Acids and Rust: A New Perspective on the Chemical Revolution

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Abstract

This paper uses scientific perspectivism as a lens for understanding acid experiments from the Chemical Revolution. I argue that this account has several advantages over several recent interpretations of this period, interpretations that do not neatly capture some of the historical experiments on acids. The perspectival view is distinctive in that it avoids discontinuity, allows for the rational resolution of disagreement, and is sensitive to the historical epistemic context.

Keywords: Philosophy of Chemistry; the Chemical Revolution; perspectivism; realism; pluralism.

Introduction

A recent exchange between philosophers and historians of science has highlighted the continued importance of, and serious challenges associated with, interpreting events during the Chemical Revolution. Klein, Kusch, and especially Chang have offered three different explanations of the disagreements marking this period (Chang 2015; 2012; Klein 2015; Kusch 2015). There are three particular points of contention that these three authors address:

1) Was there a Revolution at all?
2) What was the disagreement about?
3) Was change during the Chemical Revolution well motivated?

This article seeks to provide clarification on these questions by examining several experiments that chemists conducted during the Chemical Revolution. I will argue that 1) there was not quite a revolution, that 2) the disagreements were about the classification of several substances, and 3) that change during this period was well motivated.

In section 1, I describe some existing interpretations of the Chemical Revolution, showing where those three points of contention arise. I then discuss the view that I think advances our thinking about the Chemical Revolution: perspectivism (section 2). Section 3 examines several neglected experiments from this period, particularly the work of Cavendish, Kirwan, and Lavoisier. I show that these disagreements are best understood as perspectival and how doing so advances our characterization of this period.

1.0 Interpreting the Chemical Revolution

Sections 1.1 to 1.2 lay out the debate in interpreting the Chemical Revolution. We can crudely divide the views into two. One view argues that this period was revolutionary in a strong sense. The other views emphasises the opposite: that there was great continuity in chemical theory and practice.

1.1 The Revolution was Revolutionary

One broad interpretive approach to the Chemical Revolution characterizes this period as revolutionary. Kuhn advocated this view (1976), as did Siegfried (Siegfried 2002; Siegfried and Dobbs 1968) and more recently Chang (2012). Chang’s view has sparked recent debate over how to interpret this period and I focus on his view with an eye toward advancing the debate between him, Kusch, and Klein.

Chang calls his reading of the Chemical Revolution normative pluralism (Chang 2015, 2012). This is the most thorough and extensive account this paper explores and so it is worth understanding the view. The thinking is scientists work in different systems of practice that are defined by their own set of methods, goals, and explanations. Because each system has its own aims, we should evaluate the activities or explanations produced by members of the practice according to its internal aims, but not according to the aims of other practices. Aims consequently function like internal incommensurable standards. There are strong similarities between systems of practice and perspectives as described in section 1. Both emphasize methods and explanations, for example. However, there is a crucial difference. While perspectivism allows for standards that can be assessed outwith a perspective or practice, normative pluralism allows only for the internal of knowledge claims (so no cross-perspectival standards).

These systems of practice more generally are incommensurable and cannot be evaluated in terms of one another, which may also bring to mind Kuhnian paradigms (Kuhn 1976).

However, unlike a paradigm a system of practice can co-exist with another system. A system of practice also offers an analysis based on activities first and propositions second, if at all, which is another point of departure from perspectivism. Another key feature of normative pluralism is the normativity. We should, Chang argues, promote a variety of systems and resist the imperialist tendency to follow just a single practice.

Chang is the main foil for this paper, but it is worth mentioning that there is a sociological reading of this period that is also commited to a pluralist reading of the Chemical Revolution. Kusch offers such a reading. He (2015) rejects two features of Chang's analysis: the normativity and role of social factors. For normativity, should the revolution taken the course it did? Was it justified? Kusch argues, contra Chang, that there were good reasons, social reasons, for adopting the oxygen system of chemistry. To do this, Kusch appeals to experimental work in Germany that won over chemists to the oxygen system. He uses Hufbauer (1982, 77) to argue that social factors provided sound motivation for adopting oxygen chemistry.[1] Chang has responded (Chang 2015) that the social motivation does not provide sufficient reason to abandon a system of practice like phlogiston chemistry. Despite this exchange, both Chang and Kusch think revolutionary change marked this period, i.e. there was extensive experimental and conceptual revision. They are both consequently offering pluralist readings of the Chemical Revolution.

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[1] Hufbauer actually claims he is assuming a social analysis and does not claim to be arguing for it. It seems unlikely that Kusch can actually use Hufbauer's research as evidence for a social reading given this admission.

1.2 The Revolution was not very Revolutionary

Both Kusch and Chang consider the Chemical Revolution a period of drastic change. Klein (2015) criticizes this interpretation, arguing instead that this period in chemistry saw more gradual change. She is not alone. Holmes (1995), Multhauf (1962), and Chalmers (2013; 2012) have also argued in different ways for thinking change during the Chemical Revolution was more gradual.

There are two arguments for this reading. One is that chemists throughout the 18th century endorsed the same ontology of substances; Klein and Chalmers in particular defend this view. The other argument is that the methods chemists used during this period do not change in a way that resembles a revolution. We might attribute this view to Multhauf (1962). If one accepts this broad view that there was no revolution, we must consider the changes we see during this period as relatively minor and part of a more continuous practice of chemistry.

Chang responded (2015) to Klein's criticism by acknowledging that there was indeed no drastic ontological change—and hence he agrees there was no revolution in this regard—but he argues the changes in methods and standards of judgement were quite drastic and that there were important semantic changes; if we defined a revolution in these terms, this period was revolutionary (2015, 92). Because Chang is the primary foil in this article, I am going to follow him in assuming that a revolution consists in changes in standards of judgement, semantics, and methods.

From this debate on how to characterize the chemical revolution, there are several main points of contention I wish to distil out:

1) Was there a Revolution at all?

2) What did change consist in?

3) Was the change well motivated?

A perspectival analysis will provide some answers to these questions by acknowledging the seriousness of the differences between oxygen and phlogiston chemists, but while also making room for resolution and continuity. The idea here is that past disagreements did become resolved (or at least scientists moved past them) and an account of those disagreements should reflect the fact that there was resolution and that the resolution can be understood rationally, i.e. that there reasons for it that can be understood epistemically.

