

Chapter 1

Water and the Chemical Revolution

Abstract It was through the Chemical Revolution of the late eighteenth century that water first came to be recognized as a compound, having been considered an element since ancient times. In this chapter I offer a revisionist account of that momentous event. A systematic appraisal shows that the old phlogistonist system of chemistry was not clearly inferior to Lavoisier's oxygenist system of chemistry. Lavoisier's system actually suffered from significant empirical and theoretical problems already recognized at the time, and there was significant methodological incommensurability between the two systems, though only mild semantic incommensurability. Aside from the effective and ruthless campaigning by the Lavoisierians, the demise of phlogiston (which was not as sudden or complete as often imagined) was most of all due to the advent of *compositionism* as a dominant trend in chemistry, into which phlogistonist practices did not easily fit. With the demise of phlogiston, many valuable elements of knowledge were lost; in effect, these were recovered and developed later with the help of different concepts (e.g., potential energy and electrons), but I argue that it would have been better for science if the phlogistonist system had been allowed to continue its work. This conclusion also anticipates the more general argument for pluralism in science, to be given fully in Chap. 5. In order to give more precision in the articulation and defence of these ideas, I introduce and use the notion of *system of practice* as a unit of analysis.

1.1 The Premature Death of Phlogiston

In the middle of the eighteenth century people still considered water as an element. For Europeans this idea dated at least back to the ancient Greeks at the time of Aristotle, according to whom water was one of the four basic elements (along with earth, air and fire) which constituted all the substances making up the terrestrial world. Thales had even postulated that water was *the* element from which everything was made. We now know that water is not an element, but a compound made

up of oxygen and hydrogen. This chapter tells the story of how we came to know that, as a result of the Chemical Revolution just over 200 years ago. It is a story that has been told very many times, and knowledgeable readers may wonder why I should try to tell it again. The reason is simple: it is all too often told all wrong—“wrong” in several senses: incorrect about the historical circumstances, ignorant of the relevant scientific arguments, judgmental on the basis of deep misunderstandings, and philosophically naïve and simplistic. The best available insights are usually buried in specialist works that are neglected even by the majority of professional historians and philosophers of science.

I hope that my re-telling of this story will enhance your interest in the Chemical Revolution as an exciting topic of historical, philosophical and scientific thinking. As will be the case in each chapter of this book, the first section (Sect. 1.1) will provide a brief surface-level presentation of the main ideas, intended to be accessible to readers without much background in the relevant philosophical, historical and scientific areas. My hope is that even non-specialist readers will become intrigued enough to go on to read the second section (Sect. 1.2), and even some of the third section (Sect. 1.3).

1.1.1 *Joseph Priestley*

Our story begins with Joseph Priestley (1733–1804), a paragon of eighteenth-century amateur science.¹ A dissenting preacher and political consultant, Priestley flourished in the benign exclusion imposed by the Anglican orthodoxy. He was one of those British men of science who never went near a university, either for learning or teaching. His scientific research was done at home, initially in the “warm mice-ridden Yorkshire cottage kitchens” (Crowther 1962, 218). His great work in chemistry began when he moved to Leeds in 1767, where he had the good fortune of “inhabiting a house adjoining to a public brewery” (Priestley 1970, 94). Experimenting with the “fixed air” that was found collecting in the fermenting vats (what we now call carbon dioxide), he became a celebrity all over Europe when he found a way of making artificially carbonated water. This work also marked the beginning of his long-term research programme in “pneumatic chemistry”, the chemistry of gases—or “airs”, as he and his contemporaries more commonly conceived them.

Priestley was the most prolific discoverer and manufacturer of new airs. Not long before his work, most people had considered air to be a pure element as much as water, though some isolated observations of certain gases different from ordinary air had been reported before. After his work, there was no doubt left that ordinary air had at least two components, and that different types of air could be produced by various chemical reactions. Priestley’s *Experiments and Observations on Different*

¹ For further details on Priestley’s life and work, see Chang (2012a) and references therein. The most authoritative and extensive biography is Schofield (1997, 2004).

Kinds of Air (1774, 1790) is a sheer delight for those who share a sense of fascination about all the diverse phenomena of nature and a childlike wonder at our own ability to call them forth.

Priestley was the first person to make and bottle what we now call oxygen and tell the wide world about it. In Carl Djerassi and Roald Hoffmann's amusing play *Oxygen* (2001), it is left uncertain who should win the first "Retro-Nobel Prize" in Chemistry for the discovery of oxygen. But those authors, or anyone else well enough informed, would not deny Priestley's priority over Carl Wilhelm Scheele (1742–1786) in publication, and over Antoine-Laurent Lavoisier (1743–1794) in deed. Priestley's excitement is palpable as he reports, in his letter of 15 March 1775 to James Pringle, the President of the Royal Society of London: "the most remarkable of all the kinds of air that I have produced . . . is one that is five or six times better than common air, for the purpose of respiration, inflammation, and, I believe, every other use of common atmospherical air." First he tested this new air by burning things in it. And then, "to complete the proof of the superior quality of this air, I introduced a mouse into it; and in a quantity in which, had it been in common air, it would have died in about a quarter of an hour, it lived, at two different times, a whole hour, and was taken out quite vigorous." (Priestley 1775, 387–388) After that he found the courage to breathe the new air himself. "The feeling of it in my lungs," Priestley reported, "was not sensibly different from that of common air, but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it."

On the new site of the Mill Hill Chapel in Leeds, for which he preached for several years during his scientific heyday, a blue plaque proudly proclaims (Fig. 1.1): "Joseph Priestley, discoverer of oxygen, was minister here 1767–1773." Such commemoration would have annoyed Priestley, for he did not call his new gas "oxygen". He called it "dephlogisticated air", and that was not just a matter of words. By that phrase he really meant common air cleansed of the "phlogiston" that is normally mixed up in it. What was phlogiston? In short, it was the principle of inflammability; "principle" here did not mean a fundamental rule, but rather a fundamental substance that combined with other substances and gave them its characteristic properties. Phlogiston was the principle that imparted combustibility to combustibles. A combustible substance was rich in phlogiston, and when it burned it released its phlogiston, which then manifested itself in the flame that came out.

Certain experiments seemed to indicate that metals, too, were rich in phlogiston, and that it was phlogiston that gave them the characteristic metallic properties, such as their shiny luster, their malleability and ductility, and their electrical conductivity (and their inflammability actually, under the right circumstances). When a metal was deprived of phlogiston, it lost its key metallic properties and became an earthy substance called "calx" (which we would now identify as rust or oxide). All of this sounds much too fanciful to our modern ear. Let us see if we can make the phlogiston-theorist to see some common sense. If calx is really metal that has lost its phlogiston, then you should be able to turn it back into metal by giving some phlogiston to it. Can you do that? "Sure", says the phlogistonist. That is what smelters have been doing for



Fig. 1.1 Memorial plaque to Priestley at the Mill Hill Chapel, Leeds (photo by Hasok Chang)

thousands of years. Take a metallic ore, which often contains calx rather than pure metal, and mix it up with a phlogiston-rich substance, say, charcoal; heat the mixture strongly, to effect a transfer of phlogiston from the charcoal to the calx. And there it comes, the shiny metal! Similar work on the interconversion of sulphur and sulphuric acid by the German physician and chemist Georg Ernst Stahl (1659–1734) was one of the founding experiments of phlogistonist chemistry. This work elicited the admiration of Immanuel Kant, who chose it as one of the three prime illustrations of how empirical science began to grapple with nature in a principled way, in his *Critique of Pure Reason*: when “Stahl changed metals into calx and then changed the latter back into metal by first removing something and then putting it back again, a light dawned on all those who study nature.”²

Similarly, Priestley had produced his oxygen (dephlogisticated air) through a process in which he thought mercury calx was “revivified” into its metallic form by absorbing phlogiston from air. As a result, the air would have been “de-phlogisticated”.

² Kant [1787] (1998), 108–109. This passage occurs in the Preface to the second edition (B xii–xiii). My attention was drawn to this passage by its quotation in Bensaude-Vincent and Simon (2008), 87; they quote from the classic translation by Norman Kemp Smith, who committed an interesting anachronism by rendering the German “Kalk” as “oxide”. Kant’s other examples are Galileo on inclined planes and Torricelli on barometric pressure.

Such an air should be an exceptionally good supporter of combustion, since it would re-absorb phlogiston very eagerly. And so it was, when Priestley tried the experiment. He also reasoned, because respiration was a process in which the phlogiston produced by the workings of the body was removed from the lungs, dephlogisticated air should be particularly good to breathe. And so it was.

One can see why Georges Cuvier quipped that Priestley was a father of modern chemistry, but “a father who never wanted to acknowledge his daughter.”³ Priestley is often seen as a tragic figure, possessed of consummate experimental skill and full of good scientific intentions, but blinded by a dogmatic adherence to an outmoded way of thinking. His chemical misfortune was compounded by political injustice, when a reactionary mob ransacked his house and laboratory in Birmingham in 1791 for his support of the French Revolution, on the second anniversary of the storming of the Bastille. After that he tried a life in London, but in the end only found refuge in America. It was sad but inevitably right, it is said, that his work was swept aside by the tide of scientific progress brought in by Antoine-Laurent Lavoisier, the young urbane Parisian with brilliance and ambition in equal measure. Lavoisier had a different way of accounting for Priestley’s experiments and observations. Combustion was combination with oxygen, as was calcination (turning metal into calx). Where Priestley saw de-phlogistication, Lavoisier saw oxidation. Having seen the light shown by Lavoisier, chemists have never looked back to phlogiston...

Even Thomas Kuhn, who famously refused to say that the losing side in a scientific revolution was simply wrong, was surprisingly downbeat about Priestley. Although he denied that Priestley’s resistance to Lavoisierian chemistry was ever “illogical or unscientific”, Kuhn thought he was “unreasonable” to resist as long as he did; the historian “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, 159). The tale of stubborn old Priestley blinded by phlogistic dogma has captured many people’s imagination, but it is a misleading story on so many levels. By the time you finish reading this chapter, I hope I will have given you some considerable doubt about that version of events. The best way to start seeing the problem is to ask: what was really so wrong with Priestley’s stance? To the end of his life he continued his chemical research and published well-informed and closely reasoned defences of phlogiston (Priestley [1796] 1969, 1803). He continued to find the phlogiston theory a sensible and fruitful framework for understanding new phenomena (such as electrolysis, as I will discuss further in Chap. 2), and there were still no phenomena which in his view clearly refuted the phlogiston theory. And there are similar stories to be told about each of the other brilliant and dedicated scientists who refused to let go of the phlogiston theory, some of whom we shall meet shortly.

³Quoted in the *Encyclopaedia Britannica*, 9th edition, vol. 5 (1876), 462.

1.1.2 Water

A decisive moment in the competition between the oxygen theory and the phlogiston theory was Lavoisier's argument that water was not an element at all, but a compound of oxygen and hydrogen. The irony of the situation is worth noting. No one knew better than Priestley how to make oxygen (dephlogisticated air), and hydrogen (called inflammable air) had been discovered and studied in 1766 by his compatriot and fellow phlogistonist Henry Cavendish (1731–1810), by dropping pieces of metal into acids (see Jungnickel and McCormmach 1999, 202ff). Cavendish also discovered how to explode those two airs together to make water, an experiment which Priestley repeated successfully. It was Priestley, and Cavendish (via his friend Charles Blagden), who taught Lavoisier how to do these things. Still, the common story goes, it was Lavoisier who came up with the correct interpretation of what these gases were and what happened when they reacted with each other.

In fact, there is no better case than the composition of water for illustrating the *surprising* cogency of the phlogiston theory. Cavendish and Priestley both thought for a time that hydrogen, or “inflammable air” as they called it, was pure phlogiston, driven from the metal by the action of acid (the metal thereby turned into calx, and dissolved into the acid to form a salt⁴). If a calx was put into the acid, it dissolved without the production of inflammable air, because the calx did not contain phlogiston. A more considered version of this view served to explain the formation of water. The later Cavendish–Priestley view was that inflammable air was “phlogisticated water”, that is, water containing an excess of phlogiston. As for oxygen, or dephlogisticated air, that was “dephlogisticated water”. When phlogisticated water and dephlogisticated water combined with each other, the excess and deficit of phlogiston cancelled out and plain water was produced. To summarize: there were (at least) two competing views on the formation of water, both of which were cogent and self-consistent:

- (1) Hydrogen + Oxygen → Water
- (2) Phlogisticated water + Dephlogisticated water → Water

The second account here is not a fairy-tale that phlogiston theorists simply manufactured in order to avoid the refutation of their theory by the facts about the composition of water. Cavendish and Priestley had good reasons to think that water was an essential constituent of gases. At the most basic level this idea is suggested by the fact that vapors are produced from liquids. Might the situation not be similar with other kinds of gases, too? Lavoisier ([1789] 1965, ch. 1) also readily agreed that gases were produced from water and other liquids, by the addition of caloric (the matter of heat). Priestley (1788, 154) was entirely comfortable with “the supposition of water entering into the constitution of all the kinds of air, and being, as it were, their proper basis, without which no aëriform substance can subsist”. He noted

⁴ “Salt” here designates a whole class of chemical substances, including common salt and many others.

(p. 152) that he did not know of any ways of producing inflammable air without water, and surmised that “the same may be true of every other kind of air, since water is used in the production of them all.”⁵ Within this framework of thinking, it made perfectly good sense to think that phlogistication or dephlogistication would affect the process of aerification of water, resulting in different types of airs.

Historically well-informed philosophers have struggled to say what exactly was wrong with Priestley’s stance. We must resist the impulse to say “we *know* he was wrong, because phlogiston simply doesn’t exist”, since that only begs the question of how we know *that*. The complaint that it was not possible to isolate phlogiston in its pure form has no force. If we always required such material isolatability, science would look very different, as we would have to renounce a whole range of concepts from quarks to energy. And at the core of Lavoisier’s own theory was *caloric*, the matter of heat, which was not isolatable in its pure form, either. It also won’t do to say that phlogiston was an illegitimate scientific concept because it was unobservable. However one may define “observable”, science to this day is full of unobservable entities that are postulated because of theoretical necessities (dark matter and superstrings come to mind). And it is not clear that phlogiston was not observable; to the phlogistonists, phlogiston was not only observable (in the flame that comes out of combustion, for example), but even directly manipulable (when it was transferred from one substance to another, as in smelting or in the production of inflammable air by the solution of metals in acids). This feeling was evident within the Lunar Society, that remarkable association of scientific people around Birmingham, which included Priestley.⁶ In 1782, as the Chemical Revolution began to move into its last phase, Matthew Boulton, James Watt’s business partner, wrote to the potter Josiah Wedgwood, marveling about Priestley’s new experiment in which a calx turned into metal by “imbibing” inflammable air (hydrogen), which Priestley then considered pure phlogiston: “We have long talked of phlogiston without knowing what we talked about, but now Dr Priestley hath brought ye matter to light. We can pour that Element out of one Vessell into another, can tell how much of it by accurate measure is necessary to reduce a Calx to a Metal” (Boulton quoted in Musgrave 1976, 200).

A related complaint against phlogiston concerns weight. The crude version of the complaint is that phlogiston is an “imponderable” substance (meaning a weightless substance, not an unthinkable one), and therefore it should not be accepted in science. But isn’t today’s physics quite sanguine about weightless particles, such as photons? And in the days of phlogiston, other imponderables were postulated with impunity, such as the electric fluid(s), and not least of all Lavoisier’s caloric. Another version of the complaint focuses on the weight gain in calcination: a metal gains weight in becoming a calx, which would not happen if it were losing something, namely phlogiston. The weight gain is explained very nicely by the oxygen theory,

⁵ Priestley even gave an experimental estimate that “fixed air consists of about half its weight of water”, from his experiment of generating fixed air by passing steam over *terra ponderosa aerata* in a red heat, noting that heating alone did not yield fixed air.

