Going back to the late 1780's we may consider the position of someone like Priestley who provides a great contrast in character and outlook to Lavoisier. An outspoken Christian minister of religion (albeit a heterodox one), a man of modest means and democratic principles, the contrast is striking. Priestley was also modest in his claims and his language. Some of his discoveries he presented as having stumbled on by accident.[42] He was usually content to use the language of ordinary discourse in his science, apologising if he felt it necessary to employ a new term. Yet this was certainly not from lack of learning -he was proficient in Latin, Greek, Hebrew and several modern languages.

Priestley must have seen Lavoisier as very elitist and in more ways than one. First, as an Academician, Lavoisier was one of a very small number of chemists recognised by the French state. His position hardly bears comparison with Priestley, who had merely stepped over the very low fence surrounding the membership of the Royal Society. In the eighteenth century no great proficiency in science was required to become F.R.S. and there was no restriction on numbers. The select members of the French Academy, on the other hand, drew a line between themselves and other practitioners of science. (Being a member of the Academy had helped Lavoisier enormously in the early propagation of his theory.) Priestley was anxious to present science as being open to everyone of ordinary intelligence and modest means. These ideas are most clearly expressed in the introduction to his *History of Electricity*. Therefore, quite independently of the oxygen theory, the two chemists belonged to contrasting traditions. They viewed the natural world and society from completely different standpoints.

One feature which unified Priestley's career in religion, politics and science was his hostility to authority. Already in 1790, reacting to the growing influence of the new theory of chemistry, he advocated

> putting an end to all undue and usurped authority in the business of religion as well as of science.[43]

He expressed a similar idea even more forceably when writing to the French chemists in 1796 from Pennsylvania. Asserting his customary independence almost for the last time he asserted that:

> no man ought to surrender his own judgement to any mere authority, however respectable.[44]

Persuasion, Priestley said, was possible by two means, by brute force, or by argument. Obviously in science, as in politics, the latter was preferable. Therefore, addressing the French chemists, he said:

> As you would not, I am persuaded, have your reign to resemble that of Robespierre ... we hope you had rather gain us by persuasion than silence us by power.[45]

A similar theme was taken up by Elizabeth Fulhame, the wife of a doctor and the author of *An essay on combustion*. She said that science should be open to everyone and argued against any 'dictatorship in science'.[46] After referring to 'M. Lavoisier and other great names' she said that she was

> persuaded that we are not to be deterred from the investigation of truth by any authority, however great, and that every opinion must stand or fall by its own merits.[47]

Priestley as a democrat could not fail to have been impressed by the *numbers* of people supporting the rival theory. In the early days the numbers game would obviously have favoured theories of phlogiston but increasingly in the early 1790s any count of numbers of supporters would have favoured the *oxygen* theory. Priestley admitted in 1800 that 'great numbers'[48] in Britain as well as in France supported the new theory. Therefore he could no longer use numerical arguments to support phlogiston. It is ironic that in his final defence of the phlogiston theory in 1800 he himself fell back on the argument from authority. He cited the names of the German chemists, Crell, Westrumb, Gmelin and Mayer, saying:

> No person needs to be ashamed of avowing an opinion which has the sanction of such names as these.[49]

Unfortunately, by this time there were very few active chemists *outside* the German states who could be cited as still supporting a theory of phlogiston. Phlogiston had never been killed outright but it was clearly now on its deathbed. Yet it would probably be a mistake to present Priestley simply as a defender of the phlogiston theory. He was much less concerned with theory in general than Lavoisier. As a plain Englishman he often said that he was concerned only with the 'facts'.[50] Hence a part of his antagonism to Lavoisier was based on the Frenchman's increasing concern with *theory*. It was the resentment of the practical man towards the theoretician, a feeling which is much more general than the particular case we are discussing.

Black and his editor, Robison.

A British chemist of greater seniority than Priestley was Joseph Black and it would be interesting to know what he made not only of Lavoisier's theory but also of his propaganda. On the first point we know that he accepted the oxygen theory by 1790.[51] Lavoisier privately assured him that he would regard his support as decisive and Black may have been taken in by such flattery.[52] As regards Black's feelings about Lavoisier's approach, we have to fall back largely on the work of John Robison, who edited Black's lecture notes for publication. Robison wrote that Black

> always expressed a high opinion of M. Lavoisier's genius and sound sense but was much displeased by the authoritative manner in which the junto of chemists at Paris announced everything, treating all doubt or hesitation about the justice of their opinions as of the want of common sense.[53]

In this latter comment Robison was saying that Lavoisier and his colleagues felt that their critics were lacking in logic, but the main point being made here was once more resentment about the authority assumed by the French chemists.

