The Hidden History of Phlogiston

How Philosophical Failure Can Generate Historiographical Refinement

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Abstract: Historians often feel that standard philosophical doctrines about the nature and development of science are not adequate for representing the real history of science. However, when philosophers of science fail to make sense of certain historical events, it is also possible that there is something wrong with the standard historical descriptions of those events, precluding any sensible explanation. If so, philosophical failure can be useful as a guide for improving historiography, and this constitutes a significant mode of productive interaction between the history and the philosophy of science. I illustrate this methodological claim through the case of the Chemical Revolution. I argue that no standard philosophical theory of scientific method can explain why European chemists made a sudden and nearly unanimous switch of allegiance from the phlogiston theory to Lavoisier's theory. A careful re-examination of the history reveals that the shift was neither so quick nor so unanimous as imagined even by many historians. In closing I offer brief reflections on how best to explain the general drift toward Lavoisier's theory that did take place.

Keywords: *Chemical Revolution, phlogiston, history and philosophy of science, scientific change, Lavoisier.*

1. Introduction

Many historians of science have felt for some time that standard philosophical doctrines about the nature and development of science are not adequate for representing the real history of science. This is one of the major obstacles standing in the way of productive collaboration between historians and philosophers of science. I would like to turn the historians' frustration on its head: when philosophers of science fail to make sense of certain historical events, that may not always be the philosophers' fault; it is possible that there is something wrong with the accepted historical descriptions of those events,

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precluding any sensible explanation of why they took place. If that is the case, philosophical failure can serve usefully as a guide for improving historiography. From this point of view, we can easily see that historians and philosophers of science will have much to talk to each other about, because these philosophical failures are quite abundant!

I will use my current work-in-progress on the Chemical Revolution in order to illustrate this mode of history-philosophy interaction.[1] The task at hand is to explain the decisions that scientists made in the Chemical Revolution. For philosophers and philosophical historians of science, such explanations need to be given in the framework of some philosophical theory of scientific method (or rationality, or progress, or at least something related to the basic nature of science). So, my thesis of philosophical failure amounts to the claim that no theory of scientific method has been able to explain the event that most philosophers of science have understood the Chemical Revolution to be. In the words of Paul Thagard, this is what the event consisted in:

> In 1772, when Lavoisier first began to form his views, the dominant theory in chemistry was the phlogiston theory of Stahl (1723/1730). By 1789, when Lavoisier published his *Traité* […] the vast majority of chemists had gone over to Lavoisier's oxygen theory, which gave a very different explanatory account of the phenomena of combustion, calcination, and respiration […]. [Thagard 1990, p. 184]

Although there is a more sophisticated historical literature on the Chemical Revolution that I will be drawing from later on, the notion of the Chemical Revolution expressed by Thagard is pretty much the standard view of historians, too – rather than an instance of the careless and gross distortions of history that one sometimes finds in the philosophical literature. John McEvoy's recent overview of the historiography of the Chemical Revolution notes: "The Chemical Revolution has generally been regarded as the very paradigm of a scientific revolution." What a scientific 'revolution' means, of course, is a point of great contention, but McEvoy observes that "the suddenness, brevity and pace of the Chemical Revolution, together with the burst of new discoveries and foundational conflicts that accompanied it, marked it in the minds of many commentators as arguably the best example of a classic revolution in the history of science." (McEvoy 2010, pp. 18-19)

Therefore I take it as a broadly accepted historical view that whatever else the Chemical Revolution was, it consisted in a rather sudden and nearly unanimous switch of allegiance by late 18th-century European chemists from the phlogiston theory to Antoine-Laurent Lavoisier's 'anti-phlogistic' theory. Describing the failure of philosophers to explain why this abrupt change took place is the remit of Section 2 below. And then I will move on to use the philosophical failure historiographically in the way promised above. That is to say, I will advance a revised description of the Chemical Revolution in Section 3, followed by an explanation of the newly described event in Section 4. I will close with some general and abstract reflections on the history-philosophy relation in Section 5.

2. Philosophical failures in explaining the Chemical Revolution

2.1 Basic empiricism

Some of the philosophical explanations of the Chemical Revolution on offer can be disposed of quite easily.[2] Some people think that the phlogiston theory deserved to be consigned to the dustbin of history because phlogiston was just an imaginary entity, not based on anything empirical. This is a basic misconception, as phlogiston had some detailed links with observed phenomena and with very concrete practical operations. And Lavoisier's theory relied essentially on caloric, the material fluid of heat, which was just as unobservable or hypothetical as phlogiston.

Even many of those who do recognize the respectable empirical character of phlogiston think that the phlogiston theory was, in the end, factually inadequate. As space is limited I will only discuss the most sophisticated version of this argument known to me, due to Philip Kitcher. Kitcher (1993, p. 272) sets out to demolish the view that "there was no cognitively superior reasoning available to the participants, which would have decided the issue in favor of Lavoisier". He wishes to "argue that this fashionable picture is a myth" and in fact less adequate than the old view that "the phlogiston theory crumbled under the cumulative force of Lavoisier's evidence". An improved version of this old view is what Kitcher tries to provide, more successfully in my view than anyone else who has tried to do the same. Kitcher is clearly aware of the various merits of the phlogiston theory and, like various other well-informed commentators, grants that there was initially no clear difference between the empirical adequacy of the phlogiston theory and Lavoisier's theory (*ibid*., p. 273). However, Kitcher argues, the phlogiston theories were unable to deal with the new empirical evidence that emerged in the 1780s.

As in many other arguments (starting with Lavoisier's own) designed to show the empirical inadequacy of the phlogiston theory, Kitcher focuses on the weight relations in key chemical reactions including combustion and calcination. Intuitively, the main point is that in combustion/calcination nothing (such as phlogiston) is emitted, but something (oxygen) is absorbed, as shown by the fact that the reaction products, added together, weigh more than the combustible substance (or the metal) before the reaction. Kitcher avoids the common mistake of assuming that the phlogistonists simply ignored the evidence, or that they fled into the idea of the negative weight of phlogiston (which a small number of people did entertain). Rather, he correctly notes (*ibid*., p. 277): "they do something that is far more reasonable: to wit, accept Lavoisier's claim that something from the air is absorbed and try to combine this concession with the traditional idea that phlogiston is emitted." But this defensive strategy ran into dead-ends eventually, Kitcher argues. He focuses on the work of Richard Kirwan, who tried to accommodate all observed phenomena by postulating that a calx may contain water or 'fixed air' (our carbon dioxide), and that the combination of oxygen and hydrogen can make water or fixed air depending on the temperature (Kirwan 1789; Kitcher 1993, pp. 283-288). Kitcher is correct in noting that Kirwan's story ended in complex tangles, even inconsistencies with some experimental results, such as the demonstration that no fixed air could be extracted from calxes unless there were carbon impurities present, regardless of the temperature to which they were subjected. Thus Kitcher concludes that it was right that Kirwan himself accepted defeat and gave up the phlogiston theory.[3] Kitcher contends that "the rest of the story is more of the same". But that needs to be shown, not assumed. For example, Kitcher does not assess the mature phlogiston theory advanced by Henry Cavendish (1784), which was free of any contradictions or inordinate complexities as far as I can see. Instead he discusses (*ibid*., p. 284) Cavendish's earlier view (1766), according to which inflammable air was pure phlogiston, which was problematic considering that inflammable air clearly had weight. As I will discuss further in Section 2.4, Cavendish had a simple and straightforward view about what the combination of hydrogen and oxygen would make (water, not fixed air); his view did not have the sort of ambiguity that created trouble for Kirwan.

We can meaningfully engage in an in-depth dispute about just how much the empirical adequacy of the phlogiston theories was compromised by various observations, especially regarding weight. However, we need to do so without losing sight of a far more important point: the relevant question of empirical adequacy is a comparative one, not an absolute one. The question is not whether the phlogiston theory was absolutely flawless (to which the answer is 'of course not'), but whether its empirical adequacy was better or worse than its competitors at the time, particularly Lavoisier's theory. We really need to lose the habit of treating 'phlogiston theory got *X* wrong' as the end of the story; we also need to ask whether Lavoisier's theory got *X* right, and whether it did not get *Y* and *Z* wrong.