⁶ About the Lunar Society, see Schofield (1963) and Uglow (2002).

in which calx is metal combined with oxygen, hence obviously heavier than the metal by itself. However, this does not quite work as a refutation of the phlogiston theory, as there are ways of accounting for the weight gain. It was not necessary to resort to the much-ridiculed idea that phlogiston had “levity”.⁷ A much more serious explanation, advanced by Priestley and also by Richard Kirwan (c.1733–1812), was that in calcination the metal combined with water, while losing phlogiston. When the calx was reduced back to metal, it gave out the water and absorbed phlogiston. If there was no external source of phlogiston, the calx sometimes took it from the water that it was emitting, which means that what it emitted was dephlogisticated water (oxygen, in Lavoisier’s terms)!

1.1.3 *The Trouble with Lavoisier*

So we can begin to see how it was that Priestley could hold on to his phlogiston theory, in quite a rational way. What is actually more difficult to see is why nearly everyone else should have signed up to Lavoisier’s position, or stuck to it for any length of time. In order to free up our thinking from old clichés about the Chemical Revolution, I must begin by pointing out *just how wrong Lavoisier was, if we judge him from the view of modern chemistry and physics*.⁸ As John McEvoy says (1997, 22–23), it is “a simple fact” that already “by the end of the eighteenth century, almost every major theoretical claim that Lavoisier made about the nature and function of oxygen was found wanting.” Similarly, Robert Siegfried (1988, 35) states that “the central assumptions that had guided his work so fruitfully were proved empirically false by about 1815”. Let us take a closer look. Three major pillars of Lavoisier’s system of chemistry marked clear departures from previous chemistry: the theory of acids, the theory of combustion, and the caloric theory. All three are clearly wrong, from the viewpoint of modern chemistry, or even from the viewpoint of nineteenth-century chemistry. (I will be discussing more lasting parts of Lavoisier’s achievements later.)

Even the most robust Lavoisier-enthusiasts will easily concede that his theory of acids was mistaken. Lavoisier said that all acids contained oxygen, but the Lavoisierians knew as well as anyone that there was no evidence of oxygen in certain acids, including muriatic acid (in modern terms, hydrochloric acid, HCl) and prussic acid (hydrocyanic acid, HCN). As the twentieth-century Oxford chemist Harold Hartley put it (1971, 110), “the rigid acceptance of this doctrine” was “responsible for so much make-believe in chemists’ minds”, including their failure to recognize chlorine as an

⁷ Guyton de Morveau, before he “converted” to the Lavoisierian cause, gave a very reasonable version of this idea, that phlogiston had *relative* levity; since it must be lighter than air, its addition to an object would reduce the object’s *apparent* weight. See passage included in Crosland (1971), 135, from *Dissertation sur le phlogistique* (1770), and also the exposition in Poirier (1996), 62.

⁸For an extended discussion in this vein, see Chang (2009b).

element for 20 years after its isolation by Scheele.⁹ Was this theory of acids just an unfortunate non-essential adjunct to the rest of Lavoisier's system, which could safely be discarded?¹⁰ At least Lavoisier himself didn't think so, as we can glimpse from the way he named his beloved "oxygen", meaning "acid-generator".¹¹

Even more central to Lavoisier's "antiphlogistic" system was his theory of combustion. Surely this undeniably essential bit of Lavoisier's system was correct, and is still preserved in modern chemistry? To grant that would be to participate in an amnesia that pro-Lavoisierian historiography has carefully orchestrated. It is in fact quite incredible that any modern person can think: "Combustion is combination with oxygen, that's what causes the emission of heat and light, and Lavoisier discovered all that." What on earth does oxygen have to do with heat and light? Lavoisier's explanation of the production of heat in combustion was that it was the freeing of caloric fluid from oxygen gas, the caloric that was responsible for the gaseous state of the oxygen in the first place. But it was widely recognized by Lavoisier's contemporaries (and even Lavoisier himself) that there were serious difficulties with this story. Thomas Thomson (1773–1852), the leading Scottish chemist of the period just after the Chemical Revolution, gave a calm and devastating summary of well-known difficulties in his *System of Chemistry* first published in 1802 (vol. 1, 354–358). These difficulties included cases of combustion without oxygen in the gaseous state (such as the explosion of gunpowder), and combustion without any oxygen at all.¹² Thomson judged that "upon the whole, it cannot be denied that Lavoisier's theory does not afford a sufficient explanation of combustion." (p. 358) Thomson was not advocating a return to phlogiston, but he wanted chemistry to move on beyond Lavoisier, less than 15 years after the culmination of the Chemical Revolution.

There was also growing discontent with Lavoisier's caloric theory of heat in more general terms, 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. It is important to remember that caloric in Lavoisier's system was not merely a device for explaining the release of heat in combustion; rather, it was an essential element in his cosmology, for example in explaining the three states of matter. Lavoisier ([1789] 1965, 175) clearly considered caloric a cornerstone of his chemical system, putting it (along with light) at the top of his list of chemical elements (see Fig. 1.2), and devoting

⁹For details on the story of chlorine and its elementary nature, see Ashbee (2007) and Gray et al. (2007), which are the first two chapters in Chang and Jackson (2007). See also Brooke (1980) and Crosland (1980).

¹⁰It was certainly possible to accept the rest of Lavoisier's theory without accepting the theory of acids. Even Claude-Louis Berthollet, one of Lavoisier's closest associates, seems to have done so, as Homer Le Grand (1975, 69) argues. Ramón Gago (1988) states that this was a widespread attitude in Spain.

¹¹William H. Brock (1992, 125) goes as far as to suggest: "Once Lavoisier had the concept of a gas, it was the issue of acidity, not combustion, that led him to oxygen—as its very name implies."

¹²See also Mauskopf (1988), 93–118.

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

Fig. 1.2 Lavoisier's table of simple substances; the *left-hand column* gives Lavoisier's neologisms, and the *right-hand column* gives the corresponding old terms (from the original French edition of 1789, p. 192)

the entire first chapter of his definitive textbook of new chemistry to the elucidation of the nature and role of caloric. It was a beautiful and sensible system, but only as much as the phlogiston theory was. Both theories are equally wrong, from the modern point of view.

Recognizing that the debate was not a simple matter of truth and falsity, various philosophers and historians of science have attempted to meet the challenge of

explaining why the vast majority of chemists went over to Lavoisier's side. Alan Musgrave's classic paper on the subject discusses and dismisses several common attempts (1976, 182–186). It is not the case that Lavoisier's new chemistry was established by induction from observations. It is equally wrong to say that the phlogiston theory was simply falsified by facts like the increase of weight in metals when they are oxidized (or, calcined). What may be more appealing but equally fallacious is the notion that Lavoisier's theory won because it was inherently simpler than the phlogiston theory. The crudest version of this idea, which Musgrave calls "simplicism" (or conventionalism) in his critique, says that the phlogiston theory unnecessarily complicated things by postulating an unobservable substance, phlogiston; this argument ignores the fact that Lavoisier had to postulate an equally unobservable substance, caloric.

After dismissing these explanations, Musgrave proposes that the crucial factor was that the oxygen research programme was more progressive, with "progress" as defined in Imre Lakatos's philosophy. Musgrave argues that it was rational for chemists to abandon the phlogiston theory because after a certain point it stopped making successful new predictions; it only continued to make *ad hoc* hypotheses—excuses made up in order to protect a failing theory. Unfortunately, I do not think this Lakatosian explanation works, as I will explain in more detail in Sect. 1.3.1. Musgrave states: "Between 1770 and 1785 the oxygen programme clearly demonstrated its superiority to phlogistonism: it developed coherently and each new version was theoretically and empirically progressive, whereas after 1770 the phlogiston programme did neither." (p. 205) I find this claim difficult to substantiate, attractive as it is. The idea that the progress of the phlogiston program ended in 1770 (while the oxygen program continued in its smooth progress) is contradicted by Musgrave's own statements earlier in the paper (p. 199): "While Lavoisier was failing, Priestley was having great success with the 1766 version of phlogistonism. . . the most impressive experiment of all came in early 1783." This was the confirmation of the phlogistonist prediction that calxes would be reduced to metals by heating in inflammable air. In order to sustain Musgrave's thesis convincingly, we need to find successful novel predictions that Lavoisier's research program made after the phlogiston program stopped making any, and that is 1783, not 1770. Where are these predictions? Are we thinking of the prediction that the oxidation of inflammable air (hydrogen) would produce an acid?¹³ Or the prediction that muriatic (hydrochloric) acid would be decomposed into oxygen and the "muriatic radical" (see Fig. 1.2, second group)?

After the failure of all these attempts to argue for the rationality of the consensus in favour of Lavoisier, the story gets more involved. Two common lines of thought are often pursued: defend the presumed rationality of the Chemical Revolution by an appeal to a complicated version of simplicism, or shift attention away from the traditional philosophical concern with rationality by seeking social explanations. I will consider these lines of thought in Sects. 1.3.1 and 1.3.2, and why I do not follow

¹³ Again, Musgrave (1976, 199–200) himself points out this failed Lavoisierian prediction, and how long Lavoisier had struggled with it.

them will already be clear by then from the statement of my own position given in the second section of this chapter (Sect. 1.2). For now, however, let me pursue the implication of the conclusion that I am going to reach after the full deliberation: *the phlogiston theory was prematurely killed*.

1.1.4 *Could Water Be an Element?*

The burden of judgment is a responsibility to act: if I really believe that the phlogiston theory was discarded prematurely, I must consider what could have been achieved by keeping it. My main aim is not the counterfactual history of “what if” (see Sect. 1.3.5 for more on this issue). I am ultimately advocating a more activist type of scholarship, in which we actually open up the possibility of reviving the unjustly discarded line of thinking, and see what comes of it. What I seek is a complete view: I want to know what contributions to scientific knowledge the phlogiston theory did make, what contributions it could have made if it had been kept longer, and what contributions it could still make if it were revived. If all of those categories of contributions have been lost or missed because of the premature abandonment of phlogiston theory, then we should recover, imagine and create them. If you would object that such an enterprise is neither history nor philosophy nor real science, so be it: I call it “complementary science”, as I explained in my previous book (Chang 2004, esp. ch. 6). I aim to give a novel function to history and philosophy of science, without denying its traditional functions. And of course I am not proposing to do away with oxygen or the tradition of modern chemistry descended from Lavoisier, even if such a thing would be possible. No, the whole enterprise is a pluralistic one, as I will be explaining further in Sect. 1.2.4, and in Chap. 5 in more general terms.

There are various questions we need to ask. (1) Was there any knowledge that scientists lost when they rejected the phlogistonist system? (From here on I will say “system” instead of “theory”, in order to emphasize that there is more than just theory involved.¹⁴) In other words, was there something good that the phlogistonist system did that the oxygenist system could not do? Kuhn thought that there was typically such knowledge-loss when a scientific revolution happens; this has been dubbed “Kuhn loss” in his honour.¹⁵ (2) Was there any knowledge that could have come from keeping the phlogistonist system, whose development was delayed or prevented because of its demise? (3) Was there a beneficial effect of having both phlogistonist and oxygenist systems present, in terms of what was produced by interactions between them? (4) Would there have been further beneficial

¹⁴ This seems consistent with the usage of the time. There were various textbooks of chemistry that were called “Systems”, for example Thomson (1802), which I have already mentioned. Other examples included the *Système des connaissances chimiques* (1801) by Lavoisier’s colleague Antoine Fourcroy. I will give a more thorough definition of a “system of practice” in Sect. 1.2.1.1.

¹⁵ This is a term coined by Heinz Post, according to Ioannis Votsis.

interactions between the oxygenist system and the phlogistonist system, if the latter had been maintained?

I will attempt to answer these questions in some detail in Sect. 1.2.4, but here are some preliminary thoughts to stimulate your imagination. Although a lot of the explanations of chemical phenomena given in the phlogistonist system were successfully co-opted into the oxygenist system, that was not always possible. For example, the phlogiston theory gave a nice explanation of why all the metals had similar properties, and very little about that could be said in Lavoisier's theory. The identification of phlogiston with (negative) electricity was a promising avenue of thought shut down by the demise of phlogiston. While phlogiston theorists are often faulted for not having paid sufficient attention to the balancing of weights in chemical equations, phlogiston did serve as a useful reminder that not everything in chemistry could be explained by weights. The eminent English chemist William Odling opined in 1871 that phlogiston had been a clear precursor of chemical potential energy. These ideas should at least give us a pause, and plenty of food for thought.

My idiosyncratic view of the Chemical Revolution will make many people uncomfortable. If the rejection of phlogiston theory was premature and unwarranted, so was the rejection of the idea that water is an element. But surely water is not an element? If you believe something so outlandish, the whole weight of modern science will crush you. But could there really not be a sensible system of science in which water is an element? The easy answer is: "Yes, and it was called the phlogistonist system"—or, really, every scientific system before the Chemical Revolution itself. The tougher question, however, is whether there is a system of science based on elementary water that we, here and now, could plausibly believe, or at least work with and profit from. In considering this question we must, again, resist the temptation to say "of course *we* now *know* that water is a compound of hydrogen and oxygen; any theory that says otherwise is simply false, and not worth considering". We only think like this because we are prisoners of a scientific worldview which is founded on premises such as the compound nature of water. The truly pluralistic challenge is to ask whether we could step outside that worldview and find another one that is not premised on the compound nature of water; and, if there is one, whether there would be any benefit in developing it. As Léna Soler puts it (2008, 230), following Ian Hacking (2000), the general question is whether well-established scientific results are inevitable or contingent: is it "possible for there to be a science that is . . . as successful and progressive as ours but radically different in content"?

A terminological insight, trivial in itself, will make this prospect seem infinitely more plausible. When we talk about "elements" in modern science, for example when we say that oxygen is a chemical element, we do not mean ultimately simple bodies that cannot be broken down any further, or even bodies that have not been broken down yet. Such simple-minded views of chemical elements had to be rejected in later science—just like the view that water was an element and could not be broken down any further. According to modern science the atoms and molecules of oxygen, hydrogen, or water—each and every chemical substance—is made up of a number of other, simpler particles, such as neutrons, electrons, and protons. If we take modern physics really seriously, even an electron is not a "simple" particle; it

is not a “particle” at all in its ordinary sense, but a packet of energy exhibiting wave–particle duality, or not even that, but some fluctuating state of the quantum field. Never mind that—the point is that “Water is an element” is really only about as wrong as “Oxygen is an element”. So there is no reason to feel so desperately apologetic in considering the potential merits of a theory that includes elementary water, any more than in considering the merits of a theory that includes elementary oxygen. The real question is what “element” means in a system of chemistry, in terms of its functions. When Priestley and Cavendish maintained that water was an element, they clearly meant it as a substance that could be *modified*, by the addition or subtraction of phlogiston, for example. We can certainly consider whether there are potential insights we can gain from that way of thinking.

Let us take the case of water in the Chemical Revolution as a reminder of the poverty of single vision. The tale of phlogiston illustrates how actual and potential knowledge can be lost if we insist on having only one way to pursue it.

