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The Chemical Revolution revisited

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ABSTRACT

I respond to the critical comments by Martin Kusch and Ursula Klein on my account of the Chemical Revolution. I comment along three different lines: descriptive, explanatory, and normative. (1) I agree with Klein that Lavoisier did not introduce drastic changes in chemical ontology, but maintain that there was methodological incommensurability in the Chemical Revolution; in response to Kusch's view, I maintain that Lavoisier's victory was slow and incomplete. (2) Admitting that there were many causes shaping the outcome of the Chemical Revolution, including the convenience of Lavoisier's theoretical scheme and various complicated social factors, I still think that the general rise of compositionism was an important factor. (3) I defend my normative pluralist view on the Chemical Revolution, denying Kusch's argument that chemists had overwhelmingly good reasons to trust Lavoisier and his allies over the phlogistonists. Overall, I agree with Kusch that it would be desirable to have a good descriptive–normative sociological account of the Chemical Revolution, but I also think that it should be an account that allows for divergence in individuals' and sub-communities' self-determination.

1. Introduction

It is a great honour to have my work critically examined in such depth and detail by two of the scholars that I respect most, and to have an opportunity to re-examine my own views. I would like to begin by summing up the spirit of my response. With Ursula Klein's description of the event usually known as the Chemical Revolution, I actually have no significant disagreements; however, convincing her (and others) of that will be an interesting challenge. Concerning Martin Kusch's points I do have some disagreement, and my challenge there is to render the disagreement productive, as he also wishes to do.

I have organized my comments around three different types of critique that Kusch and Klein have raised: the descriptive, the explanatory, and the normative. What exactly did happen in the "Chemical Revolution"? What is the best explanation of why it happened? And was it a good thing that it happened? I will try to argue that the answers I have given to these questions in *Is Water H_2O?* and various other publications still stand, but I will also try to indicate where the critique by Kusch and Klein points to lines of research and thinking that I should have pursued further.

2. What happened?

Klein and Kusch both find my description of the Chemical Revolution lacking, but in different ways. I will take their critique in turn, and then attempt a summing-up.

2.1. Klein: was there a revolution?

Ursula Klein's main message is that there was no real revolutionary change in the event that historians, philosophers and chemists have often called the "Chemical Revolution": "If we define scientific revolutions as changes of scientists' ontologies, types of causal explanation, and paradigmatic types of methods and instruments, Antoine-Laurent Lavoisier's contribution to chemistry did not amount to a scientific revolution." (Klein, 2015, in this issue, p. 80, abstract). This contention is based on a very detailed and careful study that she and Wolfgang Lefèvre have made in their

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book *Materials in Eighteenth-Century Science* (2007). I endorse what they say about Lavoisier in that notable book, and have said so in print, though in a review published in a journal that has little currency in history and philosophy of science (Chang, 2010a).

But Klein's sense of what a revolution should mean is not the only possible one, and the substance of her discussion belies even her own definition of revolution, as her focus is very strongly on the point about ontology. This ontological emphasis does sit well with Thomas Kuhn's extensional notion of incommensurability from the later years of his life, which focused on cross-cutting boundaries of classificatory categories. However, it is too narrow to fit with Kuhn's earlier ideas, and it is the early Kuhn of *The Structure of Scientific Revolutions* that used the Chemical Revolution as one of the prime examples of scientific revolutions.[1] I would go along with Klein's conclusion entirely, if it would be just slightly rephrased as "in such and such respects, Lavoisier's contribution was not revolutionary."

Kuhn's original notion of scientific revolutions was broad and imprecise, and his original notion of incommensurability multi-dimensional. While I agree with Klein that Lavoisier did not introduce such a drastic change in the ruling chemical ontology of the day, I think he did introduce other major changes. I have argued elsewhere (Chang, 2012a) that there was clear methodological incommensurability between the phlogistonist and oxygenist paradigms, though only mild semantic incommensurability. Lavoisier certainly introduced sufficient changes in the problem-field and the judgment-criteria in chemistry, so as to make it difficult to reach paradigm-independent judgments of merit. That difficulty of impartial judgment is exactly what constitutes incommensurability, according to the Kuhn of *Structure*. And chemical ontology did change a bit, too. First of all, competing sides in the Chemical Revolution disagreed sharply about what counted as elements and what were compounds (metal or calx, sulphur or sulphuric acid, etc.), and this was a difference that was considered very important by many chemists of the time, though it is seen as minor by Klein. In addition, as I will explain further below, phlogistonists did tend to retain the idea of phlogiston as a "principle", an ontological category that would disappear as Lavoisierian chemistry developed.