John Robison was professor of natural philosophy from 1774 to 1805 in the University of Edinburgh and therefore a respected figure in academic circles and a former colleague of Black.[54] In 1797 he had published an extremist tract claiming the French Revolution as a universal conspiracy.[55] Although mainly an attack on French Jacobinism, portrayed in caricature, it also attacked those in Britain who supported the revolution, notably Joseph Priestley. In this complex story, however, we are focussing on allegations about French chemistry.

I will not waste time quoting the absurdities of Robison's political pamphlet, although it was surprisingly well received at the time of publication. I will quote only from his correspondence and the notes which Robison added to Black's university lectures, which one might assume to be a sober academic source. Robison claimed that the new French chemistry was not something worked out and published by private individuals. Rather

> It was propagated as a public concern; and even propagated in the way in which that nation always chose to act - by address and with authority. Everything pertaining to the system was treated in council, and all the leading experiments were documented by committees of the Academy of Sciences.[56]

In defiance of strict chronology Robison elsewhere referred to the new nomenclature of 1787 as the work of a 'Revolutionary committee'[57]:

> The new language in chemistry was not so much intended for instructing the world as for securing the sovereignty in science to the French Junto .[58]

As further evidence of his conspiracy theory Robison mentioned the publication of the *Annales de chimie* 'in concert' and the introduction of a new chemical language -just as the French revolutionaries had introduced a republican calendar with its strange names for the months. Indeed by mentioning certain individuals like Guyton and Hassenfratz he had evidence of an overlap between the chemists and the legislators. Referring to the new nomenclature, Robison claimed:

> A determination to be the founder of a system and a sect of philosophers seems to have seduced M. Lavoisier and made him acquiesce in measures which may be called violent and unbecoming.[59]

In his reference to a 'sect of philosophers' Robison was using the same language as Burke.

This edition of Black's lectures was reviewed by Henry Brougham, who applauded Robison's decision to comment on the behaviour of the French chemists. He wrote:

> We rejoice that this subject is fairly brought before the public; and on whichever side the decision may finally be given, the history of science, as well as the political

> history of our times, is likely to be illustrated by the discussion. That the French chemists formed themselves into a junto for the propagation of their system; that like all juntos, they delivered their doctrines with an authoritative tone, highly indecorous in matters of science; and that they even displayed somewhat of a spirit of persecution towards those who, from ancient habits, or from a predilection for their own theories, refused their assent to the antiphlogistic doctrines, are facts which cannot be disputed.[60]

Brougham, however, was happy to accept the oxygen theory and thought that Robison had taken too far his support of Black and his hostility to the thrust of Lavoisier's chemistry but only as a reasonable 'approximation' to the French. Part of Robison's attitude was based on his feeling that the French chemists had not attached sufficient importance to the work of his fellowcountryman. He was most anxious 'that Dr Black should not appear like the humble pupil of Lavoisier.'[61] But the concept of illegitimate authority and official backing is a common theme among British critics of Lavoisier. While on the subject of Robison I may refer briefly to another extremist writer, Robert Harrington, who spoke very respectfully of Priestley,[62] but considered the new French chemistry as a conspiracy, their machinations extending to a regular system.[63] According to Harrington oxygen was a sort of conjuring trick[64] and he advised the English reader to

> stick close to his King, his County, his Constitution, his Religion and his God: and oppose all the French revolutionary doctrines, both scientific, politic, deistic and atheistic.[65]

Further accusations about the sinister implications of the association of the French chemists was made by Richard Chenevix, who spoke of a 'junto of French chemists', who had transformed the language of chemistry. He alleged that they hoped that 'the universal currency' of the French language, 'which had so often furthered their political ends, might help them in their scientific intrigues'.[66]

Davy's reactions.