1.2 Why Phlogiston Should Have Lived

Having raised some doubts and questions concerning the outcome of the Chemical Revolution, I will now give a full and free exposition of my views, as I will do in Section 2 of each chapter. Here I assume a fair amount of background knowledge (some of which was provided in Sect. 1.1), and discuss matters with little regard to disciplinary boundaries or worries about objections that might come from various specialists; afterwards, Sect. 1.3 will deal with anticipated objections and try to position my views with respect to existing literature. I will begin, in Sect. 1.2.1, with a systematic appraisal of the evidential situation in the choice between oxygen and phlogiston. The verdict of that appraisal will be along the lines already indicated in Sect. 1.1: there were no conclusive reasons of evidence to reject the phlogiston theory in favor of Lavoisier’s theory. This raises a question of historical explanation: why, then, did chemists make that unjustified choice? Section 1.2.2 will argue that chemists at the time did not actually reach a simple, rapid and universal agreement in favor of Lavoisier. Still, admitting that a clear majority did eventually reject phlogiston, Sect. 1.2.3 will attempt to explain why that collective decision was reached, by reference to a larger and longer-term trend underlying the Chemical Revolution, namely the advent of “compositionism”. Providing an explanation does not mean advocating what one has explained. After all the evaluations and explanations, Sect. 1.2.4 will take a better-informed look at the fate of phlogiston, and ask what benefits might have followed from retaining it in chemistry.

1.2.1 *Phlogiston vs. Oxygen*

Was there sufficient scientific justification for the rejection of phlogiston-based chemistry in favor of Lavoisier’s oxygen-based chemistry? Was there enough

evidence to support that decision?¹⁶ My sense is that there has not yet been a truly systematic assessment of this matter. Now, that is a grandiose claim to make about a subject that has been studied so thoroughly, so I need to explain what I have in mind more carefully. First of all, much of the existing historical literature on the Chemical Revolution is not concerned with the questions of justification. And most of the philosophical analyses have been carried out from particular perspectives, each of which neglects or obscures certain aspects of the situation, as I will explain further as I go along. So I will begin by outlining a more commodious framework of analysis.

1.2.1.1 Evaluating Systems of Practice

At least in the Anglophone traditions, philosophical analyses of science have been unduly limited by the common habit of viewing science as a collection of propositions, focusing on the truth-value of those propositions and the logical relationships between them. The premier subject of discussion in philosophy of science has been *theories* as organized bodies of propositions. This has led to the neglect of experimentation and other non-verbal and non-propositional dimensions of science in philosophical analyses. Many historians, sociologists and philosophers have pointed out this problem, but so far no clear alternative philosophical framework has been agreed upon to provide a language for fuller analyses of scientific practice. A serious study of scientific practice must be concerned with what it is that we actually *do* in scientific work. This requires a change of focus from propositions to actions. I begin with the recognition that all scientific work, including pure theorizing, consists of actions—physical, mental, and “paper-and-pencil” operations, to put it in Percy Bridgman’s terms (1959, 3). Of course, all verbal descriptions we make of scientific work must be put into propositions, but we must avoid the mistake of only paying attention to the propositional aspects of the scientific actions.

I propose to frame my analyses in terms of “systems of (scientific) practice” that are made up of “epistemic activities”.¹⁷ (I will also use the phrase “system of knowledge” interchangeably with “system of practice”, especially where it seems there is no danger of forgetting that knowledge is rooted in practice.¹⁸) An *epistemic activity* is a more-or-less coherent set of mental or physical operations that are intended to contribute to the production or improvement of knowledge in a particular way, in

¹⁶ What exactly “evidence” means is, of course, a serious philosophical question, which I will address as I go on. For the time being I rely on an intuitive notion that seems sufficiently widespread and coherent, of epistemic justification that a theory receives from observations or facts.

¹⁷ I have made preliminary attempts to spell out these notions in Chang (2011a, d), from which I draw here. Some further details are provided in those papers.

¹⁸ In my relatively informal discussion in Sect. 1.1, there was already a felt need to consider more than just theories, and I began to use the term “system”, following the contemporary chemists who often spoke of the “phlogistic system” and the “antiphlogistic system” (or the “French system of chemistry”).

accordance with some discernible rules (though the rules may be unarticulated). An important part of my proposal is to keep in mind the aims that scientists are trying to achieve in each situation. The presence of an identifiable aim (even if not articulated explicitly by the actors themselves) is what distinguishes activities from mere physical happenings involving human bodies, and the coherence of an activity is defined by how well the activity succeeds in achieving its aim. Common types of epistemic activities include measurement, prediction, and hypothesis-testing. Some epistemic activities are primarily mental. There is such a thing as theoretical practice, and in chemistry it consists of activities such as classification, equation-balancing, and the modeling of molecular structures. In reality, most epistemic activities are both mental and physical at once. When we start thinking of scientific work as a collection of activities, an immediately obvious thing is the sheer variety in the types of epistemic activities that scientists engage in. Here is a partial list of types of epistemic activities: describing, predicting, explaining, hypothesizing, testing, observing, detecting, measuring, classifying, representing, modeling, simulating, synthesizing, analyzing, abstracting, idealizing.

Epistemic activities normally do not, and should not, occur in isolation. Rather, each one tends to be practiced in relation to others, constituting a whole system. A *system of practice* is formed by a coherent set of epistemic activities performed with a view to achieve certain aims. For instance, Lavoisier created a system of chemistry whose activities included collecting gases produced by chemical reactions, measuring the weights of the ingredients and products of reactions, combusting organic substances for analytical purposes, and classifying compounds according to their compositions. The overall aims of this system included determining the composition of various substances, and explaining chemical reactions in terms of the composition of the substances. Similarly as with the coherence of each activity, it is the overall aims of a system of practice that define what it means for the system to be coherent. The coherence of a system goes beyond mere consistency between the propositions involved in its activities; rather, coherence consists in various activities coming together in an effective way toward the achievement of the aims of the system. Coherence comes in degrees and different shapes, and it is necessarily a less precise concept than consistency, which comes well-defined through logical axioms.

It may seem difficult to make a sharp distinction between an epistemic activity and a system of practice, and this is intentional. The distinction is only relative and context-dependent. In each situation in which we study a body of scientific practice, I am proposing to call the overall object a *system*; when we want to study more closely more specific aspects of that system, we can analyze it into different subordinate *activities*. What we take as a whole system in a given situation may be seen as a constituent activity of a larger system, and what we see as a constituent activity in a given situation may in a different situation be analyzed as a whole system made up from other activities. (When I initially introduced the notion of epistemic activity two paragraphs ago, I used the expedient of saying that an activity was made up of operations; the terminology of “operation” is strictly speaking unnecessary, though it is convenient when we want to keep *three* levels of description in view all at once.)

In this way, my framework is applicable at all levels, and can be zoomed in and out to suit any level that we want to focus on. At each focus-point, we call the overall practice “system” and its constituents “activities” (and their constituents “operations”), without intending to stick those categorical labels to anything on a permanent basis.

I do not propose to go deeply into the metaphysics of action, but it will be useful to stress the non-reductive nature of the relationship between the levels of description here. The structure of actions and processes is not atomistic in a reductive way, unlike the structure of things and statements.¹⁹ Each epistemic activity can itself be analyzed as a system of activities, but the “component” activities are not necessarily simpler than the “whole” activity in an absolute sense, and the analysis can go on indefinitely. For example, take the combustion-analysis of a chemical substance. This can be analyzed as consisting of various other activities: burning the target substance; absorbing the combustion-products using other chemicals; weighing with a balance; making percentage-calculations; etc. And those component activities in themselves consist of other activities; for example, the activity of weighing with a balance consists in placing samples and weights on balance-pans, reading the number off the scale, etc. Now it may seem that we are getting to simpler and simpler activities as we continue in our analysis of actions, hopefully to reach a rock-bottom of atomic operations. But we are forgetting something less convenient. The activity of weighing with a balance also includes a certification-activity, to support our assumption that what we are handling are the correct standard weights; without that, the whole activity of weighing with a balance becomes incoherent. This certification-activity may consist in ordering the weights from a reliable supplier, or comparing them to a more trusted set of weights, or checking them against certain natural phenomena (e.g., the weight of a certain volume of water at a certain temperature). Whichever option we go with, it is clear that this component activity is not simpler in any clear sense than the main activity of weighing with a balance. The relation between various epistemic activities is ultimately non-reductive and reticular, although in many situations we can gain useful insights from analyzing an activity into its apparent components. There is no lowest level of description, and no clear end to the process of activity-analysis. Rather, the analysis should be carried out wherever, and as far as, it is productive.

Before I begin to apply the concepts of epistemic activity and system of practice to the case at hand, a few more general remarks will help clarify the framework further. First, the focus on activities and their aims throws new light on the old philosophical preoccupation with propositions and their truth. Of course propositions are important in science, including observation statements, empirical laws and theoretical principles. However, what is really important is to understand how they function within various epistemic activities. Evaluating the correctness of statements

¹⁹ There are, of course, some reasons to be skeptical about the atomistic–reductive structure of things and statements, too, but that is a story for another day.

is surely one of the most important epistemic activities; however, the standards of correctness can easily vary from system to system (see Chap. 4, Sect. 4.3.1 for further reflections on this issue). If we are talking about “Truth with a capital *T*” that is not at all dependent on the system of practice one works in, it is doubtful that there are any actual scientific activities that are concerned with it. The success of each activity or system needs to be judged first of all in terms of how well it achieves the aims that it sets for itself; in addition, we may make judgments on the value of the aims themselves. But there will be precious few occasions on which “Truth” is an operable aim or standard of judgment in science.²⁰

Finally, it will be instructive to draw a comparison-and-contrast between systems of practice and some other notions already current in the literature of the history and philosophy of science. The most obvious comparison is the Kuhnian concept of a paradigm. Despite some clear similarities, I think there are also some significant differences, sufficient to warrant the articulation of a new concept. As Kuhn freely admitted, he used the term “paradigm” in two main senses.²¹ The first, of an “exemplar”, does not match my sense of “system”. The second sense, the “disciplinary matrix”, is akin to my concept, but for two main reasons I do not find it helpful. First, I think we need a concept that is more definite and orderly than the Kuhnian disciplinary matrix, which incorporates all kinds of elements ranging from fundamental metaphysical principles to institutional structures, with no definite indication of how the whole thing holds together. Hacking’s (1992, 44–50) characterization of laboratory practice also has a similar difficulty, specifying 15 different kinds of elements that enter into experimental practice without elaborating how they combine and interact with each other. In addition, Kuhn’s paradigm concept is too closely tied to his insistence that a paradigm does and should enjoy a monopoly over an entire scientific discipline in normal phases of science; as explained further in Chap. 5, I have both descriptive and normative objections to that presumption of monopoly. Another interesting comparison is with John Pickstone’s “ways of knowing”, which I think are *types* of systems of practice (or, system-types). A *system-type* is a class consisting of various systems all of which share some core activities. A system-type is not an actual system, not even a very general one, but an incomplete characterization of a system with blanks consciously left to be filled in (and similarly for activity-type, a notion I used briefly above). Depending on how the blanks are filled, we have different instantiations of the type. A system-type defines a class of systems consisting of these instantiations. I will have occasion to make use of this concept in Sect. 1.2.3.2 (for further details see Pickstone 2000, 2007; Chang 2011d).

²⁰ This is not to deny the value of Truth as a regulative principle—or, less grandiosely, a rhetorically or motivationally effective purpose.

²¹ This explication occurs in the 1969 Postscript included in the second edition of *The Structure of Scientific Revolutions* (Kuhn 1970, 180–191).

1.2.1.2 Problem-Fields

Having clarified my framework of analysis, let me come to the task at hand: to compare the relative merits of the phlogistonist²² and oxygenist systems of chemistry as they existed in the late eighteenth century. In attempting to answer these questions, it is important to keep a clear head about *whose* judgment we are considering. There are various viewpoints to consider: what each of the competing sides in the dispute thought, and what we ourselves (various people) as historiographical and philosophical inquirers think. Which of these judgments we focus on depends on our purpose in making the inquiry. My present purpose is to assess whether there were good reasons for abandoning the phlogistonist system, not only whether past scientists themselves thought so. Therefore, I must give my own judgments in the end, though it would be silly to ignore the historical actors' judgments completely. The significance of that distinction will become clearer as I proceed with my concrete assessment.

I will try to be *comprehensive* in my appraisal, because what we have seen so far indicates that there never was a simple isolated “clincher” or a fatal flaw on either side that settled, or should have settled, the argument. That means there were probably a number of important factors relevant to the decision, all of which need to be considered carefully and weighed up against each other. So let us start by asking, very generally: what were the important scientific problems facing the phlogistonist and oxygenist systems, and how well did each system solve them? Answering these questions will require that we address two further questions: by which standards, or epistemic values, should the solutions to those problems be evaluated; and how exactly should each epistemic value be applied in the concrete situations under consideration? All of these questions were raised very clearly by Kuhn (1970, 1977) in his discussions of paradigm-choice in the face of incommensurability (see Sect. 1.3.3 for further thoughts on incommensurability in the Chemical Revolution).

In *The Structure of Scientific Revolutions* it was one of Kuhn's main points regarding incommensurability that different paradigms have different lists of problems they consider legitimate and important.²³ This most indisputable element of incommensurability often got swept aside in the excited discussions about its other aspects,²⁴ and it seems that Kuhn himself abandoned the concern with problem-fields in the reference-based discussions of incommensurability that he gave later in life. But I do not see how we can even start a reasoned debate about theory-choice or paradigm-choice without identifying the problem-fields recognized by the

²² I should say “phlogistic” to be more faithful to contemporary usage, but I prefer “phlogistonist”, which works well in parallel with “oxygenist” (rather than “oxygenic” or some such formulation to fit with “phlogistic”). My terms conform well enough to standard usage among historians, as can be seen in the discussion of the “phlogistonists” in Brock's standard-setting *Fontana/Norton History of Chemistry* (1992, 78).

²³ On the early Kuhn's emphasis of this aspect of incommensurability, see Hoyningen-Huene (1993), 208–209.

²⁴ Among the philosophical commentators on the Chemical Revolution, Andrew Pyle (2000, 104) is one of the exceptional people who stress the importance of this factor.

Table 1.1 The division of the problem-field in the Chemical Revolution

Problems considered important by both sides	Problems considered (very) important only by phlogistonists	Problems considered (very) important only by oxygenists
Understanding of combustion, calcination/reduction, and respiration	Explaining properties of compounds (including metals) in terms of properties of ingredients	Theory of heat and changes of state
Theory of acids	Mineralogy; geology	Chemistry of salts
Constitution of various substances	Meteorology Nutrition; ecology	

competing sides. Theories are not simply “tested against evidence”; we must always choose *where* they ought to be tested against evidence—which is to say, where we most wish them to be empirically successful. The focus on problems is particularly congenial to a pragmatist view of knowledge, which will inform my discussion in Chaps. 4 and 5. For now, it is sufficient to recall John Dewey’s (e.g., 1938) inclination to analyze knowledge in terms of processes of *inquiry*.

Table 1.1 shows the division of the problem-field in the Chemical Revolution. It is important to have this laid out in front of us, as that was literally the battlefield of the Chemical Revolution. Some problems were commonly acknowledged to be important by both sides, and others were considered important by only one side. Both the oxygenist system and the phlogistonist system assigned great importance to the understanding of three processes commonly recognized as closely related to each other: combustion, calcination, and respiration. The theory of acids was also something that both sides considered important and debated seriously.²⁵ Closely related to these issues was the *constitution* of various substances including water, metals and calxes, and various kinds of “airs”, and also of various non-metallic substances that had been considered to be phlogiston-rich. Kirwan (1789, 6–7) thought that constitution was the key to the whole dispute: “The controversy is therefore at present confined to a few points, namely, whether the *inflammable principle* [phlogiston] be found in what are called phlogisticated acids, vegetable acids, fixed air, sulphur, phosphorus, sugar, charcoal, and metals.” Similarly, in his latter-day defence of the phlogiston theory published from his exile in Pennsylvania, Priestley ([1796] 1969) focused almost exclusively on constitution—of metals and water most of all, but also of carbon, nitrogen and fixed air. In these works Kirwan and Priestley were responding to Lavoisier’s contention that he had refuted the phlogistonist conceptions on the constitution of these substances.

