Phlogiston as a Case Study of Scientific Rationality

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Abstract: A number of prominent defenders of the phlogiston theory identified phlogiston with hydrogen in the late eighteenth century, and I argue that this identification was fairly well-entrenched by the early nineteenth century. In light of this identification, I examine the ways in which retaining phlogiston could have retarded scientific progress, and also the ways in which it could have benefited science. I argue that it was rational for chemists to eliminate phlogiston, but that it also would have been rational for them to retain it. I situate my arguments for these claims in relation to Hasok Chang's recent work on the Chemical Revolution. And I conclude that there is a sense in which scientific rationality concerns what is permissible, as opposed to what is required, so that retention and elimination may, at least sometimes, both be rationally permissible options.

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1 Introduction

The literature on the Chemical Revolution is voluminous, and historians and philosophers of science have had much to say about it. For that reason, one might suspect that everything that can be said about this episode has already been said. Hasok Chang's recent work, however, shows that this is not the case.[1] Perhaps the most exciting aspect of Chang's work concerns the bold and original conclusion for which he argues, namely, that phlogiston was killed prematurely. More specifically, Chang's view is that chemists working in the early nineteenth century should have retained phlogiston, just as they did oxygen, and that science could have benefited from this pluralistic approach.

Chang recognizes that his re-telling of the story of the Chemical Revolution bears on the issue of rationality. Although he holds that the Chemical Revolution "was a fairly rational affair," he locates an element of irrationality, not in the chemists who continued to hold on to phlogiston, but in those who embraced Antoine Lavoisier's oxygen theory too readily (Chang, 2012, 56). On Chang's understanding of rationality, if one were to admit the rationality of the response of these latter chemists, this would threaten his claim that phlogiston was killed too soon (ibid.: 51).

In this paper, I will examine and critically evaluate the arguments that Chang puts forward in order to defend his view that chemists should have retained phlogiston. My aim in doing so is two-fold—in short, I hope to shed some light on the Chemical Revolution in particular, and on scientific rationality more generally. Regarding the former, I take it that Chang is correct that it would have been rational for chemists working in the early nineteenth century to retain phlogiston, though my way of supporting this claim will differ from Chang's. My view will differ from Chang's in another respect, insofar as I will defend the claim that it was also rational for chemists to eliminate phlogiston. On my view, then, the decision to retain phlogiston and the decision to eliminate it would have both been rational, and the rationality of eliminating phlogiston needn't threaten Chang's claim that it would have been rational for chemists to retain it. My second aim is to use this view of the Chemical Revolution to illustrate something about scientific rationality more generally, namely, that there is a sense in which it concerns

[1] This work includes Chang's 2009, 2010, 2011, and chapter 1 of his 2012. For critical reviews of the latter, see Mauskopf (2013) and Schummer (2013).

what is permissible, as opposed to what is required. When it comes to deciding whether to retain or eliminate a given entity, there are cases (like that of phlogiston) in which both options are rationally permissible.

To that end, I'll proceed as follows. In section 2, I'll summarize Chang's reasons for thinking that phlogiston suffered a premature death, and that science could have benefited if chemists had retained it. In section 3, I'll argue that it's likely that the retention of phlogiston would not have led to the benefits that Chang discusses. This is because a number of chemists identified phlogiston with hydrogen in the late eighteenth century, and, as I will argue, this identification became rather well-entrenched by the early nineteenth century. It's likely that retaining phlogiston after this point would have brought mixed results. It could have benefitted science in ways that Chang does not discuss, but it could also have retarded scientific progress in other ways. In section 4, I'll use this identification of phlogiston with hydrogen in order to draw some conclusions about the rationality of the Chemical Revolution. I'll argue that it would have been rational for chemists to eliminate phlogiston once they found that various substances thought to be rich in phlogiston contain no hydrogen. On the other hand, I'll argue that this identification could also have supported the rationality of retaining phlogiston, since, insofar as it was rational to retain hydrogen, it would have been rational to retain phlogiston. Finally, in section 5, I'll end with some conclusions about scientific rationality more generally.

2 Chang on Retaining Phlogiston

In his recent work on the Chemical Revolution, Chang claims that phlogiston was killed prematurely. Before examining his evidence for this claim, it's worth noting that, contrary to what many historians and philosophers have held, Chang argues that the Chemical Revolution did not consist in a quick conversion of the vast majority of late-eighteenth-century chemists to Lavoisier's oxygen theory. As Chang emphasizes, there were, in fact, many anti-Lavoisierians who continued to entertain the phlogiston theory well into the nineteenth century (2010, 62–68; 2012, 29–34). Chang's claim, then, is that even after we take this into account, the death of phlogiston was still premature.

