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The epistemic value of Dalton's theory of atoms

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John Dalton's achievement for chemistry could not be perceived more differently by scientists and philosophers of science. Whereas Dalton's atomism (1803) figures as an indispensable part of what chemists teach their students as their discipline's history, Dalton's work has recently been marginalised by philosophers of science. Needham (2004a, 2004b), for instance, claims that Dalton's theory was non-explanatory, that it 'merely repeats what has to be explained on a smaller scale' (2004a, p. 1041) and that it was thus tautological. Even though Chalmers (2005a, 2004b, 2008) doesn't go as far as Needham, he concurs that Daltonian atomism 'yields little else that is experimentally testable beyond what is a consequence of the laws of proportion themselves' (2005a). Moreover Chalmers claims that the formulas used in organic chemistry that became the main focus of chemical research from the 1820s onwards and which did not yield unequivocal formulae assignments to compounds until the 1860s was 'a precondition for rather than a result of atomism in chemistry' (ibid.). In this paper I shall seek to demonstrate that both Needham and Chalmers are mistaken in their claims. Dalton's theory was indeed explanatory and pivotal for the subsequent development of chemistry.

The historian Rocke (1978, 1984) has argued for a distinction between a chemical and a physical component of Daltonian atomism. Whereas the former (very roughly) refers to the simplest part of chemical substances that enters chemical reactions, the latter includes interatomic forces and caloric spheres, which Dalton used in his meteorological investigations and his derivation of the law of partial pressures, explaining air as a *mixture* (rather than a combination) of gases with various weights. Whereas Chalmers has accepted this distinction, Needham (2004a)—in supposed charity to Dalton—has tried to apply Dalton's physical reasoning to the laws of constant proportions, understandably without much success. Needham thus concludes that Dalton's theory didn't really provide any explanation of the laws of constant proportions, since, saying that certain elements combine in constant weight proportions (say, hydrogen with oxygen in one to eight parts) is so because their atoms combine in constant weight proportions does not add anything that was not known before, apart the postulation of entities whose existence couldn't be directly be verified anyway. Chalmers, in contrast, thinks that even though "Dalton's theory does not explain why substances *combine* in constant proportions ... [it explains] why they do so in *constant proportions*" (Chalmers 2005). That is, whereas explaining the combination of atoms would require a theory to say something about "attractive forces", "affinities" or the like, an explanation of the combination of substances in constant proportions is free from this requirement. Even though I agree with Chalmers that Dalton's theory was explanatory, I don't think his argument is sufficient for making a good case for that. Rather, I want to suggest the following: Dalton's theory not only predicted the law of multiple and reciprocal proportions, but, more importantly, it brought those laws and the law of constant proportions under one umbrella. The unification of the phenomena has long been discussed by philosophers as a genuine explanatory property (see Friedman 1974, Kitcher 1989). I thus want to claim that it was precisely the unification of the three laws of constant proportions in terms of atoms, which made Dalton's theory explanatory. It is therefore no wonder that Needham's objection may be defensible in the case of a *single* law: the objection misses out on the unificatory nature of Dalton's explanation. However, even if one were to reject such a notion of explanation, the epistemic value of Dalton's theory is not exhausted by its explanatory power.

Both Needham and Chalmers in their accounts of Dalton's theory of atoms make an assumption that is historically not justified. They presuppose that the law of constant proportions was firmly established *before* Dalton's theory entered the stage.

And yet, this is not the case, as historians have argued convincingly (Meldrum 1910, Kapoor 1965, 1972, Mauskopf 1972). Contrary to popular folklore, the debate between Proust and Berthollet did not witness a clear victory on either side. Not only did Proust try to demonstrate the law of constant proportions for only a handful of cases, he also rather dogmatically defended the false view that elements combine in exactly *two* proportions. I want to suggest that it was Dalton's theory of atoms which gave chemists good reasons to believe in the laws of constant proportions, because it made the latter, in itself merely a contingent fact about the world, "plausible": Dalton's theory explained why the law *should* hold. It thus provided chemists with a research programme that encouraged them to seek to extend the applicability of law to other compounds and it gave them good reasons to hold onto the law against anomalies, very much in the fashion of a Lakatosian "hard core". Note that Dalton's theory also "corrected" Proust's law. In Dalton's theory there is nothing that prohibits two elements to enter into more than two proportions. In fact, it explicitly allows this in the form of the law of "multiple proportions", according to which the weights of elements, which combine in more than one way, form ratios of small whole numbers. Dalton's theory thus possessed epistemic virtues, which Needham and Chalmers, due to the use of idealised history, fail to realise.

Despite the above, someone being in favour of Needham's and Chalmers' view about the irrelevance of Dalton's theory of atoms for the establishment of atomism within the scientific community may want to refer to the lack of commitment to atomism amongst chemists at the time. Rather than adopting Dalton's terminology of atoms, chemists often preferred to speak of "equivalent weights", "combining volumes", "proportions", and the like (Buchwald 1959, Gardner 1976, Whitt 1990, Knight 1967). Trying to deflect such an objection, Rocke (1978, 1984) has surveyed the chemical systems of those chemists who were reluctant to commit to atoms. Finding that these systems were operationally equivalent to Dalton's, Rocke concludes that chemists, despite their lack of *explicit* commitment, implicitly adopted chemical atomism through their practices. But this is a weak argument. Rather than following Rocke's conclusion, one may as well draw the opposite conclusion from his analysis, namely, that Dalton's system, like the systems of his contemporaries who used notions of 'equivalent weights and the like', was actually merely conventional. I therefore would like to revisit some of the arguments made by the former president of the Chemical Society A. W. Williamson (1869) which seem to be much better suited for the agenda Rocke has in mind but which he fails to implement convincingly. I will furthermore reconsider an objection that Bernatowicz (1970) has raised against Dalton's theory. Bernatowicz believes that Dalton's rule of greatest simplicity is not only arbitrary (which is generally agreed) but also that is not even operational: given multiple compounds of the same elements, Dalton's rule is incapable of determining the *simplest* compound of those elements. Bernatowicz's objection has sat in the literature for almost 40 years. Nevertheless, I will demonstrate that Bernatowicz's conclusion is due to a confusion between equivalent weights and atoms, and I thus will effectively argue against all those who think that Dalton's theory is nothing more than a theory about the former rather than the latter.

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