In contrast to the above problems, some others were not universally considered important. The phlogistonists shared a widespread preoccupation of pre-Lavoisier

²⁵ See Brock (1992), 125. Richard Kirwan’s classic exposition of the phlogiston theory was titled “An Essay on Phlogiston and the Composition of Acids”. In that text, Kirwan (1789, 38) considered that Lavoisier’s work on acidity was an important contribution: “With respect to the nature and internal composition of acids, it must be owned that the theory of chemistry has been much advanced by the deductions and reasonings of Mr. Lavoisier.”

chemistry, which was “to account for the qualities of chemical substances and for the changes these qualities underwent during chemical reactions.” (Kuhn 1970, 107)²⁶ More specifically, phlogistonists sought to explain the key properties of many substances in terms of the “principles”, particularly phlogiston, that entered into their composition (see Sect. 1.2.3.2 for more on this). One salient case was the explanation of why metals (which were compounds for phlogistonists) had a set of common properties (Kuhn 1970, 148). Actually by the onset of the Chemical Revolution this was no longer a research problem in the phlogiston paradigm, as it was accepted almost as common sense that metals had their common metallic properties (including shininess, malleability, ductility, electrical conductivity) because of the phlogiston they contained.²⁷ The oxygenist side seems to have rejected not so much this answer as the question itself; chemistry reclaimed this stretch of territory only in the twentieth century. There were also some other problems neglected by the oxygenists that were considered important by some phlogistonists, although not all phlogistonists were equally concerned about them. These included various problems in mineralogy, geology, meteorology, and the circulation of nutrients in the environment.

On the other side, there were some problems considered much more important by oxygenists than by phlogistonists. Thermal phenomena were noted by all chemists and various phlogistonists tried to give some account of the nature of heat, but it was Lavoisier, building on Joseph Black’s work on latent heat, who really brought heat (caloric) centrally into chemistry. Closely related to the theory of heat were questions regarding what we now call changes of state, about which Lavoisier had a very definite theory, to which he gave a very prominent place in his system of chemistry.²⁸ A similar case is the chemistry of salts. This was a common preoccupation of eighteenth-century chemistry, but the phlogiston theory had relatively little to offer here. In contrast, this area of research held out much promise for the new chemistry on the basis of Lavoisier’s notion that “oxygen formed the glue or bond of dualistic union between acid and base to form salts”.²⁹ Lavoisier displayed his enthusiasm for this line of research by devoting an entire third of his *Elements of Chemistry* to it.³⁰

Having surveyed the problem-field of the Chemical Revolution, we can now tackle our main question: how do the performances of the two sides compare? First

²⁶ We shall see in Sect. 1.3.4 how Lavoisier himself was quite pre-Lavoisierian in this regard!

²⁷ See Kirwan (1789), 168.

²⁸ See the very first chapter of Lavoisier ([1789] 1965); Black’s ideas were widely known but only published posthumously (see Black 1803). Some individual phlogistonists were also deeply concerned about heat and changes of state (e.g., Cavendish on boiling and evaporation), but these thoughts did not tend to affect their chemical theories. An apparent exception to the last statement is Jean-André De Luc, whose opposition to Lavoisier’s chemistry seems to have been motivated by his ideas regarding the theory of rain; however, De Luc’s chemistry was idiosyncratic and it is tricky whether to classify him as a phlogistonist, though he was clearly against Lavoisier.

²⁹ Brock (1992), 216.

³⁰ This is part 2 (of 3) of Lavoisier’s text ([1789] 1965, 173–291), with the title “of the combinations of acids with salifiable bases, and of the formation of neutral salts.”

of all, can we say which side was focusing on more important problems? My own judgment is that all of the problems listed in Table 1.1 above were important and worthwhile. Now, when we make such judgments, we ride roughshod over the historical actors' judgments of what was important. Historians will tend to be wary of such judgments, but it is not one we can avoid. Philosophers wishing to make an epistemic assessment certainly cannot avoid it, and even historians must judge what is worth writing about. Simply going along with the historical actors' judgments is not a solution, especially in cases like this where important scientists at the time disagreed with each other. The most pernicious option is to go simply with the historical winner's judgment.³¹

Accepting that all the problems considered important by either side were important (at least *prima facie*), we can ask how well each side did in solving them. Rather predictably, and just as Kuhn said, each side tended to provide good solutions to the problems that it considered important, and not such good solutions to other problems; that does not give us much of a basis for preferring one side to the other. It does seem to me that there were a larger number of important problems dealt with by the phlogistonist system but not by the oxygenist system, than conversely. Other things being equal, that circumstance would tend to recommend the phlogistonist system over the oxygenist system. Possibly more important is whether either side did clearly better in solving the common problems, which both sides recognized as important. In the Chemical Revolution there was no shortage of common problems, but each side thought it was doing quite well on all of them, and rather better than the other side. Proponents of the two systems offered substantively different solutions to common problems, and understood each other quite well, but disagreed on the evaluation of the relative qualities of those solutions. This brings us to the question of the criteria of judgment, or, the epistemic values that were in operation in the debates.

1.2.1.3 Divergent Epistemic Values

The phlogistonist and oxygenist systems gave very different solutions to most of the problems they shared, and they differed sharply in their judgments of who gave better solutions to these problems. Here we come into a different dimension of incommensurability, which has one important source in divergent epistemic values. Let us examine this situation more closely.

The most important factor was an opposition between *simplicity* and *completeness*. Oxygenists, particularly Lavoisier himself, prized simplicity greatly—especially the kind of simplicity that one could describe as elegance. Phlogistonists, particularly Priestley, saw more importance in completeness, wanting to account for all the observed phenomena in a given problem-area and for all the observed aspects of those phenomena. Lavoisierians liked to focus their attention on paradigmatic cases in which their theoretical conceptions worked out beautifully, setting the messier cases aside. In contrast, Priestley and some of his fellow phlogistonists endeavored

³¹ For my reasons against aligning our judgments with the historical winners', see Chang (2009b).

to explain all the major phenomena they produced and observed, even if the explanations got cumbersome in the more difficult cases. It is not that either side failed to recognize the desirability of either simplicity or completeness, but there were clear differences in the degree of emphasis, or preoccupation, with those competing values.

A good example to illustrate this point is the calcination and reduction of metals. The red calx of mercury, which Priestley had initially used to produce dephlogisticated air, was taken up by Lavoisier and his colleagues as the paradigm case showing that calcination and reduction were processes of oxidization and de-oxidization. Mercury could be turned into this red calx by heating in ordinary air; the calx could then be turned back into metal simply by a higher degree of heat (produced by a large burning lens), yielding oxygen and producing or absorbing no other detectable substances. This wonderful exhibition of oxidization and reduction was cited over and over by the Lavoisierians. Priestley protested ([1796] 1969, 24): “But this is the case of only this particular calx of this metal”. In his view, the Lavoisierians were distorting the whole picture by focusing on one exceptional case. Other metals behaved differently; Priestley (p. 31) pointed out that no calx of iron could be revived “unless it be heated in inflammable air, which it eagerly imbibes, or in contact with some other substance which has been supposed to contain phlogiston.” Even for mercury, there was another type of calx³² “which cannot be completely revived by any degree of heat, but may be revived in inflammable air, which it imbibes, or when mixed with charcoal, iron-filings, or other substances supposed to contain phlogiston.” (p. 24)

This divergence between simplicity and completeness also played an important role in the debates regarding combustion. The difficulties of Lavoisier’s theory of combustion, which I discussed briefly in Sect. 1.1, deserve some detailed attention here, especially as they tend to be neglected even in well-informed historical treatments. As background, recall that Lavoisier understood combustion as involving a *decomposition* of oxygen gas into “oxygen base” and caloric, the oxygen base combining with the combustible substance, and the caloric being released. The sensible heat generated in combustion came from the oxygen gas, and it was essential that the oxygen enabling combustion was in a gaseous state to begin with, since it was the abundance of combined caloric which put a substance into the gaseous state. The production of light in combustion was explained in a similar way, though more vaguely.

Let me return to Thomas Thomson’s summary of objections to Lavoisier’s theory of combustion (Thomson 1802, vol. 1, 354–358).³³ Thomson reckoned that, following Lavoisier’s view, “one would naturally suppose, that when the product [of combustion] is a gas, all the caloric and light which existed in the oxygen gas would be necessary for maintaining the gaseous state of the product”. But, for example, when charcoal is burned the product is a gas, yet the combustion still yields a great deal of heat and light. According to Thomson, Lavoisier was aware of this problem, but did not provide a convincing solution. Thomson also noted a converse problem: “One would

³² Priestley explained that this was “that which remains after exposing turbith mineral to a red heat”; the modern editor of his text adds that “turbith mineral” is “basic mercuric sulphate”.

³³ See also the discussion of Thomson’s arguments by Partington and McKie (1937–1939), 340–342, and by Morris (1972).

naturally suppose that in every case of combustion the oxygen employed must be in the state of a gas. But this is very far from being the case". For example, "a very rapid combustion" takes place when nitric acid is poured on certain oils, but oxygen only enters that reaction in a liquid state, not a gaseous one. Or consider the explosion of gunpowder, which happens without the help of ambient oxygen gas, the oxygen being present in the solid state in the nitre (saltpeter) contained in the gunpowder itself. Thomson also pointed out that from Lavoisier's theory "one would naturally expect that caloric and light would be emitted during the condensation of other gases as well as oxygen: but this never happens unless oxygen be concerned." For example, when hydrogen and nitrogen gases combine, there is no heat or light emitted; ammonia gas and hydrochloric acid gas combine to make a "concrete salt", producing very little heat and no light. Thomson also noted that there was an emission of a good deal of caloric and light (i.e., combustion, to all appearances) in some reactions that did not involve any oxygen (nor any gases) as reactants: for example, when sulphur combines with certain metals, and when phosphorus and lime combine with each other.

Such problems did not move Lavoisier and his colleagues to modify their doctrines, not to mention abandon them. It is not that Lavoisier and his colleagues were *blind* to anomalies. For example, as Seymour Mauskopf (1988) relates in illuminating detail, Lavoisier was very interested in the chemistry of gunpowder, and made a few different attempts to explain its workings in terms of his theory. (Lavoisier's interest in gunpowder is no surprise after all, as he was a commissioner of the Royal Gunpowder Administration from 1775 and in that capacity set up his residence and laboratory at the Paris Arsenal.³⁴) These attempts were not very successful, and no less than Claude-Louis Berthollet (1748–1822) used the gunpowder case against Lavoisier's theory of combustion, citing precisely the difficulty that Thomson later reported. Lavoisier was himself never quite satisfied with the clever yet clumsy defence he was able to give, although Berthollet was pacified on this point after his "conversion" to Lavoisier's system for other reasons.³⁵ After that stumble in the middle of Paris, the Lavoisierian theory of combustion marched on and out, unaltered. Thomson reported that Luigi Valentino Brugnatelli (1761–1818) had solved some of the difficulties with his concept of "thermoxygen", which is oxygen that combines with other substances while retaining its light and caloric. This troubled Thomson as a blatantly *ad hoc* move, but it was a way for Lavoisierian theory to keep its main doctrine while addressing an important anomaly; such ideas were entertained by Lavoisierians when necessary, but they were not admitted to the core of their system.³⁶

³⁴ Guerlac (1975), 65–66.

³⁵ Mauskopf (1988, 110–111) highlights Berthollet's objection, and relates Lavoisier's ultimate discontent (p. 115). See Le Grand (1975) on Berthollet.

³⁶ As Lakatos said, fluctuations in the "protective belt" consisting of auxiliary hypotheses do not and should not affect the "hard core" of a research programme. It is interesting to note that Brugnatelli's solution here was probably only a rehash of the idea that Lavoisier himself had come up in order to fend off Berthollet. In fact, as Partington and McKie (1937–1939, 341–342) point out, Berthollet reminded Thomson that this issue had been addressed by Lavoisier. See Mauskopf (1988, 113–114) for an explanation of Lavoisier's work on this.

Thomson's conclusion on Lavoisier was clearly negative: "Upon the whole, it cannot be denied that Lavoisier's theory does not afford a sufficient explanation of combustion." Nor was Thomson alone in this sort of judgment. Numerous other chemists who accepted that oxygen combined with combustibles remained skeptical of Lavoisier's explanation of the heat and light in combustion; these "late phlogistonists" often maintained a system in which oxygen and phlogiston happily co-existed, the latter still being given the role of explaining what we would now identify as the energy relations in combustion (see Sect. 1.2.2 for further details).³⁷ But Lavoisier was more concerned about maintaining the simplicity of his theory than fitting all the known facts. Later celebratory historiography of the Chemical Revolution has nearly managed to obliterate this aspect of the story from collective memory.

In addition to simplicity and completeness, there were also broader types of epistemic values at play. A sort of epistemic conservatism was one of the values upheld by many phlogistonists, while oxygenists were taken with the idea of reform or novelty in itself. There is an interesting passage from Cavendish illustrating this point (1784, 152): "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."³⁸ Cavendish's temperament shown here is surely an informative contrast to Lavoisier's youthful enthusiasm declaring to himself in 1773 that his investigations were "destined to bring about revolution in physics and chemistry", before he had published even his first attack on phlogiston.³⁹

On the other hand, it cannot be that the leading phlogistonists were simply opposed to scientific change, as they surely delighted in making new discoveries and crafting some new theoretical ideas, too. Many arguments made on behalf of phlogiston were motivated not by conservatism but by *pluralism*, in reaction against Lavoisierian dogmatism. This is quite contrary to the common notion that the phlogistonists were blinded by dogma. In Sect. 1.2.2 I will say more about this, but for now Priestley will serve as an emblem of the scientific pluralism found on the phlogistonist side. His 1796 defence of phlogiston is a moving testimony in this regard. Declaring that "free discussion must always be favorable to the cause of truth", he reminded the reader of the non-dogmatic path he had walked in science:

No person acquainted with my philosophical publications can say that I appear to have been particularly attached to any hypothesis, as I have frequently avowed a change of opinion, and have more than once expressed an inclination for the new [Lavoisierian] theory, especially that very important part of it *the decomposition of water*. (Priestley [1796] (1969), 21)

³⁷ See Partington and McKie (1937–1939), part 4; also Allchin (1992).

³⁸ Cavendish added that there was one other consideration in addition to this prudence or conservatism: "it is more reasonable to look for great variety in the more compound than in the more simple substance", in relation to plants and their composition.

³⁹ Lavoisier quoted in Donovan (1988), 219. Donovan cautions against reading Lavoisier's "revolution" to mean what we now commonly associate with the term; my current point is not affected by the precise meaning of "revolution".

He dedicated his book to “the surviving answerers of Mr. Kirwan” (the post-Lavoisier leaders of French chemistry, namely Berthollet, Laplace, Monge, Guyton de Morveau, Fourcroy and Hassenfratz), and requested an answer from them to his objections to the oxygenist system. Priestley drew an ominous parallel between the politics of science and the larger politics that had put a premature end to Lavoisier’s life at the guillotine in 1794, for his part in the business of tax-collecting: “As you would not . . . have your reign to resemble that of *Robespierre*, few as we are who remain disaffected, we hope you had rather gain us by persuasion, than silence us by power.”⁴⁰

I do not think that all this was retrospective self-fashioning or a loser’s spiteful plea for survival. Priestley had expressed similar epistemic views even at the height of his fame and success. For example, in the 1775 letter in which he announced the discovery of dephlogisticated air (oxygen), Priestley wrote (1775, 389):

It is happy, when with a fertility of invention sufficient to raise *hypotheses*, a person is not apt to acquire too great attachment to them. By this means they lead to the discovery of new facts, and from a sufficient number of these the true theory of nature will easily result.

