**To Save the Noumena**

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Van Fraassen's criticisms of realism are valuable, if for no other reason, because they indicate how much work we realists still have to do.

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## TO SAVE THE NOUMENA *

IF realism is the thesis that to have good reason to believe that a theory is empirically adequate is to have good reason to believe that the entities it postulates are real and, furthermore, that we can and do have such good reasons for some of our theories, then realism seems hamstrung. Many argue that reports of what is observed are theory-dependent and, hence, that the evidence for a theory is biased by that theory itself, so that the choice of theory is arbitrary. But, even if there were a theory-independent body of phenomena, many incompatible theories could account for it, and, hence, again the choice of theory is arbitrary. My belief is that both parts of the anti-realist argument fail, but the central contention of the argument from underdetermination remains problematic.

Although arguments against the cogency of the distinction between what is observable and what is not are unconvincing, the thesis that there are phenomena to be saved—that there are data that a theory must account for—does not depend on that distinction. It depends only on recognizing that some of the quantities and properties with which a theory deals are those we know how to determine reliably for some systems without using the theoretical principles in question. Such determinations may be only approximate and they may use *other* theoretical principles. Of course observation enters somehow into many such determinations, but we do not have to know just how in order to see the error of the first part of the anti-realist argument.

The problematic notion of "all possible evidence" is clear enough if we understand by it the physically possible values of those quantities pertinent to a theory but determinable independently of it, perhaps generalized to all systems or to all systems of some specified kind. It cannot harm the realist case if too much is included within "all possible evidence." That many theories may save a common

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body of phenomena is initially plausible enough; it is plausible, too, that if $T$ and $T'$ both save a body of phenomena but $T \& H$ saves a larger body of phenomena whereas $T' \& H$ does not, there often (always?) exists an $H'$ such that $T' \& H'$ also saves the enlarged body of evidence. Still, expanding the phenomena and adding auxiliary hypotheses may reduce underdetermination, because $T \& H$ may be intertranslatable with $T' \& H'$ even while neither $T$ nor $H$ is intertranslatable with $T'$ or with $H'$. There are, for example, alternative, distinct, but empirically equivalent gravitational theories such that, when the appropriate electrodynamic theories are conjoined with the respective gravitational theories, there result two intertranslatable theories.

It is wrong to think that the evidence can provide no reasons to discriminate among theories that equally save the phenomena. Hypotheses in a complex theory are often confirmed by obtaining instances of them from the phenomena. Such instances are obtained by using other hypotheses (and sometimes even the very hypothesis to be tested) to determine values of "theoretical" quantities from values of "observational" quantities. With this strategy, two theories that equally well save the phenomena may not be equally well tested by the phenomena. The strategy explains part of the traditional requirements of simplicity, variety of evidence, and explanatory power; there are, then, aspects of all these methodological preferences which amount to preferences for better tested theories.

It is difficult to confine skepticism to the noumena, for we often have as much or more reason to believe our theories as to believe any particular piece of evidence. That is why it is sometimes licit to discard as erroneous data that do not fit an otherwise adequate functional relation. Indeed, for some theories—Newtonian gravitational theory, for example—we have more reason to believe in the theoretical relations than in the theory's empirical adequacy (in Van Fraassen's sense).

Because the phenomena to be saved are uncertain and we lack an account of when a theory may reasonably contradict a datum, and because of the several different respects in which one theory may be better tested than another, we have no ready answer to the question of underdetermination. We do not know how methodologically desirable features covary, or even if there is a justifiable priority among them. In spite of these difficulties there are cases where we can discover underdetermination. All these, however, are cases in which the theories are not, in Van Fraassen's terms, empirically minimal. In effect they are all cases in which some theoret-

ical quantity is not determinable from the phenomena or in which the theory is equally well tested whatever value is assumed for the quantity. This sort of indeterminacy is not so shocking as a thoroughgoing underdetermination of all theoretical relations. We may hope that there is no more underdetermination than the kind these cases reveal. Anyone can hope.

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## A METHOD FOR ONTOLOGY, WITH APPLICATIONS TO NUMBERS AND EVENTS *

ONTOLOGY is supposed to give us an exhaustive classification of what exists, whose categories embody the metaphysically essential and epistemologically central properties of existents. A successful ontology is a systematic picture of reality at the highest level of generality. It is achieved in two steps: first, we need an exhaustive inventory of all there is; second, we must frame the categories of the classification. We are concerned here with the inventory only, for which W. V. Quine has proposed the following method. Starting with the platitude that a thing exists just in case the assertion that it exists is true, Quine proposes that we find out what there is by assessing the ontological commitments of the (a?) true, complete theory of the world. The desired inventory is the natural offspring of truth and ontological commitment. Thus if truth and ontological commitment can be assessed before explicitly tackling ontological issues, real progress has been made. Quine's explication of ontological commitment is this:

(O) $T$ is ontologically committed to $a/F$s iff $T$ logically implies '$(\exists x)\ (x = a)$'/'$(\exists x)Fx$'.[1]

(O) makes ontological commitment dependent solely upon the success of our logical theory. If we can do logical theory independently

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[1] (O) assumes that $T$ has been regimented as a first-order theory. I will follow Quine in making this assumption. Attributing (O) to Quine is somewhat speculative; sometimes he writes as if ontological commitment is extensional, contrary to (O).