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Scientific Pluralism and the Chemical Revolution

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Abstract: In a number of papers and in his recent book, Is Water H2O? Evidence, Realism, Pluralism (2012), Hasok Chang has argued that the correct interpretation of the Chemical Revolution provides a strong case for the view that progress in science is served by maintaining several incommensurable "systems of practice" in the same discipline, and concerning the same region of nature. This paper is a critical discussion of Chang's reading of the Chemical Revolution. It seeks to establish, first, that Chang's assessment of Lavoisier's and Priestley's work and character follows the phlogistonists' "actors' sociology"; second, that Chang simplifies late-eighteenth-century chemical debates by reducing them to an alleged conflict between two systems of practice; third, that Chang's evidence for a slow transition from phlogistonist theory to oxygen theory is not strong; and fourth, that he is wrong to assume that chemists at the time did not have overwhelming good reasons to favor Lavoisier's over the phlogistonists' views.

1. Introduction

This paper is a critical discussion of the recent work of Hasok Chang, especially his important 2012 book *Is Water H_2O? Evidence, Realism, Pluralism* (cf. Chang 2009, 2019, 2011). Chang's monograph is a fascinating, bold and thought-provoking plea for a *prescriptive* version of scientific pluralism. That is to say, Chang seeks to make a case for a science policy that supports a plurality of incommensurable scientific practices. *Is Water H_2O?* is also meant to exemplify "integrated history and philosophy of science". Accordingly, Chang analyses key historical stages on the long road towards the view that water is H_2O. The most important of these case studies concerns the Chemical Revolution. Chang writes that "I became a pluralist ... because I could not honestly convince myself that the phlogiston theory was simply wrong ..." (2012, p. 253)

I shall focus my critical examination of Chang's pluralism on his reading of the Chemical Revolution. My argument will unfold in four steps. The first step concerns Chang's efforts to correct the allegedly highly uncharitable treatment of phlogistonists at the hand of previous generations of historians of chemistry. I argue that Chang's attempt to rehabilitate the phlogistonists ends up adopting the latters' propaganda against Lavoisier. Chang insinuates a moral and intellectual superiority of phlogistonists over Lavoisians that is questionable. Second, Chang misconstrues late-eighteenth-century chemical debates by reducing them to an alleged conflict between two "systems": "the phlogistonist system" and "the oxygenist system". Phlogistonist ideas, theories and practices never formed a "system" by Chang's own definition of a "system of practice". This fact undermines both Chang's comparisons between "Lavoisier's system and the best versions of the phlogistonist system" (2012, p. 28), and his claims regarding what might have resulted if only the phlogistonist system had been kept alive longer. Third, Chang's argument for a slow transition from the phlogistonist system to the oxygen system does not support his pluralism. Most importantly, while Chang pays a lot of attention to late criticisms of aspects of Lavoisier's programme, he ignores the reasons "converts" gave for shifting their allegiance to the oxygen theory. Fourth, I shall challenge Chang's contention that chemists in late eighteenth century Europe did not have conclusive reasons to favor Lavoisier's over the phlogistonists' views. I shall argue that this claim has some plausibility only on too narrow an understanding of reasons. If we construe the relevant reasons more broadly – such that they include reasons to trust – then the case for adopting central pillars of Lavoisier's program was strong. The fourth criticism is the most important: it is here that work on testimony and trust in science, as well as the "Experimenters' Regress", will prove crucial.

The predominantly critical tone of this paper will perhaps give some readers the impression that I find little of value in Chang's work in general and his studies on pluralism and the Chemical Revolution in particular. This would be a total misunderstanding. I consider Chang's aspirations to integrate the history and philosophy of science to be one of the most exciting projects in Science Studies today; I support his calls for detailed attention to scientific practice; I respect his project of developing a plausible form of scientific pluralism; and I applaud many of his specific critical analyses of influential positions in contemporary philosophy of science. Indeed, it is probably precisely because – on a general level – Chang's and my own views are fairly close, that I react (all too) passionately to the few specific points on which we disagree. Disputes within the family can sometimes be more heated than disputes with strangers.

2. Chang on Scientific Pluralism

Chang calls his position "active normative epistemic pluralism" (2012, p. 268) and characterizes it as "... the doctrine advocating the cultivation of multiple systems of practice in any given field of science" (2012, p. 260). Chang offers three general motivations for pluralism. The first focuses on humility and prudence: since the world is inexhaustibly complex, we are better off with multiple approaches (2012, p. 255). The second motivation centres on a social-political idea. Pluralism is central to liberal democracy. Liberal democracy is the best form of political organisation. Science is a polity of sorts. Ergo, pluralism should be central to science (2012, p. 264). And the third line of thought highlights the (alleged) failure of reductionism: there is no end to the sequence of ever more basic units; and wholes are sometimes simpler than their parts (2012, p. 257).

The benefits of pluralism are of two kinds: there are “benefits of toleration” and “benefits of interaction” (2012, pp. 279-284). The former are “hedging our bets” (it is prudent to have multiple lines of inquiry); “division of domain” (it is wise to use different theoretical tools in the same domain; cf. the ways we use both classical mechanics and quantum mechanics); “satisfaction of different aims” (no one scientific system can satisfy all needs and values); and “multiple satisfaction” (“epistemic abundance should delight us”). Benefits of interaction are “integration” (as envisaged for instance by Otto Neurath); “co-optation” (think of Lavoisier using some of Priestley’s experimental results); and “competition” (preferably conducted in front of a wider audience).

Chang’s main analytic tool is his concept of a “system of practice”. A system of practice is a “coherent set of epistemic principles”. (2012, p. 16). The concept of a system of practice is central to Chang’s ultimate statement of his position: “Each system of practice is conducive to revealing particular aspects of reality, and by cultivating multiple incommensurable systems we stand to gain most knowledge.” (2012, p. 218)

3. The Chemical Revolution – A Primer

In this section I shall give a brief overview of the central technical content of the Chemical Revolution.[1] The study of gases – or “airs” – was a neglected topic in seventeenth-century

[1] The papers and book upon which my narrative is based are: Beretta 1993, Boantza 2013, Boantza and Gal 2011, Conant 1950, Crosland 1995, Donovan 1993, Eshet 2001, Frercks 2008, Golinski 1992

chemistry. The situation began to improve in the eighteenth century. For instance, Stephen Hales (1677-1761) studied the volumes of gases released when different materials were heated, and Joseph Black (1728-1799) in 1756 described the chemical effects of so-called "fixed air" (i.e. CO_2). Even more important was Henry Cavendish's (1731-1810) work. In 1766 he isolated what we call "hydrogen", or – as he dubbed it – "inflammable air", and equated it with so-called "phlogiston". (Siegfried 2002, pp. 153-157.)

The term "phlogiston" had been coined earlier by the German chemist Georg Ernst Stahl (1659-1734). The term comes from the Greek word for combustible, "φλογιστός". Phlogiston was introduced to make sense combustion and "calcination" (e.g. rusting); these were thought of as processes in which a substance loses its phlogiston content. For metals and their "calxes" the convers process was also taken to be possible: this process was called the "revivification" of the metal.

Joseph Priestley (1734-1804) enters the story because of his interpretation of the following phenomenon: when a body is burnt in a closed vessel, the burning will often stop *before* the body is fully burnt. Priestley offered the following explanation. When a body burns in a closed vessel, phlogiston is released from the body and absorbed by the ambient air in the vessel. This turns the ambient air into "phlogisticated air". When the air is saturated by phlogiston, the burning stops. This observation and interpretation allowed Priestley to develop his "nitrous air test" for air quality (Boantza 2013). Priestley also distinguished the behaviour of different "airs" on the basis of their alleged different degrees of phlogistication.