Chang's evidence for this claim comes in four varieties. First of all, he argues that the elimination of phlogiston resulted in the elimination of "certain valuable scientific problems and solutions" (2012, 47). Chang's central example is familiar from discussions of so-called 'Kuhn loss.'[2] While the phlogiston theory provided an explanation of the similarity of the metals in terms of their shared phlogiston, oxygen theorists not only failed to provide a solution, but ignored the very problem that the phlogiston theorists had attempted to solve (ibid.: 21, 43–44).[3] The retention of phlogiston, then, would have served as a reminder of certain problems and purported solutions.

Secondly, Chang argues that there were productive interactions between oxygen and phlogiston that could have continued if the latter had been retained (ibid.: 48–50). Chang points out that it's unlikely that Lavoisier would have achieved what he did without building upon work by phlogiston theorists like Joseph Priestley and Henry Cavendish. Chang sees no reason why such productive interactions would have ceased if phlogiston had been retained.

Chang's third source of evidence is also related to the issue of productive interactions. Chang argues that the elimination of phlogiston "close[d] off certain theoretical and experimental avenues for future scientific work" (ibid.: 47). More specifically, he argues that if chemists had retained phlogiston alongside oxygen, it would have been possible to make more rapid progress in theorizing about electricity and energy. These productive interactions, in turn, lend support to Chang's more general advocacy for pluralism in science, which involves maintaining competing systems, and his rejection of monism, which involves the elimination of all competing systems except for the 'winner' (2011, 425–428; 2012, ch. 5).

Fourthly, Chang argues that by the early nineteenth century, phlogiston and oxygen were on more-or-less equal footing, theoretically speaking. What justified chemists in retaining oxygen was really the set of operations that they relied on in carrying out various experiments. This justification, in Chang's view, would have applied equally well to phlogiston (2011, 420).

These latter two sources of evidence deserve more detailed discussion, to which I will now turn.

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[2]For Kuhn's own discussion of this example, see his 2012/1962, 156.

[3]Though I will sometimes write of *the* phlogiston theory, I don't mean to deny that there were distinct and mutually inconsistent phlogiston theories at the time, some of which I will discuss in section 3.1. And when I write of the phlogiston theorists, I don't mean to imply that all such theorists defended the same theory. These points apply to *the* oxygen theory and oxygen theorists as well.

2.1 Phlogiston, Electricity, and Energy

Chang claims that the elimination of phlogiston resulted in the elimination of various theoretical and experimental possibilities which would have been beneficial for scientists to pursue. More specifically, Chang believes that, by retaining phlogiston, scientists could have made more rapid progress regarding electricity, on the one hand, and energy, on the other. According to Chang, if we were to engage in some truly whiggish history of science, there are two entities with which we would identify phlogiston, namely, free electrons and chemical potential energy (2009, 246–250; 2011, 412–423; 2012, 43–48).

To begin with, there is the identification with free electrons. In Chang's view, the phlogiston theorists were right that metals are similar to one another by virtue of some shared constituent, and that this is the same thing that is released in combustion. As it turns out, it is free electrons. This isn't just a *post hoc* identification because, as Chang points out, many phlogiston theorists, some of whom I will discuss in more detail in section 3.1, posited a connection between phlogiston and electricity. They did this, not merely out of a desire to have some grand unified theory of all of the imponderable fluids, of which phlogiston and electricity were two,[4] but for experimental reasons as well. For example, Chang notes that it was known that electricity could be used to change calxes into metals, a process that phlogiston theorists understood in terms of gain in phlogiston. Chang claims that if phlogiston had been retained, along with its posited connection with electricity, chemists would have continued to use any methods that they could think of in order to isolate it. In light of this, Chang argues that it's not unreasonable to think that various electrical phenomena could have been uncovered sooner, and even that the discovery of the electron might have constituted the discovery of phlogiston.

Chang also argues that, if it had been retained, the concept of phlogiston would have been split, in which case it would also have been identified with chemical potential energy. His claim is that gain and loss of phlogiston can be understood in terms of gain and loss of potential energy, and that retaining phlogiston could have contributed to more rapid progress being made regarding energy. Insofar as phlogiston was conceived of as a principle, as opposed to a component, and insofar as it was conceived of as an imponderable fluid, the phlogiston theorists had a way of tracking what we would now classify as

[4] Or three, if one prefers a two-fluid theory of electricity.

energy considerations. The oxygen theorists, on the other hand, did not. In accordance with the idea of the conservation of matter, they focused on the weights of substances before and after chemical reactions had taken place, and gain and loss of energy is not something that one can keep track of in this way.

