Realism without the Real

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DISCUSSION:

REALISM WITHOUT THE REAL*

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1. Background. Clyde Hardin and Alexander Rosenberg recently published a critique (1982) of an essay of mine (1981) in this journal. Where I had sought to raise some doubts about the ability of scientific realism to explain some key features of science which had been said (by Putnam, Newton-Smith and Boyd) to be uniquely explicable by realist epistemology, Hardin and Rosenberg maintain that realism is amply provided with explanatory resources for handling the sorts of cases I discussed. It appears to me, however, that the Hardin-Rosenberg approach buys what explanatory power it can claim at a high cost; namely, by a more or less wholesale repudiation of much that the scientific realist holds dear. Indeed, it seems to me that the position they actually defend is so attenuated a form of 'realism'—if it be realism at all—that it is scarcely distinguishable from that of an instrumentalist. In this brief reply, I want to show why Hardin and Rosenberg, despite their intentions to the contrary, seem to be cutting the ground out from under classical scientific realism.

Before I can deal with their arguments in detail, however, some background needs to be sketched in briefly. Scientific realism, at least as developed by Sellars, Putnam, Boyd, and Newton-Smith, seems to rest on the following epistemic intuition: the theories of the "mature sciences" (e.g., contemporary physics) are sufficiently well-tested that it is reasonable to assume that the world *is* substantially the way those theories say it is. This realist construal extends not only to the 'directly observable' or 'directly testable' parts of our theories, but equally (one might almost say especially) to the explanatory and often 'unobservable' portions of our well-tested theories. I shall call this view *the thesis of hard-core scientific realism*. That thesis rests, in turn, on the legitimacy of a particular form of inference, often called 'inference to the best explanation'. Vir-

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tually all the arguments for the thesis of hard-core realism involve varieties of inference to the best explanation. Indeed, it is only because the realist sets such store by inference to the best explanation that he is able to motivate the characteristic realist inference from the well-testedness of a theory to the claim that there is a warrant for accepting it as true.

In the essay of mine to which Hardin and Rosenberg take exception, I challenged the intuitions which motivate the realist enterprise by arguing (among other things) that many (now discredited) scientific theories of earlier eras exhibited an impressive sort of empirical support, arguably no different in kind from that enjoyed by many contemporary physical theories. Yet we now believe that many of those earlier theories profoundly mischaracterized the way the world really is. More specifically, we now believe that there is nothing in the world which even approximately answers to the central explanatory entities postulated by a great many successful theories of the past. My approach in that essay, which we might call *the historical gambit,* was to show that these historical cases call into question the realist's warrant for assuming that *today's* theories, including even those which have passed an impressive array of tests, can thereby warrantedly be taken to be (in Sellars' apt image), "cutting the world at its joints". In the process of elaborating this historical gambit, I spent some time examining certain apparent flaws both in the realist theory of reference and in the realist semantics of approximate truth, flaws which it is not necessary to summarize here. Finally, I tried to show in a general way that the historical gambit exhibits the unreliability of inference to the best explanation as a strategy for warranting truth claims on behalf of scientific theories.

Hardin and Rosenberg reply by asserting that the realist has two plausible responses to the historical gambit: (a) the realist can readily concede, if he likes, that the central explanatory concepts of many superceded theories have failed to refer (in the denotative sense) and yet still maintain his belief that those theories were 'approximately true' and empirically successful; and (b) that the realist, by adopting a functional, *non-denotative* theory of reference, can even hold that the central explanatory concepts of past theories genuinely "referred", despite the fact that there is apparently nothing in the world which even approximately answers to the descriptions which those theories gave of their central concepts.

I think that both of those suggestions are ill-advised, precisely because they undermine the very thesis of hard-core scientific realism which they were apparently intended to advance. The object of this reply is to show how (a) and (b) fail to enhance the case for scientific realism.

2. The Divorce of Reference and Success. Scientific realists have long maintained that, if a theory is a highly successful explainer and predictor of phenomena, that is, if it has stood up to a demanding battery of empirical tests, then it is reasonable to assume that the world's structure is substantially the way that theory claims it to be. As Sellars once observed, the realist is one who says, "if the kinetic molecular theory is highly confirmed, then there really are molecules". Hardin and Rosenberg are evidently prepared to jettison this key tenet of scientific realism, at least with respect to theories of the past. Specifically, they are ready to say that there can be theories which have been highly successful, even "approximately true", even though there is no reason to think that there is anything like the basic entities which those theories postulate.