There has been a great tendency, among philosophers and historians alike, to ignore and minimize the things that Lavoisier's theory could not explain (or got wrong by modern standards).[4] One might say that this is testimony to the effectiveness of Lavoisier's rhetorical offensive, which seems to have won over generations of later commentators as well as his contemporaries. Kitcher (1993, p. 278) is much more aware of this trap than most detractors of phlogiston are, and he does note that there were difficulties with Lavoisier's theory of heat and his theory of acidity. Yet Kitcher's concession, that giving a verdict in favor of Lavoisier's theory "is not to say that his own analysis is free of problems", is made briefly with no details, and then left behind.[5] And then he goes right back to an elaborate discussion highlighting those issues on which the phlogiston theories had the most difficulties. This biased emphasis, along with a near-dismissal of Lavoisier's problems, gives apparently strong support to Kitcher's assessment that by the mid-1780s Lavoisier had developed "a general account which deals, in a unified and consistent way, with a far greater range of the experimental results than any extant version of the phlogiston theory." (*Ibid*., p. 278) At the end Kitcher (*ibid*., p. 289) does again acknowledge the need for a comparative viewpoint, and briefly discusses how Lavoisier had to repel Kirwan's attack concerning Lavoisier's table of affinities of oxygen.

A closer look at the primary literature reveals that there were a number of other observations and experiments known at the time which Lavoisier's theory failed to explain. First of all, there was a profusion of curious and weird anomalous phenomena reported by various phlogistonists that were simply brushed aside by Lavoisier and his colleagues. To get a flavor of these phenomena, it would be sufficient to have a casual glance through Priestley's volumes on air or Scheele's collected papers. For example, Priestley reported on an experiment in which he "impregnated" distilled water with "nitrous vapour": "the water presently became warm, then began to sparkle very much, air issuing from all parts of it very copiously; and after this it assumed a light blue colour"; in a later run of the same experiment, the water went on to become green, "about which time the emission of air ceased; and lastly, after the green colour had deepened very much [...] a yellowish tinge was perceived to be diffused through the green colour." (Priestley 1790, pp. 336-8) Priestley continued to produce a stream of diverse phenomena in the laboratory right up to his last years. For example, he became excited about Volta's invention of the battery, and reported that he could not electrolyze water devoid of dissolved air, and also that he had dissolved a gold wire in plain water by using it as the anode in electrolysis (Priestley 1802).

Or look at Scheele's 1774 paper on manganese, which is remembered now for the discovery of chlorine. What Scheele called 'manganese' was pyrolusite, or manganese dioxide (MnO_2) probably in an impure form. In reacting this mineral with muriatic acid (or marine acid – hydrochloric acid, HCl, in modern terms) he produced a poisonous yellow-green gas (chlorine), which he called ‘dephlogisticated muriatic acid’ because he thought it was the result of the removal of phlogiston (hydrogen?) from the acid by the manganese. He also went on to make a number of other observations. For instance, Scheele (1931, pp. 24-25) reported that the reaction of manganese with acids was facilitated by the presence of high-phlogiston substances such as sugar, honey, gum arabic, or hartshorn jelly; manganese filings dissolved only partially in acid alone, but dissolved completely when combustibles like sugar were added to the acid.[6] Lavoisierians ignored all the complicated observations, and attempted to contradict Scheele’s interpretation of chlorine by claiming that it was ‘oxygenated muriatic acid’.[7]

Even aside from such unruly phenomena there were other significant anomalies, which were recognized by Lavoisier himself and his allies. When pressed, even the great Lavoisier-enthusiasts of today will admit that he was quite mistaken in his theory of acids, in which he proposed that oxygen was the essence of acidity. Apologists tend to dismiss Lavoisier’s theory of acids as an incidental part of his chemistry that can be safely discarded while preserving the other, good parts. But Lavoisier was so enamored with his theory of acids that he named his beloved oxygen to mean ‘acid-generator’. He was not dissuaded by critics, including Cavendish (1784, p. 153), who pointed out that muriatic acid and the ‘acid of tartar’ could not be deprived of their acidity “by any union with phlogiston” (or, what came to the same thing for Cavendish, by any attempt to extract oxygen from them). While Lavoisier knew that there were certain acids that could not be made to yield any oxygen, he was confident that improved techniques would extract oxygen from them eventually. With such confidence he included in his table of chemical elements the ‘muriatic radical’ (what one would produce by removing oxygen from hydrochloric acid, HCl, in modern terms), as well as the ‘fluoric radical’ and the ‘boracic radical’.

In Lavoisier’s table of simple substances (Figure 1) we also have a reminder of another problematic part of Lavoisier’s theory, namely caloric, at the top of the table along with light. Unlike the theory of acidity, ideas about caloric were undeniably central to Lavoisier’s system, occupying the very first chapter of his definitive textbook of the new chemistry (Lavoisier 1789). There are many modern apologists, including Kitcher (1993, p. 278, footnote 70), who try to downplay Lavoisier’s belief in the reality of caloric. They ignore the key role that caloric played in his theoretical system, and mistake as genuine and specific doubt what was merely common lip-service to proto-positivistic caution about all theoretical entities.[8] The points I want to make here become the clearest in relation to Lavoisier’s theory of combustion, which was indeed one of the most important points of contention between Lavoisier and the phlogistonists. And this is also where we encounter the most important and most incredible anti-phlogiston prejudice in most modern commentators.

	Noms nouveaux.	*Noms anciens correſpondans.*
Subſtances ſimples qui appartiennent aux trois règnes & qu'on peut regarder comme les élémens des corps.	Lumière	Lumière.
	Calorique	Chaleur. Principe de la chaleur. Fluide igné. Feu. Matière du feu & de la chaleur.
	Oxygène	Air déphlogiſtiqué. Air empiréal. Air vital. Baſe de l'air vital.
	Azote	Gaz phlogiſtiqué. Moſete. Baſe de la moſete.
	Hydrogène	Gaz inflammable. Baſe du gaz inflammable.
Subſtances ſimples non métalliques oxidables & acidifiables.	Soufre	Soufre.
	Phoſphore	Phoſphore.
	Carbone	Charbon pur.
	Radical muriatique.	Inconnu.
	Radical fluorique	Inconnu.
	Radical boracique	Inconnu.
Subſtances ſimples métalliques oxidables & acidifiables.	Antimoine	Antimoine.
	Argent	Argent.
	Arſenic	Arſenic.
	Biſmuth	Biſmuth.
	Cobolt	Cobolt.
	Cuivre	Cuivre.
	Etain	Etain.
	Fer	Fer.
	Manganèſe	Manganèſe.
	Mercure	Mercure.
	Molybdène	Molybdène.
	Nickel	Nickel.
	Or	Or.
	Platine	Platine.
	Plomb	Plomb.
	Tungſtène	Tungſtene.
	Zinc	Zinc.
Subſtances ſimples ſalifiables terreuſes.	Chaux	Terre calcaire, chaux.
	Magnéſie	Magnéſie, baſe du ſel d'Epſom.
	Baryte	Barote, terre peſante.
	Alumine	Argile, terre de l'alun, baſe de l'alun.
	Silice	Terre ſiliceuſe, terre vitrifiable.

Figure 1: Lavoisier's table of simple substances (Lavoisier 1789, p. 192; p. 175 in the English translation).

Here we have to remember that Lavoisier understood combustion as involving a decomposition of oxygen gas into 'oxygen base' and caloric, the oxygen base combining with the combustible, and the caloric being released. The heat generated in combustion comes out of the oxygen gas, and it is essential that the oxygen is in a gaseous state to begin with, since it is the abundance of combined caloric which puts matter into the gaseous state. The various difficulties of this theory of combustion may not be discussed very often in histories of the Chemical Revolution nowadays, but they were well known at the time. Thomas Thomson (1802, vol. 1, pp. 354-8), for instance, summarized them in his popular and authoritative textbook of chemistry. The empirical anomalies of Lavoisier's theory included cases of combustion without involving oxygen in the gaseous state, and some cases involving no oxygen at all. The phenomena simply did not follow Lavoisier's dictate that the production of heat was caused by the liberation of caloric from a gas as it became condensed to a solid or liquid state (and similarly with light); there were cases of heat production when the reaction products were gaseous (*e.g.* the burning of carbon), and cases in which gases were condensed by chemical combination without much heat production. One high-profile difficulty, discussed in illuminating detail by Seymour Mauskopf (1988), concerned the combustion of gunpowder, no doubt prominent in Lavoisier's own mind as he worked from his laboratory at the Paris Arsenal. No less than Claude-Louis Berthollet, later to be one of his most loyal allies, challenged Lavoisier's theory using this example: gunpowder combusted very well in the absence of ambient oxygen gas; there was oxygen contained in the gunpowder itself, but that was in the solid state.