Before moving on, it's important to be clear about Chang's view regarding these benefits of retaining phlogiston. While Chang claims that the elimination of phlogiston retarded scientific progress in various ways, he also admits that we've already realized the benefits that he discusses (ibid.: 43, 47). Chang, therefore, is not advocating that we resurrect phlogiston today, but that these benefits would have been realized more quickly if phlogiston had been retained.

2.2 Phlogiston Was Not Any Worse Than Oxygen

Chang also argues that, in light of the fact that oxygen and phlogiston were on more-or-less equal footing by the early nineteenth century, it would have been rational to retain the latter as well as the former. In order to understand Chang's argument, we must first look at Lavoisier's oxygen in a bit more detail. As Chang emphasizes, by the early nineteenth century, almost every theoretical claim that Lavoisier made about oxygen was proven to be false (2011, 415–420; 2012, 8-10).[5]

Lavoisier's oxygen theory was a theory of combustion, among other things. Lavoisier explained the heat and light that result from combustion in terms of the decomposition of oxygen gas, which involves the separation of oxygen base from caloric. By the early years of the nineteenth century, this explanation was found wanting. If oxygen gas is supposed to be the sole supporter of combustion, then Lavoisier needed an explanation for why other gases, all of which contain caloric combined with some base or other, do not support combustion. Even more damning is the fact that chemists found instances of combustion that did not involve oxygen gas at all, and so the latter could not be the sole supporter of combustion.

The oxygen theory was not just a theory of combustion, though—it was also a theory of acidity. For Lavoisier, oxygen was the principle of acidity, that which renders the substances with which it combines acidic (Lavoisier,

[5] Chang draws support from similar claims made by McEvoy (1997, 22–23) and Siegfried (1988, 35).

1965/1789, 65). But by 1810, Humphry Davy had put the nail in the coffin of the oxygen theory of acidity by showing that muriatic acid (hydrochloric acid, HCl), contains no oxygen.

In light of all of these theoretical failures, one might well wonder why oxygen was retained at all. Chang's answer is that the meaning of 'oxygen' can be fixed operationally in such a way that there is continuity from Lavoisier's time to our own (2011, 419). His basic idea is that all of the operations by which Lavoisier produced oxygen gas work just as well today as they did in the late eighteenth century.

Returning now to the case of phlogiston, Chang's claim is that we can tell essentially the same story (ibid.: 420). Even in light of various theoretical failures, there is operational continuity. For example, Priestley proposed to produce phlogiston by converting metals into calxes. Although today, we would understand this reaction in terms of converting metals into oxides, the operations are the same. And we can fix the meaning of 'phlogiston' operationally, in terms of what the metals give off when they are converted. Chang concludes from this that "there was no convincing reason for chemists to kill phlogiston in the late eighteenth century—at least no more convincing reason than there was to kill oxygen in the early nineteenth century" (ibid.: 420).

3 Evaluating the Benefits of Retaining Phlogiston

I find much with which to agree in Chang's work on the Chemical Revolution, and in the remainder of the paper, I'll indicate these points of agreement. My primary goal in this section, however, is to argue that, if phlogiston had been retained, the benefits that Chang points to regarding electricity and energy would likely not have materialized. My argument hinges on the fact that a number of phlogiston theorists in the late eighteenth century identified phlogiston with hydrogen. I'll attempt to support the claim that this identification was rather well-entrenched by the early nineteenth century. And I'll argue that, as a result, retaining phlogiston would have most likely brought about mixed results. It could have brought about some benefits which Chang does not discuss, but it could have retarded progress in various ways as well.

3.1 Phlogiston and Hydrogen

In the later years of the eighteenth century, two of the foremost proponents of the phlogiston theory, namely, Joseph Priestley and Richard Kirwan, identified phlogiston with inflammable air.[6] As early as 1782, Priestley makes this identification in a letter to Josiah Wedgwood, which describes an experiment which, in Priestley's view, "seems to prove, that what we have called *phlogiston* is the same thing with *inflammable air in a state of combination with other bodies*" (in Bolton, 1892, 33). Some years later, Kirwan makes the same identification in his *Essay on Phlogiston and the Constitution of Acids*, when he claims that "inflammable air, before its extrication from bodies in which it exists in a concrete state, was the very substance to which all the characters and properties of the phlogiston of the ancient chymists actually belonged" (1789, 4–5).

The oxygen theorists, on the other hand, identified inflammable air with hydrogen gas. For example, Lavoisier, in his commentary on Kirwan's *Essay*, identifies the base of inflammable air (i.e., inflammable air minus caloric) with hydrogen. He writes of Kirwan's view that certain substances "all contain the base of inflammable air, that is to say hydrogene [sic]" (in Kirwan, 1789, 22). It follows that hydrogen gas, for Lavoisier, is inflammable air.