We come finally to Humphry Davy, whose very first published paper was an immature attempt to replace Lavoisier's caloric with light; he even introduced the neologism *phosoxygen*.[67] However, even in his maturity, in his famous paper of 1807 on the decomposition of the fixed alkalis, Davy added a note to the effect that it was still possible to defend a theory of phlogiston.[63] In his Bakerian Lecture for 1808 he proudly referred to this speculation that inflammable substances might contain something like hydrogen or phlogiston.[69] Davy was of course prepared to accept the main truth.[70] He expected chemical theory to change in the future and I believe it was always part of his ambition to challenge the new French chemistry. He probably came nearest to this when he showed that the fixed alkalis are the oxides (or hydroxides) of previously unknown metals. This was a further blow to that part of Lavoisier's theory which associated oxygen with acidity. For the young Davy, however, Lavoisier was a figure of the past. The Frenchman with whom he consciously competed in the most active year of chemical research was Gay-Lussac, an exact contemporary, since both had been born in 1778. The sharpness of the competition was increased by the fact that France and Britain were at war. When Davy announced his spectacular isolation of sodium and potassium using the electric pile of the Royal Institution, French government funds were used to construct a bigger and better pile at the Ecole Polytechnique.

Since Davy was once quoted as saying that, if the governments of France and Britain were at war, the men of science were not [71], it may be worth quoting from a draft lecture he prepared in 1810 but never actually delivered, which tells a different story. He wrote:

> The scientific glory of a country may be considered in some measure as an indication of its innate strength. The exaltation of Reason must necessarily be connected with the exaltation of the other faculties of the mind and there is one spirit of enterprise, vigour and conquest in science, arts and arms.[72]

This passage suggests that Davy sometimes saw science in nationalistic terms and this would have been all the more understandable around 1810 when Napoleon had conquered half of Europe. There were many people in England who had a great admiration for France's contribution to western civilisation, who nevertheless saw Napoleon as a monster whose ambition seemed to amount to little less than world domination.

It is in this context that I would like to introduce a part of a letter written by Davy in the spring of 1814 to a Swiss correspondent. In

the previous winter in Paris Davy had carried out some studies of iodine on a visit to Paris almost under the nose of his rival Gay-Lussac. Both have claims to have discovered the elementary nature of iodine.[73] It was only a few years earlier that Davy had unequivocally proclaimed the elementary nature of the gas known as 'oxymuriatic acid' and which he now proposed to call 'chlorine'.[74] The irony here is that Gay-Lussac had reached such a conclusion earlier but had been prevailed upon to claim this as no more than a possibility.
We are now in a position to understand the letter by Davy, who referred to iodine as:

> a very useful ally in my endeavour to establish the independence of chlorine and to do away [with] the imperial despotism of oxygen.[75]

I think that the really interesting phrase here is 'the imperial despotism of oxygen'. Everyone in Britain would have been familiar with the concept of imperial despotism' as applied to Napoleon. But it took a chemist to use this phrase as a metaphor and apply it to the role of oxygen in his science. Oxygen was certainly at the centre of the new chemistry and Davy was here expressing his resentment. Twenty years after Lavoisier's death one could hardly still name him as the leading chemist and the other French chemists were a rather heterogeneous group covering at least two generations. Better to focus on the element that Davy had wrestled with throughout his whole chemical career, whether in its elementary state or as a compound with alkalis or as a supposed constituent of chlorine. The chemistry of the time was dominated by oxygen and British chemists inevitably saw this as a French achievement, even a French imposition. When countries are at war it is only too easy to characterise any situation in nationalistic terms.

## Conclusion

We does all this amount to? There will be some who will want to say that I have been discussing little more than random metaphors used by British chemists in the period 1789-1815. Although some of the quotations I have used are obvious metaphors, they are anything but random. They illustrate a common theme, a common resentment against the style as much as the content of the new French chemistry. Priestley made the accusation that Lavoisier and his colleagues were arguing from authority rather than seeking a consensus. They were using force rather than persuasion to convert people to the new chemistry. It was probably understandable that someone on the losing side in an argument should claim that he was suffering from oppression, just as it was easy for Lavoisier on the winning side to claim objectivity. The claims of the oxygen theory may not have been viewed by the opposition mainly in philosophical terms. They did not stand back as might the modern student and ask, for example, which theory provided a simpler explanation of the observed phenomena. Some like Priestley could not help thinking in more political terms - what right had Lavoisier and his colleagues to change the basis of chemistry? What right did Lavoisier have to-ride roughshod over the traditional theory of chemistry and substitute his own ideas? Lavoisier had started with a certain authority as a member of the Royal Academy of Sciences but by the late 1780s he had assumed a much greater authority which drew some resentment from those who did not agree with his quantitative takeover bid of chemistry.