Cavendish's and Priestley's work on airs soon met with a lot of interest in France. And the young Antoine-Laurent Lavoisier (1743-1794) entered the race to become *the* French expert in pneumatic chemistry. Early in 1772 Lavoisier learnt from Guyton de

& 1995, Guerlac 1976, Holmes 2000a&b, Hufbauer 1982, Kim 2003,2008 & 2011, Kuhn 1962, McCann 1978, McEvoy 1978, 2010, Mauskopf 2002, Melhado 1985, Miller 2004, Musgrave 1976, Norton Wise 1993, Perrin 1988a&b, Roberts 1995, Schaffer 1986, Schofield 2004, Siegfried 1964, 2002, Simon 2005, Ströker 1982, Toulmin 1957.

Morveau (1734-1804) that metals gain weight when calcined – that is, that *metals gain weight while releasing phlogiston*. Lavoisier quickly set to work on an experimental program to capitalize on this observation. With the help of a burning glass he heated phosphorus or sulfur in an inverted glass over water. The result was that the phosphorus or sulfur increased in weight, and that the volume of air inside the inverted glass was reduced. In three famous sealed notes deposited with the French Academy, Lavoisier put forward the following hypotheses. The first note claimed that the phosphorus absorbs air. The second note added that it does as so as it releases phlogiston. And the third note suggested that the whole process can be framed solely in terms of air, and that no reference to phlogiston is needed to explain the experimental outcome (Musgrave 1976, p. 191, Siegfried 2002, pp. 161-167).

Put differently, at first Lavoisier tried to solve the weight problem without giving up the phlogiston theory. He hypothesized that the calx contains a "matter of air", and that, when the calx is heated with charcoal, the "matter of air" is released from the calx, and combines with the phlogiston to form a gas. This makes sense of the resulting metal weighing less than the calx. In slightly later work Lavoisier offered a new rendering. First, he now assumed that atmospheric air has two components: "pure air" (a gas supporting combustion) and "mephitic air" (a gas not supporting combustion). Second, he maintained that the calx contains pure air. And third, in reduction over charcoal, the pure air is released from the calx – hence the weight loss.

Priestley took the next important step in August 1774. He heated the red calx of mercury in an inverted glass over water. This resulted in the production of a gas that was easy to breath, and – as the nitrous air test showed – was four to five times better than normal air. Priestley reasoned that the calx had absorbed phlogiston from the air inside the glass, leaving behind this new gas. He called it "dephlogisticated air". The Swedish pharmacist Carl Wilhelm Scheele (1742–1786) had isolated the same gas already in 1772, though he did not publish his results until 1777. Lavoisier knew about Scheele's and

Priestley's experiments, did them again, saw his prediction concerning "pure air" confirmed, and claimed priority (Musgrave 1976, p. 191, Siegfried 2002, p. 161).

It is important to be clear about the theoretical differences between Lavoisier and Priestley at this stage. We have already seen how Lavoisier theorised the revivification of a metal when charcoal is used as the source of heat. In revivification without charcoal, that is, in revivification with the burning glass, the pure air is released from the calx – hence the weight loss of the calx. And thus pure air – what Priestley calls "dephlogisticated air" – is found.

Priestley's theory differed in both cases. Consider first the reduction with the help of charcoal. Priestley claimed that the calx contains both dephlogisticated air and phlogisticated air as fixed air. When heated with charcoal, the metal retains the phlogiston but loses dephlogisticated air. And some of the phlogiston from the charcoal is released as fixed air. When the revivification is done without charcoal, some of the fixed-air phlogiston becomes part of the metal, and some is released – in addition to the dephlogisticated air – as fixed air.

Note that at this point Priestley and Lavoisier did not just disagree over the interpretation of experiments that both judged to have been competently performed. They disagreed over what the outcome of competently performed experiment should be. Priestley said that the careful experimenter would always detect some phlogiston in the form of fixed air. Lavoisier claimed that any fixed air found in the process was due to contaminated equipment or substances. I shall later analyse the difference between Priestley and Lavoisier as an instance of the "Experimenters' Regress".

The next important step was the introduction of the name "oxygen" meaning "acid-former". The term was a direct expression of Lavoisier's belief that "this pure air is the constitutive principle of acidity". With this claim Lavoisier connected pneumatic chemistry to the theory of salts, the main field of chemistry in Central Europe at the time.

But Lavoisier also faced plenty of problems. The most important was this. In 1783 Priestley heated the calx of lead in an inverted glass over water. Inside the glass he put inflammable air – which he took to be phlogiston. The result was that the volume of air inside the glass decreased and the water-level rose. For Priestley and his followers this was experimental proof for the phlogiston theory of revivification. Priestley claimed that the lead absorbed the inflammable air, that is, the phlogiston.

Enter water. In 1781 Cavendish mixed inflammable air (for him: phlogiston) with pure air (for him: dephlogisticated air) and ignited the mixture with a spark. This produced drops of water. Cavendish accounted for this event by suggesting that water is an element; that water is part of both pure and inflammable airs, and that in the experiment water is released from these two airs by a condensation reaction. Lavoisier gave a different explanation: Water is a compound of oxygen and inflammable air (i.e. hydrogen). This thought immediately solved two of his above-mentioned problems. Priestley's 1783 experiment could now be rendered along oxygenist lines. When the calx is heated, it gives off oxygen. And this oxygen combines with inflammable air to form water. Inflammable air is not phlogiston. The volume of inflammable air reduces, and the water-level inside the inverted glass rises. Cavendish's experiment and Lavoisier's interpretation impressed Priestley to such an extent that he abandoned the identification of inflammable air with phlogiston in 1784. Instead he adopted Cavendish's view that inflammable air is highly phlogisticated water. (Siegfried 2002, pp. 176-177, Beretta 1993, p. 178).

By around 1785 there were two main theories concerning water. According to the position of Priestley and Cavendish, pure air (i.e. a mixture of water and a small amount of phlogiston) and inflammable air (i.e. a mixture of phlogiston and a small amount of water) can be brought into a reaction that will produce water (i.e. an element) and heat. The importance given to heat was taken as an indication that the weights of two volumes of pure and inflammable airs would not necessarily equal the weight of the water. For Lavoisians water could be produced out of pure air (or oxygen) and inflammable air (i.e.

hydrogen). Lavoisians too noticed heat as a further product of the reaction, but they believed nevertheless that the weights on the two sides of the reaction would be equal (Golinski 1995, p. 77).

In order to marshal support for his view, in 1785 Lavoisier staged a big public demonstration in order to prove that the weights would add up in the analysis and synthesis of water (Golinski 1995, p. 78). In the interest of space I here describe only the analysis part. Water steam was put through a red-hot gun barrel filled with iron filings. Two substances were collected at the other end: water and inflammable air. Lavoisier's rendering was that the iron filings absorb the oxygen, and that hydrogen escapes. The weight difference of the gun-barrel (before and after) plus the weight of the inflammable air (deducting the undecomposed water) equal the weight of the water steam. Lavoisier claimed to have determined with utmost precision how much oxygen and how much hydrogen are needed to make, say, one pound of water. One needed 0.86866273 pound of oxygen plus 0.13133727 pound of hydrogen. This impressed and convinced many of the colleagues who were present. But many phlogistonists doubted both the interpretation and the precision. This form of the controversy is naturally read as yet another "Experimenters' Regress" (Golinski 1995, p. 78), i.e. as a case where two groups of scientists can agree neither on what constitutes a competently performed experiment nor on a theoretical account of the phenomena.

Let us now turn to the later stages of the controversy. First a few words about Priestley. In several important respects he eventually moved closer and closer to Lavoisier's position. For instance, from 1788 onwards he accepted the oxygen-theory of acidity. And from 1796 onwards he went along with the oxidation of metals. (McEvoy 2010, pp. 119, 254; Boantza and Gal 2011, pp. 329-30; cf. Schofield 2004, ch. 9).