There were certainly terminological differences between the oxygen theorists and the phlogiston theorists. But if we exploit Chang's insight that the meanings of these terms are fixed operationally, and if we recognize that all of these chemists produced hydrogen gas by means of a shared set of operations, it's clear that all parties were talking about the same substance, namely, hydrogen gas. In that case, there's good reason to identify Priestley's and Kirwan's phlogiston with Lavoisier's hydrogen gas, though, to be sure, this identification wasn't made explicit by either side. In what follows, I'll write of the identification of phlogiston with hydrogen, as opposed to hydrogen *gas*, both for the sake of brevity, and for the sake of avoiding commitment to the caloric theory of heat.

My claim is that this identification is significant. But as Chang notes, it was just one among many attempts to identify various posits of the phlogiston theory with various substances. Before arguing for the significance of this identification, it's worth briefly discussing some of these other attempts. One such attempt is that advocated by Henry Cavendish, and later by Priestley himself. As Chang points out, both chemists identified inflammable air with

[6] For a more detailed discussion of this identification, see Stewart (2012).

phlogisticated water (2012, 6). We can see Cavendish's view in the following passage. After stating that "inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston," Cavendish writes: "Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely" (1784, 137).

Chang goes on to note a similarity between Cavendish's view and another view, which suggests a link between phlogiston and electricity (2012, 44, 80). On Cavendish's view, inflammable air is phlogisticated water, while oxygen is dephlogisticated water, and the two combine to yield elementary water. Then there is Johann Wilhelm Ritter's view, according to which inflammable air is negatively electrified water, while oxygen is positively electrified water, and the two combine to yield elementary water. If we were to identify phlogiston with electricity, Cavendish's and Ritter's views would amount to one and the same view. And, indeed, some chemists did put forward views along these lines. Chang discusses Priestley himself, who posited a connection between phlogiston and electricity (ibid.: 80–82). After discussing a number of experiments, Priestley writes:

> These experiments favour the hypothesis of *two electric fluids*, the positive containing the principle of oxigen [sic], and the negative that of phlogiston. These united to water seem to constitute the two opposite kinds of air, viz. dephlogisticated and inflammable. (Priestley, 1802, 202)

And as Chang notes (2012, 80), George Smith Gibbes posits a similar connection when he claims that "the principle of the negative side of the galvanic apparatus resides in all combustible bodies . . . and answers exactly to the Phlogiston of Scheele" (1809, 13).

In order to evaluate the benefits of retaining phlogiston, the crucial issue comes down to the extent to which the identifications between phlogiston and various substances became entrenched and passed down to future generations of chemists. My claim is that the identification of phlogiston with hydrogen became entrenched in a way that the other identifications did not. To be sure, a full justification of this claim would require more work than I will do here, which will be limited to the discussion of a single, but important, source of evidence, namely, the work of Humphry Davy.

3.2 Davy's Phlogistic and Electrochemical Speculations

Davy's work is important to consider because, as Chang notes, he was one of a number of chemists who engaged in some "relatively maverick attempts to employ phlogiston again for various scientific purposes" (2012, 65). And of all the chemists working in the early nineteenth century, Chang singles out Davy as "perhaps the most interesting case of the new generation of anti-Lavoisier chemists" (ibid.: 33). One might suspect that Davy's enthusiasm for phlogiston, combined with his work in electrochemistry, provided the perfect conditions for identifying phlogiston with electricity in a way that would become entrenched in the practice of chemistry. Indeed, Chang references Davy's phlogistic speculations with this in mind (ibid.: 80). But Davy, in fact, maintained the identification of phlogiston with hydrogen throughout his work. And oftentimes he doesn't even bother to make this identification explicit to his readers, which provides evidence for the claim that this identification was fairly well-entrenched, not just for Davy, but for his audience as well. At this point, I'll briefly discuss Davy's phlogistic and electrochemical speculations in order to make this point clear.

To begin with, as Chang points out, Davy does engage in some speculation regarding the phlogiston theory in his 1807 Bakerian Lecture. Davy writes:

> A phlogistic chemical theory might certainly by [sic] defended, on the idea that the metals are compounds of certain unknown bases with the same matter as that existing in hydrogene [sic]; and the metallic oxides, alkalies and acids compounds of the same bases with water . . . (1808a, 33)

Davy goes on to consider the limitations of such a theory immediately after introducing it. But the fact that he mentions it at all shows that he does display some enthusiasm for the phlogiston theory. This passage is notable for another reason, though—if one did not have the identification of phlogiston with hydrogen in mind when reading this passage, it would be completely unclear why this theory is supposed to be a phlogiston theory. Hence, this passage shows that, at this stage of his thinking, if Davy identified phlogiston with anything, it was with hydrogen. Moreover, it shows that he expected that his audience had made the same identification; otherwise, he would have been more explicit about the identification and the reasons for it.