2.0 Perspectivism

Several authors have recently used in different ways perspectivism as a lens for better understanding the sciences. Giere (2006) takes a model-based approach and attempts to show how scientific knowledge is always restricted and relative to particular models. My own view is related to the perspectival account that Massimi defends (2018; 2017; 2012). The relation is based on epistemic standards; this account does not necessarily endorse other elements of Massimi's account. Massimi argues that scientists across history have occupied different perspectives, where a perspective is understood as

> A scientific practice, including the epistemic claims, methodological resources, and justification endorsed by a scientific community. (i) the body of scientific knowledge claims advanced by the scientific community at the time; (ii) the

> experimental, theoretical, and technological resources available to the scientific community at the time to reliably make those scientific knowledge claims; and
> (iii) second-order (methodological-epistemic) claims that can justify the scientific knowledge claims advanced (Massimi in Ott and Patton 2018 page 152).

Unlike some other forms of pluralism, however, a scientific practice, or a perspective, also makes use of epistemic standards, that is, standards by which epistemic claims are evaluated. Crucially—and the idea with which this paper will engage when analysing the Chemical Revolution—epistemic standards are not perspective-relative, though the particular form they take might vary from one perspective to another (Massimi 2017, 418). Such standards include concepts like simplicity, explanatory scope, accuracy, objectivity, among others. The analysis in this article is going to make use of Massimi's definition of practice and the notion of cross-perspectival epistemic standards only. These philosophical tools, especially the epistemic standards, provide a helpful way of characterizing change during the Chemical Revolution.

How do these standards work in practice? To become accepted by a scientific community, a scientific claim must satisfy at least some of these standards. I take this to mean that a claim, to be satisfactory and receive the endorsement of a scientific community, must have some kind of appeal and we can understand this appeal in terms of meeting epistemic standards. Massimi (2017) argues that this is the case for best-systems of laws (the scientific laws we should endorse are those which strike the best balance between simplicity and strength) and although Massimi does not make this claim, for my analysis I think we should understand the point more generally: the scientific explanations we should endorse (as philosophers or scientists) are those which strike the best balance among sometimes competing epistemic standards. Understood in this way, epistemic standards are not mysterious free floating ideals that science must somehow meet, rather they give us a tool for thinking about how and why scientific claims and explanations are compelling and why they become endorsed by scientific communities.

These standards offer a potentially valuable tool in the philosophical analysis of science for several reasons. For one, epistemic standards are the kinds of things to which scientists can appeal *in their own historical contexts,* so the analysis promises to be historically sensitive. For another, such standards can also be evaluated and considered from different epistemic contexts, so they can offer us as contemporary readers of science the opportunity to understand how past science has changed and why.[2] Finally, epistemic standards are more epistemically available than robust metaphysical versions of truth, which are only indirectly regulative (see Psillos 1999, chap. Introduction) and not defeasible.

These standards will form the basis of my analysis of the Chemical Revolution and I will use them to show that changes in the chemists' understanding of acids was not as radical as it may seem and that the change was well-motivated.

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[2] Massimi (2018, 354)discusses how epistemic standards can be evaluated from within a variety of scientific perspectives (or epistemic contexts).

I will begin by focusing on a few particular experiments and exchanges between three natural philosophers: Cavendish, Lavoisier, and Kirwan. I have chosen these three because Cavendish and Lavoisier conducted very similar experiments on the same substances and Kirwan brought the conclusions that Cavendish and Lavoisier reached into more explicit and direct conflict.

3.0 18th Century Chemistry

This paper takes an interest in the early work on acids, an approach that has several advantages. The first is that the experimental context became incredibly complex once phlogiston became more explicitly and centrally debated later in the 1780s. Part of the complexity stems from interconnected issues of acidity, calcination, combustion, and the composition of water and metals. The early acid experiments were less muddled by this complexity. The second advantage is that this subject is relatively self-contained and shows clearly the similarities and differences in experimental practice between very influential chemists who offered very different explanations of the same phenomena. The final advantage is that all chemists involved had extensive common commitments. This common ground narrows and emphasizes what precisely the differences were between these approaches to the study of acids.

It is particularly important for this article that we examine several experiments by different chemists because only by comparing the work of different scientists will it be possible to see where the problems are. Furthermore, any particular experiment is better understood by contrast with what other chemists were doing and why we find the similarities and the differences that we do.

3.1 Acids

In the 18th century, the acids were contrasted with the alkalis, or bases. Acids, a type of simple substance that cannot be further decomposed, effervesced when combined with an alkali and formed a neutral salt, a complex substance composed of the original acid plus an alkali[3]. The effervescence produced an air and because experimenting with acids allowed chemists to produce airs from solid substances, they were a natural substance to use when attempting to understand the different types of airs. Crudely, phlogiston and oxygen chemists offered rival explanations of what acids were and how they reacted with other substances. The former, in general, considered acids a simple substance and the latter considered it a complex substance formed by oxygen and another substance. Let’s first examine how Cavendish, a phlogiston chemist, experiments with acids and metals before turning to Lavoisier’s work.

3.1.1 Cavendish’s Early Study of Acids

To study the behaviour of acids and of metals, Cavendish used laboratory equipment that allowed him to combine substances with great control and to measure the products of the

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[3] See Siegfried 2002, chap. 4 for a discussion of this distinction.

experiment (1766, reprinted in (1921), 77). I will describe his apparatus in some detail because it will illustrate the great similarities in experimental practice that he shared with Lavoisier (discussed in section 3.1.2).

Cavendish used a bottle to hold a mixture of acids and metals. To the top of the bottle he affixed an S-shaped glass tube. This tube curved down through a large vessel full of water and then up into another bottle, this one inverted. This second bottle he filled with water and inverted so that the bottle opening was submerged in the vessel of water. In essence, Cavendish connected two bottles with a glass tube and ensured there would be no leakage by submersing key parts of the system underwater. As airs were produced in the first bottle, they would travel through the glass tube into the second bottle, where they would displace water. Based on how much water was displaced, Cavendish could measure how much gas was produced.