Regarding Lavoisier's later work we must note several important and influential texts. In 1783 Lavoisier and Pierre-Simon Laplace (1749-1827) published their *Mémoire sur la Chaleur*, a treatise on the calorimetrics of vapourisation and the idea of "caloric". In 1787 followed the *Méthode de Nomenclature Chimique*, written jointly with Claude-Louis Berthollet (1748-1822), Antoine-François de Fourcroy (1755-1809), and the afore-mentioned Guyton de Morveau. This book was a reform bill for terms in organic chemistry relying on the oxygenist system. Lavoisier's *chef-d'oeuvre* was his 1789 *Traité élémentaire de chimie*, published just as the French Revolution (that would cost Lavoisier his life in 1793) got under way. Here caloric was presented as the new "general principle". Its quantity in a body explained its state of aggregation. Important was also Lavoisier's notion of a chemical element as a substance not yet shown to be compound. The book also offered a unified framework for all of chemistry with oxygen and caloric in the centre, and a new nomenclature structured around Lavoisier's "Table of Simple Substances" (Beretta 1993, pp. 265, 184; Siegfried 1982, p. 30).

Finally, the perhaps best worked-out phlogistonist theory of the late 1780s was that of the Irishman Richard Kirwan (1733-1812). Kirwan held that inflammable air is phlogiston; that inflammable air plus dephlogisticated air equals fixed air; and that phlogiston is a component of all metals. Kirwan's theory of fixed air allowed him to accept Lavoisier's gravimetric data in the gun-barrel experiment. (Mauskopf 2002, Golinski 1995, p. 78). But Kirwan had a problem. He maintained that inflammable air plus dephlogisticated air equals fixed air. And this seemed to directly contradict Cavendish who believed that inflammable air plus dephlogisticated air equals water. Kirwan tried to get out of the conundrum by pointing to differences in temperature. In high temperatures inflammable air plus dephlogisticated air results in water, in low temperatures in fixed air (Mauskopf 2002).

Kirwan defended all this in considerable detail in his 1787 *Essay on Phlogiston and the Constitution of Acids*. He did not have to wait long for an answer. Within a year Marie-Anne Lavoisier, Antoine's wife, had translated the book into French, and Lavoisier, Berthollet, Fourcroy and Guyton had written chapter-by-chapter replies (Kawashima 2000). Kirwan conceded defeat in 1791. As his main reason he gave his knowing "of no clear and decisive experiment by which it could be shown that fixed air was formed out of vital [i.e. pure or dephlogisticated] air united with phlogiston …" (Mauskopf 2002, p. 202).

4. Chang on the Chemical Revolution and Pluralism

I now turn to a summary and interpretation of Chang's views concerning phlogiston and oxygen. I begin with his discussion of how the Chemical Revolution should or should not, can or cannot, be explained.

A first important claim is that many authors across Europe kept objecting to the oxygenist position long after the alleged watershed of Lavoisier's *Traité élémentaire de chimie* of 1789 (2012, p. 29). Some of the continued opposition came from "die-hards", that is, people who never gave up on phlogiston. Other opponents were "fence-sitters" or authors who did not feel able to decide between the two main theoretical options. And a third category consisted of "new-anti-Lavoisians", that is thinkers who rejected the oxygenist position on grounds unrelated to phlogistonist concerns. The following table from Chang's book (2012, p. 31) is meant to sum up the numbers in the different rubrics.

Die-hards	*Fence-sitters*	*New anti-Lavoisierians*
J. Hutton, 1726-97	P.-J. Macquer, 1718-84	B. Thompson, 1753-1814
J.-A. De Luc, 1727-1817	H. Cavendish, 1731-1810	G. Smith Gibbes, 1771-1851
A. Baumé, 1728-1804	G.-C. Lichtenberg, 1742-99	T. Thompson, 1773-1852
J. C. Wiegleb, 1732-1800	L. Crell, 1745-1816	J.W. Ritter, 1776-1810
J. Priestley, 1733-1804	C.-.L. Berthollet, 1748-1822	H. Davy, 1778-1829
T. Bergman, 1735-1784	J. Gadolin, 1760-1852	
J. Watt, 1736-1819	F. Gren, 1760-98	
B.-G. Sage, 1740-1824		

C.W. Scheele, 1742-1786

J.C. Delamétherie, 1743-1817

J. B. Lamarck, 1744-1829

A. Crawford, 1748-1795

J.F. Westrumb, 1751-1819

R. Harrington, 1751-1837

The relevance of this table to the case for pluralism is obvious. On the one hand, the data seems to show that there was a lot more *de facto pluralism* than historians and philosophers of science have allegedly allowed for. And, on the other hand, the data suggests that, for many writers at the time, there were no compelling reasons to “convert” to the oxygen theory. And thus phlogiston should have been allowed to live on.

As far as earlier explanations of the Chemical Revolution are concerned, Chang criticizes both philosophical and sociological accounts. On the philosophers’ side he rebuts attempts by Alan Musgrave (1976), Philip Kitcher (1993) and Andrew Pyle (2000) to show that giving up the phlogiston theory was rationally mandated (Chang 2012: 52-54). For example, Kitcher (1993) rationalizes the rejection of the phlogiston theory by drawing attention to the inconsistencies in Kirwan’s position. Chang points out that this leaves Priestley and Cavendish in the running (2012, p. 52). Again, it is easy to see why Chang feels the need to oppose these philosophical accounts: if the switch to oxygen had been rationally mandated, then it could not be maintained that both oxygenist and phlogistonist systems should have lived on.

Chang also has little sympathy for “social explanations of the Chemical Revolution” which he places “on the opposite side of the spectrum from the rationality-obsessed philoso-phers” (2012, p. 56). He praises Alan Musgrave’s “effective rebuttal of three social factors that are often invoked as explanations of scientists’ various decisions in the Chemical Revolution: nationality, age, and nomenclature” (2012, pp. 56-57; Musgrave 1976, pp. 206-7). Chang confines himself to offering some additional observations in support of Musgrave's arguments. As concerns nationality and age, Chang's target is H. Gilman McCann (1978) who claims that nationality and age were "significant causes of paradigm choice". Chang finds little of interest in the issue of age; for him it boils down to the trivial observation that "the young are, on the whole, more prone than the old to adopting new stuff" (2012, p. 57). Nationality is a more substantive factor, but, as Chang stresses, it is not able to account for the debates *within* France.

Chang treats the issue of nomenclature as one aspect of "the well-organized campaign … that Lavoisier and his colleagues ran, utilizing all kinds of institutional and rhetorical means at their disposal" (2012, p. 57). Chang emphasizes two points. On the one hand he warns against giving "Lavoisier's ruthless campaign" too much explanatory weight: after all, Lavoisian chemistry continued to be successful even after Lavoisier's death (2012, p. 58). On the other hand, Chang believes that this campaign was the major source of "irrationality" in the Chemical Revolution; it made too many chemists go along with Lavoisier (2012, p. 56).

In addition to age, nationality and nomenclature Chang also mentions "interest-based explanations for following Lavoisier" though without telling his reader whose explanations he has in mind. He deems them helpful "only … for those [historical actors] caught up in the narrow politics of Parisian science" (2012, p. 56).

In related, earlier work (2004, 2010) Chang expresses his stance vis-à-vis SSK and sociological explanations of scientific decisions more generally. He distances himself from SSK's alleged attempt to deflate "the special authority of science as a whole by reducing the justification of scientific beliefs to social causes" (2004, p. 248). Chang declares himself unwilling to "share the methodological commitment … that all explanations in the history of science must be social" (2010, p. 69).