This passage directly followed his proposal that “nitrous acid is the basis of common air, and that nitre is formed by a decomposition of the atmosphere”, to which he added: “But I may think otherwise to-morrow.” One can almost hear an echo of Montaigne finishing his thoughts with “though I don’t know”.⁴¹ In contrast, there was a clear absolutist impulse on the oxygenist side, perhaps most egregiously manifested in the ceremonial burning of Stahl’s phlogistonist text. As described by Justus Liebig (1851, 25), this was “a festival in which Madame Lavoisier, robed as a priestess, committed to the flames on an altar, while a solemn requiem was chanted, the phlogistic system of chemistry.”

What are *we* to conclude, having seen these arguments from both sides? My sense is that Lavoisier did clearly fail in giving a complete account of combustion. Does that mean his theory was simply wrong, and therefore its pleasing simplicity was immaterial? Or, to be a bit more subtle: shouldn’t we say, following Bas van Fraassen’s view of theory-assessment (1980, 87), that Lavoisier’s theory of combustion was not empirically adequate, and no amount of simplicity, which is only a pragmatic virtue, could redeem it? The matter is not so straightforward, because empirical adequacy comes in bits and pieces (in various problem-areas treated by each theory), and moreover comes in degrees in each little bit and piece. The discomfort of a strict hierarchy of values between empirical adequacy and the pragmatic virtues becomes clear when we ask whether a tiny advantage in empirical adequacy would

⁴⁰ Priestley [1796] (1969), 17–18. He signed off his dedication with unflagging loyalty to the cause of the French Revolution: “I earnestly wish success to the arms of France, which has done me the honour to adopt me when I was persecuted and rejected in my native country. With great satisfaction, therefore, I subscribe myself Your fellow-citizen, Joseph Priestley.”

⁴¹ See Bakewell (2010), 43, and Chapter 7, for a nice exposition of this aspect of Montaigne’s thought. Perhaps there is a fruitful comparison to be made between Montaigne’s *Essays* and Priestley’s *Experiments and Observations on Different Kinds of Air*. A Cartesian quest for certainty is clearly eschewed in both.

not be outweighed by a great deal of additional simplicity or other virtues.⁴² The problem is worsened when we recognize that empirical adequacy itself is not a single-valued variable in any case; how do we weigh up an advantage in empirical adequacy in one area against a disadvantage in another area? All I can say, at this stage, is that we cannot say anything definitive about the oxygen–phlogiston choice, at least until we have taken in a comprehensive view of all the relevant problems and all the epistemic values relevant in evaluating the solutions.

Where different values jostle with each other, the epistemologist is in uncomfortable territory. How are we to say which of the values cherished by different historical scientists were more valuable? Do we have any right to make such judgments? In my view, this is not a question of rights, as we are not going to be *doing* anything to the past actors themselves by our judgment. Rather, I think the question is about the present, and I believe that we have a *duty* to ourselves to make such judgments. As philosophers of science, or as any responsible citizen who considers matters of knowledge, we need to make judgments on the basis of epistemic values in the present. Then it is impossible to avoid applying these judgments in some ways to the past, just like it is impossible to keep our present ethical values entirely out of our studies of history in general. (It will not do to say that we have no right to judge whether a past act, say a genocide, was right or wrong.) Conversely, it is important to remain alert to the judgments we do have about epistemic values operative in past science, because they will affect our judgments about present science. What we celebrate and condemn in past science, however implicitly or subtly, cannot remain safely separate from how we deal with present science. So it is not irrelevant to ask questions such as whether simplicity or completeness was the superior virtue in the phlogiston–oxygen choice. My honestly biased way of posing the question would be: which attitude was (and is) more rational or scientific, between adapting theories to new phenomena that we learn about, and giving dogmatic dominance to a favoured theory?

1.2.1.4 Divergent Instantiations of the Same Value

Having addressed different weights given to different epistemic values by the opposing sides in the Chemical Revolution, we must now examine a further complication raised by Kuhn (1977, 331), that even one and the same epistemic value may be interpreted and instantiated in divergent ways, leading to very different conclusions and even mutual accusations of betrayal of the value in question. There are a few significant cases of divergent value-instantiation in the Chemical Revolution.

Both sides valued unity, and each side cited the kind of unity it was able to achieve as persuasive evidence in its own favor. There was some convergence in this, since both systems unified combustion, calcination and respiration in similar ways. But beyond that there was significant divergence in what was unified, and

⁴² In Chap. 5 I will come back more strongly to the question of whether empirical adequacy really ought to be considered a value that is more important than others.

how. Lavoisier's caloric theory linked together explanations of combustion and of changes of state. His ideas on oxygen connected combustion and acidity, since many combustion-products were acidic. On the phlogistonist side, there was a pleasing theoretical unity concerning the behavior of metals: their common properties, their calcination/reduction, and their reaction with acids. The phlogiston theory was also more conducive to a grand unity of all imponderable substances, as manifestations of "elementary fire": phlogiston, electricity, light, magnetism, and so on (for the connections between phlogiston and electricity, see Chap. 2, and Sect. 1.2.4.1).

Similarly but even more strikingly, the adherence of both sides to the value of *systematicity* is manifested in each side accusing the other of being arbitrary and haphazard. From the phlogistonist side, the accusation against the Lavoisierians was that they failed to adhere to the rule of assigning like causes to like effects. Both Priestley ([1796] 1969, 33) and Kirwan (1789, 281–282) used this argument in constitutional debates, to combat the oxygenist refusal to recognize the common presence of phlogiston in various substances. For his part, Lavoisier had a clear disdain for the continual complications and mutually conflicting changes that various phlogistonists introduced to their theories in their attempts to meet the challenges posed by various new phenomena:

Why, therefore, need we have recourse to an hypothetical principle, the existence of which is ever supposed, and has never been proved; which in one case must be considered as heavy, and in another as void of weight, and to which, in some cases, it is necessary even to suppose a negative weight; a substance which in some instances passes through the vessels, and in others is retained by them; a being which its maintainers dare not rigorously define, because its merit and its convenience consist even in the uncertainty of the definitions which are given of it?⁴³

Now, as I will discuss further in Sect. 1.3.1, Lavoisier's statement is misleading to the extent that it implies that each phlogistonist or each version of the phlogiston theory held these mutually contradictory beliefs about the nature of phlogiston; rather, there were different versions of the phlogiston theory, in some cases successive in time, that did not agree with each other in all their details. (My own comparative assessment here is between Lavoisier's system and the best versions of the phlogistonist system.) Still, Lavoisier was making a reasonable demand that the phlogiston system as a whole should be developing in a more systematic way. In contrast to Lavoisier, no leaders on the phlogistonist side had the will or the wherewithal to make everyone on their side sing from the same hymn sheet.

An examination of the arguments on both sides also reveals their common allegiance to what I will call "empiricism": a commitment to avoid invoking extraneous hypotheses, to stay close to observable facts and ideas derived from those facts. In the passage quoted above, Lavoisier and his colleagues denounced phlogiston as a hypothetical entity whose existence was "ever supposed, never proved" (unashamed of their own *lumière*, *calorique*, and *radical muriatique*). In their own theory, they

⁴³ This passage occurs in a report to the Paris Academy by Lavoisier, Berthollet and Fourcroy, which Lavoisier quotes in his comments on Kirwan's treatise on phlogiston (1789, 15). See also Lavoisier (1786).

claimed, “nothing is admitted but established truths”; theirs was “a doctrine which explains all the facts of chemistry without any supposition.”⁴⁴ Most phlogistonists were no less adamant about their empiricism. I have already quoted Priestley as saying that he did not have strong attachments to hypotheses, and regarded them mostly as means for eliciting new facts.

Having reviewed those divergent value-instantiations, the question of our own judgment raises its head again: which do we think was the more valuable kind of unity, the more useful kind of systematicity, and the more genuine kind of empiricism? In each case, I cannot see any irrefutable way to argue that one was more important than the other. That is my honest judgment, although it may raise a worry about not being able to close the debate.

1.2.2 *What Really Happened in the Chemical Revolution?*

The evidential assessment made in the last section leaves us with a very uncertain verdict. It seems clear that each of the oxygenist and the phlogistonist systems had its own merits and difficulties, and that there were different standards according to which one or the other was better supported by empirical evidence. In a way, this is only an indication that evidential support is not a straightforward matter of logical or probabilistic connections between theory and observation, but a complex relationship mediated by epistemic values, which can be divergent and contextual. My own judgment is that both systems were partially successful in their attempts to attain worthwhile goals, and that there was no reason to clearly favour one over the other (I will say more about the implications of that judgment in Sect. 1.2.4). But if there was no clear justification for the choice of oxygen over phlogiston, then why *did* chemists make that choice? Why did the Chemical Revolution happen? In Sect. 1.1 I only touched briefly on this question. One reason I did not enter into a full discussion of it is that I think philosophers and historians have wasted a lot of time and energy by tackling an illusory question. There is great futility in the enterprise of explaining why the vast majority of chemists quickly went over to Lavoisier’s side—because actually that is not quite what happened. I would like to set out a different and more accurate story of the Chemical Revolution, before I launch into further discussions of its causes, its rationality, and its consequences.

There is an extensive literature on the Chemical Revolution, which I will not be able to survey with any comprehensiveness here; instead I refer the reader to John McEvoy’s (2010) up-to-date and thorough critical review of this literature. My own aim here is to advance a particular revisionist thesis: the Chemical Revolution did *not* consist in a swift and nearly universal conversion of the chemical community to Lavoisier’s theory. I have made that argument more fully elsewhere (Chang 2010), so I will present a shorter summary. First of all we need to resist being taken in by triumphalist declarations emanating from Lavoisier, from his contemporary advo-

⁴⁴ This passage is from the preface to the French translation of Kirwan’s treatise on phlogiston: Kirwan (1789), xiii.

cates, and from his posthumous glorifiers. But declarations of a clean victory can be found in some quite unexpected places, too. For example, take the opening sentence of Priestley's 1796 defence of the phlogiston theory: "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] 1969, 1) Perhaps that was just an exaggerated lament from the loser,⁴⁵ but strangely the same idea can also be found in the works of some very expert historians, such as Robert Siegfried (1989, 31): "Of all the well known revolutions in the history of science, the chemical is perhaps the most dramatic . . . Only 20 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." The impression of unanimity is also voiced by Larry Holmes (2000, 751): "all but Priestley himself eventually came over to the side of the French chemists". McEvoy notes (2010, 18–19): "the suddenness, brevity and pace of the Chemical Revolution" as some of the key factors which "marked it in the minds of many commentators as arguably the best example of a classic revolution in the history of science." Similar ideas can be found in the most careful of the philosophical commentators, too: Andrew Pyle (2000, 105) judges that "by 1800, all its [phlogiston theory's] defenders had effectively been swept away", and Alan Musgrave (1976, 205) says that by 1796 "the Chemical Revolution was over".

A reasonably close look at the primary literature from about 1790 onward, however, should make it evident that there were numerous chemists who declined to jump on the Lavoisier bandwagon, whom I will call "anti-anti-phlogistonists". Although Lavoisier was sanguine enough to declare by 1790 (in a letter to Benjamin Franklin) that "a revolution has taken place in an important area of human knowledge", he knew full well that there were still many who were unconverted, especially in Germany and Britain; even the French chemists were still "divided", as he acknowledged in the same letter.⁴⁶ Many of the anti-anti-phlogistonists were respectable and respected chemists, not old men driven by sheer conservatism or dogmatism. There were at least three different types of dissenters in the period *after* the publication of Lavoisier's *Elements of Chemistry* in 1789, which is often seen as the point at which the Chemical Revolution was irreversibly consolidated (see Table 1.2).

First of all, there were indeed some die-hards. Priestley tops this list, but he is only a small part of the picture. Scheele did not survive long enough for his "die-hard" credentials to be tested truly, but up to his death in 1786 he showed no sign of relinquishing the phlogiston theory. One of the most striking figures is Jean-André De Luc, whose objection was based on his theory of rain postulating the transmutation of atmospheric air into water, as well as his general dislike of the rash and the revolutionary in science and politics alike (e.g., De Luc 1803). De Luc maintained

⁴⁵ Or Priestley may have felt disheartened by the rather sudden change of heart by some of the German phlogistonists in the mid-1790s (for this, see Hufbauer 1982).

⁴⁶ Quoted in Guerlac (1975), 112; Donovan (1993), 184.

Table 1.2 Varieties of anti-anti-phlogistonists, in the order of birth in each category

Die-hards (“elderly holdout”, some not so elderly)	Fence-sitters	New anti-Lavoisierians
James Hutton (1726–1797)	Pierre-Joseph Macquer (1718–1784)	Benjamin Thompson, Count Rumford (1753–1814)
Jean-André De Luc (1727–1817)	Henry Cavendish (1731–1810)	George Smith Gibbes (1771–1851)
Antoine Baumé (1728–1804)	Georg-Christoph Lichtenberg (1742–1799)	Thomas Thomson (1773–1852)
Johann Christian Wiegleb (1732–1800)	Lorenz Crell (1745–1816)	Johann Wilhelm Ritter (1776–1810)
Joseph Priestley (1733–1804)	Claude-Louis Berthollet (1748–1822)	Humphry Davy (1778–1829)
Torbern Bergman (1735–1784)	Johan Gadolin (1760–1852)	
James Watt (1736–1819)	Friedrich Gren (1760–1798)	
Balthazar-Georges Sage (1740–1824)		
Carl Wilhelm Scheele (1742–1786)		
Jean-Claude Delamétherie (1743–1817)		
Jean-Baptiste Lamarck (1744–1829)		
Adair Crawford (1748–1795)		
Johann Friedrich Westrumb (1751–1819)		
Robert Harrington (1751–1837)		

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.⁴⁷ In 1796 Priestley identified the Lunar group as the only remaining adherents to phlogiston that he knew of, in addition to Adair Crawford, who had just died (Priestley [1796] 1969, 20); he may have been unaware of Robert Harrington, who would go on to issue a “death warrant” of French chemistry (1804). On the German side, Karl Hufbauer (1982, 140–144) 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). Even right there in Paris there

⁴⁷ Middleton (1965, 115–131) gives a discussion of De Luc’s theory of rain. See De Luc (1803), 1–306, for his detailed objections to the new chemistry, first in itself and then in relation to meteorology. For broader contexts of De Luc’s work, see Heilbron (2005) and Tunbridge (1971).

remained significant anti-Lavoisierian figures, including Jean-Claude Delam  therie, 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; Arthur Donovan (1993, 174) considers him to have been “the most determined and effective of Lavoisier’s French opponents”.⁴⁸ 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, 62) also adds Antoine Baum   and Balthazar-Georges Sage.

The second category of dissenters sought compromise, or deliberate neutrality. Allchin, in his aptly titled paper “Phlogiston After Oxygen” (1992), 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 classic series of papers on the phlogiston theory (1937–1939, 125–127, 143–148), 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 (1982) study of the German chemical community in the eighteenth 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 Pierre-Joseph 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.⁴⁹ There were many others who clearly saw some merit in Lavoisier’s chemistry but did not consider it sufficient to reach a clear verdict in favor of it. As discussed above, Cavendish (1784, 150–153) 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, 239–241) explains how Georg Christoph Lichtenberg made a strong case that there was not enough knowledge yet for a decisive verdict even in the 1790s, 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. Donovan (1993, 168) articulates the Lavoisierian gambit as follows: “His strategy [in “R  flexions sur le phlogistique”] was to force the reader to choose between two alternative theories he offered as the only possible choices”; thereby he “attempted to score a knockout, a *coup de science*.”