With this apparatus, Cavendish investigated the reactions of three metals with three acids. The metals were zinc, iron, and tin. The acids were vitriolic acid, spirit of salt, and nitrous acid. He dissolved the metals in the acids, one acid and one metal at a time, in the first bottle and captured the gas produced in the second bottle. Generally, he found inflammable air was produced. Or, when not inflammable air, some kind of acidic fumes (Cavendish in 1921, 78–79). Although he does not here discuss how he identifies the gas as inflammable air, we might infer from later passages (*Ibid*, 80) that Cavendish did so by observing whether the gas ignites when lit with a flame. Cavendish measured precisely by weight how much metal he used and how much gas was produced.

We can see in this work that Cavendish was highly quantitative, something he was well known for (McCormmach and Jungnickel 2016, 171). He starts his experiments with careful measurements and diligently weighs the results. This clearly shows he is familiar and has great facility with quantitative chemistry, a skill often attributed to oxygen chemists, but less often to phlogiston chemists.

Cavendish offers the following interpretation (1921, 79) of his experiments on these acids and metals:

> It seems likely from hence, that, when either of the above-mentioned metallic substances are dissolved in spirit of salt, or the diluted vitriolic acid, their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are dissolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiston unites to part of the acid used for their solution, and flies off with it in fumes, the phlogiston losing its inflammable property by the union. The volatile sulphureous fumes, produced by uniting these metallic substances by heat to the undiluted vitriolic acid, shew plainly, that in this case their phlogiston unites to the acid; for it is well known, that the vitriolic sulphureous acid consists of the plain vitriolic acid united to phlogiston.

Cavendish interprets this experiment as showing that two types of acid—spirit of salt and vitriolic acid, dissolve both zinc and iron and when they do, they effervess, releasing an inflammable air and this air is phlogiston.[4] Crucially, the metal, and not the acid, supplies the phlogiston that is released. The acid is here a simple substance that functions to drive out the phlogiston from the metal, leaving behind another simple substance: the calx. In its gaseous state, the phlogiston forms inflammable air, which can be ignited by flame. This would be nicely consistent with the idea that phlogiston is involved in all forms of combustion. However, if the acid is *nitrous* acid, the phlogiston combines with some part of the acid once the metal is dissolved and forms the acidic fumes that Cavendish isolated. As it joins the acids, the phlogiston loses its inflammable property and therefore cannot be ignited by flame. At the end of this passage, Cavendish appeals to what he takes to be a well-known fact: that vitriolic sulphurous acid is formed by the combining phlogiston with vitriolic acid. We can assume that he takes this for granted and is not directly testing it as an experimental hypothesis.

The classificatory choices Cavendish made, i.e. the way he individuated substances, was based upon what he was able to isolate, in this case inflammable air. Because the reaction of metals and acids seemed to produce a product that could be isolated, it is natural to think that metals are complex substances that give off the inflammable air. Cavendish classified metal as complex on the basis of what the experiment produced. His choice of what to measure led him to this conclusion. As we will see shortly, this is a markedly different

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[4] This is not the only interpretation of the relationship between phlogiston and air during the 1770s. Priestley (Priestley 1775), for instance, does not associate it with inflammable air.

strategy for classifying metals when compared with Lavoisier's work and is one important basis, I argue, of the disagreement between oxygen and phlogiston chemistry.

It is tempting at this point to follow Ladyman and Blumenthal (2017) in ascribing to Cavendish two "mistakes."[5] One was neglecting to weigh everything he should and the other was to appeal to a hypothetical entity, phlogiston, in analysing his results. It is a temping analysis because, to the modern eye, it is natural to weigh everything and phlogiston is not hypothetical, it is non-existent. However, we should resist this temptation because it is subtly anachronistic. Although Cavendish did not weigh all he could have, there is scant evidence that his contemporaries identified this as a mistake, suggesting his actions fit contemporary scientific norms. It is also difficult to see how appealing to phlogiston could be a mistake. Phlogiston was accepted as a legitimate explanation since Stahl (1730) and there was as yet no salient reason to *avoid* appealing to phlogiston. There is also a more general problem with an analysis that calls these "mistakes." It suggests that scientists can conduct their work while making no assumptions, taking no background knowledge for granted, and letting no hypothesis go untested at any time. Such an ideal just does not sit well with the practicalities of science. Every scientist must take at least some things for granted.

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[5] I am grateful to an anonymous reviewer for bringing this to my attention.

3.1.2 Lavoisier’s Lime, Chalk, and Acid

Lavoisier’s early work on acids was motivated by an interest in how elastic fluids became fixed and separated from inelastic substances (Lavoisier 1776, 221).[6] An elastic fluid is fixed when it is somehow bound up in a solid (or inelastic substance) and it is associated with airs when it is separated from solids. Cavendish, recall, used the term “factitious” to talk about substances that are sometimes in or part of solids and at other times in or part of airs. The similarity between these interests should be striking: both were investigating the relation between airs and solids. To do this, Lavoisier conducted a series of experiments that resemble very closely Cavendish’s work, with a subtle difference that led to very different conclusions and a very different classification of substances.

For the first experiments, Lavoisier dissolved iron power in nitrous acid to its saturation point (1776, 293). He carefully weighed both the acid and the iron before mixing and diluting the solution with distilled water. He separated out two portions of this solution and added chalk to one, lime to the other (1776, 294–95). Upon adding the chalk or lime, iron rust precipitated out of the solution. Had he not added the chalk or lime to the iron solution, the iron would not have precipitated out and would have remained dissolved. He uses lime

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[6] Lavoisier had previously worked on other topics in chemistry (see Guerlac (1966) or Holmes (1988) for example), but the acid work is particularly relevant here because of its connection to Cavendish and because Lavoisier’s evolving views on oxygen emerge here.

and chalk as a kind of control; the results cannot be just a peculiar feature of lime, for example, because the results are similar when using chalk.

