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Demystifying Underdetermination

Pure logic is not the only rule for our judgments; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable.

—Pierre Duhem[1]

Introduction

This essay begins with some good sense from Pierre Duhem. The piece can be described as a defense of this particular Duhemian thesis against a rather more familiar doctrine to which Duhem's name has often been attached. To put it in a nutshell, I shall be seeking to show that the doctrine of underdetermination, and the assaults on methodology that have been mounted in its name, founder precisely because they suppose that the logically possible and the reasonable are coextensive. Specifically, they rest on the assumption that, unless we can show that a scientific hypothesis cannot possibly be reconciled with the evidence, then we have no epistemic grounds for faulting those who espouse that hypothesis. Stated so baldly, this appears to be an absurd claim. That in itself is hardly decisive, since many philosophical (and scientific) theses smack initially of the absurd. But, as I shall show below in some detail, the surface implausibility of this doctrine gives way on further analysis to the conviction that it is even more untoward and ill argued than it initially appears. And what compounds the crime is that precisely this thesis is presupposed by many of the fashionable epistemologies of science of the last quarter cen-

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tury. Before this complex indictment can be made plausible, however, there is a larger story that has to be told.

There is abroad in the land a growing suspicion about the viability of scientific methodology. Polanyi, Wittgenstein, Feyerabend and a host of others have doubted, occasionally even denied, that science is or should be a rule-governed activity. Others, while granting that there are rules of the 'game' of science, doubt that those rules do much to delimit choice (e.g., Quine, Kuhn). Much of the present uneasiness about the viability of methodology and normative epistemology can be traced to a series of arguments arising out of what is usually called "the underdetermination of theories." Indeed, on the strength of one or another variant of the thesis of underdetermination, a motley coalition of philosophers and sociologists has drawn some dire morals for the epistemological enterprise.

Consider a few of the better-known examples: Quine has claimed that theories are so radically underdetermined by the data that a scientist can, if he wishes, hold on to *any* theory he likes, "come what may." Lakatos and Feyerabend have taken the underdetermination of theories to justify the claim that the only difference between empirically successful and empirically unsuccessful theories lay in the talents and resources of their respective advocates (i.e., with sufficient ingenuity, more or less *any* theory can be made to look methodologically respectable).[2] Boyd and Newton-Smith suggest that underdetermination poses several prima facie challenges to scientific realism.[3] Hesse and Bloor have claimed that underdetermination shows the *necessity* for bringing noncognitive, social factors into play in explaining the theory choices of scientists (on the grounds that methodological and evidential considerations alone are demonstrably insufficient to account for such choices).[4] H. M. Collins, and several of his fellow sociologists of knowledge, have asserted that underdetermination lends credence to the view that the world does little if anything to shape or constrain our beliefs about it.[5] Further afield, literary theorists like Derrida have utilized underdetermination as one part of the rationale for "deconstructionism" (in brief, the thesis that, since every text lends itself to a variety of interpretations and thus since texts underdetermine choice among those interpretations, texts have no determinate meaning).[6] This litany of invocations of underdeterminationist assumptions could be expanded almost indefinitely; but that is hardly called for, since it has become a familiar feature of contemporary intellectual discourse to endow underdetermination with a deep significance for our understanding of the limitations of methodology, and thus with broad ramifications for all our claims to knowledge—insofar as the latter are alleged to be grounded in trustworthy procedures of inquiry. In fact, underdetermination forms the central weapon in the relativistic assault on epistemology.

As my title suggests, I think that this issue has been overplayed. Sloppy formulations of the thesis of underdetermination have encouraged authors to use it—sometimes inadvertently, sometimes willfully—to support whatever relativist conclusions they fancy. Moreover, a failure to distinguish several distinct species of underdetermination—some probably viable, others decidedly not—has encouraged writers to lump together situations that ought to be sharply distinguished. Above all, inferences have been drawn from the fact of underdetermination that by no means follow from it. Because all that is so, we need to get as clear as we can about this slippery concept before we can decide whether underdetermination warrants the critiques of methodology that have been mounted in its name. That is the object of the next section of this paper. With those clarifications in hand, I will then turn in succeeding parts to assess some recent garden-variety claims about the methodological and epistemic significance of underdetermination.