All in all, the empirical adequacy of Lavoisier's new chemical theory was highly questionable, and often questioned. It is genuinely difficult to say whether Lavoisier's theory was more or less empirically inadequate than the phlogiston theory. This is by no means a knock-down case of one theory being so clearly superior to another that a careful consideration is not necessary. In order to give a clear verdict, we would need an agreed-upon quantitative measure of empirical adequacy that can give us a composite index from the variety of phenomena that a theory covers more or less well. In the absence of such an empirical adequacy measure, we may not be able to go beyond Cavendish's assessment at the time, which we would do well to remember, at least (1784, p. 152): "as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it […] it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phenomena, at least as well as Mr. Lavoisier's, I have adhered to that."[9] (What exactly Cavendish meant here will be explained below.)

2.2 Kuhnian revolution

In many ways, Thomas Kuhn's account of the Chemical Revolution is more informative than the basic empiricist line examined above. References to the Chemical Revolution are scattered throughout *The Structure of Scientific Revolutions* and elsewhere in Kuhn's work, but a convenient and insightful synthesis can be found in a paper by Paul Hoyningen-Huene (2008), on which one can rely at least for most purposes. Kuhn clearly recognizes the difficulty involved in trying to say which side was better in the Chemical Revolution. He notes the mismatch in the problem fields handled well by the competing sides, and emphasizes that there were different standards of judgment employed by them; these and other paradigm-based differences clearly constitute an instance of methodological incommensurability, although it is more debatable whether there was any significant semantic incommensurability involved. Kuhn in fact highlights Priestley's resistance to Lavoisier's new paradigm as a case illustrating the lack of super-paradigmatic criteria of rationality in science:

> Though the historian can always find men – Priestley, for instance – who were unreasonable to resist as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has *ipso facto* ceased to be a scientist. [Kuhn 1970, p. 159]

Ironically, these merits of Kuhn's account of the Chemical Revolution also constitute its greatest defect, for those who would seek philosophical explanations of scientific behavior. Kuhn says that Priestley's resistance was never irrational, and that it was only unreasonable because he was being stubborn even after the great majority of chemists had gone over to Lavoisier's side. But why did the majority of chemists change their minds, in the first place? It is well known that a prominent group of sociologists of science took Kuhn's ideas to their logical conclusion (or, I should say, *one* of their possible 'logical' conclusions), and declared that all scientific decisions should be explained by reference to social factors (*e.g.*, Barnes 1982).

If one (perhaps irrationally) wanted to resist that ascent to sociology, then it might seem that the only other way to go is to fall back to the strategy of finding something, *anything*, that is wrong with the losing side so that we can feel good about the majority going with the winning side. An intriguing example of this reactionary strategy can be seen in Howard Margolis' book *Paradigms and Barriers*. Margolis' 'habits of thought' is an interesting adaptation of Kuhn's ideas, especially referring to pragmatic roots of conceptual habits. Margolis's starting point is a puzzle: "the puzzle is to understand why men as able as Priestley and Cavendish, and indeed Lavoisier himself, found it so hard to give up the idea of phlogiston." (Margolis 1993, p. 43) Margolis wants an explanation as to why "even Lavoisier himself was slow to make arguments against phlogiston", and why "when he did give an argument that seems convincing today, no chemists followed." (p. 46) This way of thinking is premised on the idea that Lavoisier's theory was really so much better than the phlogiston theory, and Margolis secures that premise mostly by selective attention, for example conveniently not mentioning caloric in Lavoisier's explanation of combustion (*ibid*., p. 44). Margolis' answer to the puzzle is that there was a habit of thought, based on the intuitive idea that in combustion something (phlogiston) is emitted, that worked as a cognitive obstacle to progress: the transition from phlogiston to oxygen was "logically […] exhilarating", but "cognitively it was plainly painful for most chemists" (*ibid*., p. 49). This type of situation Margolis identifies as "a Kuhnian revolution: cognitively difficult though logically not so, hence best understood as turning on the presence of a barrier habit of mind." (*Ibid*., p. 50)

Margolis' analysis is not exactly faithful to Kuhnian ideas, nor is it intended to be. However, it does accentuate some fundamental difficulties in broadly Kuhnian explanations of revolutionary episodes. The Kuhnian framework naturally explains agreement in normal science and disagreement in extraordinary science. So, following Kuhn, we can easily explain why disputes between competing paradigms can persist, but we have difficulty explaining why and how those disputes do get resolved and end in agreement. This difficulty is not felt in Margolis' analysis, because he has no compunctions about assuming that all chemists should have seen the light and followed Lavoisier; then follows the semi-Kuhnian explanation, that they would have done so, except for their attachment to the 'phlogiston escaping' habit of thought. For those who follow Kuhn more faithfully, it is not easy to be so cavalier about the explanation of why those who 'converted' to Lavoisier did so. So we come back to the general complaint about Kuhn's treatment of revolutions: it seems to provide no convincing reasons as to why a scientist does or should go with one or the other of the competing paradigms.

2.3 Simplicity and unity

Those disappointed by the lack of explanations for revolutionary change have tried to get beyond Kuhn in various ways. For the moment, let me set aside the possibility that we really need 'social' explanatory factors, which I will come back to later. Kuhn (1977) himself moved on to considering certain basic epistemic values shared even by scientists in different paradigms. It is possible to argue that when a revolutionary struggle in science does reach a resolution in the triumph of one paradigm over another, that agreement is generated because the winning paradigm is superior to the losing one in terms of some of these super-paradigmatic values. Simplicity is one epistemic value that has been invoked time and again in attempts to explain the Chemical Revolution. On the surface, it is quite an appealing notion that Lavoisier's theory won because it was inherently simpler than the phlogiston theory. The crudest version of this idea says that the phlogiston theory unnecessarily complicated things by postulating the existence of an unobservable substance, phlogiston. But that is, again, to ignore the fact that Lavoisier had to postulate the existence of an equally unobservable substance, caloric.

Perhaps the most sophisticated of these simplicity-based arguments comes from Andrew Pyle (2000). The sophistication of Pyle's position is already evident in his handling of the weight-gain issue: "the weight-gain phenomenon posed a genuine difficulty [for the phlogistic chemists], and one which generated a number of very different responses. It could not, however, be described as a knock-down refutation." (*Ibid.*, p. 109) In agreement with Kitcher and Margolis, and with Alan Musgrave, whose ideas will be discussed shortly, Pyle (*ibid.,* p. 110) emphasizes that up to about 1783 Lavoisier's theory had little overall advantage. So it makes sense that few people converted up to that point, and that Lavoisier himself did not launch an aggressive campaign. All of this changed when Lavoisier arrived at the attractive new hypothesis about the composition of water, namely that it was a compound of hydrogen and oxygen, not an element as the phlogiston theorists (and everyone else) had assumed. Unfortunately, Pyle's explanation of why most chemists did convert to the oxygen theory quickly after 1783 is not satisfactory. One problem is that Pyle only picks out rational-looking parts of the story. But even if we allow his selection of events for the moment, his argument about their rationality is very thin.

Pyle notes, quite rightly, that the phlogiston theorists had to concede that while the metals lost phlogiston in the process of calcination, something else (such as water or fixed air) became combined with the metal to give it extra weight. But why invent and hold on to such complicated stories, when there was a simpler story that did the job? Pyle also makes much of the fact that mainstream phlogiston theory after 1783 was of a hybrid nature, that is, acknowledging a clear chemical role for oxygen (by whatever name), while maintaining the existence of phlogiston. And then, in the midst of this highly nuanced discussion, Pyle suddenly descends into a simple-minded point about simplicity:

> By 1800, the old phlogiston theory was dead, and the outstanding dispute was between Lavoisier's theory and a spectrum of compromise-theories. How might such a debate be settled? Here the factor of simplicity comes into play on the side of Lavoisier. His theory of combustion is objectively simpler than compromise theories in that it represents combustion in terms of 3 factors rather than 4. [Pyle 2000, p. 113]

I take it that the three factors that Pyle identifies in Lavoisier's theory are: the combustible, oxygen base, and caloric. On the phlogiston side, the factors involved must be all of those, plus phlogiston. I am not sure why Pyle thinks that phlogistonists necessarily needed caloric rather than using phlogiston to account for heat (there were diverging opinions on this point among phlogistonists), and why he is letting Lavoisier off the hook by ignoring the fact that he also postulated the existence of *lumière*, the substance of light, which was the very first item in his table of simple substances (see Figure 1 above). Depending on how one counts, the substance count could easily be four to three in favor of the phlogistonists. In any case, it does not seem right to choose the fundamental theory of chemistry on the basis of whether it postulates X or $X+1$ substances. We would first need a good story about why that kind of simplicity is so important.