The point in discussing such detail is to show that thus far Lavoisier’s work is very similar to Cavendish’s, apart from precipitating the rust. Lavoisier has dissolved a metal in an acid, just as Cavendish did. He also weighs his starting ingredients and his results. However, Lavoisier is at this point more interested in the metal than the vapour and makes an important departure from Cavendish. Lavoisier washes the iron rust in distilled water and weighs it (1776, 295). He finds that the calx is heavier than the metal was before being dissolved in the acid. This is true for both the lime and chalk solutions, though the chalk solution produced heavier iron rust than the lime solution. Cavendish does not isolate and weigh the calx he produced; he was principally concerned with what experimental products he could isolate and measure. Lavoisier concludes the following:

> The results of these experiments are, 1st, that iron and mercury dissolved in the nitrous acid, acquire in general a remarkable increase of weight, whether they be precipitated by chalk or by lime. 2ndly, that this increase is greater in respect to iron than to mercury, 3rdly, that one reason for thinking that the elastic fluid contributes to this augmentation is, that it is constantly greater when an earth is employed saturated with elastic fluid, such as chalk, than when an earth is used which has been deprived of it, as lime (Lavoisier 1776, 295).

Calxes, such as iron rust, are formed from metal and an elastic fluid and are therefore compounds. Because the calx is heavier than the metal, it stands to reason that there is more substance to the calx than the metal when the quantity of metal is sufficiently controlled. Because the amount of metal was controlled in this experiment, the extra weight must have come from somewhere. Lavoisier attributes the gain in weight to the elastic fluid in part because chalk contains more of it than lime. Recall that the iron rust produced by using chalk was heavier than the rust produced by lime. Lavoisier demonstrated this difference between lime and chalk in a series of earlier experiments (Lavoisier 1776, chap. 1).

Thus far Lavoisier seems to have shown that metals are simple and calxes compound, which is opposite to the conclusion that Cavendish reached. Lavoisier makes no appeal to phlogiston, though he also as yet makes no reference to oxygen, except as the “elastic fluid” that binds with metal to form rust.[7] But the crucial difference here is that Lavoisier classifies metals as simple substances whereas Cavendish classifies them as complex. This is the crux of the difference between these interpretations, though other important differences exist. The problem is therefore a classification and identity problem and it emerges from a heavily shared experimental and classificatory practice. This appeal to phlogiston and oxygen in these rival explanations is secondary to the classificatory problem because the appeal is only

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[7] Lavoisier choice of terms changes. Eventually he uses the term “oxygen” (Lavoisier in 2015) but in earlier work we can find him referring to an “elastic fluid” (1776) or “pure air” (1777).

possible once a chemist had made a choice about what to measure. Phlogiston only offered explanatory interest *after* Cavendish isolated the products of his experiment just as oxygen only had explanatory value *after* Lavoisier measured the weight of metal and calx and found the latter heavier.

So far I have shown that classificatory differences emerged in chemistry over the problem of acids and metals. There is, consequently, reason for thinking that there were some differences in practice between oxygen and phlogiston chemists. They were, to use the terms introduced in section 1, part of different perspectives because they used different justifications for different epistemic claims. However, I have not as yet shown any disagreement. I now turn to this topic to show that not only was there extensive shared background prior to the disagreement, there was also a shared interest in resolution, in establishing how best to approach explanation in chemistry, and a shared appeal to epistemic standards.

3.2 Oxygen Confronts Phlogiston

Cavendish and Lavoisier, although engaged in very similar projects, are unaware of one another's experiments and alternative conclusions about the nature of metals. This difference becomes apparent later and is particularly striking in the work of Richard Kirwan (1733-1812) who, like Cavendish, was a phlogiston chemist, but communicated regularly with oxygen chemists.

By the time Kirwan (1787) addressed questions associated with metals and acids, the differences between Lavoisier and phlogiston chemists became far more complex and connected to a number of related problems. One striking criticism that Kirwan levels against Lavoisier concerned the basis for classifying metals and calxes. By this time oxygen chemistry became much more explicit and Lavoisier no longer refers to the "elastic fluid" present in calxes in general terms, but calls it "oxygen." Acids, both phlogiston and oxygen chemists claimed, were compounds. The oxygen chemists claimed they were formed from oxygen combined with a base, whereas phlogiston chemists claimed that phlogiston was a component of acids.[8] This is a departure from Cavendish's earlier work, which did not consider phlogiston a component of the acids.

Kirwan's main criticism comes from his 1787 *Essay on Phlogiston.* This text provided the most recent and powerful defence of phlogiston theory. Lavoisier's wife, Marie-Anne, translated Kirwan's text into French. Added to Kirwan's passages were responses from leading antiphlogiston chemists, including Lavoisier. The French version with those responses was translated back into English two years later by William Nicholson of London. A crux argument in the text concerned affinity tables and how Lavoisier used them. It is worth discussing these tables in detail because they were an important part of chemical explanations.

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[8] Kirwan offers a brief discussion of these two views of the composition of acids (Kirwan 1789, 38–39).

3.3 Affinity Tables and Their Use

Affinity tables were a widely used explanatory tool in 18th and early 19th century chemistry.[9] Klein has argued (1994) that affinity tables formed the basis of the concept of composition during this period. The first affinity table was constructed by Geoffrey (1718) and made much more systamtic by Bergman in 1778.[10] The use of these tables declined after concerted criticisms by Berthollet that began shortly before 1800.[11] Before its decline, the basic principle of the affinity table was that substances are attracted to one another and combine because of their affinities. Different substances have different, but regular, affinities for one another. For example, if three substances are mixed, then the two with the strongest affinity for each other will combine, to the exclusion of the third.

These tables were initially useful for several reasons. They explained why acids, metals, and bases behaved the way they did; they provided order to the wide range of substances that chemists studied; and they allowed chemists to make predictions about how much of which substances would combine. Between Bergman's table and Berthollet's criticism of affinity

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[9] As Holmes points out (1962), the concept of affinity had different uses. One was to explain the combining of different substances. Another use was to provide a systematic ordering of substances, as in the affinity tables.

[10] This was translated into English a few years later (Bergman 1785).

[11] For a discussion of the fate of affinity tables, see (Holmes 1962).

tables, a number of chemists—including Kirwan, Morveau, and Fourcroy—worked on filling in the tables with more detail and more substances while attempting to also provide quantitatively measurements of affinity (Kirwan 1789, 173).