Davy continues his phlogistic speculations in another paper, published in 1808. After admitting that the oxygen theory is superior to the phlogiston theory, he claims that "the only good arguments in favour of a common principle of inflammability, flow from some of the novel analogies in electrochemical science" (1808b, 363). He goes on to spell out what he has in mind:

> Oxygene [sic] is the only body which can be supposed to be elementary, attracted by the positive surface in the electrical circuit, and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable proportion of oxygene [sic]. Hydrogene [sic] is the only matter attracted by the negative surface, which can be considered as acting the opposite part to oxygene [sic]; may not then the different inflammable bodies, supposed to be simple, contain this as a common element? (ibid.: 363)

If we keep in mind the identification of phlogiston with hydrogen, we can see these "novel analogies" as suggesting a version of the phlogiston theory. And indeed, Davy goes on to identify phlogiston with hydrogen more explicitly, when, in a discussion of the metals, he writes of "the adherence of their phlogiston or hydrogene [sic]" (ibid.: 364).

Shortly after this passage, Davy engages in some further speculation, and considers "other hypotheses [which] might be formed upon the new electrochemical facts, in which still fewer elements than those allowed in the antiphlogistic or phlogistic theory might be maintained" (ibid.: 368). This way of framing his electrochemical speculations makes it clear that, for Davy, these hypotheses are not elaborations of either the oxygen theory or the phlogiston theory. That said, Davy's motivation for engaging in these electrochemical and phlogistic speculations appears to be the same. Robert Siegfried (1964, 118–119) has argued that Davy entertained various phlogistic theories because of his desire to reduce the number of chemical elements, and the same point applies to the hypotheses that Davy mentions here.

The hypothesis that Davy goes on to consider is based on his observation of a coincidence between chemical states and electrical states. Acids, being attracted to the positive surface of a Voltaic apparatus, are negative, while the alkalies and inflammable substances are positive. Moreover, acids lose their acidic properties when they are positively electrified, while the alkalies lose their alkaline properties when negatively electrified. Davy concludes that, "in these instances the chemical qualities are shewn to depend upon the electrical powers; and it is not impossible that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms" (1808b, 368). Such a hypothesis, then, would admit fewer elements, since the very same element may exhibit different properties depending on its electrical powers.

In a footnote to this passage (ibid.: 368–369), and in some unpublished notes (quoted in John Davy, 1836, 405–406), Davy engages in some additional electrochemical speculation, again with the goal of reducing the number of chemical elements. He entertains the idea that water is an element, but adopts a view which is the opposite of Ritter's, namely, that positively electrified water is hydrogen, and that negatively electrified water is oxygen. The metals, charcoal, sulphur, phosphorus, and nitrogen are constituted of unknown bases and hydrogen, while the acids, oxides, alkalies, and earths are constituted of unknown bases and oxygen. The elements on this theory, then, are water and these unknown bases.

Given that Davy engaged in these electrochemical and phlogistic speculations, one might expect him to posit some kind of connection between phlogiston and electricity of the kind put forward by Priestley and Gibbes. Davy's speculations generally involve the idea that metals and inflammable substances contain hydrogen. And since Davy identifies phlogiston with hydrogen, and since he often makes use of the idea that hydrogen is positively charged, one might expect him to identify phlogiston with some kind of electrical power. But an examination of Davy's work would frustrate these expectations. For some time, Davy continues to entertain the phlogistic idea that inflammable bodies contain hydrogen (e.g., 1809, 103; 1810a, 69). And his electrochemical speculations also appear in subsequent work (e.g., 1810a, 62). But Davy never identifies phlogiston with electricity, or indeed with anything other than hydrogen. And one plausible explanation for this fact is that Davy saw the identification with hydrogen as too well-entrenched to attempt to displace.[7]

At this point, if I've established anything at all, it's that Davy identified phlogiston with hydrogen throughout his phlogistic speculations, and that he wrote as if he expected his audience to have the same identification in mind. And while I take it that Davy's work provides evidence for my claim that this identification was, by the early nineteenth century, more well-entrenched than any other, I acknowledge that this latter claim requires additional work to fully support. That said, in the remainder of the paper, I will take this

[7]For a more detailed discussion of Davy's phlogistic speculations, see Siegfried (1964).

claim for granted, and see what follows from it. But before moving on, it's worth briefly discussing the shape of the further work required to support this claim, and in doing so, I'll indicate some reasons to be optimistic about the prospects.