Previous studies have given information about the conversion of chemists, noting which ones accepted the oxygen theory by a particular date. This is obviously a matter of great interest. But perhaps now it is possible to go further and raise the question of reluctant converts. Conversion in science, as in religion or politics, can take a number of different forms. We are all familiar with the newly converted who show a greater enthusiasm than the people whom he joins; there are always some who are 'more royalist than the king'. But, if the conversion is brought about at the expense of a deep resentment, one is likely to obtain recruits who are *less* royalist than the king. The importance of this in our story is that supposed 'converts' such as Davy may continue to try to wriggle out of the new system. Davy taught the oxygen theory but nothing would have pleased him better in his research than to force a major reappraisal of the theory.

I have ventured to suggest that science may sometimes be connected with politics. One connection is through questions that have been raised about power and authority. There has been much discussion recently about the importance of *persuasion* in science. I agree that this is an important issue to examine but I would not want to confine the agenda to rhetoric. This is only a part of the story of the rise of the oxygen theory. There is the question of a new language which might be interpreted as thought control. More fundamental than the style of Lavoisier's *Traité* is his decision to write a textbook at all and, arguably more important than the *Traité*, was the *Annales*, not a single volume but an on-going publication soon to become monthly, which hammered home the new chemistry.

I would also want to argue that there is further evidence in the story I have told of national styles in science. Several of the British critics saw an *association*, however indirect, between the French government and science, which they considered morally indefensible. Living under a constitutional monarchy acting in conjunction with a parliament, British writers made much of the rhetoric of liberty, and would criticise the French people living in the 1780s under an absolute monarchy. When this political system was replaced by the Jacobins and later by the rise of Napoleon, British critics had further examples of extreme authoritarianism. It was easy for critics to claim that similar authoritarianism existed in science. Indeed the *dirigisme* of the French state may well have influenced the presentation of French science and may be contrasted with a British *laissez-faire* philosophy. Through official agencies such as the National Institute, there seemed to be an *official* science in France and the French were trying to spread their ideas in other countries. There only needs to be a grain of truth in an idea for it to be believed by critics - especially in time of war. The same comment may be made about the accusation that the French chemists constituted a junto, an accusation that may seem very strange to us today. It informs us first about a certain image but this image was not totally unwarranted by the facts. In the *Methode de nomenclature chimique* of 1787 and in the *Annales de chimie* from 1789 onwards the French chemists had presented themselves as a group, united in advocating the new chemistry and the new nomenclature. We may contrast this association with the position of the British chemists who worked and published very much as individuals. This is not the least important conclusion to draw from evidence I have provided.

It is possible to think of science as a game, played according to generally agreed rules. Unfortunately, in the case of the oxygen theory the French and the British played the game by different rules. The British had a keen sense of fair play and it was not long before they were crying 'foul'. It is not our business today to award points to each side but this analogy may help us to understand the difference in attitude of the French and the British chemists.

Finally I am fully aware that the extensive list of claims I have given for Lavoisier's authority do not correspond exactly with the British contemporary reactions I give in the latter part of my paper. I think it would have been too limiting to aim for an exact fit. In the review of British reactions questions of nomenclature, co-operative activity and nationality came to the fore while some other issues were only hinted at or not mentioned at all. It was not so much that Priestley complained about the expense of Lavoisier's apparatus as that it belonged to a different world.[76] In any case, long drawn-out quantitative experiments were just not Priestley's style. Towards the end of his career he preferred to concentrate on one or two awkward experiments of his own. So I make no apology for the lack of symmetry. Sometimes historians reconstruct the past in too tidy a fashion. At best I have probed a little beneath the surface to explore some assumptions. Lavoisier's methodology was only partly explicit. In so far as it has been examined by historians it has been mainly to look at his experimental work. I have been more concerned with his propaganda. In some ways he was more successful in persuading his compatriots than chemists in other countries. In any debate it is often the case that people do not agree because they start from different premises. This is an important point to bear in mind in considering reactions to the work of Lavoisier.