Pyle (*ibid.*, p. 114) also reinforces a slightly different simplicity-based argument in favor of Lavoisier, which is more about the constancy and uniformity of opinion rather than simplicity as such. Originally this was an argument that Lavoisier himself made with much rhetorical effect: phlogiston was a 'veritable Proteus', which changed its form just as needed, and no two phlogistonists could agree about what it really was. At first glance it does seem terrible that phlogistonists could not even agree amongst themselves, while Lavoisier's school had a unified stance. But on more careful consideration this is not an argument that carries much weight. It has no force when we are trying to consider the rationality of *each* phlogistonist's position. (Should Scientology rationally convince Christians to give up Christianity because there are so many mutually conflicting variants of the latter?) We also need to recognize that the anti-phlogistic camp was not completely united, either. There was no great and lasting unity among those who accepted oxygen and caloric (for example, about whether light was a separate substance from caloric, or about whether caloric was made up of particles). There were also many other people who used neither caloric nor phlogiston, preferring their own ideas about the nature of 'elementary fire' and such. And there was considerable ontological discomfort and indecision in general about the imponderables, on which not even all Lavoisierians were in agreement. The Chemical Revolution was not a Manichean conflict between the Lavoisierians and the phlogistonists. When all these facts are taken into account, there is not much of substance left in the arguments based on simplicity or unity for the rationality of the Chemical Revolution.

2.4 Lakatos, Musgrave, and progress

In my view, the best available philosophical treatment of the Chemical Revolution is still Alan Musgrave's paper of 1976, which makes an application of

Imre Lakatos' methodology of scientific research programs to this case. Musgrave argues that after a certain point the phlogiston research program ceased to be progressive, while the oxygen research program continued to be progressive. Musgrave's explanation is framed explicitly in Lakatosian terms, so what he means by 'progress' is the production of successful novel predictions, and the rational thing for scientists to do is to choose the most progressive of available research programs. So the Chemical Revolution is seen as a perfectly rational affair, and thereby also vindicates Lakatos' philosophy of science.

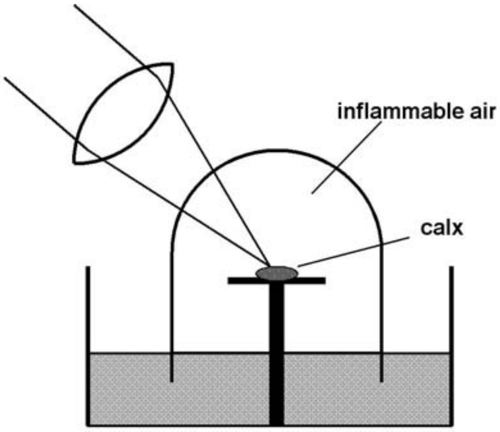

Figure 2. A schematic representation of Priestley's experiment demonstrating the reduction of lead calx by heating in inflammable air.

Musgrave (1976, p. 199) sets up the crucial moment of truth very nicely. The phlogiston program was highly progressive for a time, up to Joseph Priestley's prediction in 1783 that a metallic calx (or, oxide) would be reduced (turned back into shiny metal) through heating in inflammable air, which he considered to be phlogiston itself at that time. This prediction received a stunning corroboration in Priestley's experiment of heating minium (lead calx) in inflammable air by means of a large burning lens (see Figure 2). Priestley declared: "I could not doubt but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of *phlogiston*." The moment of high drama came when Lavoisier turned this apparent phlogistonist triumph upside down by exploiting Cavendish's new work on the production of water by the combustion of inflammable air (*ibid.*, p. 201). Characteristically, Lavoisier began his counter-offensive by noting that the lead calx in Priestley's experiment would have lost some weight in turning back into metal, as in other cases of reduction. Then he deduced that the lost weight would have gone into the water that must have been produced in the experiment, composed of oxygen coming from the calx, and the ambient inflammable air (which he then re-named 'hydrogen').

Ironically, Musgrave points out, it was Priestley himself who confirmed Lavoisier's prediction (or retrodiction) that water must be (must have been) produced in the experiment, by performing the experiment over mercury, instead of water as in the original setup. But as Lakatos might have predicted from the general nature of research programs, the phlogiston program was actually not conclusively defeated at this point. Priestley, instead of converting to Lavoisier's theory at this point as Kitcher and Margolis would see fit, switched to Cavendish's new version of the phlogiston theory, which hinged on the ingenious *post hoc* modification to the effect that inflammable air was not phlogiston but phlogisticated water, while oxygen (or, dephlogisticated air) was dephlogisticated water. However, Musgrave argues (*ibid.,* pp. 203-6), from this point on the phlogiston theory was forever on its back foot, adjusting itself this way and that way to accommodate inconvenient new findings but not managing to make any successful novel predictions. As he puts it (p. 203), "a degenerating programme can soldier on, and phlogistonism did just that", using Cavendish's new ideas. But at that point the Lakatosian verdict kicks in: it is irrational to hold on to a degenerating research program; it was rational for chemists to abandon the phlogiston program after 1783 or so, and most chemists were indeed rational in that way, leaving behind the 'elderly hold-outs' like Priestley and Cavendish.

Musgrave's argument certainly has some plausibility, but there is a problem: where are the successful novel predictions made by the oxygen program after the phlogiston program stopped making them? Musgrave (*ibid.,* p. 201) counts Lavoisier's deduction that water must have formed in Priestley's 1783 experiment as a novel prediction. But this is a difficult claim to sustain. Lavoisier's analysis was only made in retrospect, though it can be said that in the logical sense his theory 'predicted' the production of water, which Priestley had failed to observe in the original experiment. But, as Musgrave acknowledges, the same 'prediction' was also made by Cavendish's theory, and it was more likely the phlogistonist Cavendish, not Lavoisier, who occasioned Priestley to repeat the experiment over mercury. Lavoisier had not predicted the production of water in the experiment of exploding hydrogen and oxygen together, and his hypothesis about the composition of water was itself a *post hoc* adjustment made in order to explain the unexpected production of water in Cavendish's experiment (exploding a mixture of hydrogen and oxygen gases). According to Lavoisier's original position, the product of this reaction should have been an acid, since it contained oxygen; Lavoisier tried in 1777 and 1781-82 repeatedly to produce an acid by the combustion of inflammable air, without success and without detecting the water produced in it, either. Musgrave (*ibid.,* p. 199) tells us all of that, with perfect clarity. Lavoisier's account of the composition of water started its life not as a novel prediction, but as a classic *ad hoc* hypothesis (lacking use-novelty as well as temporal novelty).

Were there any successful novel predictions made by Lavoisier? Musgrave (*ibid.,* p. 203) gives us one: "water, traditionally used to put out fires, should, since it contains oxygen, support slow combustion and yield hydrogen. Iron filings immersed in water did indeed rust and hydrogen was collected." But, again, this was just as deducible from Cavendish's 1784 version of the phlogiston theory: if iron gave its phlogiston to water, that would produce phlogisticated water, which is hydrogen. The same can be said about Lavoisier's famous decomposition of water vapor by hot metal: the transfer of phlogiston from the metal to the water would cause the former to turn into a calx, and turn the latter into inflammable air (phlogisticated water). So these novel predictions do not quite qualify as crucial experiments, and I cannot see any other significant candidates for successful post-1783 novel predictions made by the Lavoisierian research program.