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, from the year 1800. William Herschel (1738–1822) 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

⁴⁸ On Delam  therie’s opposition to Lavoisier, see Guerlac (1975), 105–106.

⁴⁹ On Macquer, see Holmes (2000, 752); on Berthollet, see Le Grand (1975).

Banks (1743–1820) 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.” 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.”⁵⁰ 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 on in our usual historiography. What did Banks have in mind, exactly? It’s impossible to be sure, but we can make a fairly good guess. I have already discussed the difficulties of Lavoisier’s theory of acidity, his theory of combustion, and his theory of heat. Some of Lavoisier’s new-generation critics were located in Banks’s London at the time, including Humphry Davy and Count Rumford, in addition to old dissidents like Cavendish and De Luc.

Perhaps the most interesting case of the new generation of anti-Lavoisier chemists was Humphry Davy (1778–1829). A mere lad of about 10 when Lavoisier’s *Elements of Chemistry* was published, Davy was one of those who grew up with the Lavoisierian orthodoxy but came to reject it upon further consideration. He made his name not only with laughing gas and electrochemistry (more on the latter in Chap. 2), but by putting a nail in the coffin of Lavoisier’s theory of acids with his argument that chlorine was an element and that muriatic acid (hydrochloric acid) did not contain oxygen, only hydrogen and chlorine.⁵¹ After the acceptance of Davy’s work, Lavoisier’s oxygen theory of acidity fell clearly out of favour among chemists. Along with Rumford, Young and Cavendish, Davy was also one of those who mounted serious challenges to the Lavoisierian caloric theory of heat, whose dominance was never total.⁵² As Siegfried (1964) reports in some detail, Davy actually entertained various systems of chemistry involving the revival of phlogiston. David Knight remarks (1978, 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 Sir 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.⁵³ Another who entertained such expectations was the

⁵⁰ Banks to Herschel, 24 March 1800, and Herschel to Banks, 26 March 1800, both quoted in Lubbock (1933), 266–267.

⁵¹ See Gray et al. (2007).

⁵² 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 really a thing of the past.

⁵³ See Golinski (1992, 213), who calls Gibbes “perverse” for this.

1720 1730 1740 1750 1760 1770 1780 1790 1800 1810 1820 1830 1840 1850

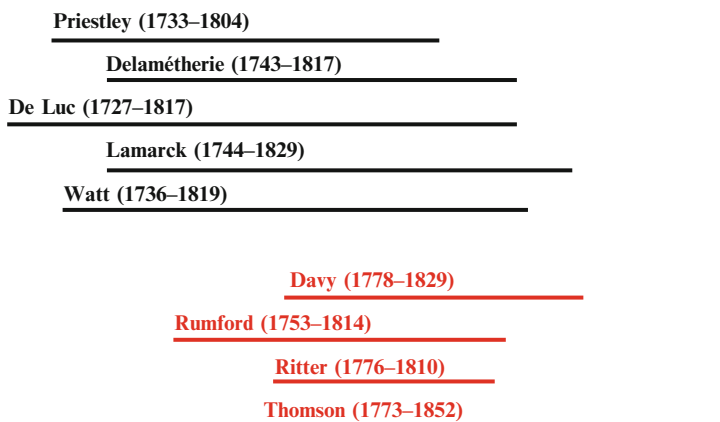


Fig. 1.3 The overlap between old and new anti-phlogistonists (salient figures from the first and third groups from Table 1.2)

young Michael Faraday (1791–1867), who wrote in 1812, citing Davy as an authority: “I would wish you not to be surprised if the old theory of Phlogiston should be again adopted as the true one”.⁵⁴

Taking all of that into consideration, what can we say the Chemical Revolution really consisted in? We still have to admit that a considerable number of chemists did become fully “converted” to Lavoisier’s chemistry at least for a time,⁵⁵ 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 conversions, in many cases with a retention of phlogiston. 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 those 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 Fig. 1.3). Knight (1978, 29) actually understates the case when he says, in reference to a couple of later episodes: “As had happened with gothic architecture, this phlogiston survival was almost contemporaneous with the phlogiston revival”.⁵⁶ Phlogiston was never truly and successfully revived, and there are indeed many senses in which Lavoisier and his colleagues brought about a “revolution” in chemistry, but it was not a sudden and clear-cut affair. It was a many-sided struggle that neither ended in unanimous agreement nor established any immutable orthodoxy.

⁵⁴ Michael Faraday to Benjamin Abbott, 11 August 1812, in James (1991), 17.

⁵⁵ See McCann (1978) for a thorough count.

⁵⁶ Knight refers to Stevenson (1849) and Odling (1871); the latter work will be discussed further in Sect. 1.2.4.

1.2.3 *Weights, Composition, and Chemical Practice*

Having modified the description of the manner in which the oxygenist system triumphed over the phlogistonist system, I now return to the task of explanation. Even in my revisionist history it is the case that a clear majority of chemists did eventually abandon phlogiston, and that fact remains to be explained. (If you are inclined to exclaim “It was all social!”, see Sect. 1.3.2.) In short, my answer is that the victory of oxygen over phlogiston only came as part of a much larger development, namely the slow and steady ascendancy of what I will call “compositionism” in chemistry.

1.2.3.1 The Importance of Weight

There is something of crucial importance about the Chemical Revolution that we have not fully considered yet. Many well-informed historians and chemists have argued that Lavoisier’s most important contribution to chemistry was his emphasis on weight: his recognition of its importance, and his use of precision measurements to trace it through chemical changes. According to this view, oxygen was not really the center of Lavoisier’s chemical system, much as Lavoisier himself was enamored with it; rather, it was a consistent focus on weights that made the new system superior to the phlogistonist system. It does seem that what oxygenists considered the most decisive set of arguments against the phlogistonist system was based on the consideration of weights in chemical reactions. In contrast, phlogistonists were not so centrally concerned with weight, though some of them did recognize it as a relevant property and even knew how to measure it extremely well.⁵⁷ Where did this difference come from, and how did the difference on weight-related issues affect the evidential status of the competing systems? The focus on weight did not enter much into the discussion in Sect. 1.2.1, because it is not really an epistemic value. An attempt to puzzle out what sort of thing it is will bring us to a whole other dimension of evidential reasoning.

One can get a stark view about which side was right in the Chemical Revolution, if one shares Lavoisier’s preoccupation with weight and his conception of weight as a conserved quantity. Take the decomposition and recomposition of water. Lavoisier’s view is crystal-clear: we take 100g of water, and we make 15g of hydrogen and 85g of oxygen out of it (I am using Lavoisier’s own figures here)⁵⁸; and then, we can

⁵⁷ Kirwan devoted the first chapter of his book on phlogiston (1789) to the consideration of weights. Cavendish was unsurpassed in the precision measurements of all things, including weights. Bergman even made quantitative analyses of metals for their phlogiston contents (Brock 1992, 180).

⁵⁸ These numbers were given in Lavoisier’s commentary on Kirwan’s work (Kirwan 1789, 16). I should note that his pre-Revolutionary “g” was “grain”, not the metric “gram” that he actually helped to put into place.

put those *precise* amounts of hydrogen and oxygen together, and make 100g of water again. What better proof than this could one have for the idea that water is a compound made up of hydrogen and oxygen? In contrast, consider the phlogistonist story: hydrogen is meant to be “phlogisticated water” and oxygen “dephlogisticated water”. Cavendish and Priestley thought that water was the base of all gases, but gave no good story about why phlogistication should make water less dense than dephlogistication does, or a precise measure of how much phlogiston went into the part of water that became hydrogen (or came out of the part that became oxygen). One can easily see the force of Lavoisier’s account.

We must not be over-hasty in our judgment, however. Did chemists during the Chemical Revolution have good reasons to accept Lavoisier’s weight-based arguments about the constitution of certain key substances? This question is crucial, because weight-based arguments regarding constitution provided perhaps the only evidence available at the time that could have rationally compelled the phlogistonists to give up. Priestley and some others clearly rejected the weight-based arguments. Why did they do so, and were they wrong to do so? Lavoisier’s reasoning given above rested on two very significant assumptions:

- (a) Weight is a good and proper measure of the amount of all chemical substances.
- (b) Weight is conserved.

If these assumptions were accepted, then there would be a compelling case for the use of weights as the main source of evidence for constitutional ideas. But was there good evidence to support assumptions (a) and (b) themselves?

In considering that question, we must start by losing the prejudice that weight is *of course* the important thing to focus on, and that weightless things do not belong in good chemistry. Assumption (a) did not hold universally even within Lavoisier’s own chemistry, since the first two in his list of simple substances, light and caloric, did not have weight. Assumption (b) is also tricky. Contrary to common intuitions, there never was any deep metaphysical reason why weight must be conserved (and indeed, $E = mc^2$ proclaims that it is not). Of course, not all measurable quantities are conserved. Some turn out like temperature; temperature values cannot be added up to each other meaningfully, and they are not conserved. In any case, Priestley and some others were not ready to accept the precise weight-conservation claimed by Lavoisier in the decomposition and recombination of water. This was particularly problematic given that the force of Lavoisier’s argument was meant to derive from the rigorous precision of his experiments. William Nicholson (1753–1815), who did accept Lavoisier’s system, gave some detailed reasons to doubt the claimed degrees of accuracy in Lavoisier’s weight measurements. This was one aspect of Lavoisier’s work that made him annoyed and doubtful:

I must beg leave to observe, that these long rows of figures, which in some instances extend to a thousand times the nicety of experiment, serve only to exhibit a parade which true science has no need of: and, more than this, that when the real degree of accuracy in

experiments is hidden from our contemplation, we are somewhat disposed to doubt whether the *exactitude scrupuleuse* of the experiments be indeed such as to render the proofs *de l'ordre demonstratif*.⁵⁹

For those with modern knowledge, it is nearly impossible not to sympathize with Nicholson here, in the face of Lavoisier's utter conviction that 85:15 was the correct oxygen–hydrogen ratio in water, rather than anything like 8:1.

1.2.3.2 Compositionism vs. Principlism

Let us recap the argument given so far: if one accepts the primacy of Lavoisier's accounting of weights, then there is indeed very strong empirical evidence for the oxygenist system over the phlogistonist system; but why should one accept the primacy of weight considerations? At the core of that question is whether and why one should accept assumptions (a) and (b) above. We have seen that they are not self-evident. Nor were they proven by direct empirical evidence. Rather, they were *presumed* by Lavoisier and his followers, and used as part of the fabric of the experimental practices by which they produced empirical evidence. The tracking of chemical changes by means of weight was a key epistemic activity within the oxygenist system, and it cohered well with many of Lavoisier's other practices. But it did not fit well into the phlogistonist system, although it was not entirely absent there. The arithmetic of chemical weights arose within a tradition of chemical knowledge that I call *compositionism*, which is to be contrasted to the *principlism*⁶⁰ underlying the phlogistonist doctrines. Each system incorporated a significant metaphysical doctrine about the fundamental ontology of chemical substances, which differed from each other sharply. I am going to argue that an increasing tendency toward compositionism created a climate that was mostly congenial to the oxygenist system and distinctly unfavorable to the phlogistonist system. I believe that this was the most important factor responsible for the oxygenist victory.

In identifying the compositionist and principlist traditions, I am drawing from the well-established work of many historians of chemistry, especially Siegfried, although the exact terminology is my own.⁶¹ (I have given a previous exposition of these ideas (Chang 2011d), which I develop slightly here.) When I say “traditions” above I am using the term in a loose sense. To be more precise within my own

⁵⁹ This is from Nicholson's preface to Kirwan (1789), xi. Jan Golinski (1995) notes the same point and explores the contexts for such claims of precision.

⁶⁰ The term “principalist” has been used in some secondary sources, but I think it is more correct to spell it as “principlist”, as we are referring to principles, not principals.

⁶¹ See, for example, Siegfried and Dobbs (1968), Siegfried (1982), Klein (1994), and Siegfried (2002). Klein (1996) clearly demonstrates the interaction between the experimental and the theoretical dimensions.

analytical framework, I should say that these traditions are instances of what I characterized as *system-types* in Sect. 1.2.1.1. The phlogistonist system was a particular instantiation of principlism, and the oxygenist system was a particular instantiation of compositionism. (However, as will be explained further in Sect. 1.3.4, the situation is more complicated than that, since there were some significant principlist elements in Lavoisier's system and some compositionist elements in the phlogistonist system, too.)

A fundamental epistemic activity of the compositionist system-type was describing chemical substances as either elements, or compounds made up of those elements.⁶² In addition, there were the more experimental activities of decomposing compounds into their elements, and recomposing them from those elements. When one could do both decomposition and recomposition,⁶³ that was regarded as the best proof of the presumed composition of a substance. These practices required the presumption that the components were stable units that are preserved through chemical reactions. That presumption also grounded the activity of explaining chemical reactions as the rearrangement of distinct and stable building-blocks which retain their identity throughout even when their properties are not manifest in a state of combination.

For those who have been educated in modern science, compositionism may seem like either common sense or a necessary condition of chemistry itself. So it helps to know what alternatives there were to compositionist chemistry, and the main eighteenth-century contender was principlism. This is a system-type formed around the concept of *principles*, namely fundamental substances that impart certain characteristic properties to other substances. The defining epistemic activities in principlism were: classifying substances according to observable properties; explaining the properties of substances by reference to principles; and effecting transformations of substances by the application (or withdrawal) of principles. Like compositionism, principlism was a system-type that had many instantiations, which all shared these three core activities. It is important to note that the principlist ontology presumed an asymmetry between principles and the other substances that are transformed by them, principles being active and the others passive. There are some old echoes in principlism, including the old metaphysics of the substratum of elements modified by the influence of principles, even of matter being given form. It may make sense to trace principlist chemistry back to the Aristotelian notion of the mixt,

⁶² As explained in Chang (2011d), compositionism constitutes an analytical “way of knowing” in John Pickstone’s terminology, because it rests on a commitment to “reduce complex . . . objects to configurations of elements” (Pickstone 2007, 494). Also implied in Pickstone’s characterization of the analytical way of knowing is the activity of laboratory analysis, or better, decomposition: applying various methods to take a substance physically apart into its constituent parts.

⁶³ These are terms used by Lavoisier himself; see, for example, his remarks quoted in Kirwan (1789), 16. I avoid the terms “analysis” and “synthesis” here, because “analysis” could also mean the detection of substances without actual decomposition, and “synthesis” came to take on other significance later.

which Duhem attempted to revive philosophically at the start of the twentieth century (see Bensaude-Vincent and Simon 2008, 125). Or we may indeed trace principlism through the alchemical notion of the transmutation of substances; Priestley once did write Benjamin Franklin: “You will smile when I tell you, that I do not absolutely despair of the transmutation of metals.”⁶⁴ However, perhaps the instance of principlism most vivid to the modern historians of science is the phlogistonist system of chemistry, originating from Johann Becher (1635–1682) and Georg Ernst Stahl (1660–1734) and practiced up to the time of Lavoisier, most famously by Priestley.

It is not that chemical operations in themselves could unequivocally be classified as principlist transformation or decomposition–recomposition; that would be to assume that the experimental practices were completely non-theoretical. Many chemical operations were easily amenable to both interpretations; and this is not a mere matter of armchair interpretation, since the conceptions did affect the actual experiments, too. In this vein it is interesting to note an early debate regarding the cogency of the practice of analyzing substances by the application of fire: some had compositionist confidence that fire would only effect decompositions, while others showed principlist worries that the fire would transform the substances being analyzed (Debus 1967; Holmes 1971).