## NOTES

1. Thus Fourcroy in February 1789 spoke of 'la révolution que la chimie a éprouvé depuis quelques années', Lavoisier, *Traité élémentaire de chimie*, Paris, 1789, vol.2, p.629. See also Henry Guerlac, 'The chemical revolution. A word from Monsieur Fourcroy', *Ambix*, 23(1976), 1-4.
2. Berthollet, *La révolution chimique. Lavoisier*, Paris, 1890.
3. Crosland, 'The image of science as a threat: Burke versus Priestley and the "Philosophic Revolution"', BJHS, 20(1987), 277-307.
4. Burke, *Reflections on the revolution in France* (1790), ed. Conor Cruise O'Brien, Harmondsworth, Middx., 1981, p.370.
5. *Ibid.*, p.90.
6. T. Rutt (ed.), *The theological and miscellaneous works of Joseph Priestley* (26 vols., London, 1817-32), vol.18, p.544.
7. Parliamentary History, 2 March 1790, 28, col.438n.
8. Burke, *Letter to a noble Lord.* E. J. Payne (ed.), *Select works of E. Burke*, Oxford, 1898, vol.5, p.142.
9. It is ironic that Priestley himself was intensely concerned with moral questions.
10. Although these remarks were published in 1814, they were probably written about 1800, since Yorke refers to volume 28(1799) of the *Annales de chimie* as being the most recently published.
11. R. Yorke, *Letters from France*, London, 1814, p.314.
12. The present author is currently engaged on an extensive study of the *Annales de chimie*, the first series (96 vols.) of which covered the period 1789-1815 with a gap in publication in the years 1794-96.
13. The other original editors were Dietrich, Hassenfratz and Adet. Lavoisier contributed much less material than the others but may be seen as the organising genius.
14. *Elements of chemistry*, Edinburgh, 1790, Introduction. He repeatedly took mathematics as a model, e.g. pp.xv, xviii.
15. *Ibid.*, p.xviii.
16. *Ibid.*, p.xx.
17. *Ibid.*, p.xxvi.
18. Crosland, 'Nature and measurement in eighteenth-century France', *Studies on Voltaire and the eighteenth century*, 87(1972), 277-309.
19. *Elements*, p.xvii.
20. *Ibid.*, p.xxxvi.
21. *Ibid.*, p.xxii.

22. When foreign chemists were very occasionally mentioned in his memoirs their names were often misspelt.
23. ...'I have been prevented from doing that justice to my associates, and more especially to foreign chemists, which I wished to render them', *Elements*, p. xxxii.
24. *Ibid.*, p.xxxiii.
25. Especially Priestley but also to some extent Black, particularly represented by Robison - see later discussion.
26. *Elements*, pp.xxxiii-xxiv.
27. Harry Collins, *Changing order. Replication and induction in scientific Practice*, London, 1985, e.g.p.l9.
28. *Elements*, p.xxxv.
29. Ibid., p.308.
30. Plate VIII
31. Plate XI
32. Crosland, Priestley memorial lecture. 'A practical perspective on Joseph Priestley as a pneumatic chemist', *B.J.H.S.*, 16(1983), 223-238.
33. *Elements,* p.297.
34. The reason why Hales half a century earlier had failed to recognise the new gases he had prepared was that, as a blinkered Newtonian, he was concerned obsessively with the quantity of 'airs'.
35. Joseph Black, *Lectures on the elements of chemistry*, ed. John Robison, Edinburgh, 1803, vol.2, p.217.
36. The committee consisted of Baumé, D'Arcet, Sage and Cadet de Vaux.
37. 'Rapport sur la nouvelle nomenclature' (extrait des Registres de l'Académie Royale des Sciences du 13 juin 1787), *Méthode de nomenclature chimique*, Paris, 1787, pp.238-252.
38. Deyeux was a pharmacist who accepted the new chemistry. He joined the editorial board of the *Annales de chimie* in 1800.
39. A. Smeaton, *Fourcroy, chemistry and revolutionary, 1755-1809*, Cambridge, 1962, pp.213ff.
40. Crosland, *Science under control. The French Academy of Sciences, 1795-1914*, Cambridge, 1992, pp.236-241.
41. Introduction to *Annales de chimie*, 1(1789).
42. One of the most often quoted of Priestley's chemical writings relates to the role of chance in scientific discoveries. See *Experiments and observations on different kinds of air* (3 vols., London, 1774-77), vol.2, p.29.
43. *Ibid.*, vol.l, p.xxiii. In religion it was obviously the special authority of the Church of England to which he objected. I first publicly discussed some of the implications of this and similar

quotations in one of three Priestley Memorial Lectures given at the University of Leeds in 1983 to mark the 250th anniversary of Priestley's birth.