As Chang notes, Davy was just one of a number of anti-Lavoisierian chemists working in the early nineteenth century (2010, 63–68; 2012, 30–34). A complete justification for my claim would therefore involve looking at these other chemists. Among them are some whom I've already discussed in section 3.1, for example, Ritter, Priestley, and Gibbes. Ritter is unlikely to have been able to establish a more well-entrenched identification of phlogiston with something other than hydrogen, since, as Chang points out, his work on elementary water was rejected by most chemists (2012, 87–94). It's not clear that Priestley's posited connection between phlogiston and electricity would have fared any better, since, as Chang notes, it's not clear how much attention his 1802 paper received (ibid.: 82). And while Chang lists Gibbes as one of the anti-Lavoisierians, he does not include Gibbes in a subsequent figure which focuses on "salient figures" from the previously-mentioned list (ibid.: 31, 34). If Chang's judgment regarding salience is on point, Gibbes would not have had the influence necessary to entrench his posited connection between phlogiston and electricity. Chang lists a number of other anti-Lavoisierians, but Davy surely stands out as one of the most prominent and influential. And given his phlogistic and electrochemical speculations, his work is likely the most significant when it comes to supporting my claim regarding the entrenchment of the identification of phlogiston with hydrogen. So although I've only discussed a single source of evidence for my claim, it's a significant one.

3.3 Benefits and Harms of Retention

At this point, we can evaluate Chang's claim that, if chemists had retained phlogiston, science would have benefited. If I am right that the identification of phlogiston with hydrogen was well-entrenched by the early nineteenth century, then if phlogiston had been retained, so would its identification with hydrogen. And so, if we are to engage in an evaluation of the benefits of retaining phlogiston, we must keep this identification in mind.

To begin with, I think Chang is correct about some of the benefits that he discusses. Even if we keep the identification of phlogiston with hydrogen in mind, the retention of phlogiston would have served as a useful reminder of unsolved problems and potential solutions, like the idea that the metals are similar to one another by virtue of their shared phlogiston. This, in turn, would have served as a potential productive interaction between phlogiston and oxygen, and so I think Chang is also correct that retaining phlogiston would have lead to subsequent productive interactions between the two systems.

That said, if the identification with hydrogen was well-entrenched by the early nineteenth century, it's unlikely that retaining phlogiston would have lead to the other benefits that Chang discusses, namely, more rapid progress regarding energy and electricity. There doesn't seem to be any kind of direct path from phlogiston *qua* hydrogen to these benefits, and so it's likely that they would not have materialized.[8] There may have been a more indirect path to such benefits, for example, one that took into account various electrochemical phenomena like the charge of hydrogen in experiments using the Voltaic pile. But in this case, it is hydrogen, as opposed to its identification with phlogiston, that would be doing the work. In order to count as benefits *of retaining phlogiston*, Chang's argument requires some fluidity regarding the identifications between phlogiston and other substances that could have become entrenched in the practice of chemistry. And if my argument in section 3.2 is on point, this kind of fluidity was not available by the early nineteenth century. It would have been difficult, but perhaps not impossible, to identify phlogiston with electricity, energy, or anything else, in a way that would become more well-entrenched than the identification with hydrogen.

There are, however, still some unaddressed issues. It could be that the retention of phlogiston, along with its identification with hydrogen, would have brought about some benefits that Chang does not discuss. And it's also possible that retaining phlogiston would have brought about harms. In my view, the retention of phlogiston would have been a mixed bag, and it's likely that it would have brought about both benefits and harms. A useful starting point is Kirwan's framing of what is at issue in the opposition between the phlogiston theorists and the oxygen theorists:

> The controversy is therefore at present confined to a few points, namely, whether the *inflammable principle* be found in what are called phlogisticated acids, vegetable acids, fixed air, sulphur,

[8] Mauskopf (2013, 625) makes a similar evaluation when he claims that Kirwan's phlogiston theory, which identified phlogiston with hydrogen, did not have the potential to bring about the benefits that Chang discusses.

phosphorus, sugar, charcoal, and metals. (1789, 6–7)

Kirwan held that the inflammable principle, or hydrogen, is a constituent of all of these substances, and we can inquire into the benefits and harms of retaining a view like this.