How did compositionism become dominant? Various historians of chemistry have written instructively about the origin of compositionism, tracing it back to experimental practices of decomposition–recomposition as well as ideas about the “elective affinities” between various chemical substances. As Siegfried (1982, 2002) and Klein (1994, 1996) have explained in detail, the origin of compositionist chemistry goes back at least to the mechanical philosophers of the seventeenth century, becoming fully operative by the late eighteenth century. Robert Multhauf (1962, 1996, chs. 14–16) has elucidated the connections between the origin of compositionist concepts and the development of certain processes in metallic and heavy-chemicals industries. Larry Holmes (1971) and Allen Debus (1967) have stressed the linkage with methods in analytical chemistry. The origin of compositionism was multifarious, and its dominance was the result of the embedding of various practices that occurred at glacial speed. In this ascendancy of compositionism, principlist thinking gradually lost its appeal as it conflicted with the building-block ontology of compositionism, in which all pieces of matter had equal ontological status. It is probably fair to say that the firm establishment of compositionism in chemistry occurred with the eighteenth-century doctrine of affinity, epitomized in Etienne-François Geoffroy’s affinity table published in 1718.⁶⁵ Geoffroy’s system of chemistry

⁶⁴ Priestley to Franklin, 13 February 1776, quoted in Schofield (2004), 124. Schofield also quotes Franklin’s witty reply, dated 27 January 1777, saying that Priestley should lose the philosopher’s stone should he ever find it, as it will only aid mankind’s wickedness in slaughtering one another—conversation fit for Revolutionary times!

⁶⁵ On the history of affinity chemistry in connection with compositionism, see Klein (1994, 1996), Klein and Lefèvre (2007), Kim (2003) and Taylor (2006).

was firmly based on the compositionist framework, representing compounds as combinations of indestructible parts, and chemical reactions as *rearrangement* of those parts. To that Geoffroy added the concept of affinity and its relative strength, which enabled the explanation of why certain chemical combinations took place in preference to others.

I believe it was the general adoption of compositionism that gave assumptions (a) and (b) above their appearance of self-evidence, and thereby led to the triumph of Lavoisier's weight-based chemistry. And it is not that weight was the only possible parameter to track chemical components, but it was the only one that worked out well enough for Lavoisier and his contemporaries.⁶⁶ Was compositionism itself supported by sufficient empirical evidence? That is the wrong question to ask, as noted already: assumptions like (a) and (b) *enable* the production of empirical evidence; they are not themselves tested by the evidence.⁶⁷ Principlist and compositionist ways of thinking were linked up with different experimental practices. Most importantly, principlist thinking was linked with the laboratory practices of *transformation*; compositionist thinking was linked with the laboratory practices of *decomposition and recomposition*. When I say "linked", what I mean is that the conceptual and the experimental aspects reinforced and shaped each other, rather than one causing the other in a unidirectional way; this expresses what I mean by the "coherence" of a system of practice. Priestley's pneumatic chemistry was a fundamentally transformative experimental practice, as an examination of his main treatise on the subject shows clearly. Before he started making the discoveries of all the various different airs that made him famous, he was reporting on things like "air *infected* with animal respiration, or putrefaction" and "air *infected* with the fumes of burning charcoal" (Priestley 1774, Contents). Priestley would not have had any particular ideas at that point of any substance *combining* with the air in the process of respiration or combustion. Rather, respiration was seen as a transformative operation, and that was that. When he was recounting the previous works that formed the background to his own research, Priestley extolled the virtue of Joseph Black, who had discovered that the input of fixed air *rendered* calcareous substances mild (Priestley 1774, 3).

This way of thinking also combined well, at least initially, with the Stahlian notion that phlogiston was the principle that imparted inflammability and metallicity to substances. As he proceeded with his own investigations, Priestley strongly adopted this principlist way of thinking about phlogiston, speaking routinely of how air was transformed by the addition of phlogiston in a "regular gradation from dephlogisticated air, through common air, and phlogisticated air, down to nitrous

⁶⁶ Whether something works out or not in this way is how nature exercises its guiding hand on science, as I will explain further in Chap. 4. Such guidance can only be obtained pragmatically in specific situations; Chap. 3 will show that in later and different settings, volume and even specific heat also successfully performed similar tracking functions as weight.

⁶⁷ Hence they qualify as "metaphysical principles" or "ontological principles" as I termed them in Chang (2008, 2009c). This is a neo-Kantian point of view, which has in recent times been championed by Michael Friedman (e.g. 2001).

air” (1775, 392). This series was not an idle piece of theorizing, but something rooted in his minute laboratory operations. In his numerous experiments leading to (and based on) these conceptions, Priestley took note of all sorts of properties that were given to substances as they were modified (such as color, smell, elasticity, and chemical reactivity with various other substances) but weight was not something he noted very frequently. Changes of weight seemed capricious in relation to phlogistication, which meant that weight was not taken as a reliable variable for the purpose of extracting stable patterns in nature’s behavior.

What kind of experimental practices were linked to Lavoisier’s thinking? He was also driven by pneumatic chemistry, but in a very different way from Priestley. Lavoisier’s fascination, right from his early “crucial year”, was about how air was absorbed and given out by solid substances in certain chemical reactions. His thinking was based on the presumption that chemical substances had *components*: stable units that are preserved through chemical reactions. This focus on the tracing of input and output, combined with Lavoisier’s quantitative bent, resulted in his “balance-sheet” method (Poirier 2005),⁶⁸ or his “algebraic” inclination (Kim 2005). Having thus stepped into the compositionist way of thinking, Lavoisier sought and developed experiments conforming to the old compositionist practice of decomposition-and-recomposition, which was the most convincing method of ascertaining compositions.

In this context it is interesting to examine Lavoisier’s experimental work on heat, in which he collaborated with Pierre-Simon Laplace (1749–1827).⁶⁹ Although Lavoisier and Laplace acknowledged that heat had no detectable weight (and in this paper they do not even commit strongly to the material reality of caloric), it is clear that they assumed heat to be a conserved quantity, and aimed to find a quantitative measure of it to make it amenable to compositionist thinking. They invented the ice calorimeter for this purpose, behind which was Lavoisier’s chemical notion that liquid water was a compound of a definite amount of caloric (latent heat) per unit amount of ice. The ice calorimeter turned out to be difficult to use in practice, and compositionist practice concerning heat did not become very fruitful in Lavoisier’s chemistry.

The balance was more successful as an instrument of quantification. It allowed a convenient and precise tracking of chemical components that had weight, and fostered a powerful system of compositionist practice. The balance, as it were, was the perfect instrument for managing the balance-sheet. This arithmetic of chemical weights was the key to Lavoisier’s filling-out of the schema given by compositionism. In his chemical accounting of weights, chemical reactions were studied by keeping

⁶⁸ Thinking of the balance-sheet of weights in chemical reactions must have been pleasing to the commercial–bourgeois sensibilities of Lavoisier and his middle-class scientific community, though the oxygenists did not all share the same class background, and I have no means of supporting a real causal link here in any case.

⁶⁹ Lavoisier and Laplace [1783] (1920). See Guerlac (1976) for an informative discussion of this collaboration, and also Chang (2004), 134–136, and references therein.

track of the weights of the ingredients and products, and composition was determined with the help of the assumption that if a substance became heavier after a reaction then it must have become compounded with another substance. This weight-based compositionist system had its striking success in pneumatic chemistry where, before Lavoisier, the chemical role of various gases had been noted but their weights had not received sufficient attention. The success of Lavoisier's weight-based compositionist practice reinforced the conviction that weight was the most important variable in chemical reactions; however, those not operating in that system of practice would have found it difficult to share that conviction.

To summarize: the clear evidential advantage of the oxygenist system on the basis of weight considerations only holds if one accepts compositionism; phlogistonists disregarded weight-based arguments because they were principlists. The Chemical Revolution makes much more sense when we see it as a ripple riding on a large wave, which was the very gradual establishment of compositionism. It is important to see beyond the clash between phlogiston and oxygen. If we should want to conceive of the Chemical Revolution as the event that gave rise to "modern chemistry", we must follow Robert Siegfried and Betty Jo Dobbs (1968) in identifying it as a compositionist revolution, whose endpoint was not Lavoisier, but Dalton. More will be said about that in Chap. 3.

1.2.4 *What Good Is Phlogiston?*

Having refined the descriptive account of the Chemical Revolution and considered the explanations for it, I am now ready to address the normative question: was it right for late eighteenth-century and early nineteenth-century chemists to reject the phlogistonist system of chemistry? (see Chap. 5 if you are inclined to say: "It doesn't matter—there is enough evidence *now*!") In Sect. 1.2.1.1 I argued that the phlogistonist system had a slight advantage over the oxygenist system because it covered a broader problem-field. In Sect. 1.2.1.2 I examined the stand-off between divergent epistemic values, especially with simplicity on the oxygen side and completeness on the phlogiston side; my own sympathy there was with completeness, though that did not amount to a decisive verdict. In Sect. 1.2.1.3 I examined the divergent instantiations of the shared epistemic values of unity, empiricism and systematicity, and saw that I could not come down on either side. In Sect. 1.2.3 I noted that weight-based arguments regarding constitutions were clearly on the side of oxygen, but that there were no compelling immediate reasons of evidence for phlogistonists to accept the *premises* of those arguments. On balance, in all honesty, I cannot see that there were good enough reasons for a decisive rejection of the phlogistonist system. I am convinced that the death of phlogiston, however slow it might have been, was premature, as I have discussed in further detail in Chang (2011b).

Let us now follow through the implications of that conclusion. If I really stand by my verdict, then I have to live with its consequences. If I want to argue that the phlogiston should not have been killed, then I am obliged to say what good would

have come of keeping it (see Sect. 1.3.5 on a more careful statement of this necessity for counterfactual thinking). Here I need to make a more serious attempt to answer the four questions raised toward the end of Sect. 1.1:

- (1) Was there any knowledge that scientists lost when they rejected the phlogistonist system?
- (2) Was there any knowledge that could have come from keeping the phlogistonist system, whose development was delayed or prevented because of its demise?⁷⁰
- (3) Was there a beneficial effect of having both phlogiston and oxygenist systems present, in terms of what was produced by interactions between them?
- (4) Would there have been further beneficial interactions between the oxygenist system and the phlogistonist system, if the latter had been maintained?

Questions (1) and (3) concern historical actuality, and questions (2) and (4) concern potentiality. Questions (1) and (2) are about the merits of the phlogistonist system in itself, compared with the merits of the oxygenist system in itself; questions (3) and (4) concern the merits of an interactive–pluralistic way of doing science in comparison to a monistic way. Also, given what was said in Sect. 1.2.3, each of the questions needs to be dealt with at two different levels: first in relation to the competition and interaction between the phlogiston and oxygenist systems, and secondly in the wider context of the competition between principlism and compositionism. I will deal with questions (1) and (2) together in Sect. 1.2.4.1 below, and with (3) and (4) in Sect. 1.2.4.2.

1.2.4.1 Benefits of Phlogiston

There was one clear area of “Kuhn loss” in the Chemical Revolution, which I have mentioned already. Phlogistonists explained the common properties of metals by saying that all metals were rich in phlogiston⁷¹; this explanation was lost through the Chemical Revolution, as it does not work if we make the familiar substitution of phlogiston with the absence of oxygen (or, as Lavoisier had it, a strong affinity for oxygen).⁷² As Paul Hoyningen-Huene puts it (2008, 110): “Only after more than a 100 years could the explanatory potential of the phlogiston theory be regained in modern chemistry. One had to wait until the advent of the electron theory of metals”.⁷³

⁷⁰ One also ought to ask whether an actual revival of an unjustly dismissed system of knowledge could produce some knowledge now. I think that is indeed possible; however, I do not highlight this question here because in the case of the phlogistonist system I think its full potential has actually been realized in the end, as I explain in Sect. 1.3.5. But I may be wrong in this judgment.

⁷¹ Kuhn (1970), 157.

⁷² Or I should say “oxygen base”, to be precise. For Lavoisier’s view on this, see his commentary in Kirwan (1789), 15–16. For a detailed discussion of whether a translation between the phlogiston and the oxygen theories can be worked out, see Chang (2012b).

⁷³ At least, it is interesting that this came to be regarded as a legitimate scientific problem again, over a century later!

Not only that, but the phlogistonist account actually has a close resonance with the modern notion that all metals share metallic properties because they all have a “sea” of free electrons. If we were to be truly whiggish, we would recognize phlogiston as the precursor of free electrons.⁷⁴ The phlogiston–electricity connection is actually not at all a retrospective fabrication by whiggish historians or philosophers. Allchin (1992, 112), following William M. Sudduth (1978), identifies no fewer than 23 people who postulated a close relationship between phlogiston and electricity in the eighteenth century. There were some good motivations for this identification (even aside from the common desire, expressed for example by James Hutton (1794), to find a grand unity among all the imponderable fluids): for example, it was found that electricity could be used to reduce calxes to metals, which was a role performed by phlogiston. For such reasons, the English chemist John Elliott (1780, 92) even proposed that phlogiston should be re-named “electron”.⁷⁵ (Later on, when the electrolysis of water in 1800 ended in a puzzle about why the oxygen and hydrogen gases were produced at separate places, Johann Wilhelm Ritter’s answer was that hydrogen gas was a compound of water and negative electricity, and oxygen a compound of water and positive electricity; this lined up exactly with Cavendish’s earlier notion that hydrogen was phlogisticated water, on making the identification of phlogiston with negative electricity; this episode will be explored in more depth and detail in Chap. 2).

To show that it is not only mad philosophers of science or completely bygone scientists who have had these wild thoughts about phlogiston, I refer to the great American chemist Gilbert Newton Lewis (of the “octet rule” and the still-current definition of acidity). In his Silliman Lectures at Yale University, Lewis (1926, 167–168) declared that the phlogistonists made “the next great step in chemical classification” after the work of Boyle, “through a study of the phenomenon which we know as reduction and oxidation, but which was first called phlogistication and dephlogistication”. Lewis thought that the demise of phlogiston in the Chemical Revolution constituted a great lost opportunity for chemistry:

If they [the phlogistonists] had only thought to say “The substance burning gives up its phlogiston to, and then combines with, the oxygen of the air,” the phlogiston theory would never have fallen into disrepute. Indeed, it is curious now to note that not only their new classification but even their mechanism was essentially correct. It is only in the last few years that we have realized that every process that we call reduction or oxidation is the gain or loss of an almost imponderable substance, which we do not call phlogiston but electrons.⁷⁶

Lewis’s statement here is a good reminder that the modern meaning of “oxidation” has nothing inherently to do with oxygen; in this area of chemistry, it might have been more sensible to stay with the basic conceptual structure behind the terminology of phlogistication/dephlogistication (gain/loss of electrons), instead of the rather confusing terminology of reduction/oxidation. In other words, if the idea of phlogiston

⁷⁴ For a full consideration of such a whiggish view, which lines up its judgments with the best modern science but not any science from the past triumphant in its own time, see Chang (2009b).

⁷⁵ This quirky fact I owe to Partington and McKie (1937–1939, 350).

⁷⁶ I thank Patrick Coffey for alerting me to this passage.

had been maintained, it would have been easier for chemists and physicists to come to grips with that mysterious substance playing crucial roles in chemical transformations, which is abundant in metals among other places. This potential is clearly suggested in Allchin's (1997) report of success in using the concept of phlogiston in teaching about redox reactions to modern-day students.