Meanwhile, there were some distinctly un-progressive aspects of the oxygen research program in the 1780s and beyond, including some embarrassingly unsuccessful predictions, and some unexpected new phenomena which Lavoisier and his followers could only accommodate *without* the desired by-products of successful novel predictions. As mentioned earlier, on the basis of his oxygen theory of acids Lavoisier confidently predicted that muriatic acid (our hydrochloric acid) would be decomposed into oxygen and the 'muriatic radical'. Lavoisierian responses to similar anomalies of prussic acid (HCN, in modern terms) and sulphuretted hydrogen (H_2S) also had no progressive outcomes. In neutralizing Berthollet's challenge about the combustion of gunpowder, Lavoisier only managed *ad hoc* hypotheses (in the Lakatosian sense of not resulting in successful novel predictions).[10] And Lavoisierians made pretty un-progressive responses to the discovery that not only oxygen but also chlorine gas supported combustion, but no other known gases did. So, if we stick to Lakatos' criterion of progressiveness, I think the verdict between phlogiston and oxygen is actually quite ambiguous. In the end, Musgrave does not even give us a convincing 'rational reconstruction' of the post-1783 phase of the Revolution.

3. How we have got the explanandum wrong

The discussion in the preceding section can be summarized with succinct pessimism: I am not aware of any philosophical account that is sufficiently successful in explaining why the vast majority of European chemists signed up to Lavoisier's theory, and I do not think it is likely that there will be a much better account forthcoming. Faced with the kind of philosophical failure, there are a few possible reactions. First, we could just keep trying out new philosophical explanations; this would require a degree of optimism verging on the desperate. Second, we could give up on philosophical explanations altogether, and try for social explanations. This is a tempting option, but it does not work out all that well for the Chemical Revolution,[11] and I also have some general objections to the flight to the social, which I will explain in Section 4. I would like to suggest a third option, which is based on the suspicion that perhaps we are not finding any good explanations because we are trying to explain something that did not actually happen. (Imagine all the fun we could have trying to explain, say, why Germany won the First World War despite the entry of the U.S. into the war.)

If my suspicion is corroborated by independent historiographical work, then we will have made productive use of a philosophical failure to improve historiography, as promised. The history-philosophy interaction in this process will be the subject of Section 5, but here let me just outline how it works out in the case of the Chemical Revolution. My historical thesis, which I will attempt to demonstrate in this section, is that the Chemical Revolution did *not* consist in a swift and near-complete conversion of the chemical community to Lavoisier's theory. Here we need to resist being taken in by triumphalist declarations of a clean victory originating from Lavoisier himself, his contemporary advocates, and some posthumous glorifiers of Lavoisier.[12] The assumptions of a clean victory can be found in some quite unexpected places, too. For example, this is what Priestley himself said, in the opening sentence of his latter-day defense of the phlogiston theory issued from his exile in America in 1796:

> There have been few, if any, revolutions in science so great, so sudden, and so general, as the prevalence of what is now usually termed *the new system of chemistry*, or that of the *Antiphlogistians*, over the doctrine of Stahl, which was at one time thought to have been the greatest discovery that had ever been made in the science. [Priestley 1796]

Maybe this was an exaggerated complaint from the loser, but strangely, the same idea can also be found in the works of some very careful historians. For example, Robert Siegfried says:

> Of all the well known revolutions in the history of science, the chemical is perhaps the most dramatic [...]. Only twenty years separate Lavoisier's first

> explorations of the chemistry of gases and the public capitulation of Richard Kirwan, the last significant European defender of the phlogistic views. [Siegfried 1989, p. 31]

The impression of suddenness is shared by Carleton Perrin:

> Few of the major conceptual shifts in the history of science rival the chemical revolution for compactness in time and consequent sense of drama. As usually defined, the episode spanned a mere twenty years. [Perrin 1981, p. 40]

The impression of unanimity is voiced by Larry Holmes (2000, 751): "all but Priestley himself eventually came over to the side of the French chemists".

Now, it may well be that scientific revolutions usually take much longer than 20 years, so the Chemical Revolution was a quick one in relative terms, but a reasonably close look at the primary literature should make it evident that there were numerous chemists who decided not to jump on the Lavoisier bandwagon even by 1790 and beyond, whom I will call 'anti-anti-phlogistonists'. And this has indeed been noted in various historical accounts, although to the hapless philosopher looking for some historical work to draw from, these scattered sources will not be easily visible. So, if nothing else, the service I want to render here is to make a convenient and useful summary of facts that are well-known to some experts here and there. Many of the anti-anti-phlogistonists were respectable and respected men of science, not just old men driven by sheer conservatism or dogmatism. There were at least three different types of these dissenters in the period *after* the publication of Lavoisier's *Elements of Chemistry* in 1789, which is usually seen as the point at which the Chemical Revolution was more or less complete, or at least irreversible (see Table 1).

First of all, there were indeed some die-hards. Priestley tops this list, but he is only a small part of the picture. One of the most striking figures is Jean-André De Luc, whose objection was based on his theory of rain, which postulated the transmutation of atmospheric air into water.[13] De Luc also maintained close connections with various anti-Lavoisier figures in Germany, particularly Göttingen, and also with Priestley's associates in the Lunar Society of Birmingham, including James Watt. In 1796 Priestley identified the latter group as the only remaining adherents to phlogiston that he knew of, in addition to Adair Crawford, who had just died (Priestley [1796] 1969, p. 20). On the German side, Karl Hufbauer (1982, pp. 140-4) notes that most chemists there either converted to the Lavoisierian side or at least gave up any active resistance by 1796, but allows that there were some remaining phlogistonists, including Johann Christian Wiegleb and Johann Friedrich Westrumb, who were 'virtually ostracized'. And then there were people like Torbern Bergman in Sweden and James Hutton in Scotland, whose concerns were mineralogical and geological above all else. Hutton, for example, had a notion of the circulation of phlogiston in the environment which smacks of modern ecology's understanding of the cycles of carbon and energy, according to Douglas Allchin (1994). Scheele did not survive long enough to prove his 'die-hard' credentials, but up to his death in 1786 he showed no sign of relinquishing the phlogiston theory. Even right there in Paris there remained significant anti-Lavoisierian figures, including Jean-Claude Delamétherie, the editor of the prestigious *Journal de physique* (called *Observations sur la physique* before 1794), who followed Priestley's ideas and cultivated a connection with De Luc.[14] There was also Jean-Baptiste Lamarck, whose idiosyncratic chemical ideas are understood by Leslie Burlingame (1981) as belonging to the natural-historical tradition of French science. To the list of French die-hards, Perrin (1981, p. 62) also adds Antoine Baumé and Balthazar-Georges Sage.

Table 1. Varieties of anti-anti-phlogistians, in the order of birth in each category

Die-hards ('elderly hold-out', some not so elderly)	Fence-sitters	New anti-Lavoisierians
James Hutton (1726-1797)	Pierre-Joseph Macquer (1718-1784)	Count Rumford (1753-1814)
Jean-André De Luc (1727-1817)	Henry Cavendish (1731-1810)	George Smith Gibbes (1771-1851)
Johann Christian Wiegleb (1732-1800)	Georg-Christoph Lichtenberg (1742-1799)	Thomas Thomson (1773-1852)
Joseph Priestley (1733-1804)	Lorenz Crell (1745-1816)	Johann Wilhelm Ritter (1776-1810)
Torbern Bergman (1735-1784)	Claude-Louis Berthollet (1748-1822)	Humphry Davy (1778-1829)
James Watt (1736-1819)	Johan Gadolin (1760-1852)	
Carl Wilhelm Scheele (1742-1786)	Friedrich Gren (1760-1798)	
Jean-Claude Delamétherie (1743-1817)	Jeremias Richter (1762-1801)	
Jean-Baptiste Lamarck (1744-1829)		
Adair Crawford (1748-1795)		
Johann Friedrich Westrumb (1751-1819)		

The second category of dissenters sought compromise, or deliberate neutrality. Allchin (1992), in his aptly titled paper 'Phlogiston After Oxygen', makes a persuasive case that many chemists admitted the existence of oxygen for gravimetric considerations, while keeping phlogiston for what we would call energy considerations. J.R. Partington and Douglas McKie, in their series of papers on the phlogiston theory (1937-39, pp. 125-7, 143-8), already pointed to a large number of people in this category, many of them German or German-speaking, including Friedrich Gren, Lorenz Crell, Jeremias Richter and Johan Gadolin. Hufbauer's study (1982) of the German chemical community in the 18th century has elaborated further on that point. More generally, people often accepted Lavoisier's theory only partially, picking and choosing what made sense to them. The old phlogistonist P.J. Macquer was taking this kind of approach when he died in 1784, and even Lavoisier's close colleague and ally Claude-Louis Berthollet remained skeptical about some of Lavoisier's ideas, especially his theory of acids.[15] There were many others who clearly saw some merit in Lavoisier's chemistry but did not consider the evidence sufficient to reach a clear verdict in favor of it. As discussed above, Cavendish (1784, pp. 150-3) gave a clear-headed view of how both theories could explain the phenomena he observed, while expressing a preference for staying with phlogiston. Alfred Nordmann (1986) explains how Georg Christoph Lichtenberg made a strong case that there was not enough knowledge yet for a decisive verdict, and how annoyed he was by the Lavoisier group's attempt to legislate the language of chemistry, by which act they forced other people to make a premature choice.