As for the benefits of retention, acids and sugars do contain hydrogen, and so the expectations of phlogiston theorists like Kirwan would have paid off. Lavoisier agreed that sugar contains hydrogen (1965/1789, 132), and so this benefit was available even with the elimination of phlogiston. The acids, however, are a different story. It's difficult not to conclude that the oxygen theory retarded progress in determining the composition of acids, as it guided chemists to look for oxygen in acids like muriatic acid (hydrochloric acid, HCl) and prussic acid (hydrocyanic acid, HCN). Perhaps if the phlogiston theory had been more widely held in the early nineteenth century, chemists would have determined the composition of these acid, and abandoned the oxygen theory of acidity, more quickly than they, in fact, did. I take it that this is a plausible benefit of retaining phlogiston, and it's noteworthy that Davy claims that Carl Wilhelm Scheele's view, according to which muriatic acid contains phlogiston and chlorine, "may be considered as an expression of facts," while the oxygen theory "rests in the present state of our knowledge, upon hypothetical grounds" (1810b, 237). Regarding the acids, the phlogiston theory was actually much closer to the truth than the oxygen theory. Based on two of our three current definitions of acidity, namely, the Arrhenius definition and the Brønsted-Lowry definition, it is hydrogen ions, and not oxygen, that play an essential role in acids. It's admittedly a bit of a long shot to conclude that these definitions of acidity could have been put forward sooner if phlogiston had been retained. But it's at least worth considering, and it may represent another potential benefit of retaining phlogiston, even after chemists had determined the composition of muriatic acid and prussic acid.

As for the harms of retention, fixed air (carbon dioxide, CO_2), sulphur, phosphorus, charcoal, and the metals do not contain hydrogen, and so the expectations of phlogiston theorists would have been frustrated. Just as the oxygen theory retarded progress regarding the composition of the acids, it's likely that retaining phlogiston would have retarded scientific progress regarding the composition of these substances. After all, it would have guided chemists to continue to attempt to isolate the hydrogen that these substances purportedly contain, even after experiencing failure in doing so. It's plausible, then, that eliminating phlogiston actually benefited scientific investigation into the composition of these substances.

In sum, the retention of phlogiston would likely have been a mixed bag. Now that we have what I hope is a clearer picture of the benefits and harms of retaining phlogiston, we can examine the issue of rationality.

4 The Rationality of Eliminating/Retaining Phlogiston

As Chang recognizes, his claim that phlogiston suffered a premature death bears on the issue of rationality. While he admittedly lacks a comprehensive theory of rationality, he does briefly make three points that, in my view, suffice for the purposes of his discussion, and I'll adopt them in what follows (2012, 51). First of all, rationality is not about truth, but about making good judgments and decisions based on what one believes. Secondly, rationality involves following rules and methods with which one's community agrees. And thirdly, rationality is instrumental, and must make reference to achieving one's goals.[9]

Although Chang holds that the Chemical Revolution "was a fairly rational affair," there was an element of irrationality, which he locates "not in the refusal of some chemists to go along with Lavoisier, but in the readiness of too many others to do so" (ibid.: 56). His concern is that, if it was rational for chemists to embrace Lavoisier's oxygen theory, this would be telling evidence against Chang's claim that phlogiston suffered a premature death (ibid.: 51). Chang considers, and ultimately rejects, a number of arguments in the literature which purport to show that such a conversion was rational (2010, 49–61; 2012, 51–56). The important point, for my purposes, is that, unless Chang held the view that the rationality of elimination precludes the rationality of retention, and *vice versa*, he wouldn't be concerned with objecting to these arguments.

One of the conclusions that Chang draws from his discussion of the Chemical Revolution involves a related point. Chang claims that "sometimes scientists retain an epistemic object [like phlogiston or oxygen] (with modifi-

[9]These latter two points correspond to the deontological and consequentialist conceptions of rationality that are often discussed in the literature on that topic. See Samuels, Stich, and Faucher (2004, 166) for a good introduction to these two conceptions.

cations) when they could also decide to eliminate it, and sometimes they eliminate it when they could retain it" (2011, 426). He goes on to lament what he sees to be "an unwarranted and unproductive tendency toward elimination" (ibid.: 426), which indicates that, though scientists *could* eliminate, there's a sense in which they *shouldn't*. While Chang doesn't frame this in terms of rationality, it clearly lines up with the view of rationality discussed in the previous paragraph.

This view of rationality is one that I wish to question, and ultimately replace, in what follows. My own view is that it was rational to eliminate phlogiston, and it also would have been rational to retain it. I'll now attempt to show why both elimination and retention would have been rational, and in doing so, I'll once again make use of the identification of phlogiston with hydrogen.

I'll consider the rationality of elimination first. Once again, Kirwan's account of the controversy of the phlogiston theory will serve as a useful way to frame my discussion (1789, 6–7). As I've already noted above, Kirwan held that fixed air (carbon dioxide, CO_2), sulphur, phosphorus, charcoal, and the metals all contain hydrogen. By 1791, Kirwan's failure to isolate the hydrogen that he presumed these substances to contain led him to abandon the phlogiston theory:

> I know of no single clear decisive experiment by which one can establish that fixed air is composed of oxygen and phlogiston, and without this proof it seems to me impossible to prove the presence of phlogiston in metals, sulphur or nitrogen . . . (quoted in Partington, 1961, 664)

While Kirwan could have held out for longer, I take it that, by sometime in the early nineteenth century, it was rational for chemists to eliminate phlogiston for the reasons that Kirwan cites. More specifically, as the evidence against the existence of hydrogen in these substances grew, it would have been rational to eliminate phlogiston.