Linking Chang's rejection of sociological explanations to his case for pluralism is not straightforward. I suspect the connection is as follows. Chang thinks that to explain a scientific belief or decision sociologically is to frame it as irrationally formed. Attributing this view to Chang seems plausible in light of his just-quoted conviction that sociological, SSK-style explanations reduce epistemic justifications to social causes and thereby deflate the authority of science. After all, to deflate the authority of science surely is to show that scientific beliefs are not rationally formed. It follows that, *if* the history of the Chemical Revolution could be fully captured in an externalist-sociological account then there would be little room for prescriptive pluralism. Remember that, according to prescriptive pluralism, historical actors – like the chemists of 1780s – had *good reasons* to pursue more than one theory, never mind whether they recognised this or not. But if there is a compelling account that reduces all reasons to irrational social causes, then there is no point in saying that the historical actors should have recognised the good reasons for a pluralist science.

Chang himself favours an account of the Chemical Revolution in terms of a long-term trend he calls “compositionism”. Compositionism is the view that chemical substances divide into “elements” and “compounds”. All elements are equally fundamental and compounds are determinable through the use of the balance. The alternative to compositionism is “principlism”. This is a dualistic conception involving imponderable fundamental principles on the one hand, and passive substances on the other hand (2012, p. 40). Chang submits that the oxygenist system fitted better into the compositionist trend than the phlogistonist system. He also alleges that the trend towards compositionism was well under way decades before the Chemical Revolution. And thus Lavoisier’s victory was but “a ripple riding on a large wave …” (2012, p. 42).

The relevance of this proposal to the argument for pluralism seems to be this. Compositionism and principlism are too basic, too fundamental, for them to be supportable by evidence. Moreover, the success of the one does not tell decisively against the other. And thus we cannot rationally decide between them.

Turning from questions of explanation to comparison and evaluation, let me put the latters' importance to the pluralist case up front. Chang wishes to establish a *symmetry* between the phlogiston and the oxygen theories: both are "equally wrong, from the modern point of view" (2012, p. 10), and both were "partially successful". There thus was "no reason to clearly favour one over the other" (2012, p. 29). Both should have been pursued.

Chang lists a number of problems that were considered important by both sides (e.g. understanding of combustion, calcination or reduction), as well as problems that were "(very) important" only to the phlogistonists (e.g. meterology) or only to the Lavoisians (e.g. the chemistry of salts). Chang insists that *all* of these problems were important, and that "each side tended to provide good solutions to the problems it considered important" (2012, p. 22). As far as differences in epistemic values were concerned, oxygenists gave pride of place to simplicity, novelty and monism, while phlogistonists held high completeness, epistemic conservatism and pluralism (2012, pp. 23-26). Chang finds it difficult to give an overall verdict on which bundle of values one should prefer. There is every reason to keep both, and be a pluralist.

Finally, Chang engages in some counterfactual reasoning to tell us what good would have resulted if the phlogiston theory had lived longer. Chang follows William Odling (1871) in maintaining that the phlogiston theory would have made possible much faster progress in the study of energy (2012, p. 47). He also sides with Gilbert Newton Lewis (1926) who suggested something similar for the study of electricity. Chang goes beyond these authors by emphasising important intellectual ties between the phlogiston system and the study of electricity. And he adds the photoelectric effect, cathode rays and radioactivity to the list of discoveries phlogiston theories might have facilitated in the nineteenth century (2012, p. 44).

5. Pluralism, the Chemical Revolution, and the Sociology of Scientific Knowledge

I now come to the main part of my paper. My aim in the pages to follow is to show that Chang's historical case study does not provide support for prescriptive scientific pluralism. Some elements of my criticism draw on ideas from the social history of science in general, and SSK in particular.

5.1. Chang's Phlogistonist Actors' Sociology

Social historians of science have often noted that parties to scientific controversies frequently put forward critical, "asymmetrical", sociological observations regarding their opponents. Such observations cast these opponents in a negative light, that is, as blinded by power or ideology. Let us call such homespun propaganda "actors' sociology". Oddly enough, in numerous passages Chang adopts the English phlogistonists' actors' sociology in his portrayal of the Chemical Revolution. Priestley and the phlogistonists, we are told, were "mature" (2012, p. 238), "pluralist" (2012, p. 26), "brilliant and dedicated" (2012, p. 5) "amateur scientists working individually, often using inexpensive equipment procured with their own resources (unlike Lavoisier …)" (2012, p. 49). The latter was "immature" (2012, p. 238), "absolutist" (2012, p. 26), "dogmatic" (2012, p. 27), "ruthless" (2012, p. 58) and made "everyone on [his] side sing from the same songsheet" (2012, p. 28). And the experiments of Lavoisier were so expensive that he needed government support (2012, p. 49).

This portrayal cannot count as a symmetrical treatment of the Chemical Revolution. Perhaps Chang thinks that two wrongs make a right, and that he needs to compensate for a traditional historiography that unfairly favours Lavoisier over Priestley. I am not convinced. I cannot find that this traditional rendering plays much of a role in today's historical work. And even if it did, this would not justify portraying Lavoisier as an immature dogmatic thinker.

Let me offer some evidence against Chang's assessments of Lavoisier and Priestley. Arthur Donovan, the author of the single best biography of Lavoisier, describes the latter's character as follows: "… his contemporaries were free to pick and choose which parts of this system they believed to be true. … He understood that true scientists must be persuaded rather than compelled …" (Donovan 1993, p. 164)

Remember also that even Lavoisier's closest allies – Guyton, Fourcroy, and Berthollet – disagreed with him on central points in public, and even in joint publications; as we saw above, Chang himself lists Berthollet as a "fence-sitter". Or consider the Lavoisians' response to Kirwan's book. Chang follows the tradition that speaks of a "demolition". However, the most detailed historical study of the episode by Seymour Mauskopf (2002) emphasises the "respectful" tone on all sides.

Chang draws a contrast between the inexpensive studies of the phlogistonists and the costly investigations of Lavoisier. His sympathies are clearly with the former. Here too Chang picks up on a phlogistonist theme of the time. True, some of Lavoisier's experiments were indeed very expensive. But let us not forget that he paid for most of his instruments himself (Frercks 2008, p. 218). As a multimillionaire he could afford it. And if being a multimillionaire counts as morally dubious, remember that Cavendish and Kirwan were superrich, too (Wilson 1851, p. 160; Reilly and O'Flynn 1930, p. 3).

I am also unhappy with of Chang's portrayal of Priestley. Of course, Priestley's income was modest compared with Lavoisier's. Nevertheless, Priestley's work was financially supported by the multimillionaire William Petty, the second Earl of Shelburne. In 1793 Priestley's laboratory was valued at £600 (Schaffer 1986), which in today's money comes to £60,000 or even £700,000, depending on whether we use as the basis of calculation the retail price index or average earnings.

Moreover, there is no easy inference from Priestley's frequent and public expressions of modesty to the conclusion that he actually and genuinely displayed such virtue in his scientific or private life. After all, modesty was a conventional trope in English natural history at least since Robert Boyle (cf. Shapin & Schaffer 1985). Furthermore, Priestley was quite ready and able to "demolish" competitors who questioned instruments (e.g. "eudiometer") that he marketed. Indeed, his work on airs was directly linked to his considerable commercial interests in the instrument-builders market in England (Schaffer 1984, 1986).