Let me keep up the whiggish fantasy for a little bit, to see where else it might lead. If my thoughts about the missed theoretical opportunities seem too speculative, we can at least admit that the preservation of the phlogiston concept would have stimulated some experimental enquiries that were not taken up under the Lavoisierian umbrella. If phlogiston had survived, and its association with electricity maintained, I am confident that nineteenth-century scientists would have made attempts to isolate the electric fluid from phlogiston-rich substances such as metals, using any plausible means at their disposal. Would it not have occurred to someone to hit the surface of a metal with powerful ultraviolet rays (already discovered in 1802) in an attempt to disengage phlogiston? As soon as there were sensitive enough electrometers, the photoelectric effect would have been detected. What about trying to run an electric current between two electrodes across a near-vacuum, a very familiar sort of thing from the traditional practice of drawing sparks from static electricity? In fact Davy, and also Jöns Jakob Berzelius, had the experimental knowledge that electricity could be passed through a vacuum. Davy even assessed that the passage of electricity was easier in vacuum than in air, and for Berzelius all this was evidence for the materiality of electricity (see Russell 1963, 145). But electrical discharge in rarefied gases, despite its striking visual manifestations, did not receive serious and widespread scientific attention till the 1870s, when Eugen Goldstein identified "cathode rays" as such (see Darrigol 2000, 274ff). Is it too irresponsible to speculate that cathode rays would have been discovered and investigated very early on if the phlogiston–electricity line of investigation had been encouraged further? Elliott would have been pleased to congratulate my imaginary investigators for the experimental isolation of the "electron".

We might also say that the phlogiston theory gave a good explanation of the production of flame in combustion. As Knight (1978, 33) points out, Davy was unhappy about the neglect of light in Lavoisier's theory, writing in his youthful piece published in 1799 that there were two defects of Lavoisier's theory, namely the assumption of material caloric and "the total neglect of light". Allchin (1992, 111–112) tells us the same about Hutton and others. But did phlogiston do any better here? In modern theory, flame is a plasma, which is mostly a mixture of positive ions and electrons. As I have just mentioned, in a whiggish understanding of the phlogistonist theory of metals, there is a clear reason to identify phlogiston with free electrons. That fits in nicely here, if we take the release of flame as a result of the dissociation of phlogiston (electrons) from the combustible substance. In the early nineteenth century William Brande (1814) at the Royal Institution carried out experiments showing that flame was subject to electrostatic attraction, but this work failed to open up a new line of research; progress would have been facilitated much better in a phlogistonist–electronist framework. The phlogistonist account, of course, needs modification here, as it tended to treat flame as just phlogiston rather

than phlogiston mixed in with dephlogisticated stuff and also did not specifically explain why the plasma should glow, but still it was less hopeless than the Lavoisierian story that flame was some mixture of two imponderable chemical substances, *calorique* and *lumière*, both disengaged from oxygen gas.

If phlogiston had been retained, it would also have served as a reminder that there was more to chemical reactions than the grouping and re-grouping of gravimetric building-blocks. Whiggishly speaking, phlogiston served as an expression of chemical potential energy, which the weight-based compositionism of the oxygenist system completely lost sight of. The Lavoisierian tradition was actually quite unstable on this count. For example, Lavoisier sowed the seed of the destruction of his own theory of combustion, by putting so much emphasis on weight and then assigning no weight to caloric. Lavoisier's theory of combustion in fact never got very far in explaining the release of heat and light in combustion, without the concept of energy available. To start thinking about energy, chemists should not have needed to wait for help from the likes of Mayer, Joule and Helmholtz and things like the link between heat and mechanical work. If phlogiston had lived, it would have given chemists a productive open end to start thinking about something like energy. I have already noted in Sect. 1.2.2 that a number of phlogistonists tried to preserve phlogiston for something like energy considerations, while admitting oxygen to their systems for weight-related considerations. This move, whether somehow remembered or freshly re-invented, was not lost on some of the Victorian chemists. In the 9th edition of the *Encyclopaedia Britannica* (1876), F. H. Butler identified phlogiston as another name for potential energy: "The supposed subtraction of phlogiston in the calcination of metals . . . was yet a loss of potential energy, by virtue of the combination of the metal with the [oxygen] gas; and the gain of phlogiston was an increase of potential energy, attendant on the removal of oxygen." Butler recognized this notion as a distinct advancement made in the phlogistonist period: "It was only in the latter part of the eighteenth century that such airy nothingness became commonly regarded as an intimate and necessary constituent of various solid and fluid bodies."⁷⁷

William Odling made the same point in a most interesting paper from 1871. Although not a household name today, Odling was one of the leading theoretical chemists of Victorian Britain, and at that time the Fullerial Professor of Chemistry at the Royal Institution. According to Odling (1871, 319), the major insight from the phlogistonists was that "combustible bodies possess in common a power or energy capable of being elicited and used", and that "the energy pertaining to combustible bodies is the same in all of them, and capable of being transferred from the combustible body which has it to an incombustible body which has it not". Lavoisier had got this wrong by locating the energy in the oxygen gas in the form of caloric, without a convincing account of why caloric contained in other gases would not have the ability to cause combustion. Odling (p. 322) thought that

⁷⁷ Vol. 5 (1876), p. 461. Butler's piece was the "Historical Introduction" to the entry on "Chemistry", jointly authored overall by "Prof. [Henry] Armstrong, R. Meldola and F. H. Butler".

“the Stahlians, though ignorant of much that has since become known, were nevertheless cognisant of much that became afterwards forgotten.” He also cited Alexander Crum-Brown (1866), another leading Victorian chemist, as having the same view that “there can be no doubt” that potential energy was what the earlier chemists “meant when they spoke of phlogiston” (p. 322). In fact Odling and Crum-Brown had been anticipated by Liebig two decades earlier (1851, 49–50), before the widespread use of the energy concept in chemistry: “Many chemists, even at the present day, find it impossible to do without certain collective names, analogous to the word phlogiston, for processes which they regard as belonging to the same class, or determined by the same cause. But . . . they employ, since the time of Berthollet, terms which designate what are called ‘forces.’”

Admitting that phlogistonists tended to conceive of phlogiston as a material substance, Odling questioned whether this was meant in such a standard way (pp. 323–324): “though defining phlogiston as the principle or matter of fire, . . . they [Stahlians] thought and spoke of it as many philosophers nowadays think and speak of the electric fluid and luminiferous ether”, the still-surviving imponderable fluids of the mid-nineteenth century. In any case, Odling thought this substance-talk could be pardoned (p. 323):

That Stahl and his followers regarded phlogiston as a material substance, if they did so regard it, should interfere no more with our recognition of the merit due to their doctrine, than the circumstance of Black and Lavoisier regarding caloric as a material substance, if they did so regard it, should interfere with our recognition of the merit due to the doctrine of latent heat.

Although phlogiston was clearly not exactly chemical potential energy as understood in 1871, Odling (p. 325) argued that “the phlogistians had, in their time, possession of a real truth in nature which, altogether lost sight of in the intermediate period, has since crystallized out in a definite form.” He ended his discourse by quoting Becher: “I trust that I have got hold of my pitcher by the right handle.” And that pitcher (or *Becher*, cup?), the doctrine of energy, was of course “the grandest generalization in science that has ever yet been established.”

All in all, I think it is quite clear that killing phlogiston off had two adverse effects: one was to discard certain valuable scientific problems and solutions; the other was to close off certain theoretical and experimental avenues for future scientific work. Perhaps it’s all fine from where we sit, since I think the frustrated potential of the phlogistonist system was quite fully realized eventually, by some very circuitous routes. But it seems to me quite clear that the premature death of phlogiston retarded scientific progress in quite tangible ways. If it had been left to develop, I think the concept of phlogiston would have split into two. On the one hand, by the early nineteenth century someone might well have hit upon energy conservation, puzzling over this imponderable entity which seemed to have an elusive sort of reality which could be passed from one ponderable substance to another.⁷⁸

⁷⁸ As Fox (1971) documents in great detail, the caloric theory was having a good deal of difficulty by the early nineteenth century, which would have fostered willingness to consider alternatives.

In that parallel universe, we would be talking about the conservation of phlogiston, and how phlogiston turned out to have all sorts of different forms, but all interconvertible with each other. This would be no more awkward than what we have in our actual universe, in which we still talk about the role of “oxygen” (acid-generator, *Sauerstoff*) in supporting combustion, and the “oxidation” number of ions. On the other hand, the phlogiston concept could have led to a study of electrons without passing through such a categorical and over-simplified atomic theory as Dalton’s. Chemists might have skipped right over from phlogiston to elementary particles, or at least found an alternative path of development that did not pass through the false simplicity of the atom–molecule–bulk matter hierarchy. Keeping the phlogiston theory would have led chemists to pay more attention to the “fourth state of matter”, starting with flames, and served as a reminder that the durability of compositionist chemical building-blocks may only be an appearance. Keeping phlogiston alive could have challenged the easy Daltonian assumption that chemical atoms were physically unbreakable units.⁷⁹ The survival of phlogiston into the nineteenth century would have sustained a vigorous alternative tradition in chemistry and physics, which would have allowed scientists to recognize with more ease the wonderful fluidity of matter, and to come to grips sooner with the nature of ions, solutions, metals, plasmas, cathode rays, and perhaps even radioactivity.

1.2.4.2 Benefits of Phlogiston–Oxygen Interaction

The actual and potential merits of the phlogistonist system in itself are not the only things we should consider. As I will discuss further in Chap. 5, there may be important benefits arising from plurality itself. More specifically, here I want to consider the actual and potential benefits of having the phlogistonist system in interaction with the oxygenist system (or having principlism in interaction with compositionism). Might there not be something to be gained from cross-fertilization or from competitive jostling? We easily grant such benefits in the realm of cross-cultural interactions; why not in science?

There are some concerns to be dispelled, before I consider the possible benefits of maintaining multiple systems. (These points will be discussed in their full detail and generality in Chap. 5, but some preliminary remarks are in order here.) First of all, wouldn’t the co-existence of different systems cause confusion and prevent effective research? There is little evidence of such harmful confusion caused by the co-existence of phlogiston and oxygen, and there isn’t any convincing reason to think that the situation would have got much worse later if phlogiston had been kept. Second, doesn’t the maintenance of too many competing scientific systems dissipate valuable resources? That is a valid general concern, but at least on the

⁷⁹ This “Daltonian” assumption is not truly Dalton’s, since Dalton thought that atoms had an internal structure: a small hard core of ordinary matter, surrounded by an “atmosphere of caloric”. But since Dalton thought that the material core of each atom was indestructible, the “Daltonian” designation is fair enough.

phlogistonist side it would not have been a serious worry, since the advocates of phlogiston were mostly amateur scientists working individually, often using inexpensive equipment procured with their own resources (unlikely Lavoisier and his colleagues relying effectively on state support). Third, won't arguments about fundamentals divert scientists' energy and attention, preventing them from launching into specialist research? Again, in the case of the phlogiston–oxygen debate there is no indication that the scientists who engaged in the debate on fundamentals were any less productive in producing specialist research than those who did not debate fundamentals.

Having neutralized those concerns for the moment, let me turn to the question of productive interaction. There is a simple-minded point that contains the germ of something much more sophisticated: how would Lavoisier have done what he did, if Priestley hadn't made oxygen and showed him how to do it, and if Cavendish hadn't made water from hydrogen and oxygen and let Blagden tell Lavoisier about it?⁸⁰ Maurice Crosland (1983, 238) is quite categorical on this point: "Certainly without the benefit of the work done on gases by British men of science, and notably by Cavendish and Priestley, Lavoisier would not have been able to build chemistry anew." Of course, we cannot rule out the possibility that Lavoisier and his friends might have stumbled on those particular experiments on their own eventually, but the point is more general. Priestley, Scheele, Cavendish and other phlogistonists of the late eighteenth century far outstripped any other group (including Lavoisier and his colleagues) in their ability to make new experimental discoveries in pneumatic and metallic chemistry, the very discoveries that Lavoisier built on so effectively. The point is more about principlism, rather than phlogiston *per se*. Aside from the genius of the individuals involved, I believe there was something about the principlist practice of transformations that was very conducive to these experimental innovations and discoveries, as it encouraged chemists to apply various powerful agents (thought to be, or to contain, various principles) to various substances, to see what happens. On the other hand, it was the compositionist re-interpretation of these results, rather than their assimilation into principlist theories, that brought about the undoubtedly great achievements of Lavoisier and his colleagues. I submit that such in-tandem progress would not have been possible without the co-existence of the principlist and the compositionist traditions, separate but interacting. Was the fruitful interaction a mere accident? At least I like to think of it as an affordance of serendipity by plurality.

It may be less exciting to think about how the oxygenist system challenged and enriched the phlogistonist system, but it is important to raise the topic at least, for the sake of symmetry and completeness. There is no doubt that the phlogistonist system was challenged and stimulated by the opposition from Lavoisier's theory.

⁸⁰ There is an open question about a similar point regarding Scheele and oxygen, since we cannot be sure how much Lavoisier knew about what Scheele had done. As Anthony Butler (1984) argues, there is evidence that Scheele sent Lavoisier a letter about oxygen at about the same time as Priestley's visit to Paris.

The hybrid systems discussed in Sects. 1.2.2 and 1.2.4.1 were healthy developments arising out of this opposition. Even for those who kept to the phlogistonist system without admitting oxygenist ideas explicitly, their phlogistonism came to be developed in more compositionist ways with more attention to weights. Such hybrid systems were not compatible with the post-Revolutionary insistence that phlogiston be expunged altogether from any system of chemistry. The consolation, on the other side, is that the benefits already reaped by the oxygenist system from its interaction with the phlogistonist system were there to stay, and not eliminated by the demise of phlogiston itself.

What about further benefits that could have come from maintaining the phlogiston–oxygen interaction for longer? Actual history provides a guide and a starting point for our thinking on that question. I have already mentioned the extinction of the hybrid systems, which accepted oxygen for weight considerations and retained phlogiston for energy considerations. These hybrids would have marked just the right path in Odling’s view (though Odling does not seem to have been aware that there was actually a hybrid tradition at the time of the Chemical Revolution): “Chemists nowadays are both Stahlian and Lavoisierian in their notions; or have regard both to energy and matter.” (1871, 323) That kind of complete view of chemistry had already been in place, before it was rooted out by the Lavoisierian orthodoxy. If the interaction between phlogiston and oxygen had been kept up, it seems to me that chemists would have been able to maintain a very dynamic hybrid tradition throughout the nineteenth century.

A productive tension between principlism and compositionism would have helped maintain a healthy pluralism in chemistry. As compositionism developed into a purer form and became increasingly dominant in its march from Lavoisier to Dalton and beyond, there was growing temptation to think of the foundations of chemistry as a matter of stark choice: subscribe to a simple-minded atomism, or renounce any ontological discourse about chemical substances. This is what some commentators have picked up as the nineteenth-century opposition between atomism and positivism. If chemists had resisted such dichotomies and remained more aware of phlogistonist–principlist successes while holding on to basic compositionism, they would have been able to develop a more flexible view of “elements” and a more nuanced take on “imponderables”, leading to an easier incorporation of electricity and thermodynamics into chemistry. Conversely, there is also something I call the “lacuna effect”. There are many things that the phlogistonist system failed to explain satisfactorily, but the oxygenist system did not even try to explain. In such cases one benefit of keeping the phlogistonist system would have been to serve as a reminder of the unsolved problems.

I would like to close with a plea for imagination, regarding what might constitute new scientific knowledge and where it might come from. I have serious intentions in my discussion of the phlogistonist system of chemistry, about its forgotten merits and the benefits it could have brought into the nineteenth century. However, even if every single one of my specific ideas about the lost future of phlogiston turned out to be futile, I would still be hopeful for a broader effect of following my musings: the freeing of the reader’s scientific imagination.