Lavoisier was interested in using affinity tables, not to accurately predict reactions as many other chemists did, but merely to provide order and explanation. For him, affinity tables could explain how oxygen, the elastic fluid of his early experiments, behaves. Oxygen, as the principle of acidity, has an affinity for other substances and therefore it was quite natural to explain its behaviour with the already widely used affinity tables. Substances combust, so this explanation goes, because of their affinity to oxygen and they stop when the source of the oxygen is at equilibrium with the combusting substance (Holmes 1962).

The problem with Lavoisier's tables was that they had many exceptions that could be easily demonstrated through experiment. This suggested Lavoisier's table was inadequate. Without a satisfactory affinity table, oxygen chemistry might have been a much less compelling account because it would be unclear why a substance would combust and why it would stop combusting. The phlogiston chemists could neatly provide this explanation by appeal to phlogiston: a substance burns when phlogiston is released and stops when the phlogiston is depleted. But why should oxygen bind with iron when affinity tables suggest it does not? And why should combustion stop at any point once it has started? Kirwan writes:

> Besides, though iron and zinc are the only metals which by Mr. Lavoisier's table have a greater affinity to the oxigenous principle, than inflammable air has to that principle; yet inflammable air is also set loose during the solution of other metals,

> which by that table have a weaker affinity to the oxigenous principle than inflammable air has to it (1789, 176).

Kirwan points out that that oxygen chemistry is committed to the idea that when metals are dissolved in a water and acid solution, inflammable air is produced. Their explanation for this is that because water and acids are at least partly composed of inflammable air and oxygen.[12] The metal binds with the oxygen to form a calx and the inflammable air is released as a vapour. However, there is a tension in the oxygen account, so Kirwan's criticism goes, because Lavoisier's affinity table suggests that only iron and zinc have a strong enough affinity for oxygen to separate it from the inflammable air, and yet when other metals are mixed in acid, inflammable air is released.

Kirwan's alternative explanation (Kirwan 1789, 45) is that when water and metal are combined, the water drives inflammable air out of the metal. Calxes are what remains after the inflammable air is driven off. The water explains the fact that calxes are heavier than metals. This explanation may be reasonable because water is heavier than inflammable air and it does not encounter the contradictions that Lavoisier's affinity tables seemed to face. We can see here that Kirwan's explanation is more consistent with Cavendish's early work in

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[12] A related problem is the question of the composition of water. Oxygen chemists believed water was compound while many phlogiston chemists, including Kirwan, believed water was simple.

that both Cavendish and Kirwan argue metals are compound substances and that part of their composition includes inflammable air *and* that inflammable air is phlogiston.

There are two things to note about Kirwan's explanation of the composition of metals. First, the notion of which substance is simplest is somewhat obscured because both calxes and metals are not simple and isolated substances (metals are partly composed of phlogiston and calxes have great quantities of water in them). This suggests Kirwan was not as interested in composition as he was in other topics like affinity tables. Second, Kirwan did not take issue with Lavoisier's weight data. He accepted that calxes were heavier than metals and that this was puzzling. But he did disagree over what type of inference this data permitted. Any conclusion, Kirwan seemed to have thought, required a consistency with affinity tables. Affinity tables, in other words, constrained the type of inferences one could make from the data, a constraint that Lavoisier did not recognize. The main point of difference between these chemists that I would like to emphasize is this: Lavoisier distinguished the elements from the compounds by weight and Kirwan did not. I will return to this point below.

Lavoisier responded to this worry about inconsistencies by claiming that the affinity tables were not meant to account for experiment directly, but to explain why different substances combust.[13] And not only are the tables not intended to predict, they *cannot* make predictions about most experiments directly because they have not been sufficiently perfected due to a practical limitation: affinities vary with temperature and temperature is not incorporated into affinity tables. To fully explain the results of experiment requires a

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[13] Lavoisier in (Kirwan 1789, 45)

specification of temperature, which would lead to a new affinity table for each incremental change in temperature.[14] Such a project is impracticable. So Lavoisier is not committed to affinity tables that provide consistent, experimentally verified predictions.

This discussion so far has shown two things. First, all chemists I have so far discussed were interested in the reactions of acids and metals and the gases produced. Second, there were important differences in the conclusions each chemist reached despite this common interest. The difference was in how these chemists classified those substances they studied. These differences suggest to me that we can assert that Lavoisier and the phlogiston chemists were part of different perspectives because they offered rival classifications of the same substances and their methods for assembling those classifications differed, though subtly.

3.4 Evidential Reasoning

Having discussed the details of this early work on acids, let’s return to the three questions we began with, which were (1) was there a revolution; (2) what was the disagreement about; and (3) was change well-motivated. In addressing these questions, I will be critically engaging with the work of Hasok Chang because of his extensive work on the topic and directness with which he addresses these questions.

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[14] Lavoisier also studied heat in other contexts and methods to quantitatively measure it.

The easiest to answer is (2), which is that the debate was about classification, specifically the classification of primitive substances. Oxygen and phlogiston chemists used different classification methods and consequently they classified metals and acids differently.

The answer to (1) is more complex to address, but the answer will be no. A revolution suggests the extensive replacement of the conceptual and experimental practices with a new set of practices and concepts, resulting in a new conceptual scheme that is incommensurable with the old scheme. I take this to be part of Kuhn’s original thought (Kuhn 1976) and Chang also takes some defining features of revolutions to includes changes in methods, semantics, and standards of judgement, with methods in particular being crucial (2015). There is something to this interpretation, in particular I am struck by the conceptual changes we do see in the 18th century. Water, calcination, metals, and acids (to name just a few substances) came to be classified differently, suggesting the kinds of revolutionary change that Chang and Kuhn describe.

Although I do not wish to reject the importance of these conceptual changes, I think it is easy to overplay how revolutionary they were. Conceptual revision happens within a larger context of continuity and even though there was great conceptual change during the Chemical Revolution as Chang and Kuhn suggest, my analysis indicates these changes were part of a continuous chemical practice and such continuity places a very different kind of emphasis on this period, an emphasis that suggests a strong reading of revolution is not quite right.

For instance, I have shown that for the work on acids, there was extensive experimental and conceptual continuity between phlogiston and oxygen chemists. Indeed, there had to be this kind of continuity at some level because, without it, there would be no basis for disagreement and it would be impossible for phlogiston and oxygen chemists to have a meaningful discussion about the same experiment. That is, both oxygen and phlogiston chemists had to recognize at least some of the same problems and appreciate the force of the arguments that one another gave (hence standards were not fully internal). If there had been a strong break between oxygen and phlogiston chemistry, and hence a revolution, then it is hard to see how there could be a basis for disagreement. However, I do not take this article to have shown that this was the case for all of late 18th century chemistry. We have only seen that this is the case for some of the work on acids.