Perhaps most significant for Chang's overall thesis – that it would have been better if the phlogistonist system had lived longer – is the question concerning Lavoisier's and Priestley's respective experimental skills. Here too Chang adopts the phlogistonists' perspective. As he sees it, the phlogistonists were outstanding experimenters. If there had been no phlogistonists around, "Lavoisier and his friends might have stumbled on those particular experiments on their own eventually" – but the emphasis is on the "stumbled" (2012, p. 49). In support of his assessment, Chang cites a thirty-year-old paper by Maurice Crosland (1983). But Chang does not comment on the fact that such evaluations have been challenged in more recent scholarships. For instance, Frederic L. Holmes (2000a) traces in some detail how Lavoisier's reputation as a clumsy experimenter – who borrows from others without giving them credit – is a "myth that Priestley himself initiated" (2000a, p. 76). Holmes in fact shows that by the mid-1770s the situation was actually the reverse: by that stage Priestley had come to agree with Lavoisier that "theories of composition were to be adjudicated in terms of weight relations". But Priestley never managed to achieve stable measurement results. Writes Holmes:

> "The problem was Priestley's relative lack of experience in this type of investigation. Because he did not regularly subject his theoretical views on the composition of airs to this kind of test, he could not, in the long run, compete with Lavoisier's growing mastery of this mode of experimentation. There is, therefore, a deep irony in his characterization of Lavoisier's reasoning as mere 'speculation' based on facts supplied by others." (2000a, p. 86)

Recall also Chang's misgivings about Lavoisier's so-called "ruthless campaign". Here too Holmes (2000a) provides a helpful corrective: "… we need to balance our perspective on [Lavoisier's campaign] … by noting how adroit Priestley had, with the help also of his tireless advocate [Jean Hyacinthe de] Magellan, already built an international network of experimental 'philosophers' who looked to him as the founder of the modern doctrine of airs" (2000a, p. 93).

Resonances of phlogistonist propaganda can also be found in Chang's repeated insistence on "just how wrong Lavoisier was, if we judge him from the view of modern chemistry and physics" (2012, p. 8). Chang follows Robert Siegfried's (1988, p. 35) claim that "the central assumptions that guided [Lavoisier's] work so fruitfully were proved empirically false by about 1815". Unfortunately, Chang pays no attention to Carlton Perrin's criticism of Siegfried's evaluation. Perrin argues that Siegfried's view "can only be maintained by a narrow interpretation of Lavoisier's achievement". Perrin lists as such accomplishments the institution of "an new and powerful investigative program that shed light on combustion, calcination, the formation of acids, the composition of minerals, the constitution of organic substances, fermentation, respiration, heat exchanges as more". Perrin concludes: "The chemistry of 1815 and after did not negate the Lavoisian achievement but evolved from it—without a dramatic upheaval of the kind Lavoisier's efforts triggered." (1988a, p. 81; cf. Blumenthal 2011.)

Does all this matter for Chang's attempt to read the Chemical Revolution as making a case for scientific pluralism? I think it does. Chang needs to convince us that it was irrational for chemical communities of the late eighteenth century to abandon phlogiston-nism. What the last few paragraphs bring out is that one important strand of his overall argument is based on phlogistonist actors' sociology. Following Priestley's lead, Chang insinuates a moral and intellectual superiority of the phlogistonists over and against the Lavoisians. As we have seen however, the content of this propaganda is highly questionable. This is not without irony: Chang rejects the sociology offered by SSK as reductive and simplistic – only to then fall for a form of actors' sociology himself.

5.2. Chang's Systems

In order to make sense, a prescriptive pluralist needs to specify the level on which an increase in plurality of some sort is sought. Chang pitches his thesis on the level of "systems of practice". A system of practice "is formed by a coherent set of epistemic activities performed with a view to achieve certain aims" (2012, p. 16).

Throughout his case study, Chang speaks of the confrontation between "the oxygenist system" and "the phlogistonist system" (2012, e.g. p. 37). He is adamant that it would have been beneficial for the progress of science if the "phlogistonist system" had not been "killed" in the 1780s and 90s (2012, e.g. p. 65). Chang assumes something of a "zero-sum game" about the confrontation between these two systems. In particular, every argument against Lavoisier is at least an indirect indication that phlogiston was given up too soon (2012, pp. 30-34). Chang allows for a plurality of versions within both systems. With respect to phlogiston, Chang addresses Lavoisier's famous accusation, according to which the phlogiston theory was a "veritable Proteus" (2012, pp. 28, 55). Chang rejects the insinuation that the phlogiston system itself was contradictory: the contradictory claims were distributed over "different versions" of the system. Finally, Chang specifies as follows how he conducts his comparison between the strengths and weaknesses of two systems: "My own comparative assessment here is between Lavoisier's system and the best versions of the phlogistonist system" (2012, p. 28).

There are several problems with this approach. First of all, in the light of recent historical scholarship, it is highly doubtful whether it makes much sense to speak of *a* "phlogistonist system" (in the singular). Stahl's phlogiston theory, early phlogistonism in France, English and Scottish uses of the concepts of phlogiston – all these were linked to very different experimental and theoretical traditions, goals, epistemic values, principles, and disciplines. For instance, Frederic Holmes argues that it would be a mistake to construe the Chemical Revolution as the “overthrow of the reigning paradigm” of phlogistonism: “Priestley’s use of phlogiston was so loosely connected with the older phlogiston theories descended from Georg Ernst Stahl that the events at the heart of the chemical revolution should be viewed more as a competition between two rival new research programs than as the replacement of a reigning paradigm” (2000b, p. 735). Mi Gyung Kim (2008, 2011) and John Stewart (2012) develop this line of thought in considerable detail (cf. Mauskopf 2013). Stewart shows that British chemists “conceptualized a new phlogiston, empirically produced through decomposition reactions and submitted to metric measurement and algebraically controlled chemical reactions”. Such methods had played no role in the earlier French phlogistonist traditions. Moreover, where the French had ontologically associated phlogiston with the “sulfur principle and corpuscular heat”, some British authors identified phlogiston instead with inflammable air (Stewart 2012, p. 178).

Of course the variation did not end there. Cavendish, Kirwan and Priestley differed on many important questions. For instance, Kirwan and Cavendish gave inflammable air different ontological identities. These different identifications were linked to different understandings of inflammable air’s role in chemical theory. Kirwan claimed that inflammable air was central in the production of fixed air; Cavendish insisted that the lack of experimental proof for this assumption constituted a refutation of Kirwan’s whole theory of phlogiston. In his criticism of Kirwan, Lavoisier relied centrally on Cavendish’s point (Stewart 2012, p. 181).

Chang is not entitled to deal with this buzzing plurality by speaking about “versions of the same system”. According to his own definition, a “system of practice” needs a “coherent set of epistemic activities”. But the phlogistonist practices of Stahl, of the early-eighteenth-century French authors, or of the British pneumatic chemists put together did not form such a coherent set.

The observations of the last few paragraphs undermine Chang's right to be comparing "Lavoisier's system and the best versions of the phlogistonist system" (2012, p. 28). Chang in fact compares Lavoisier's system with different systems of chemistry and physics that happen to be using the term "phlogiston". The problem is compounded by the fact that Chang fails to tell us how the "best" is to be determined (Mauskopf 2013, p. 625). Usually Chang identifies phlogiston with a "material but imponderable fluid" (2012, p. 13); this is crucial to his argument that phlogiston could have lead us to electrons faster. In so doing Chang completely ignores the considerations that lead arguably outstanding phlogistonists – men like Cavendish or Kirwan – to identify phlogiston with measurable entities.

Furthermore, it is not enough to tell us that keeping alive one particular rendering of phlogiston would have led to faster progress towards electrons in the nineteenth century. It also needs to be shown that this specific understanding had a realistic chance of being noted and developed further at the time. Chang reminds us that, during the eighteenth century, more than two dozen authors commented on connections between phlogiston and electricity. This is true but misleading. The ideas on these connections were a very mixed bag, and amongst these authors there was no agreement on the nature of either electricity or phlogiston. In particular, most of the ideas linking electricity to phlogiston conflicted with the ways in which Cavendish, Kirwan and Priestley thought about phlogiston. Moreover, the leading and most respected expert on the links between electricity and phlogiston – the Dutchman Martinus van Marum (1750-1837) – decided in 1785 that the Lavoisian system was better suited for his research project in electricity than the phlogiston system (Sudduth 1978). Nor is Chang's case made for him by Gilbert Newton Lewis (1926) or William Odling (1871). In their brief comments on the potential of phlogiston they pay no attention to the variety of phlogiston theories in the second half of the eighteenth century, and they work with quick rational reconstructions of what the phlogiston theory might ideally be. In other words, to assume that electrons would have been discovered sooner if only the mixed bag of ideas on phlogiston and electricity been kept alive longer, seems based on little else than the hope – familiar from scientific realism – that “truth will out”.