Even more interesting is the third category of dissidents, who fully acknowledged that Lavoisier's system had become established but also sensed that its time was passing quickly. Very suggestive in this connection is the following snippet of scientific conversation that I happened to stumble upon recently, from the year 1800. William Herschel had just detected infrared radiation coming from the sun, which he saw as caloric rays separated from light rays by means of the prism. Joseph Banks wrote to congratulate Herschel on this momentous discovery, but had one piece of advice:

> I think all my friends are of the opinion that the French system of Chemistry, on which the names lately adopted by their Chemists are founded, already totters on its base and is likely soon to be subverted. I venture therefore to suggest to you whether it will not be better for you […] to use the term Radiant Heat instead of Caloric; by the use of which latter word it should seem as if you had adopted a system of Chemistry which you have probably never examined. [Banks to Herschel, 24 March 1800, quoted in Lubbock 1933, 266-7]

Herschel accepted Banks's advice happily: "I have the honour of your letter and shall be very ready to change the word caloric for radiant heat, which expresses my meaning extremely well."[16] Banks was a botanist and not a well-known chemist, but if the longtime President of the Royal Society and 'all his friends' were predicting the imminent demise of the French chemistry in 1800, then there must be something that we have missed out in our usual historiography.

What did Banks have in mind when he said that the French chemistry was "tottering on its base"? It is impossible to say for sure, but there are some clear things he might have had in mind. In Section 2.1 above I have explained the empirical difficulties with Lavoisier's theory of acidity and with his theory of combustion. Thomas Thomson (1802, p. 358), whom I already cited there, concluded: "upon the whole, it cannot be denied that Lavoisier's theory does not afford a sufficient explanation of combustion." Thomson was not advocating a return to phlogiston, but he wanted chemistry to move on beyond Lavoisier. There was also growing discontent with Lavoisier's caloric theory of heat in general – particularly in London, where around 1800 there was a remarkable concentration of advocates of the notion that heat was a form of motion, including Count Rumford, Humphry Davy, Thomas Young, and Henry Cavendish.

The year 1800 is also significant because it saw the invention of the battery (or the 'pile') by Alessandro Volta. The news reached England in the form of a long letter from Volta to Banks, who had it printed in the *Philosophical Transactions of the Royal Society*. While waiting for its publication Banks showed Volta's letter to his friend Anthony Carlisle, a London-based physician. Carlisle repeated Volta's experiments with the help of the scientific publisher William Nicholson, and they also used Volta's pile to effect the first decomposition of water using an electric current. This result was reported in Nicholson's own *Journal of Natural Philosophy, Chemistry and the Arts*, and caused quite a sensation. Now, the electrolysis of water into hydrogen and oxygen would seem like great news for the Lavoisierians: what could be a clearer proof of Lavoisier's hypothesis about the composition of water, now obtained without any complications involving the calcination of metals and such things? Nicholson and Carlisle would have agreed, but they added a puzzled note:

> We had been led [...] to expect a decomposition of the water; but it was with no little surprise that we found the hydrogen extricated at the contact with one wire, while the oxygen fixed itself in combination with the other wire at the distance of almost two inches. This new fact still remains to be explained, and seems to point at some general law of the agency of electricity in chemical operations. [Nicholson 1800, p. 183]

This problem was noted by many others, and in the hands of young Johann Wilhelm Ritter in Germany it became a great weapon against Lavoisierian chemistry. Ritter carried out various experiments in support of his idea that electrolysis was not decomposition at all, but a pair of synthetic reactions: negative electricity comes in at one end and combines with water, and the product of that combination is hydrogen; likewise, positive electricity combines with water at the other end, and makes oxygen. According to Ritter, water was an element after all, and hydrogen and oxygen were water-based compounds. If you think of negative electricity as phlogiston, Ritter's view on water maps very neatly onto Cavendish's earlier view that hydrogen was phlogisticated water and oxygen was dephlogisticated water. And there had indeed been many chemists who suspected a deep connection between phlogiston and electricity before this, as W.M. Sudduth (1978) records in a sadly neglected paper.[17] Ritter was the darling of the German Romanticists, and it seems that his view had some advocates abroad, too. Again, we can see Lavoisier's system tottering on its base, as a consequence of new developments in which Banks and his friends had a hand.[18]

Perhaps the most interesting case of the new generation of anti-Lavoisier chemists was Humphry Davy, who was still a boy of about 10 years when Lavoisier's *Elements of Chemistry* was published. Davy later objected to almost every major aspect of Lavoisier's chemistry. He made his name in electrochemistry, and also by putting a nail in the coffin of Lavoisier's theory of acids with his argument that chlorine was an element and muriatic acid did not contain oxygen, only hydrogen and chlorine.[19] After the acceptance of Davy's work, Lavoisier's oxygen theory of acidity was clearly dead, never to be revived again. As mentioned above, Davy was also one of those who mounted serious challenges to the Lavoisierian caloric theory of heat, whose dominance was never total.[20] As Siegfried (1964) reports in some detail, Davy actually entertained various systems of chemistry involving the revival of phlogiston. David Knight remarks (1978, p. 4): "there were widespread hopes and fears until at least 1810 that Davy would restore it [the phlogiston theory] and overthrow the French doctrines." Among those who expressed such hope in print was George Smith Gibbes, doctor and chemical lecturer in Bath, later to be physician to Queen Charlotte; in 1809 Gibbes opined that Davy's discoveries had confirmed that Lavoisier was wrong after all.[21]

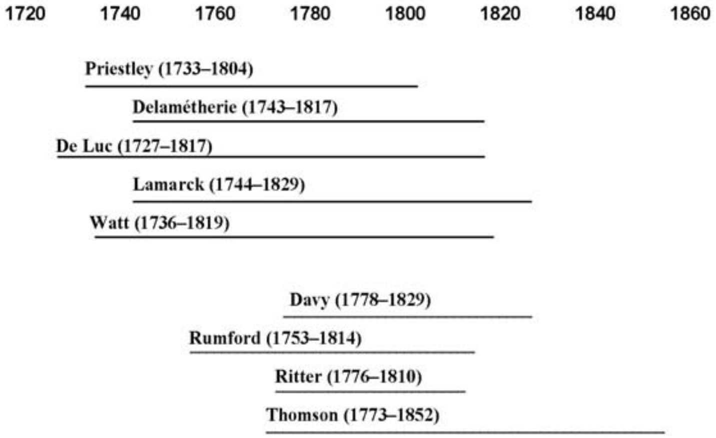

Figure 3. The overlap between old (top) and new anti-anti-phlogistonists (bottom).

What can we say after all of that, about what the Chemical Revolution really consisted in? We still have to admit that a considerable number of chemists became fully 'converted' to Lavoisier's chemistry at least for a time, and that it achieved a clear dominance in the textbooks. However, we also need to acknowledge that there were common cases of partial or half-hearted converts, and many of those retained phlogiston in their systems. Add to that not only the die-hard phlogistonists, but also the younger generation of dissidents who actually had their scientific education after Lavoisier's victory. A very interesting thing about these two generations is that they in fact overlapped significantly in time, the new generation coming up before all the die-hards had given up (see Figure 3). Knight (1978, p. 29) actually understates the case when he says, in reference to a later episode: "As had happened with gothic architecture, this phlogiston survival was almost contemporaneous with the phlogiston revival".[22] There are many senses in which there was a 'revolution' in chemistry effected by Lavoisier and his colleagues, but it was not a sudden and clear-cut affair. It was a many-sided struggle that neither ended in unanimous agreement nor established a long-lasting orthodoxy.[23]

4. How do we explain what did happen?

With the new description of the Chemical Revolution sketched in the preceding section, we can find a good philosophical explanation of why it happened. The full story is too complex to fit into this paper, as the Chemical Revolution was a complex and multi-faceted event.[24] But one key point is simple: since Lavoisier had some excellent arguments but lacked knock-down punches, it makes perfect sense that some people shifted their allegiance to him, and others did not. To borrow Kuhn's phrase (1970, p. 94), since the dispute could not be "unequivocally settled by logic and experiment alone", it makes sense that there would have been continuing attachment to phlogiston in various quarters. Having noted that there were many who were not sold on Lavoisier's chemistry, we no longer have to agonize about explaining why the vast majority of chemists converted to Lavoisier's chemistry. Many did not, and that is quite easy to explain as a rational epistemic response to the situation as it was. At the surface level, that is all we need to say by way of a philosophical explanation of the Chemical Revolution.