By the early Kuhn's lights, there was certainly a revolution here. Besides, the notion of a scientific revolution is not entirely owned by Kuhn, either, and I think it is reasonable to maintain without much harm the "revolution" label in a rather innocent or naïve way, simply meaning "rapid and fundamental change." Still, we can debate how important ontology at the level discussed by Klein is at the foundation of a science, and think about how ontology affects the rest of scientific practice and knowledge. I think that would be a more productive discussion than trying to decide whether there was a "revolution" or not. There are all kinds and degrees of continuity and discontinuity in scientific change, and it seems to me an important and interesting task for historians of science to discern them carefully.

2.2. Kusch: in defence of Lavoisier's triumph?

Martin Kusch also finds inadequacies in my description of the Chemical Revolution (if we may still use that designation). His critique pulls in a rather opposite direction from Klein's: while Klein thinks that I (and most others) exaggerate the discontinuity in the history, Kusch seems to chide me for failing to acknowledge the clear-cut triumph of Lavoisier for what it was. (It is not entirely clear to me why this matters so much to Kusch, though I will try to spell out my thoughts on that matter in the last section of this paper.)

One might say that I myself complicated matters unnecessarily by taking an exception to the prevalent descriptions of the Chemical Revolution. It would have been simpler to go along with the usual story that Lavoisier won a quick and nearly unanimous victory, and then put forward my normative pluralist thesis that he should not have. Why did I have to spoil that simple line of argument, by saying that Lavoisier's victory was actually not as decisive as often believed? Well, I am enough of a historian not to be able accept a description of the past that I see as simply inadequate. It is futile to try to offer an explanation of why something happened, if it didn't happen. Before I could get seriously down to my business of stating why the event happened and how it should have gone instead, I had to satisfy myself with a description that I could believe.

Kusch objects to my description of those who didn't entirely agree with Lavoisier, whom I classified into three classes ("die-hards", "fence-sitters" and "new anti-Lavoisierians").[2] The point of that rather elaborate story of mine was the following: "there are indeed many senses in which Lavoisier and his colleagues brought about a 'revolution' in chemistry, but it was not a sudden and clear-cut affair. It was a many-sided struggle that neither ended in unanimous agreement nor established any immutable orthodoxy." (Chang, 2012b, p. 34). I cannot see why this conclusion is so objectionable to Kusch, and the way he argues against it is not convincing to me.

He says I should consider overall numbers, rather than listing some names; doing the overall numbers would be an interesting exercise, but what we are looking at is not a majority vote. He says my account is based on the view that the situation was something of a zero-sum game (or, more like, "you're either with Lavoisier or with the phlogistonists") (Kusch, 2015, in this issue, p. 74), but that is precisely the opposite of what I am indicating with the category of "new anti-Lavoisierians" (or, "anti-anti-phlogistonists"), who were neither for phlogiston nor for Lavoisier. And the following is a loose and a rather reductionist statement (Kusch, 2015, in this issue, p. 75): "we should not forget that when the war was over Davy quickly dropped his phlogistonist project." Davy stopped talking much about phlogiston around 1810, and that does not line up with the end of the war. There is no evidence that he "quickly dropped" the "project". Most importantly, Davy's anti-anti-phlogistonist opposition to Lavoisierian chemistry did not end with the war, either; on the contrary, his identification of chlorine as an element was an achievement of which he remained proud, and that was what put an effective end to Lavoisier's theory of acidity. One thing I will admit is that it would have been better not to include Bergman, Macquer and Scheele in the list of "die-hards", as my main point was about die-hards that lasted beyond 1790.

2.3. Was there such a thing as the "phlogiston system"?

There is one point of descriptive criticism that Kusch and Klein would probably agree on, though only Kusch is explicit about it. This is the complaint that I over-simplified the field of contention in the Chemical Revolution. This is a criticism that I can readily accept, and have actually already anticipated. It was perhaps lazy of me to speak of the "phlogistonist system". As Kusch notes (2015, in this issue, p. 74), I did emphasize that there were various versions of the phlogistonist system (Chang, 2012b, p. 28). I hope that the recognition of the existence of different versions is shown clearly in various places in my discussion (esp. in Chang, 2010b; Chang, 2012b, Section 1.3.1). It is a very interesting and important task to distinguish the different versions carefully, as some of the leading

[1] See Hoyningen-Huene (2008) for the exposition of this point.

[2] Further details can be found in Chang (2010b).