5.3. Converts and Core-sets

I now turn to Chang’s above-quoted table about “die-hards”, “fence-sitters” and “new anti-Lavoisierians”. Recall that this table is meant to show that there was much more *de facto* pluralism than (allegedly) is common to assume in the historiography of the Chemical Revolution.

One problem with Chang’s argument here is that listing a number of names as die-hards or fence-sitters is not very informative unless we get a sense of overall numbers. Perrin (1988b) does the numbers for France. 45 authors accepted the central tenets of the Lavoisian system by 1789, and an additional 24 authors publicly expressed their support for its central tenets during the 1790s. 6 authors continued resistance after 1789. That is less than 10%. Note also that although Chang offers his list as evidence for the claim that resistance to Lavoisier continued after 1789, three people on his list were dead by 1786: Bergman and Macquer in 1784, Scheele in 1786. Holmes (2000a: 752) reports that by the time of his death Macquer had already moved pretty close to Lavoisier’s position.

Moreover, Chang’s inclusion of “new anti-Lavoisierians” into the argument is based on the problematic assumption of a zero-sum game between the two systems. I shall just comment on one chemist in that rubric: Humphry Davy (cf. Crosland 1995; Siegfried 1964). In 1807 Davy coined the striking expression “imperial despotism of oxygen” and flirted with a return to phlogiston. The expression “imperial despotism of oxygen” was a modification of the more widely used “imperial despotism of Napoleon”. Siegfried (1964) suggests that Davy’s attempt to resurrect the term “phlogiston” was linked to Davy’s belief that “… the scientific glory of a country may be considered in some measure as an indication of its innate strength." Judged by this criterion, chemistry could not owe much to the French. Chang (2012, p. 33) finds Davy's flirtation with phlogiston grist to his mill. But we should not forget that when the war was over Davy quickly dropped his phlogistonist project.

Chang classifies Berthollet as a "fence-sitter" on the grounds that the latter disagreed with Lavoisier's ideas on acidity. True, Berthollet did so disagree. And yet, he also was Lavoisier's closest ally. He published several attacks on the phlogiston theory and participated in the so-called "demolition" of Kirwan. If Berthollet counts as a fence-sitter, if Berthollet does not count as a Lavoisian, then who does?

Furthermore, it is important to remember that not everyone who supported, or disagreed with, the oxygen theory was part of the "core set" of investigators whose views and arguments had the potential to make a difference (I take the term "core set" from Collins 1992). German and Swedish (or Dutch or Spanish) authors following the debate were not part of the core set of the 1780s. The controversies in those countries took place five to ten years later than in France (Hufbauer 1982, Gago 1988, Lundgren 1988, Snelders 1988). For instance, the first text defending the Lavoisian system in Sweden was published only in 1795. And the Finnish chemist Johann Gadolin published the first Lavoisian textbook in Swedish in 1798. He obviously was not a fence-sitter at that stage any more. (Lundgren 1988.) Similar arguments apply to the Germans of Chang's fence-sitter category (cf. Hufbauer 1982: pp. 83 & 142 for Crell; p. 131 & 139 for Gren; p. 95 for Lichtenberg).

Taking these considerations into account the only important fence-sitter that remains is Cavendish. And that Cavendish moved from being an all-out phlogistonist to being a fence-sitter was of course a tremendous triumph for Lavoisier's side.

Chang assumes that whoever did not fully agree with every aspect of Lavoisier's system was either a fence-sitter or a die-hard. But that cannot be right: by that criterion almost everyone was a fence-sitter or a die-hard – and pluralism was rampant. Here we must remember a central aspect of Collins' analysis of core sets in scientific controversies: to wit, that there always exists plenty of uncertainty and disagreement even amongst allies.

And that is exactly what historians (like Kim 2003) and Perrin (1988b) have documented in detail for the Chemical Revolution, too.

As it stands, Chang's chart does not make a case for a prescriptive pluralism. In addition to the problems already mentioned, it also fails to attend to the reasons of those who did convert. This was the vast majority. Chang wants to be able to say that in the 1780s and 90s there were good reasons for keeping phlogiston alive and that at least some of the historical actors were aware of these reasons. Chang mentions some such reasons. But this line of inquiry demands a symmetrical treatment of those who gave up the phlogiston theory. Chang does not discuss the reasons of even one such convert even though such reasons were written down in letters and publications at the time: van Marum or Kirwan are obvious cases in point. Why does Chang think that these men were mistaken in their judgements? Were they victims of Lavoisier's "ruthless campaign"? Alas, we are never told.

5.4. The Rational and the Social

Chang maintains that neither experimental results nor theoretical considerations available to chemists at the time were sufficiently strong to rationally compel abandoning the phlogiston theory. For the purposes of this paper I shall go along with Chang's view. This should not be misunderstood as dogmatism on my part. My argument in what follows is conditional. I want to show that *even if* we grant Chang the mentioned underdetermination, his prescriptive scientific pluralism does not follow. What blocks the move from the premise to the conclusion is the fact that, by the 1780s or 90s, the vast majority of people interested in the debates over phlogiston and oxygen started to have overwhelmingly good "indirect" reasons for favouring the Lavoisian position. Such "indirect" reasons concerned markers of trustworthiness in chemical matters: track records, social indicators of reliability, institutional status, standing in the profession, plausibility of vision for the field of chemistry as a whole, … and much else besides. These “indirect” reasons were closely intertwined with what I shall call the “narrow” reasons for or against phlogiston, that is, the experimental data and proximate theoretical considerations. – All this will of course need some unpacking in what follows.

5.4.1. Trust and the Experimenters’ Regress

Trust first. For the past thirty years, scholars in SSK, social epistemology and philosophy of science have assembled extensive evidence for the idea that “scientists know so much about the natural world by knowing so much about whom they can trust” (Shapin 1994, p. 417; cf. e.g. Collins 1992, Hardwig 1985, 1991, Kitcher 1983). Little reflection is needed to recognize that scientists’ first-hand knowledge is dwarfed by their testimonial knowledge. Moreover, to acquire first-hand knowledge, scientists need second-hand knowledge (e.g. in the form of information about substances and instruments they have acquired from others).

Trust in science is not altogether blind. It is based on criteria such as track record and status-indicators: How successful has the testifier been in the past? What institution is she working at? Where has she published her papers? What prizes has she won? Who cites her work? With whom is she collaborating? Who agrees or disagrees with her? And so on. These criteria are social in that the degree to which they are fulfilled almost always depends on the judgments of people.

Trust in science is so ubiquitous and so deep that it is easily overlooked and taken for granted. It becomes visible first and foremost when it break downs. This happens in fierce and prolonged scientific controversies. Perhaps it is no coincidence therefore that the role of trust in science has been analyzed most perceptively by SSK-scholars engaged in “controversy-studies”. The most important general account of controversies in cutting-edge science is Harry Collins’ theory of the “Experimenters’ Regress” (Collins 1992).

The central parties to a cutting-edge scientific controversy – the "core set" – face an "Experimenters' Regress" when the following situation obtains. The central parties are unable to agree on questions of theory since they are unable to agree on what constitutes the competent production of data; and they are unable to agree on what constitutes the competent production of data since they are unable to agree on questions of theory. I take it to be a well-confirmed thesis that Experimenters' Regresses are a central feature of cutting-edge science (Collins 1992, 2004, 2010; Shapin and Schaffer 1985; Pinch 1986; Gooding, Pinch and Schaffer 1989; Kusch 1998). The relevant literature has also documented how scientists within, and outside of, the core set deal with this conundrum: to wit, by partly relying on, partly modifying, (their criteria for) their differential distribution of trust. The central parties to the controversies of course try to bring about such recalibration of trust as is favourable to the adoption of their position. In the end, an Experimenters' Regress is broken, and "closure" is achieved, when a new distribution of trust is in place and stable, that is, when the scientific community is in broad agreement on who are the reliable spokespersons for the recently controversial area of science. At this stage members of the core set, as well as scientists following the controversy from a greater distance, have what they think of as very good reasons to favour one of the two sides.

