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Quo Vadis Selective Scientific Realism?

Peter Vickers*

Where is selective scientific realism going? A very large number of historical challenges are out there in the literature (see e.g., the list of twenty cases in Vickers 2013) apparently showing in concrete terms how scientific success actually *can* be born of hypotheses that are not even approximately true (on any reasonable account of “approximate truth”). In the years since Laudan’s “Confutation of Convergent Realism” (1981) the scientific realist has introduced a whole host of criteria to try to dodge the “counterexamples,” or at least to reduce the number of them. But when one looks at the historical challenges, and especially the newer ones that are sensitive to contemporary selective realism, one might feel that the realist is on the back foot (to put it mildly). Indeed, many in the community tend to think that the historical challenges are overwhelming, such that the realist will only manage to dodge them if she turns realism into an empty position, or if she adds auxiliaries in an ad hoc manner to save what is in fact a degenerating research program.

By contrast, my current opinion is that the selective realist is in a strong position vis-à-vis the historical challenges. Certainly the realist needs to invoke some careful criteria for realist commitment, and various nuances concerning the nature of her epistemic commitment, and this may raise the “death by a thousand qualifications” question mark. But the concern is unfounded: the qualifications are all independently motivated, and indeed necessary given the philosophical complexity. Qualifications are to be welcomed here; often the truth is far from simple!

To illustrate, let’s start with a list of some of the most serious historical challenges that feature in the contemporary literature:

1. Successful explanations and predictions based on the caloric theory of heat.
2. Successful explanations and predictions based on the phlogiston theory of combustion.

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3. Successful explanations and predictions based on the Fresnel “luminiferous ether” theory of light.
4. Successful explanations and predictions based on Kirchhoff’s theory of the diffraction of light.
5. Successful explanations and superb predictions based on Sommerfeld’s 1916 theory of the hydrogen atom.
6. Dirac’s highly significant prediction of the positron based on the “Dirac sea” theory of negative energy electrons.

These examples are all chosen because they were extremely successful, so the realist cannot hope to answer them by raising the bar for the success necessary for a realist commitment. But what of making a distinction between the “working parts” of the theory, which are confirmed by the success because of the special role they play in bringing about the success, and the “idle wheels” of the theory, which are not so confirmed? This selective approach to confirmation is independently motivated, has wide appeal, and has been applied to at least some of the cases (1)-(6) to answer the historical threat. Just to give three relatively recent examples, Votsis and Schurz (2012) apply this approach to the caloric theory, Ladyman (2011) applies it to the phlogiston theory, and Cordero (2011) applies it to Fresnel’s theory of light. Granted, in each of these cases the application of the selective strategy varies. But this should cause no concern if we accept a “neutral” approach to the type of theoretical entity that can be confirmed (see Peters 2014): in some contexts it might be right that only abstract “structure” is confirmed, but in other contexts laws, or entities, or some mixture of things might be confirmed. What really matters is that the realist has good reasons for “selecting” the parts that are confirmed, reasons that are *completely independent* of the fact that those parts were (at least approximately) retained in the successor theory/theories. Of course, contemporary realists are sensitive to this requirement, and argue that they do indeed have independent reasons.

What of cases (4), (5), and (6)? Case (4) initially seemed to me to be a serious challenge to the realist, but after further investigation (see Vickers 2016) I have come around to the idea that the realist has a good response here (based, again, on thinking about what is really doing the work to bring about the success). Similarly case (5), on first view, seems extremely challenging to the realist (see Vickers 2012) and I have seen this case put forward at conferences and workshops as the knock-down counterexample to the realist’s success-to-truth inference. However, it turns out that physicists were themselves long perplexed by this case, and one physicist in particular worked to resolve the puzzle. His conclusion—comparing the old Sommerfeld derivation of the success to the contemporary derivation—is that “*The*

underlying symmetry of the problem intervenes in a most remarkable and essential way so as to produce the closest possible correspondence between the two (suitably formulated) calculations" (Biedenharn 1983, 14, original emphasis). There is some philosophical work to be done here, but this strongly suggests just the sort of structural correspondence between the old and the new theory that would be expected by a structural realist.

That leaves case (6), and, to my knowledge, there is currently no realist response to the challenge. However, the challenge has been put to the realist only quite recently (Pashby 2012), so it is a bit too soon to make a confident judgement. And in addition, this is only one case. Even non-realists such as Chang insist that "[o]ne case, of course, does not have much force" (2003, 910). And one might well wonder whether it would be a really serious challenge to the realist even if there are two or three "counterexamples" to the claim that the working parts of a sufficiently successful theory must be (at least approximately) true. In particular it is crucial to stress that no contemporary realist actually insists that such working parts *must* be true; instead the claim is that it is a good bet, and "counterexamples" are likely to be rare. It is acknowledged that realists have a challenge cashing out the probability judgement they are making here (e.g., Magnus and Callender 2004), but the basic intuition seems to make sense and realist responses to the "base rate" objection are emerging (e.g., Henderson 2017).

Where does this leave us? I am suggesting that cases (1)-(6) are the six most serious historical challenges to scientific realism in the literature, and that contemporary realists currently have answers to five of them that look promising, or at the very least are worth pursuing. So if the historical challenge is supposed to be a pessimistic *induction*, the realist is in a strong position since the inductive base looks rather weak. But if it is *not* an induction, then it seems to miss the mark, since the realist proposes a *defeasible* success-to-truth inference that will survive one or two historical counter-instances. For me, then, contemporary selective scientific realism is a progressive research program, and this is a progressive and fascinating debate. Wherever it is going, it is going somewhere important.

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