It is worth distinguishing here two senses of "revolution," one technical, one informal. A revolution in the technical sense concerns radical conceptual or experimental unheaval. This is the kind of revolution that interests Kuhn (1976) and Chang (2012, 2015). This is the kind of revolution that interpreters of this period are typically using and I am not in this article going to take issue with this definition. Contrast this technical kind of revolution with a more informal notion. An informal use of "revolution" involves great change, but without specifying, or requiring, conceptual or experimental revision. Chemists during and since the 18th century referred to the great changes in chemistry as a "revolution," but this is informal and possibly even rhetorical. One can consider this period revolutionary, i.e. a period of great change, without requiring systematic conceptual and experimental revision. I have argued that there was not revolution in this more technical sense, but there likely was a revolution in the informal sense.

Another reason for thinking that there was more continuity during the Chemical Revolution than we might expect concerns the evolving epistemic aims different chemists shared. One might be tempted to agree with Chang (2012) and provide a pluralist analysis of this period that shows different chemists had different interests and consequently appealed to different standards of judgement when evaluating experiments. Although epistemic aims were undoubtedly important, the full story cannot stop with this explanation because of how aims changed and how those changes informed and were informed by other considerations such as experimental results and classification. It is worth, therefore, revisiting some of those epistemic aims and the role they played in the work of Cavendish, Lavoisier, and Kirwan. There was a lot more common ground than we might expect and consequently, it is difficult to see standards of judgement that differ so fundamentally.

Cavendish, as a chemist working in the pneumatic tradition, was very concerned to isolate the gases produced by experiment. Some of his experimental choices can therefore be traced to the tradition in which he worked. The situation was more complex, however, because Cavendish had several evolving goals. The classification of “airs,” or gases, became troublesome for natural philosophers when it came to the factitious airs,[15] which possess some kind of important connection to more solid substances. This problem was probably a motivation for Cavendish, but it was not the only motivation. The unusual properties of

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[15] Factitous airs in the 18th were generally understood to be those airs which could be produced by experiment. Cavendish says more specifically that factitious airs were “[...] any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art” (Cavendish 1766, 141). This is the definition of factitious air that this paper will use.

arsenic were also a puzzle that motivated him. Cavendish’s goals for his work in chemistry, I am suggesting by appeal to these cases, varied. Any one goal was not necessarily the only goal at play and no one goal necessarily guided Cavendish through his entire experimental practice. However, it is fair to say that his experimental pedigree (pneumatic chemistry) led him to ask certain questions and to use the relevant methods.

Lavoisier also worked in the pneumatic tradition and so was also interested in isolating gases and determining their relationship to solid substances. But Lavoisier did choose to weigh aspects of his experiments that Cavendish did not, specifically the calx.

This emphasis on weight may be unsurprising given his interest in the balance, not simply as an instrument of measurement, but as a principle of natural and economic regulation. Bensaude-Vincent (1992) suggests the balance symbolizes for Lavoisier a kind of natural order. The weight that was conserved through reactions was just one example of a kind of natural regulation that provided a kind of balance and equality (*Ibid.,* p. 227). Given this background, a willingness to consider weight paramount is unsurprising. However, this inclination to measure all that could be measured did not exhaust Lavoisier’s interests. Later, Lavoisier was also interested in distinguishing the compounds from the elements, those substances which could be decomposed from those which could not. This project eventually culminated in a collaborative revision of the nomenclature of chemical substances on the basis of elements (1796; 1787). So we might say Lavoisier was interested in a unifying principle for identifying elements and providing a classification on this basis, but was not as interested in problems like anomalous metals, even though he also took an interest in the connection between gases and solids.

Kirwan (1782) also had experience measuring weight and was skilled at it. However, Kirwan also worked extensively on affinity tables and took quite seriously the explanatory power an affinity table offered (Kirwan [1782]).[16] Given this interest, it is unsurprising that Kirwan was particularly concerned with Lavoisier's table and less concerned about the other features of oxygen chemistry. And this is the case even though Kirwan has a lot in common with Lavoisier. Both, for instance, find the weight gain of calxes important.

Despite these differences, the broadly shared experimental practice and classification would have made it hard to avoid adopting oxygen chemistry in some form. I say "some form" because oxygen chemistry was not the only logical possibility, but in the second half of the 18th century, long-term adherence to the available alternatives may have been difficult. This brings us to question (3): was the disagreement well-motivated?

3.5 The Appeal of Oxygen

Chang (2012, sec. 1.2) claims that there was little reason during the Chemical Revolution to suppose weight could be used reliably in chemistry and that, as a consequence, Lavoisier's work was not as compelling as it may seem to the modern reader who is now accustomed to the conservation of mass. There is indeed a puzzle here: given that producing isolated

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[16] Affinity tables were not his only interest; he was more generally concerned with defending phlogiston against oxygen chemistry.

products and weight were both well-established features of chemical practice, why prefer one over the other? Data, a typical form of empirical evidence, fail in this case to provide the kind of straightforward confirmation one might expect. This is because different chemists chose to weigh different substances.

Despite these caveats, I will suggest that there was good reason for an 18th century chemist to use weight as a criterion for classifying reactions and substances. I will suggest that cross-perspectival standards that I discussed in section 2 provide sound motivation for using oxygen to explain the nature and composition of acids. Standards such as simplicity, repeatability, objectivity, and precision were clearly at play in Lavoisier’s use of weight and they were not foreign to the work of the other chemists. It was consequently natural to follow Lavoisier, even though his theory was new and even though there was no rational compulsion to doing so. There are three motivations for this claim, which come not only from this historical case, but also from research by Multhauf (1962, 84). These three motivations will suggests that weight satisfies cross-perspectival standards and therefore that a wide range of chemists, not just oxygen chemists, would find weight very persuasive. If this is correct, then the use of weight shows that there was a serious form of experimental and evidential continuity through the Chemical Revolution, despite the conceptual change associated with oxygen suplanting phlogiston. Let’s examine those three motivations in more detail.