What is somewhat more challenging to explain is why those people who went over to Lavoisier's side did so. So the original difficulty I started with comes back to us in a revised form: what we are seeking now is not an explanation of unanimity, but reasons that impressed particular individuals and particular groups. Large-scale social and political factors do not help the explanation in this case, as the acceptance or rejection of Lavoisier's theory easily cut across lines of nationality, age, political ideology, economic and social class, or profession. Instead, we would need to consider case-by-case the interaction of various background factors that would have influenced different individuals differently. Understanding individual cases will require in-depth studies of the individuals concerned, and that is beyond the scope of this paper. But there are some general factors that would have affected a good number of people.

I do not share the methodological commitment shown by some social and cultural historians, that all explanations in the history of science must be social. I am more inclined to seek any and all explanatory factors that will, together, deliver a good explanation. It is possible that the only correct explanations of the Chemical Revolution are social or 'external' ones, but we cannot be certain of that until we have also considered the scientific, philosophical, or 'internal' explanations and assessed their relative importance. In the spirit of considering all potentially relevant factors, let me highlight two of them, while I do not pretend to be comprehensive.[25]

First, Lavoisier and his colleagues did run an effective and well-coordinated campaign for their new chemistry, including the spreading of their new nomenclature and the controlling of institutional spaces such as the Paris *Académie* and the new journal *Annales de chimie*. They also co-opted many of phlogistonist successes and reforms, creating an exaggerated sense of revolution where there was in fact a good deal of continuity; J.B. Gough (1988, p. 15) argues that "Lavoisier owed a great deal more to his French Stahlian predecessors than he was willing to admit publicly", and John McEvoy (1988; 2010) has made a thorough assessment showing that Lavoisier's chemistry was not such a simple, abrupt departure from the chemistry that preceded it. Mi Gyung Kim's (2003, p. 390) observation is very apt: "the most enduring elements of the Revolution, such as the analytic definition of elements and the nomenclature reform, were not his."[26] Lavoisier also had the ability for a very clear and systematic exposition of ideas, which leading phlogistonists such as Kirwan and Priestley lacked. But it seems to me that these factors are not quite sufficient for explaining many of the conversions; they are all devices that could have (and were) resisted by those who had other reasons to oppose Lavoisier's theory. For instance, the new nomenclature only reinforced the habits of the already converted and indoctrinated the younger generation who had to grow up learning chemistry in its terms. As mentioned above, mature chemists who were not inclined to adopt Lavoisier's system were only irritated by the new nomenclature; they saw right through it, as an attempt by the Lavoisier gang to impose their theory on everyone before arguments had been considered carefully enough.

Secondly, the rejection of phlogiston makes much more sense when we see it as a ripple riding on a large wave, which was the very gradual establishment of the building-block ontology of chemical composition. This point has been considered important only by a small number of historians and almost no philosophers.[27] The phlogiston theory was grounded in the old chemical notion of 'principles', that is to say, basic substances which actively modified other substances and imparted certain characteristic properties to them – for instance, phlogiston was a principle which imparted combustibility or metallic properties to substances it combined with. This principlist[28] thinking did not fit well with the building-block ontology, in which all pieces of matter had equal ontological status (and the conservation of weight before and after a reaction was a major concern). It is not that the building-block ontology was entirely absent from the phlogiston theories. It was present, but in a very uncomfortable mix with principlist ontology. So we can actually imagine the metaphysical relief in being able to do chemistry entirely on the building-block basis. In fact Gough goes as far as to argue that "Lavoisier did not initiative a revolution in chemistry: rather, he seized hold of a revolution already in progress – a revolution that concerned the composition of the chemical molecule – and tacked his own colors on to it"; that revolution already in progress had been launched by the French Stahlians, within whose compositionist system the phlogiston theory created internal contradictions (Gough 1988, pp. 15, 29). Phlogiston was washed out in the tide of the weight-focused chemistry of the grouping and re-grouping of stable component units.

It is important for historians of this period to see beyond the clash between phlogiston and oxygen. If we want to conceive of the Chemical Revolution as the event that gave rise to 'modern chemistry', we must follow Siegfried and Betty Jo Dobbs (1968) in concluding that the endpoint of the Chemical Revolution was not Lavoisier, but Dalton. What we are talking about here is not the actual John Dalton immersed in the physics of caloric, but Dalton as sanitized by later atomists, simply focused on immutable atoms as chemical units with definite weights which worked as simple building blocks of the chemical universe. In this ontological revolution, which I characterize as 'compositionist', Lavoisier was actually not a thoroughly modern figure since he was still partly steeped in principlist thinking, as various historians have pointed out. William H. Brock (1992, pp. 112-3), for example, notes the irony that Lavoisier's oxygen theory of acidity was a direct descendant of none other than Stahl's idea that vitriolic acid was the 'universal acid', or the principle of acidity. One could also argue that Lavoisier's caloric was another principle, which imparted the property of fluidity and elasticity to matter. Perrin (1973, pp. 97-101) takes this point further, and argues quite persuasively that the first five simple substances in Lavoisier's table were all principles (*lumière*, *calorique*, *oxygène*, *azote* (nitrogen) and *hydrogène*; see Figure 1 above). Lavoisier's willingness to allow imponderable substances in his system was also not fully harmonious with his own emphasis on weight as the most important chemical property to keep track of.

The 'oxygen theory' as crafted by Lavoisier was a rather fragile thing, whose impressive dominance cannot be explained without reference to scientific fashions in the end. However, it is also important not to be carried away with this observation. If the demise of phlogiston had been simply due to the Lavoisier fad, then phlogiston would have returned after the Revolutionary execution of Lavoisier in 1794 and the slow dissolution of the well-disciplined band of French scientists around him. But phlogiston never did return in great force, and most of the new anti-anti-phlogistonists discussed above were not phlogistonists. It may have been Lavoisier and his friends who killed phlogiston, but there was a greater force at work which *kept it dead*. Post-Lavoisierian chemistry was resolutely compositionist, and that is what prevented the return of phlogiston even after the dismantling of some of Lavoisier's fundamental ideas.

5. The interplay between history and philosophy

I would like to close with some reflections on how history and philosophy interact with each other in the kind of work I have showcased in this paper. The interactive process can be schematized as follows. We start with facts about the past as given by existing historiography. In trying to explain those facts philosophically, we may fail. We may use that failure as a stick to beat philosophers with, but we may also use it as an occasion to re-examine the history. It is easily possible that we would decide that the historiography we started out with was defective and needs to be upgraded. (If so, we can come back to the philosophical task and see if we can provide a good explanation of the updated history.) This is what I meant about how philosophical failure can generate historiographical refinement. I think this is one important mode of productive interaction between history of science and philosophy of science.

It is my belief that there are also many more modes of history-philosophy interaction waiting to be articulated more clearly and practiced more widely. For example, here is a model of how we can use a failure of understanding at the history-philosophy juncture in order to improve philosophy (rather than history), which has been very important in my own work so far.[29] We start with existing philosophical frameworks, and find historiographical puzzles, namely episodes that are difficult to describe, and understand. In attempts to find an apposite description of these episodes, philosophers can generate new concepts and ways of thinking that they may not be led to otherwise. This is not so different from the Lakatosian use of history as an evidence-base for philosophy of science, which is taken to provide historiographical research programs (Lakatos 1976).

Now, returning to the mode of the history-philosophy interaction that I am focusing on in this paper, historians may object that philosophical failure is not necessary for the improvement of historiography, and that history can proceed and refine itself on its own. That is logically true, but in practice historians left to their own devices are not likely to have the particular type of focus that the concern with philosophical explanations generates, or reach the same kind of synthesis even when the same historical facts are discovered. For me it has been a very interesting experience to use my philosophical lines of inquiry to discover not only neglected historical facts in the primary sources, but also to unearth some sadly neglected secondary sources. This kind of heuristic function for the improvement of historiography is not restricted to philosophy; it can be performed by any field that provides an explanatory framework for historical events and trends – sociology, psychology, or economics, for instance.