While I take it that the identification of phlogiston with hydrogen supports the rationality of eliminating phlogiston, I also see a way in which this same identification supports the rationality of retaining it. In short, the basic idea is that, insofar as it was rational for chemists to retain hydrogen, it would have been rational for them to retain phlogiston. My justification for the rationality of retaining phlogiston therefore differs from Chang's, though I do think that I can appeal to one of Chang's insights in support of my claim. In section 2.2, I discussed Chang's idea that, theoretically, oxygen and phlogiston were on more-or-less equal footing. Chang claims that the retention of oxygen was justified in terms of the operations by which chemists could produce it. And given that phlogiston theorists had similar operations for producing phlogiston, Chang concludes that there was no more reason to eliminate phlogiston than oxygen. One issue with Chang's proposal is that he considers a number of competing and mutually inconsistent theories regarding phlogiston, including Kirwan's 'inflammable air' theory, Priestley's 'electric fluid' theory, and Cavendish's 'elementary water' theory. In that case, determining *the* set of operations for producing phlogiston may prove difficult.[10] But if I am right about the identification of phlogiston with hydrogen, this would present a way of determining the operations by which one produces phlogiston—they are just the same operations by which one produces hydrogen. In that case, Chang's operational justification for retaining phlogiston applies even more forcefully once one takes into account the well-entrenched nature of the identification of phlogiston with hydrogen.

In sum, chemists were rational to have eliminated phlogiston, but it also would have been rational for them to retain it. And while I've taken issue with a number of Chang's arguments, I also take it that this conclusion gives us another route to the kind of pluralism which he advocates. The rationality of both of these decisions provides some reason to think that chemists should have maintained both of these competing systems. And though I've attempted to engage in a sober analysis of the possible harms of retaining phlogiston alongside oxygen, I've also attempted to point to some of the benefits of doing so. Moreover, insofar as my conclusion presents us with an invitation to explore some paths that were not taken in the actual history of science, I take it to be of a piece with Chang's conclusions regarding the Chemical Revolution.

5 Scientific Rationality More Generally

I'll close with some brief remarks about how the argument that I've presented connects to the more general issue of scientific rationality. If that argument is on point, then we must admit that, sometimes, when scientists are faced

[10] Indeed, Mauskopf (2013, 625) has criticized Chang on the grounds that "phlogiston meant different things to late-eighteenth-century chemists."

with the decision of whether to retain or eliminate a given entity, it may be the case that both options are rational.

Bas van Fraassen (1989, 171–172) has distinguished two conceptions of rationality, and in order to clarify my own view, it will help to situate it within what he has to say. He begins by discussing two concepts of law distinguished by Oliver Wendell Holmes: the Prussian concept, according to which "everything is forbidden which is not explicitly permitted," and the English concept, according to which "everything [is] permitted that is not explicitly forbidden" (ibid.: 171). Van Fraassen goes on to draw an analogous distinction between two conceptions of the rationality of belief, and defends an English conception, according to which it is rational to believe anything that one is not forbidden from believing. My concern, like Chang's, is not so much with the rationality of belief, but of decisions and judgments. What I am advocating, then, is an English conception of the rationality of decisions, according to which one can decide in favor of any action that one is not explicitly forbidden to perform. While it may be the case that scientific rationality sometimes forbids all but a single action, I've attempted to establish that, sometimes, multiple actions are rationally permissible, as retention and elimination are in the case of phlogiston.

While I take it to be poor methodology to generalize from the case of the Chemical Revolution to the whole of scientific activity, I do think that it would be useful to bring this conception of rationality to bear on the history of science. This would allow us to engage in a more general examination of the extent to which the decisions that scientists did *not* make would have been rational. One might question whether it would have been rational to eliminate the atom, once it was found to be divisible, or whether it would have been rational to retain the luminiferous ether, once it was found that there is no preferred frame of reference. Answers to such questions would have implications regarding the extent to which the results of science are contingent or inevitable, since rational scientists could have acted otherwise and preserved their rationality.[11] Moreover, given the frequency with which such cases are discussed in the scientific realism debate, answers to such questions may have implications for that debate as well. The conception of rationality that I am advocating, then, may be most fruitful, not in the questions that it answers, but in the questions that it encourages us to ask.

[11] See Soler (2008) for an introduction to this issue.

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