First, weight is quantitative, precise, and repeatable; measurements can be retaken with the same result, substances need not be isolated to be weighed, and it is objective (different chemists weighing the same substance can get the same result). These are some reasons for thinking that weight offers a perfectly good kind of data to be used as evidence and that it could be a reliable method for determining what substances one had. Indeed, all chemists I discussed weighed substances quite diligently. It was after all not weight full stop that distinguished Cavendish, Lavoisier, and Kirwan. What differed was the persistent application of weight as a criterion for distinguishing the element from the compound that made Lavoisier's work stand apart.

Second, weight was already a well-established part of chemistry methods. We can see that Cavendish, in addition to Lavoisier, was skilled at measuring the weight of different substances. He was not alone. Kirwan and many of the other leading chemists used weight with high precision.[17] It would have been, this suggests, quite natural for Lavoisier to use weight to track oxygen and determine the composition of different substances. It was a new part of the chemistry practice, but not very unlike what others were doing. Indeed, despite initial scepticism, many chemists, including Kirwan, came to accept Lavoisier's work quickly.[18] It would have been difficult for chemists to maintain scepticism of weight data. Weight was, after all, already a trusted tool.

Thirdly, not only was weight quantitative and well established, it also fit neatly with intuitive conceptions of simplicity and complexity. It is quite natural, even trivial, to think that simple things weigh less than more complex things, all else being equal. If you take any

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[17] See Multhauf (1962) for a discussion of how widely used the balance was during and preceding the Chemical Revolution.

[18] For a discussion of Kirwan's work and his eventual conversion, along with Berthollet, to oxygen chemistry, see Mauskop (2002).

two things and put them together, be it coin, fruit, or horses, the sum weighs more than a single component. This point is quite general. The only exceptional thing that Lavoisier does with this intuitive idea is to consider it paramount and the basis of determining what is elemental. Weight takes priority, say, over affinity tables. The consequence is that someone like Lavoisier is perfectly happy to tolerate exceptions to affinity tables, something Kirwan was unwilling to do because he considered affinity tables much more important. Recall that Kirwan's explanation of the weight gain of calxes entailed that the notion of simple substance played little or no role, a consequence he accepted, but which Lavoisier would have found intolerable.

3.6 The Character of Revolutions

I argued in the last section that it was reasonable to follow Lavoisier's use of weight and perhaps difficult to endorse an alternative. It was reasonable because his use of weight can be justified by appeal to cross-perspectival standards such as simplicity, repeatability, objectivity, and precision. But this was not conclusive: dissent was not irrational, just difficult to maintain. Phlogiston chemists were able to support their classifications and conclusions with reasons and the oxygen chemists did face issues of their own. This complexity raises two lingering questions. First, have I set aside important complexities and under-emphasized important differences between the experimental practices that studied acids? Second, how should we characterize the changes during this period? Was it revolutionary? What did the revolution consist in?

Setting aside as many complexities as I did was justified because although it was a complex period, complexity is not what made this disagreement about acids distinctive. Any scientific disagreement has the potential to be become complex. Within this complexity there were certainly important differences and this paper did not explore all of them. However, my purpose was to show the neglected similarity between these chemists. Of course there were differences, but in light of the similarities, they were not as fundamental as some pluralist readings suggest.

For the second question, how to characterize the revolution, there are some reservations we may have about taking a strong interpretation of the changes during this period. The first reservation is that both phlogiston and oxygen chemists recognized the same problems and were interested, generally, in solving those problems. In other words, the differences in their practices were too small. The composition of metals was not just of interest to Lavoisier, Kirwan thought it worth exploring and arguing about. The projects were too closely related for either to ignore the other’s work. Oxygen chemistry influenced and stimulated phlogiston chemistry and vice versa. As a consequence, each chemist was able to respond to the other because of the significance each experiment had to the overall project of trying to classify and understand a set of substances. If the debate is indeed structured as I am suggesting, then it is hard to imagine how, say, an oxygen chemist could be said to be engaged in a project that differed from the project of a phlogiston chemist. The smallness of these differences between phlogiston and oxygen chemists—and the closeness of their practices—suggests perspectivism, and not a kind of strong revolution, is the more suitable analysis.

The second problem with a strong form of revolution is that, as I argued, there was extensive shared background to oxygen and phlogiston chemists, including shared standards of judgement that transcended the particular perspectives in which particular chemists worked. All worked in the tradition of pneumatic chemistry, all used broadly similar methods that involved isolating substances, weighing them, and exploring their properties. They were also all exploring substances that were not far removed from everyday life. Metals, water, and acids are not obscure substances and there was no disagreement about what were instances of these substances. The issue was how to classify them, as I argued earlier. The classificatory problems were not particularly obscure either, even though solving them was complex and difficult. Given the complexity of the task facing these chemists, it was certainly reasonable for there to be what we might call a period of uncertainty. But this is not a permanent state. Eventually, after more debate, experiments, analysis, and the revisiting of old work, the debate became resolved.

4.0 Conclusion

The first half of this paper (sections 1 and 2) argued that a recent debate about how to characterize the Chemical Revolution left room for another account that showed more continuity in chemical practice while also showing sensitivity for the historical context. I suggested we call this perspectivism. Starting in section 3, I revisited some historical experiments and showed that the disagreement was about classification and that it emerged from a broadly shared experimental practice. This discussion showed that the revolutionary reading of the Chemical Revolution is not quite right. I described how perspectivism helps further our understanding of this period and how it differs from existing interpretations. We need a view that a) treats disagreeing scientists as part of the same practice, b) that evaluates the disagreement within the historical context, and c) that shows how or why disagreements are resolved. I believe that perspectivism meets these requirements.

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Literature Cited

Bensaude-Vincent, Bernadette. 1992. “The Balance: Between Chemistry and Politics.” *The Eighteenth Century* 33 (3): 217–37. http://www.jstor.org/stable/41447871.

Bergman, Torbern. 1785. *A Dissertation on Elective Attractions*. Translated by Thomas Beddoes. J. Murray and Charles Elliot.