Their example is Mendel's theory of the gene and the phenotype. As they point out, "there is nothing like genes, or phenotypes, as Mendel or his immediate successors construed them" (1982, p. 606); ". . . genes can reasonably be asserted not to have existed" (1982, p. 606); "there is nothing, no one thing, like the gene" (1982, p. 606). Hardin and Rosenberg say that Mendel's theory can be admitted by a realist to be non-referring and yet still be held to be "approximately true", on the grounds that Mendel's theory (including the laws of segregation and independent assortment) is "close to the truth". They thus see in Mendel's theory a counter-example to my claim that the realist must construe any successful and approximately true theory as a genuinely referring one.

Quite what they mean by saying, let alone what grounds they have for holding, that Mendel's *theory* (as opposed to his laws) is "approximately true" is left opaque. But I do not want to repeat earlier arguments of mine about the technical difficulties confronting the semantics and criteriology of approximate truth. I think it would be more instructive to look at the far-reaching implications of the Hardin-Rosenberg claim that a realist can readily grant that many highly successful theories (such as Mendel's) failed to get things right at the level of basic structures. For my part, I would be the last to quibble with the claim that successful theories need not be genuinely referring; indeed much of my paper was devoted to documenting that claim. What I find curious is the belief of Hardin and Rosenberg that a realist can, consistently with his general epistemic outlook, happily acquiesce in the divorce of empirical success and referential presumption.

It seems clear that if we once accept that theories can be, indeed that they have been, highly successful even when their ontological underpinnings are very wide of the mark, then we have completely undercut the (Putnam-Boyd-Newton-Smith) claim against which my paper was directed that *only* by accepting a realist construal of theories, can we explain why those theories (in particular, and science in general) are successful. If theories can be successful even when their stories about fine structure are strikingly at odds with what there is, then how, if at all, can realism "de-mystify" the "miraculously" successful character of science? The Hardin-Rosenberg answer is apparently this: by accepting contemporary biology as true, and by showing that many of the consequences of contemporary biology are congruent with many of the consequences of Mendel's theories, we can thus 'explain' why Mendel's theories worked, even when they did not secure reference for their basic explanatory concepts. But that is precisely to beg the question. For where is the warrant for presuming that contemporary biological theory has got things right at the level of deep structure? Indeed, so soon as Hardin and Rosenberg acknowledge, as they are at some pains to do, that empirical success does not betoken genuineness of reference or truthlikeness, then it becomes unreasonable for them to take contemporary theories as benchmarks of what there is and how it behaves. Having once granted that (as they do vis à vis Mendel's theories) that inference to the best explanation is suspect, it appears unseemly to throw those suspicions to the wind when we apply that form of inference to the science of our own time. If empirical success (or its presumed epistemic counterpart, approximate truthlikeness) is *not* co-variant with genuineness of reference, then there is no reason whatever to believe that the world is the way our most successful theories say it is. And that, in turn, makes the thesis of hard-core scientific realism a non-sequitur, for that thesis asserts the legitimacy of assuming that the world is pretty much as our best-tested theories say it is.

Realists like Putnam and Boyd have resisted the Hardin-Rosenberg manoeuvre (viz., allowing that empirical success may not betoken genuineness of reference), and well they should; for if the realist once concedes that empirical success is not a reliable litmus for authenticity of reference, then he is no longer in a position to argue—as is the realist's wont—that the empirically successful theories of our time can warrantedly be held to be genuinely referring. And without that baseline as a given, he is in no position to use contemporary theories as the touchstone for deciding on the referential status of the explanatory concepts of earlier theories. Curiously, Hardin and Rosenberg are quite happy to resort to the latter strategy (witness their use of contemporary genetics to adjudicate whether Mendel's concepts refer), without realizing that their divorce of reference and empirical success undermines the rationale for that strategy.

For such reasons as these, it seems reasonable to conclude that, whatever merits may attach to the divorce thesis (and *non-realists* are likely to find it attractive indeed), it does little to strengthen the case for scientific realism.

