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Scientific pluralism and the Chemical Revolution

Martin Kusch

Department of Philosophy, University of Vienna, Universitätsstraße 7, 1010 Wien, Austria

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ABSTRACT

In a number of papers and in his recent book, Is Water H_2O? Evidence, Realism, Pluralism (2012), Hasok Chang has argued that the correct interpretation of the Chemical Revolution provides a strong case for the view that progress in science is served by maintaining several incommensurable "systems of practice" in the same discipline, and concerning the same region of nature. This paper is a critical discussion of Chang's reading of the Chemical Revolution. It seeks to establish, first, that Chang's assessment of Lavoisier's and Priestley's work and character follows the phlogistonists' "actors' sociology"; second, that Chang simplifies late-eighteenth-century chemical debates by reducing them to an alleged conflict between two systems of practice; third, that Chang's evidence for a slow transition from phlogistonist theory to oxygen theory is not strong; and fourth, that he is wrong to assume that chemists at the time did not have overwhelming good reasons to favour Lavoisier's over the phlogistonists' views.

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1. Introduction

This paper is a critical discussion of the recent work of Hasok Chang, especially his important 2012 book *Is Water H_2O? Evidence, Realism, Pluralism* (cf. Chang, 2009, 2010, 2011). Chang's monograph is a fascinating, bold and thought-provoking plea for a *prescriptive* version of scientific pluralism. That is to say, Chang seeks to make a case for a science policy that supports a plurality of incommensurable scientific practices. *Is Water H_2O?* is also meant to exemplify "integrated history and philosophy of science". Accordingly, Chang analyses key historical stages on the long road towards the view that water is H_2O. The most important of these case studies concerns the Chemical Revolution. Chang writes that "I became a pluralist … because I could not honestly convince myself that the phlogiston theory was simply wrong …" (2012, p. 253).

I shall focus my critical examination of Chang's pluralism on his reading of the Chemical Revolution. My argument will unfold in four steps. The first step concerns Chang's efforts to correct the allegedly highly uncharitable treatment of phlogistonists at the hand of previous generations of historians of chemistry. I argue that Chang's attempt to rehabilitate the phlogistonists ends up adopting the latters' propaganda against Lavoisier. Chang insinuates a moral and intellectual superiority of phlogistonists over Lavoisians that is questionable. Second, Chang misconstrues late-eighteenth-century chemical debates by reducing them to an alleged conflict between two "systems": "the phlogistonist system" and "the oxygenist system". Phlogistonist ideas, theories and practices never formed a "system" by Chang's own definition of a "system of practice".

This fact undermines both Chang's comparisons between "Lavoisier's system and the best versions of the phlogistonist system" (2012, p. 28), and his claims regarding what might have resulted if only the phlogistonist system had been kept alive longer. Third, Chang's argument for a slow transition from the phlogistonist system to the oxygen system does not support his pluralism. Most importantly, while Chang pays a lot of attention to late criticisms of aspects of Lavoisier's programme, he ignores the reasons "converts" gave for shifting their allegiance to the oxygen theory. Fourth, I shall challenge Chang's contention that chemists in late eighteenth century Europe did not have conclusive reasons to favour Lavoisier's over the phlogistonists' views. I shall argue that this claim has some plausibility only on too narrow an understanding of reasons. If we construe the relevant reasons more broadly—such that they include reasons to trust—then the case for adopting central pillars of Lavoisier's program was strong. The fourth criticism is the most important: it is here that work on testimony and

E-mail address: martin.kusch@univie.ac.at.

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trust in science, as well as the "Experimenters' Regress", will prove crucial.

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

2. Chang on scientific pluralism

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

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

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

3. The Chemical Revolution—a primer

In this section I shall give a brief overview of the central technical content of the Chemical Revolution.[1] The study of gases—or "airs"—was a neglected topic in seventeenth-century chemistry. The situation began to improve in the eighteenth century. For instance, Stephen Hales (1677–1761) studied the volumes of gases released when different materials were heated, and Joseph Black (1728–1799) in 1756 described the chemical effects of so-called "fixed air" (i.e. CO_2). Even more important was Henry Cavendish's (1731–1810) work. In 1766 he isolated what we call "hydrogen", or—as he dubbed it—"inflammable air", and equated it with so-called "phlogiston," (Siegfried, 2002, pp. 153–157).

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

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

Cavendish's and Priestley's work on airs soon met with a lot of interest in France. And the young Antoine-Laurent Lavoisier (1743–1794) entered the race to become *the* French expert in pneumatic chemistry. Early in 1772 Lavoisier learnt from Guyton de Morveau (1734–1804) that metals gain weight when calcined—that is, that *metals gain weight while releasing phlogiston*. Lavoisier quickly set to work on an experimental program to capitalize on this observation. With the help of a burning glass he heated phosphorus or sulphur in an inverted glass over water. The result was that the phosphorus or sulphur increased in weight, and that the volume of air inside the inverted glass was reduced. In three famous sealed notes deposited with the French Academy, Lavoisier put forward the following hypotheses. The first note claimed that the phosphorus absorbs air. The second note added that it does so as it releases phlogiston. And the third note suggested that the whole process can be framed solely in terms of air, and that no reference to phlogiston is needed to explain the experimental outcome, (Musgrave, 1976, p. 191, Siegfried, 2002, pp. 161–167).

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

Priestley took the next important step in August 1774. He heated the red calx of mercury in an inverted glass over water. This resulted in the production of a gas that was easy to breath, and—as the nitrous air test showed—was four to five times better than normal air. Priestley reasoned that the calx had absorbed phlogiston from the air inside the glass, leaving behind this new gas. He called it "dephlogisticated air". The Swedish pharmacist Carl

[1] The papers and book upon which my narrative is based are: Beretta, 1993, Boantza, 2013, Boantza & Gal, 2011, Conant, 1950, Crosland, 1995, Donovan, 1993, Eshet, 2001, Frercks, 2008, Golinski, 1992, 1995, Guerlac, 1976, Holmes, 2000a, b, Hufbauer, 1982, Kim, 2003, 2008, 2011, Kuhn, 1962, McCann, 1978, McEvoy, 1978, 2010, Mauskopf, 2002, Melhado, 1985, Miller, 2004, Musgrave, 1976, Wise, 1993, Perrin, 1988a, b, Roberts, 1995, Schaffer, 1986, Schofield, 2004, Siegfried, 1964, 2002, Simon, 2005, Ströker, 1982, Toulmin, 1957.