Best, Nicholas W. 2015. “Lavoisier’s ‘Reflections on Phlogiston’ I: Against Phlogiston Theory.” *Foundations of Chemistry* 17 (2): 137–51. https://doi.org/10.1007/s10698-015-9220-5.

Cavendish, Henry. 1766. “Three Papers, Containing Experiments on Factitious Airs.” *Philosophical Transactions of the Royal Society* 56: 141–84.

———. 1921. *The Scientific Papers of the Honourable Henry Cavendish: Volume II*. Edited by James Clerk Maxwell and Joseph Larmor. Cambridge: Cambridge University Press.

Chalmers, Alan. 2012. “Intermediate Causes and Explanations: The Key to Understanding the Scientific Revolution.” *Studies in History and Philosophy of Science* 43: 551–62.

———. 2013. “Review of H. Chang, Is Water H2O?” *Science and Education* 22 (4): 913–20.

Chang, Hasok. 2012. *Is Water H2O?: Evidence, Realism and Pluralism*. Vol. 293. Springer Science & Business Media.

———. 2015. “The Chemical Revolution Revisited.” *Studies in History and Philosophy of Science Part A* 49: 91–98. https://doi.org/10.1016/j.shpsa.2014.11.002.

Geoffrey, Etienne. 1718. “Table Des Differents Rapports Observés En Chimie Entre Differentes Substances.” *Histoire de l’ Académie Royale Des Sciences: Avec Les Mémoires de Mathématique et de Physique Pour La Même Année*, 202–12.

Georg Ernst Stahl. 1730. *Philosophical Principles of Universal Chemistry*. Edited by Translated by Peter Shaw. London: Osborn and Longman.

Guerlac, Henry. 1966. *Lavoisier—The Crucial Year*. Ithaca: Cornell University Press.

Holmes, Frederic L. 1995. “The Boundaries of Lavoisier’s Chemical Revolution.” *Revue d’histoire Des Sciences* 48 (1): 9–48.

Holmes, Frederic L. 1962. “From Elective Affinities to Chemical Equilibria: Berthollet’s Law of Mass Action.” *Source: Chymia* 8 (1962): 105–45. http://www.jstor.org/stable/27757221%0Ahttp://about.jstor.org/terms.

———. 1988. “Lavoisier’s Conceptual Passage.” *Osiris* 4: 82–92.

Hufbauer, Karl. 1982. *The Formation of the German Chemical Community, 1720-1795*. University of California Press.

Kirwan, Richard. 1789. “An Essay on Phlogiston, and the Constitution of Acids: To Which Are Added, Notes, Exhibiting and Defending the Anti-Phlogistic Theory; and Annexed to the French Ed. of This Work by Messrs. de Morveau, Lavoisier, de La Place, Monge, Berthollet, and de Four.” London: J. Johnson.

Kirwan, Richard. 1782. “Continuation of the Experiments and Observations on the Specific Gravities and Attractive Powers of Various Saline Substances.” *Philosophical Transactions of the Royal Society of London* 72: 179–237. https://doi.org/10.2307/106458.

———. 1787. *An Essay on Phlogiston, and the Constitution of Acids*. London: J. Davis, for P. Elmsly. https://archive.org/details/b22386816.

———. 1796. *Elements of Mineralogy Vol. II*. Second. London: P. Elmsly. https://books.google.co.uk/books?hl=en&lr=&id=AUxLAAAAMAAJ&oi=fnd&pg=PA1&dq=elements+of+minerology+vol.+2,+kirwan&ots=kAcT5K7gEC&sig=K_c6ba6Cu2cFThtCNJ7fxI4b69w#v=onepage&q=elements of minerology vol. 2%2C kirwan&f=false.

Klein, Ursula. 1994. “Origin of the Concept of Chemical Compound.” *Science in Context* 7 (2): 163–204.

———. 2015. “A Revolution That Never Happened.” *Studies in History and Philosophy of Science Part A* 49: 80–90. https://doi.org/10.1016/j.shpsa.2014.11.003.

Kuhn, Thomas S. 1976. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Kusch, Martin. 2015. “Scientific Pluralism and the Chemical Revolution.” *Studies in History and Philosophy of Science Part A* 49: 69–79. https://doi.org/10.1016/j.shpsa.2014.10.001.

Lavoisier, Antoine Laurent. 1776. *Essays Physical and Chemical*. Translated by Thomas Henry. London: Printed for Joseph Johnson.

———. 1777. “"Memoir on Combustion in General in A Source Book in Chemistry 1400-1900 (New York: McGraw Hill, 1952).” *Memoires de l’Academie Royale Des Science*, 592–600. https://doi.org/10.1001/jama.241.25.2730.

Massimi, Michela. 2012. “Scientific Perspectivism and Its Foes.” *Philosophica* 84: 25–52.

———. 2017. “Laws of Nature, Natural Properties, and the Robustly Best System.” *The Monist* 100 (3): 406–21.

———. 2018. “Four Kinds of Perspectival Truth.” *Philosophy and Phenomenological Research* 96 (2): 342–59.

Mauskop, Seymour. 2002. “Richard Kirwan’s Phlogiston Theory : Its Success and Fate” 49 (3): 185–205. https://doi.org/10.1179/amb.2002.49.3.185.

Mccormmach, Russell, and Christa Jungnickel. 2016. *Cavendish: The Experimental Life*. Edition Open Access.

Multhauf, Robert P. 1962. “On the Use of the Balance in Chemistry.” *Source: Proceedings of the American Philosophical Society* 106 (3): 210–18. http://www.jstor.org/stable/985176.

Ott, Walter, and Lydia Patton. 2018. *Laws of Nature*. Oxford University Press.

Priestley, Joseph. 1775. “An Account of Further Discoveries in Air.” *Philosophical Transactions* 65: 384–94. http://www.jstor.org/stable/106209.

Psillos, Stathis. 1999. *Scientific Realism: How Science Tracks Truth*. London and New York: Routledge.

Siegfried, Robert. 2002. “From Elements to Atoms: A History of Chemical Composition .” *Transactions of the American Philosophical Society* 92 (4): i–278. http://www.jstor.org/stable/4144909.

Siegfried, Robert, and Betty Jo Dobbs. 1968. “Composition, a Neglected Aspect of the Chemical Revolution.” *Annals of Science* 24 (4): 275–93.