5.4.2. Chang versus Hufbauer on Sociological Explanations of the Chemical Revolution in the German-speaking Lands

I claimed above that by the 1780s or 90s, the vast majority of people interested in the debates over phlogiston and oxygen started to have overwhelmingly good "indirect" reasons for favouring the Lavoisian position. Clearly, to defend this claim for all European countries would obviously take a book rather than a paper – and no doubt a lot more competence in chemistry than I have at this time. I shall therefore confine myself to the German-speaking lands of the late 1790s.

To prepare the ground for this brief case study, I first need to connect the above-mentioned work on trust, controversy and the Experimenters' Regress to Chang's argument. One of the odd features of Chang's analysis of the controversy between "the phlogistonist system" and "the oxygenist system" is that he makes almost no contact with the standard sociological work on scientific controversies in general and on the Chemical Revolution in particular. Chang only mentions McCann (1978) on the role of age and nationality, Hufbauer (1982) on nationality, and Henry Guerlac (1975), Arthur Donovan (1993) and especially Maurice Crosland (1995) on Lavoisier's "campaign". This leaves out some highly relevant recent work. I here mention only three monographs.

Jan Golinski's *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (1992) situates Priestley socially and politically in his time and contexts; explains the differences in experimental traditions in France and Britain; analyses Lavoisier's strategies of persuasion; and pinpoints the "Experimenters' Regress" in the experiments on water. David Philip Miller's *Discovering Water: James Watt, Henry Cavendish and the Nineteenth-Century 'Water Controversy'* (2004) offers a detailed historical study of the water controversy based on SSK work by Harry Collins on the Experimenters' Regress and by Augustine Brannigan (1981) on the "attributional model of discovery". And Jonathan Simon's *Chemistry, Pharmacy and Revolution in France, 1777-1809* (2005) offers an interest explanation for Lavoisier's success in France: Lavoisier's programme enabled chemists to free themselves from the dominance of pharmacy.

Chang is also setting aside key theoretical and methodological work. For instance, as we saw above, Chang equates "social" with "irrational" or "external(ist)" and "rational" with "internal(ist)". This equation has been challenged for more than thirty years, the locus classicus being Stephen Shapin (1992). Shapin points out many problems with the "internal—external" distinction; for instance, that the issue of what should count as internal or external is often at the heart of scientific debates; that the micro-sociology of a research team is "internal" to science; or that the distinction is hard to apply to forms of research that lack clear boundaries. Arguably all of these ideas are relevant to the study of the Chemical Revolution.

Chang's limited familiarity with this literature matters. It shows first and foremost in his failing to recognize a sociological analysis for what it is. This brings us to Karl Hufbauer's classic 1982-account of how the debate over phlogiston was closed in the German-speaking lands. Chang claims that "Hufbauer's explanation seems to be quite internalistic … there is nothing very sociological in Hufbauer's account of that event." (2012, p. 57)

Hufbauer explains that during the early 1790s, in the German-speaking lands, the fate of the phlogiston was decided on the basis of a single experiment, to wit, the reduction of "mercury calx per se" to mercury. ("Mercury calx per se" was mercury calx produced by simmering mercury in an open vessel (1982, p. 119).) The reduction of mercury calx had been one of Lavoisier's most important experiments: as he saw it, the heating of mercury calx produced the pure metal and oxygen. And this established – for Lavoisians anyway – that mercury calx really is mercury oxide, that calces are compounds, and that metals are elements.

In 1790 and 1791, several German phlogistonists challenged Lavoisier's handling of this experiment. They claimed that *freshly produced* mercury calx per se did not give off oxygen when heated. And they alleged that Lavoisier had used non-fresh mercury calx that had absorbed water from the atmosphere. In other words, these German phlogistonists maintained that Lavoisier was not a reliable producer of experimental facts (1982, pp. 120-122).

German antiphlogistonists were quick to respond. The leading Berlin Lavoisians (Hermbstaedt, J. Peschier, Klaproth) did numerous runs of the reduction experiment, and found oxygen every time. They also sought to prove that fresh and bottled mercury per se do not behave differently. (1982, p. 126) Impressed by this flood of results from the antiphlogistonists, by 1792 the phlogistonists shifted their position. They now agreed that oxygen was a product of the experiment. But they still denied that oxygen was an integral component of the calx. Instead it was "an extraneous by-product of the interaction between the calx and absorbed water". Moreover, the phlogistonists proposed a crucial two-stage experiment. In the first stage, all water should be expelled from the given amount of mercury calx per se. And then, in the second stage, the remaining mercury calx per se should be reduced. The phlogistonists insisted that – if only the first stage was done properly – no oxygen would ever be found in the second stage. This challenge was or course hard to meet for the antiphlogistonists: the very fact that they did find oxygen in the second stage showed that, by the phlogistonists' lights, they had not been successful in the first stage (1982, p. 130).

After some months of deadlock, in 1793 the Berlin antiphlogistonist Sigismund Friedrich Hermbstädt (1760-1833) mounted a campaign that soon proved successful. He reported many reduction experiments with fresh mercury calx per se; in each case he obtained a quantity of "vital air" whose weight equalled the loss in weight in the calx. Hermbstädt set up his experiments in ways that seemed to fit the phlogistonist design for the crucial experiment. Moreover, some of the mercury calx per se used by Hermbstädt had been sent to him by his phlogistonist opponents. He invited his phlogistonist opponents to check on his results by in turn forwarding them his calx per se (1982, p. 134). Hermbstädt performed his experiments in front of more than a dozen witnesses: these included PhD students of his phlogistonist opponents, physicists, medical doctors, and high civil servants. And he informed a wider public about his results by publishing in the high-circulation *Intelligenzblatt* (1982, p. 135).

As Hufbauer sees it, "Hermbstädt's assault overwhelmed the phlogistic spokes-men" (1982, p. 137). Although most of them initially attacked both Hermbstaedt's experiments and his way of reporting – e.g. his use of non-expert witnesses – their resistance quickly faltered over the next few months, as a wider public of chemists and scientists became convinced that Hermbstädt had clinched the case.

I can now return to Chang's claim that "nothing very sociological" is offered in Hufbauer's account of the episode. I beg to differ. To begin with, Hufbauer puts forward an explanation as to why German chemists of the time regarded each other as competent interlocutors, and even when they disagreed over fundamental questions: all key figures in the controversy were Protestants and "chemists in Protestant Germany regarded one another as the primary arbiters of chemical truth". This in turn had a lot to do with the fact that in the Protestant lands chemistry had reached a high degree of professionalization (1982, p. 142). Second, Hufbauer is struck by the fact that one single experiment was allowed by all sides to settle the fate of phlogiston: "Leading German phlogistonists were virtually unique in searching, or acquiescing in the search, for evidence that Lavoisier had botched the reduction experiment" (1982, p. 143). Hufbauer accounts for this uniqueness in terms of a strong empirical-experimental tradition in Germany. Lavoisians realised that they could not opt out of this tradition: "… the leading German antiphlogistonists readily agreed that Lavoisier's reliability was the central issue. In agreeing to limit the controversy in this way, they were probably mindful of their community's antipathy to theory" (1982, p. 143). Third, note that both sides put the emphasis on Lavoisier's reliability. Phlogistonists and antiphlogistonists agreed that the reduction experiment was a key test for Lavoisier's general standing as a trustworthy reporter of experimental facts. Fourth, Hufbauer's account highlights an "Experimenters' Regress": the two sides could not agree on the correct experimental outcome since they could not agree on what would constitute a competent performance of the experiment. And they could not agree on what would constitute a competent performance of the experiment since they could not agree on the correct experimental outcome. Indeed, Hufbauer emphasises that the debate did not had to end when it did. Fifth, Hufbauer goes on to list a number of factors that played an important role in the closure: Hermbstädt's clever publication strategies, his carefully crafted attacks, and – especially – his reaching beyond the community of chemists. Finally, Hufbauer points out that soon even the leading phlogistonists accepted Hermbstädt's conclusions (1982, p. 143.) – Religion, scientific traditions, agreements, trustworthiness of witnesses, Experimenters' Regresses, tools of persuasion targeting specific audiences – if these are not "something sociological", then what is?