44. *The doctrine of phlogiston established*, Northumberland, Pa., 1800, Preface (written 1796), reproduced in Robert E. Schofield (ed.), *A scientific autobiography of Joseph Priestley. Selected scientific correspondence*, Cambridge, Mass., 1966, p.289.

45. Ibid.,

46. Mrs. Fulhame (sic), *An essay on combustion ... wherein the phlogistic and antiPhlogistic hypotheses are proved erroneous*, London, 1794, p.xi. See also J. R. Partington, *History of chemistry*, vol.3, 1962, PP.708-9.

47. *Fulhame, op.cit.*, p.xiii.

48. Schofield, *loc.cit.*

49. *The doctrine of phlogiston established*, Philadelphia, 1800, p.vi.

50. Schofield, *op.cit.*, p.134. In a letter of 1784 to Cavendish Priestley says he will state only the *facts* about his experiments, leaving others to draw the proper inferences, ibid., p.233.

51. Letter of Black to Lavoisier, *A c.,* 8(1791), 225-9.

52. This is discussed in my forthcoming study of the *Annales de chimie*.

53. Black, *Lectures*, vol.2, p.216. Robison suggested that Lavoisier's ambition exceeded that of Newton, *ibid.*, vol.l, p.556.

54. Morrell, 'Professors Robison and Playfair, and the *theophobia gallica*: natural philosophy, religion and politics in Edinburgh, 1789-1815', *Notes and Records of the Royal Society of London,* 26(1971), 43-63.

55. *Proofs of a conspiracy against all the religions and governments of Europe, carried on in the secret meetings of freemasons, illuminati and reading societies.*

56. Black, *Lectures*, vol.2, p.217.

57. Eric Robinson and Douglas McKie (eds.), *Partners in science. Letters of James Watt and Joseph Black*, London, 1970, p.352.

58. Letter of Robison to Black's nephew, August 1800, *ibid.*, p.349.

59. Black, *Lectures*, vol.l, p.555. c.f. a letter of 9 September 1800 from Robison to Black in which the former spoke of 'the scientific dominion of the Gallic philosophers', *Ibid.*, p.352.

60. Edinburgh Review, 3(1803), 21.

61. Robinson and McKie, *op.cit.*j p.371.

62. *The death warrant of the French theory of chemistry*, London, 1804. He regarded Priestley as a great authority and quoted extensively from him.

63. *Ibid.*, p.98

64. *Ibid.*, p.127.

65. *Ibid.*, p.101.

66. *Edinburgh Review*, 34 (1820), 405. Another person outraged by the attempt to inspire a new chemical nomenclature was Stephen Dickson, *An essay on chemical nomenclature,* London, 1796, p.27.

67. 'An essay on heat, light and the combinations of light' (1799), *The collected works of Sir Humphry Davy* (9 vols., London, 1839-40) vol.2, pp.5-86.

68. *Ibid.*, vol.5, pp.57-101(89n.).

69. *Ibid.*, p.132.

70. *Ibid.*, p.89.

71. A phrase originally used by Edward Jenner in a letter addressed to the Institute in Paris. Sir Gavin de Beer, *The sciences were never at war*, London, 1960, p.197.

72. Royal Institution, Davy MSS. Box 3, folder 2, 1810, Lecture 5.

73. Crosland, *Gay-Lussac, scientist and bourgeois*, Cambridge, 1978, pp.80-87.

74. *Ibid.*, p.79.

75. Rome, 26 May 1814, Papiers Rillet, dossier G4, Geneva.

76. Priestley, *The doctrine of phlogiston established*, Northumberland, Pennsylvania, 1800, p.77 quoted by Jan Golinki, *Science as Public culture. Chemistry and the Enlightenment in Britain, 1760-1820*, Cambridge, 1992, p.138. Golinki's book is mainly concerned with chemistry in Britain but it also includes some discussion of British reactions to the French chemistry.