Coming back to philosophy, we need to ask: what is philosophical understanding, and is it a kind of thing historians can appreciate and even participate in? I want to argue that philosophical understanding is based on intellectual empathy, which the historian of science also cannot do without. This is perhaps a broader sense of philosophical understanding than people normally have in mind, so let me expand on it a little bit.

The initial argument regarding the Chemical Revolution that I made was that we had not yet made good sense of the 18th-century chemists' decisions. Then I argued that the basic problem in that situation was a mistaken notion of what those decisions in fact were. But there is also a common error in the philosophical discourse, namely the assumption that making sense of past scientists' decisions must mean fitting them into our present conception of scientific rationality; Lakatos is emblematic here, but he is by no means alone. It is understandable that historians of science tend to have a violent reaction against this mistake, but I also think that historians have tended to throw the baby out with the bathwater in that reaction. It is not necessary, or advisable, to shun all philosophical understanding as a way of avoiding the imposition of particular modern conceptions of rationality.[30]

What the philosophical understanding of past science requires is a broader and less restrictive sense of intellectual empathy, a sense that we can see why past scientists would have had the thoughts and beliefs that they had. Such intellectual empathy is both an aim and a presumption in the business of philosophical understanding, which philosophers should be very familiar with from their work in the history of philosophy. And that is not so different from the kind of understanding that anthropologists try to reach about alien cultures, and historians try to reach about the past. A common mistake in recent historiography of science is to imagine that intellectual empathy can be reached by effacing our own selves entirely. Kuhn used to say that the task of the historian of science in studying a past scientist was 'to get into his head'. (That reminds me of the wonderfully quirky movie called *Being John Malkovich*, whose protagonists discover a rabbit-hole on floor 7 1/2 of a certain office building that lands them inside John Malkovich's mind; one enjoys the privilege of being Malkovich for a little while, and then gets dumped on the side of the New Jersey Turnpike; a return visit is irresistible.) Once we get into Joseph Priestley's head, how do we navigate our way in there? Ultimately, I have to make sense of Priestley's thinking in my own way. Unless we can actually be brought up in the community of past scientists (which would require another science fiction movie), we are going to have to bring in some conceptual framework from our own lives as we try to understand the past scientists. The ideal of only using actor's categories is an impossible aim, and it can become pernicious if it is used as a blunt weapon against any attempt to reach a kind of historical understanding that accommodates the historian's inevitable rootedness in the present.

The Chemical Revolution has been important both as an inspiration and as an illustration of the particular mode of history-philosophy interaction that I have described in this paper. It has been a particularly challenging case for philosophical explanation, and this kind of challenge represents a major reason why historians of science became disenchanted with the philosophy of science. But in the case of the Chemical Revolution the philosophers have had an *understandable* difficulty, faced with an impossible thing to explain; in this case the historians have been as much to blame as the philosophers for creating and maintaining misguided accounts of events.

In such situations philosophers would be right to send the case back to the historians as it were, or to engage in some do-it-yourself history if the historians are not amenable. As long as we do not insist in an overly narrow or particular kind of rationality, the demand for rationality and the search for empathetic understanding can be useful historiographical tools. The demand for understanding can be a most effective type of challenge to misguided or distorted descriptions, leading to corrections and supplements. Therefore, philosophical pigheadedness in the face of failure of understanding can serve as an effective method of historical discovery. In that mode of critical scholarship bringing history of science and philosophy of science together, all we are ultimately doing is to insist: "This doesn't make sense – we must go back and check if the story is correct."

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Notes

[1] What is contained in this paper is an elaboration of one part of the first chapter of my book in progress, *Is Water H_2O? Evidence, Realism and Pluralism* (forthcoming from Springer).

[2] For an informative discussion along similar lines, see Musgrave 1976, pp. 182-6.

[3] For more detailed accounts of the development and downfall of Kirwan's theory, see Mauskopf 2002 and Boantza 2008.

[4] For further details on that point, see Chang 2009, section 2.

[5] Kitcher also adds an apologetic footnote quoting cautious statements from Lavoisier showing that he had less than an absolute commitment to his theories of heat and acidity. But all that these statements show is that Lavoisier knew how to distinguish theories from facts; that does not make the ill-fitting facts any less disconfirming of the theory.

[6] Scheele reasoned that manganese required additional phlogiston to enable its reaction with acid; when there was no external source of phlogiston, the outer parts of manganese filings had to take the necessary phlogiston from the inner parts, rendering the latter unreactable.

[7] For further details on chlorine, phlogiston, and muriatic acid, see Chang and Jackson 2007, chapters 1 and 2.

[8] For a detailed discussion of the real and beneficial role played by caloric in the chemistry and physics of this time, see Morris 1972 and Chang 2003.

[9] After making this statement, Cavendish gave one marginal reason (other than conservatism) that inclined him to favor phlogiston, which had to do with the composition of plant materials.

[10] For details on the debate regarding the combustion of gunpowder, see Mauskopf 1988.

[11] A brief yet useful rebuttal of some standard 'external' explanations is given by Musgrave 1976, pp. 206-7; for a fuller account, see Chang forthcoming, chapter 1.

[12] For critical reviews of posthumous myth-making on Lavoisier, see Bensaude-Vincent 1983, 1996 and Kim 2005.

[13] Middleton 1965, pp. 115-131 gives a discussion of De Luc's theory of rain. See De Luc 1803, pp. 1-306, for his detailed objections to the new chemistry, first in itself and then in relation to meteorology.

[14] On Delamétherie's opposition to Lavoisier, see Guerlac 1975, pp. 105-6.

[15] On Macquer, see Holmes 2000, p. 752; on Berthollet, see Le Grand 1975.

[16] Herschel to Banks, 26 March 1800, quoted in Lubbock 1933, pp. 266-7.

[17] John Elliott in 1780 even proposed that phlogiston should be re-named as 'electron'. See Chang 2009, Section 3, for more on this phlogiston-electricity connection.

[18] See Wetzels 1990 on Ritter's life and work in general, and pp. 208-9 on his interpretation of the electrolysis of water. For a full discussion of the early history of the electrolysis of water, see Chang forthcoming, chapter 2.

[19] See Golinski 1992, chapter 7. On chlorine, see also Chang and Jackson 2007, chapter 2.

[20] By the time the energy concept and early thermodynamics toppled the caloric theory altogether in the 1840s and the 1850s, Lavoisier's basic picture of the universe was in tatters; later the kinetic theory would fill in the theoretical vacuum in regard to the explanation of the three states of matter.

[21] See Golinski 1992, p. 213, who calls Gibbes "perverse" for this.

[22] Knight here refers to Stevenson 1849 and Odling 1871; for a full discussion of William Odling's 'revival' of phlogiston, see Chang 2009, section 3.

[23] After outlining the strategies and factors that led to the "triumph of the antiphlogistians", Perrin 1981, pp. 61-63 ends by acknowledging the complexity of the field: "Throughout this essay I have spoken of two factions, one phlogistic and the other antiphlogistic. This, of course, is an oversimplification. […] There was a wide range of attitudes toward the claims of the new theory ranging from highly sympathetic to hostile."

[24] I make my best attempt at a full story in Chang forthcoming, chapter 1.

[25] McEvoy 2010, chapter 6 gives an informative survey of various contextual accounts of the Chemical Revolution.

[26] She continues: "The oxygen theory of acids and the caloric theory of heat, which could be regarded as his unique contributions, quickly fell into disrepute." (Kim 2003, p. 390)

[27] Siegfried (1982, 2002) has perhaps done more than anyone to initiate this angle on the Chemical Revolution, which is reinforced by Klein & Lefèvre's 2007 history of materials in 18th-century chemistry. Also see Klein 1994, 1996 on the origin of the concept of chemical compound, Multhauf 1966, 1996 on the industry-concept connections, and Holmes 1971 and Debus 1967 on the history of methods in analytical chemistry.

[28] Some secondary sources use the term 'principalist' in this connection, but I do not see the need to modify the spelling in that way, which will only invite a false association with the word 'principal'.

[29] For example, much of Chang 2004 consists of research in this vein.

[30] For a further discussion of whiggism and historical judgment, see Chang 2009.

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