3. The Functional Theory of Reference. The second approach to reference which Hardin and Rosenberg explore is rather different. Here, one does not deny that a concept in a theory refers 'merely' because nothing satisfies the theory's description of the concept. Rather, one stipulates that the concepts in an outmoded theory can legitimately be taken to refer just in case we now accept the existence of entities which play many of the same causal and explanatory roles as the concepts of the outmoded theory. On this analysis, one says (for instance) that the Daltonian atom "referred" because, even though "nothing answers to the description of . . . the Daltonian atom" (1982, p. 613), it was invoked to explain many of the same things which *our* conception of the atom is used to explain. Similarly, Hardin and Rosenberg argue that we can legitimately say (counter to my claim) that the ether concept 'referred', since it was invoked to explain many of the same phenomena which we now explain by the electromagnetic field. Indeed, as they would have it, "'ether' referred to the electromagnetic field all along" (1982, p. 614), because of the similarity of causal roles played by the ether in classical physics and the electromagnetic field in contemporary physics.

The point of elaborating this account of reference, which radically separates reference from denotation, is that it allows Hardin and Rosenberg to refute my charge that many successful theories have had central explanatory concepts which were evidently non-referring. With this functional theory of reference, they are now in a position to say that, since many of my examples were drawn from the history of ether theories, one need not accept my characterization of ether theories as non-referring. (Incidentally, I took my characterization of ether as non-referring from the then-realist, Hilary Putnam (1978, pp. 20–22), who offered it as a prime example of a *non-referential* concept.)

Two things are wrong with this functional theory of reference. In the first place, it proves to be far too *tolerant* for the realist's purposes, since it countenances as genuinely referring all manner of ill-developed theories. It can be argued, for instance, that Aristotle's conception of natural place plays many of the same causal and explanatory roles as Newtonian gravitational forces. Cartesian vortices perform a similar function. Yet is anyone seriously prepared to maintain that Aristotle and Descartes, whose conceptual frameworks were so antithetical to action-at-a-distance, were really 'referring', had they but known, to gravitational attraction? What Newton, Descartes and Aristotle do have in common is a conviction that there is something which causes heavy bodies to fall. And in a trivial sense, they are thus all referring to "the cause of fall". But that establishes no interesting commonality of reference at the level of explanatory structure. Secondly, and more crucially, the Hardin-Rosenberg account of reference confuses a shared explanatory agenda (i.e., common problems to be solved) with a shared explanatory ontology (i.e., the characteristics of the postulated explanatory entities).

The fact that (a) two theories address (many of) the same problems and (b) that those theories group the problems to be solved into similar 'bundles', is surely *not* sufficient to establish (c) that the explanatory ontologies of the two theories 'refer' to the same entities. Especially for a realist, what a theory is thought to 'refer to' must have some connection with the existential claims the theory makes about deep or fine structure. To make reference parasitic on what is *being explained* rather than on what is *doing the explaining* entails that we can establish what a theory refers to independently of any detailed analysis of what the theory asserts. If, to save his theory of reference, the scientific realist is forced into such voodoo semantics, then realism scarcely seems worth the candle.

But that is only the beginning of the problems confronting the functional theory. The major objection to this approach is that, like the divorce thesis, it repudiates what I earlier called the hard-core realist intuition. By insisting that a theory may be highly successful and that its central concepts may successfully 'refer', even when there is nothing in the physical world relevantly like the entities postulated by the theory in question, Hardin and Rosenberg have conceded that the deep-structural claims of a theory can be systematically wide of the mark, although the theory itself is highly successful at those levels where it can be tested. Which is to say that there is no co-variance between the empirical success of theories and the correctness of their deep-structural claims. But to grant that is just to concede that "inference to the best explanation", which is the realist's core inferential strategy, is badly flawed.

It seems to me that Hardin and Rosenberg have not yet realized just how much of realism they are asking us to give up. They treat it as completely unproblematic, for instance, that we can 'read off' a correct account of the structure of the cell from contemporary biological theory. Indeed, it is precisely because they take modern genetic theories as correct that they can argue that Mendel's account of the gene is so badly mistaken. Yet where is their warrant, as realists, for treating our understanding of the gene as referentially kosher? So long as the realist could utilize 'inference to the best explanation', he could at least consistently argue that modern theories are more nearly right than their predecessors. But, having conceded that genuineness of reference is not a *sine qua non* for empirical success, Hardin and Rosenberg no longer have a license for treating *our* theories as any more likely to be referentially sound than those of Mendel.

In sum, if the realist once gives up the claim that impressive empirical success is a warrant for presuming the existence of the explanatory entities postulated by successful theories, then scientific realism becomes a semantics in search of an epistemology. And the realist's claim to possess an epistemology with some novel explanatory content becomes, in the Hardin-Rosenberg analysis, an ever more utopian pipe-dream.

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