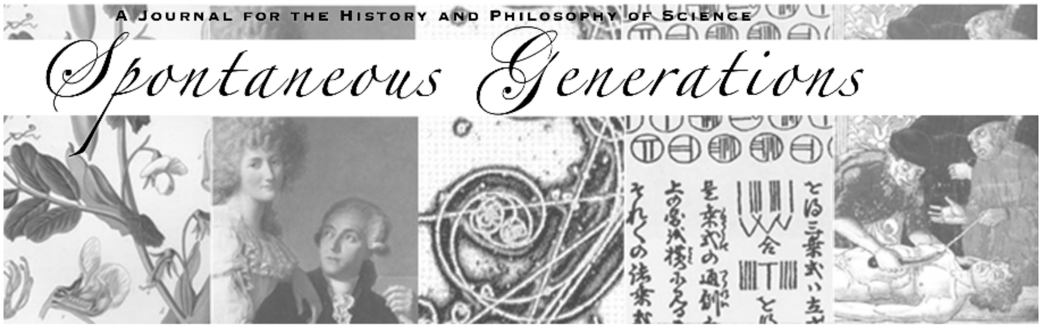

Engaging Philosophically with the History of Science: Two Challenges for Scientific Realism

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Engaging Philosophically with the History of Science: Two Challenges for Scientific Realism*

Theodore Arabatzis†

I would like to raise two challenges for scientific realists. The first is a pessimistic meta-induction (PMI), but not of the more common type, which focuses on rejected theories and abandoned entities. Rather, the PMI I have in mind departs from conceptual change, which is ubiquitous in science. Scientific concepts change over time, often to a degree that is difficult to square with the stability of their referents, a *sine qua non* for realists. The second challenge is to make sense of successful scientific practice that was centred on entities that have turned out to be fictitious.

Let me elaborate. To begin with challenge one, there are two versions of the PMI: one focusing on false but successful theories and another on evolving concepts and their shifting referents. The former was defended by Laudan (1981); the possibility of the latter was suggested by Putnam (e.g., 1982, 199). The recent debates on realism have focused on the former version of the PMI. In a recent authoritative overview of the realism debate (Chakravartty 2015), the latter version receives scant treatment. Needless to say, this remark is no criticism of Chakravartty's excellent survey article. Rather, it indicates a lacuna in the recent literature on scientific realism.

I think that Laudan's version of the PMI does not rest on a sufficiently wide evidential base. If one takes into account the stringent criteria of success imposed by realists (e.g., novel predictions), then that evidential base shrinks considerably. This is not to deny that there are significant cases where the ontology of successful theories (such as the phlogiston theory, the caloric theory, and the ether theory) turned out to be vacuous. However, I don't see this as an insuperable obstacle to scientific realism. Given that most, if not all, scientific realists are fallibilists these days, they presumably admit

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the possibility of empirically successful theories turning out to be mistaken. What more do these well-known and much discussed cases show than the actualization of this possibility?

The other version of the PMI, though, the one indicated by Putnam, may be far more threatening to the viability of a realist approach to scientific development. If scientific concepts change beyond recognition over time, as they often do, what sense can we make of their purportedly stable referents? The neglect of this problem in the recent literature is perhaps due to a feeling that it has been resolved by the causal theory of reference (CTR) and its more refined causal-descriptivist descendants (see Psillos 2012). The CTR, however, is not problem-free (see Arabatzis 2012, 154-155). Furthermore, it is not applicable to theoretical objects (e.g., black holes) with indirect causal links, or even no causal links at all, to observable phenomena or experimentally-produced effects.

The second challenge I would like to raise for scientific realism is its, *prima facie* at least, incompatibility with contemporary historiographical *bon sens*. As I have argued elsewhere (Arabatzis 2001), realists are in a historiographically awkward predicament: they are compelled to maximize the continuity between past and contemporary science. To put it another way, they are hard-pressed to portray past successful scientific theories as imperfect versions of their contemporary descendants. In other words, they are forced to embrace Whiggism, a historiographical stance rejected by the overwhelming majority of historians of science.

The Whiggish orientation of scientific realism has two untoward consequences. First, it hampers the coveted integration of history and philosophy of science. Second, it impedes the understanding of past scientific practice that involved entities, postulated by successful scientific theories, that have turned out to be non-existent.[1] Let's assume, for instance, that the ether does not exist. What then were 19th century physicists doing when they (thought they) were investigating the properties of ether and its interaction with matter? If they talked about nothing and structured their practice around a non-existent object, how was it that their investigations were so productive, leading, among other things, to the birth of microphysics (Buchwald 2000)? In those cases anti-realists seem to have an advantage over realists, because they aim at making sense of scientific practice regardless of its ability to track truth regarding the unobservable realm. And this is the terrain on which the debate on scientific realism should be played out. If we narrow our focus to issues about whether we are justified in believing

[1] This is not unanimously accepted. Some philosophers of science (e.g., Chang 2012) have advocated the resurrection of long-gone entities such as phlogiston, whereas others (e.g., Psillos 1999) have argued that obsolete entities such as the ether could be identified with their contemporary counterparts, e.g., the electromagnetic field.

in successful scientific theories (and the ontologies they sanction), then the prospects of overcoming the current standoff in the realism debate, a standoff noted by several commentators (see, e.g., Forbes 2016), are rather slim. If, on the other hand, we evaluate realism and anti-realism according to their capacity to make sense of scientific practice, both past and contemporary, hopefully we'll be able to move beyond the present stalemate.

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