K. Brad Wray. *Resisting Scientific Realism*. Cambridge University Press 2018. 236 pp. $105.00 USD (Hardcover ISBN 9781108415217).

Resisting Scientific Realism is a clearly written book, tightly argued, well researched, and economical in expression. Wray asserts that his aim is modest, to show that 'anti-realism is a more viable position than commonly thought' (206), but his strategy is more of an all-out assault on (scientific) realism. Wray defines realism as the position that we have 'adequate grounds for believing that our theories are true or approximately true with respect to what they say about unobservable entities and processes' (1), and correlatively anti-realism denies that we have such adequate grounds.

The gist of Wray's overall argument in the book is as follows. In evaluating a scientific theory one inevitably compares this theory to its theoretical competitors. Let's suppose that this comparison is favorable to the theory in that it is better satisfied by the observational evidence and exhibits more theoretical virtues than its competitor theories. As the theory possesses these epistemic merits only by comparison to its competitors, we have grounds to say that this theory is more likely to be true than its competitors in light of its epistemic merits, but cannot go further and say that the theory is, moreover, likely to be true. We can't say the latter because we're only comparing the theory to a particular set of competitors, and this set may not be representative of the best theoretical competitors at hand. In other words, a theory's extant set of competitors may be a 'bad lot' (terminology coined by van Fraassen), so comparing this theory to its competitors provides no grounds for saying that the theory is truthful about unobservables, even granting its truthfulness about observables.

This argument against realism Wray calls the 'argument from underconsideration.' As he notes, some realists have replied to this form of argument by asserting that, since scientists are able to make meaningful, evaluative comparisons between theories, it follows that they must have reliable information about unobservables, information contained in their background assumptions. In other words, their ability to ordinally rank theories in an epistemically significant way indicates that scientists possess an element of 'epistemic privilege.' The core of Wray's case against realism is then to deny that scientists possess any such 'privilege,' a denial based on the pessimistic induction. Specifically, since in the past there have been numerous theories which were empirically successful at the time and which scientists regarded as epistemically meritorious, but which were later shown to be false, it is quite possibly the case that currently, empirically successful, epistemically meritorious theories are false as well. At this stage, a realist might claim that things are different now, and assert that science has recently progressed to such a degree in terms of its productivity or methodology that it is able to neutralize this pessimistic induction. This, Wray notes, is the strategy taken by the realists Ludwig Fahrbach, Michael Devitt, amongst others. But, he cautions, such hubris is risky as previous scientists have also thought that their theories were exceptional in terms their productivity or methodology, and as we now know their theories have often been mistaken.

The above argument from underconsideration forms the backbone to Wray's argument against realism in his book. To be sure, there are numerous ways realists could counter this argument. For example, structural realists claim that despite radical theory change over historical periods certain 'structural' elements of scientific theories nevertheless persist. But as Wray understands structural realism, this continuity concerns the persistence of certain mathematical equations or empirical laws of nature, and he finds with such a view no challenge to anti-realism since such equations or laws of nature are purely observational in character and so don't concern unobservables (124). Alternatively, selective realists such as Philip Kitcher and Stathis Psillos distinguish between the parts of past theories that have been preserved in present theories, the 'working posits,' and the parts that have

been deleted, the 'idle posits' (183). However, Wray is doubtful about this strategy since he believes theory change in the past has been much more radical than the selective realists are willing to admit. In justifying this view he spends a significant amount of the book reviewing two scientific revolutions that he takes to be representative of good science: the Copernican revolution, and the more recent 'revolution' in chemistry in which the chemical elements in the Periodic Table were re-categorized by means of their atomic numbers instead of their atomic weights. In a Kuhnian spirit, Wray regards these revolutions as involving semantic alterations to the scientific terms, or as Kuhn describes it later in his career, as involving lexical changes (113-114). These changes, Wray asserts, effectively challenge selective realism since it may turn out that the preservation of a working posit is unable to survive a revolutionary semantic or lexical change.

Overall, there is much more that structural realists and selective realists could say in response to Wray's argument from underconsideration. And with Wray's argument so heavily reliant on a Kuhnian theory of scientific revolutions, there is much more that could be said about the tenability of theoretical incommensurability as well. However, instead of following up these issues, Wray turns instead to defending anti-realism from what is perhaps its main objection—how can anti-realism explain the success of science? Here he effectively counters the realist strategy of explaining the success of a scientific theory in terms of the truth of this theory. And his argument is quite strong: if one explains the success of a scientific theory in terms of the truth of this theory, then how is it possible for two theories that contradict one another to both be successful, either at the same time or at successive times? By comparison, an anti-realist 'selectionist' approach, such as the one advocated by van Fraassen, explains the success of a scientific theory in terms of the demise of unsuccessful scientific theories. Accordingly, it is easy to explain how two contradictory theories can each be successful in so far as each theory captures the relevant empirical data, in contrast to other theories that fail in this regard. Ultimately, I am inclined to endorse Wray's contention that anti-realism possesses the resources to explain the success of science: the connection between the success of a theory and its truth is much overdone by some realists.

Still, Wray's book is absent a substantive discussion of a topic that is critical to any formulation of a van Fraassen-style anti-realism: what is the nature of an observable? Without some accounting of what it means to be an 'observable,' anti-realism is an underdeveloped doctrine. What we have from Wray is the following. He adopts Kyle Stanford's depiction of what constitutes the range of unobservables: entities that are 'too fast or too slow or too rare or take place on too grand a scale for us to engage with in ordinary ways' (100, quoting Stanford). Additionally, following Kuhn, Wray views what counts as empirical success, and correlatively what counts as an observable, as socially prescribed: he comments, 'the criterion of [empirical] success is the result of social consensus in the research community' (165). However, contrary to Wray, why should we be disbarred from investigating things we don't engage with in ordinary ways or that go beyond social prescriptions? Occasionally scientists, through non-ordinary and socially novel, experimental innovation, can transform what was previously thought to be an unobservable into something that is observable. Why does Wray proscribe the examination of unobservables if this sort of progress can occur? Wray addresses this point directly, claiming that these sorts of events do not involve the revealing of unobservables, but instead the extension of 'the range of observables to hitherto completely unknown phenomena' (101).

But consider how science has developed on the issue of what we know about observables. It often takes the tack of denying that what we observe in an 'ordinary' or 'socially sanctioned' way is indicative of what is real. For example, we observe colours, but the colours we observe are often not thought by scientists to be real properties of objects—these objects have either different colours, or

no colours at all. The same with odours and tastes and texture—the real objects, scientists often say, either have different such properties, or no such properties whatsoever. Thus, for scientists, what constitutes an observable is often highly misleading and there is a drive to discern the 'reality' behind what is observed. Of course, all this is anathema for Wray, and one must wonder why Wray and his kindred anti-realists are so stuck on the epistemic priority of observables when their ontological priority is routinely questioned. My guess is that their skepticism about unobservables is based on the fact that we can never see (smell, taste, feel) unobservables directly, so we lack 'adequate grounds for believing that our theories are true or approximately true with respect to what they say about unobservable entities and processes.' But now a realist's rejection of that skepticism begins to look insightful if what we see (smell, taste, feel) 'ordinarily' or in a 'socially approved way' is itself subject to doubt – for then our theories about such things seem to lack adequate grounds for belief as well.

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