Chang's misunderstanding of Hufbauer's argument is not a slip of the pen. In fact Chang does not identify a single Experimenters' Regress in the dispute between Lavoisier and his opponents. This shows that Chang underrates the degree and the quality of underdetermination in the Chemical Revolution. He only considers cases where Lavoisians and phlogistonists disagreed over the correct interpretation of experiments that both sides took to be competently performed. But this disregards the more radical disagreements in which there was no agreement over either theory or the competent production of experimental data. Golinski (1995) shows that this was precisely the situation surrounding the analysis and synthesis of water, and I have suggested above that Lavoisier and Priestley differed in this way as concerned the reduction of metals. Priestley insisted that careful and competent chemists would be able to detect a certain amount of fixed air (containing phlogiston) resulting from the reaction. Lavoisier maintained that careful and competent chemists would not do so. As he saw it, chemists who detected fixed air during the reaction were working with contaminated equipment or contaminated substances. Lavoisier and Priestley disagreed over the existence of phlogiston and they differed over who had the experimental skill to produce reliable data. Such disagreements can be closed only through social processes of recalibrating trust.

Correcting Chang's rendering of Hufbauer's analysis has consequences for his pluralist argument. Remember that Chang is worried about sociological – "externalist" – accounts since to him these seem to erase the space for prescriptive pluralism. If social causes trump good reasons, then the prescription to be attentive to the good reasons for more than one theory makes little sense. It follows that if Chang were to admit that

Hufbauer's account of the German controversy is sociological through and through, then he would not – by his own lights – be able to use this case in support of prescriptive pluralism.

I of course reject Chang's view that in sociological accounts of the history of science social causes trump good reasons. Nevertheless, I too think that a sociological analysis like Hufbauer's makes it difficult to maintain Chang's pluralism. Chang's argument is based on the idea of two socially uncontaminated "pure" sets of good reasons for and against phlogiston. And these sets simply were not there to be had. The historical actors only had considerations that fused together experimental data, skills, tradition, religion, differential distributions of trust, and much else besides. It is hard to formulate pluralist prescriptions concerning such complex considerations.

5.4.3. Compositionism

To conclude, I should at least briefly comment on Chang's alternative – seemingly non-sociological – explanation of the Chemical Revolution in terms of the rise of compositionism. Recall that for Chang this long-term trend best explains the defeat of the phlogiston theory, and pre-empts sociological micro-historical explanations (2012: 58). (Compare here Klein, this volume.)

Chang's remarks on compositionism rely heavily on the work of the historian Robert Siegfried (esp. Siegfried 2002). Siegfried and others – in particular Ursula Klein (e.g. Klein 1994) – have indeed shown that many compositionist themes emerged throughout the seventeenth century and thus in part well before the Chemical Revolution. But other than Chang, Siegfried does not suggest that the success of Lavoisier's programme can be explained by a form of compositionism that already existed before Lavoisier.

Chang's use of compositionism as an explanatory resource is not always easy to understand. First, compositionism is supposed to explain the transition from the phlogiston system to the oxygen system. Second, Chang goes to great length to argue that Lavoisier was not a pure compositionist but held a "mixed-up position situated uncomfortably between principlism and compositionism" (2012, p. 60). Third, in the case of the phlogiston system Chang diagnoses a combination of principlist and compositionist themes as well. But here he thinks the "mix-up" was detrimental: he speaks of the "internal collapse of the phlogistonist system resulting from compositionist corruption" (2012, p. 61). This triad of theses raises important questions that, alas, are not answered: If Lavoisier himself was so steeped in principlism, how can his compositionist elements explain his victory? Since the phlogistonist system was itself moving ever closer to compositionist ideas, how could the compositionist trend lead to its destruction? And if even the most reflective chemists at the time were unable to tease apart compositionist and principlist concerns, how could the rest of the investigators use compositionist credentials to decide in Lavoisier's favour?

Siegfried's account is better able to make sense of the situation. Siegfried insists that Lavoisier fundamentally overhauled the earlier variegated compositionist elements and structured them around the principle of the conservation of weight, an experimental programme involving detailed measurements, a new compositionist nomenclature and the concept of a simple substance (2002, pp. 163-212). The overhauled compositionism was a new wave – a wave of Lavoisier's making. It does not explain Lavoisier's success, its success stands in need of an explanation. I shall not offer a specific explanation here. But in light of Hufbauer's study of the German case, or Golinski's (1992), Miller's (2004) or Simon's (2005) work, I suspect the answer will need to make reference to the kinds of social phenomena studied by the sociology of scientific knowledge.

6. Summary and Conclusion

Since the argument of this essay has been long and complex, it is best to conclude by reviewing its structure and protecting it against possible misinterpretations. I have tried to show that Chang's interpretation of the Chemical Revolution is problematic, and that putting in the needed corrections erodes the case for pluralism. My criticism consisted of four steps. I began by arguing that Chang's comparisons between the two "systems" are crassly biased in Priestley's favour. Chang adopts lock, stock and barrel the phlogistonists' actors' sociology. I then sought to criticize Chang's speaking of "the phlogistonist system". I presented results from recent historical scholarship in an effort to undermine Chang's assumption that uses of "phlogiston" were based on one coherent system of epistemic activities. Correcting this assumption weakens Chang's comparisons and his counterfactual reasoning on what might have been, had "the phlogistonist system" been kept alive longer. In my third step I questioned Chang's belief that an alleged slow transition from the phlogistonist system to the oxygen system supports pluralism. I challenged his data and noted the absence of attention to the reasons authors at the time presented for "converting" to the oxygen theory. In my final, and most important, step, I tried to show – pace Chang – that chemists in late eighteenth century Europe did have conclusive reasons to favor Lavoisier's over the phlogistonists' views. It was in this step that I drew both on historical-sociological case studies and on theoretical work on the Experimenters' Regress. – I take it that if I have been roughly successful in my criticism then Chang has not established his central claim, to wit, that the Chemical Revolution makes a case for scientific pluralism.

Needless to say, to criticize Chang's way of defending scientific pluralism is not to attack scientific pluralism *per se*. Nor should this paper be understood as suggesting that SSK is somehow principally incompatible with scientific pluralism. The claim advanced here is merely that Chang's particular historical and philosophical claims founder *inter alia* on some important lessons from SSK. I write "*inter alia*" since there may be other perspectives too from which Chang's pluralism might be successfully challenged (Blumenthal 2013, Blumenthal and Ladyman 2014, Chalmers 2013, Mauskopf 2013).

Let me also register a proviso concerning the sociological take on the Chemical Revolution offered above. A paper of this limited length cannot conclusively establish a comprehensive sociological interpretation of the Chemical Revolution. This would demand at least a book-length investigation. All I could offer here is a “proof of concept” – with “proof” taken with a large grain of salt: I hope to have made plausible that a sociological approach to the Chemical Revolution is not ruled out by Chang’s theoretical considerations or his historical data.

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