Although this paper is one of a series whose larger target is epistemic relativism in general[7], my limited aim here is not to refute relativism in all its forms. It is rather to show that one important line of argument beloved of relativists, the argument from underdetermination, will not sustain the global conclusions that they claim to derive from it.

Vintage Versions of Underdetermination

HUMEAN UNDERDETERMINATION

Although claims about underdetermination have been made for almost every aspect of science, those that interest philosophers most have to do specifically with claims about the underdetermination of *theories*. I shall use the term "theory" merely to refer to any set of *universal statements* that purport to describe the natural world.[8] Moreover, so as not to make the underdeterminationists' case any harder to make out than it already is, I shall—for purposes of this essay—suppose, with them, that single theories by themselves make no directly testable assertions. More or less everyone, relativist or nonrelativist, agrees that "theories are underdetermined" in some sense or other; but the seeming agreement about that formula disguises a dangerously wide variety of different meanings.

Our first step in trying to make some sense of the huge literature on underdetermination comes with the realization that there are two quite distinct families of theses, both of which are passed off as "the" thesis of underdetermination. Within each of these "families," there are still further differentiating features. The generic and specific differences between these versions, as we shall see shortly, are not minor or esoteric. They assert different things; they presuppose different things; the arguments that lead to and from them are quite different. Nonetheless each has been characterized, and often, as "*the* doctrine of underdetermination."

The first of the two generic types of underdetermination is what I shall call, for obvious reasons, deductive or *Humean underdetermination*

(HUD). It amounts to one variant or other of the following claim:

> HUD For any finite body of evidence, there are indefinitely many mutually contrary theories, each of which logically entails that evidence.

The arguments for HUD are sufficiently familiar and sufficiently trivial that they need no rehearsal here.* HUD shows that the fallacy of affirming the consequent is indeed a deductive fallacy (like so many other interesting patterns of inference in science); that the method of hypothesis is not logically probative; that successfully "saving the phenomena" is not a robust warrant for detachment or belief. I have no quarrels with either HUD or with the familiar arguments that can be marshaled for it. But when duly considered, HUD turns out to be an extraordinarily *weak* thesis about scientific inference, one that will scarcely sustain any of the grandiose claims that have been made on behalf of underdetermination.

Specifically, HUD is weak in two key respects: First, it addresses itself only to the role of *deductive logic* in scientific inference; it is wholly silent about whether the rules of a broader ampliative logic underdetermine theory choice. Secondly, HUD provides no motivation for the claim that *all* theories are reconcilable with any given body of evidence; it asserts rather that indefinitely many theories are so. Put differently, even if our doxastic policies were so lax that they permitted us to accept as rational any belief that logically entailed the evidence, HUD would not sanction the claim (which we might call the "*thesis of cognitive egalitarianism*") that all rival theories are thereby equally belief-worthy or equally rational to accept.

Despite these crucial and sometimes overlooked limitations of its scope, HUD still has some important lessons for us. For instance, HUD makes clear that theories cannot be "deduced from the phenomena" (in the literal, non-Newtonian sense of that phrase). It thus establishes that the resources of deductive logic are insufficient, no matter how extensive the evidence, to enable one to determine for certain that any theory is true. But for anyone comfortable with the nowadays familiar mixture of (a) fallibilism about knowledge and (b) the belief that ampliative inference depends on modes of argument that go beyond deductive logic, none of that is either very surprising or very troubling.

As already noted, HUD manifestly does *not* establish that all theories are equally good or equally well supported, or that falsifications are inconclusive or that any theory can be held on to, come what may. Nor, finally, does it suggest, let alone entail, that the methodological enterprise

* For a rehearsal of some trivial arguments for HUD, see the section "Humean Underdetermination" in the commentary on chapter 3.

is hopelessly flawed because methodological rules radically underdetermine theory selection. Indeed, consistently with HUD, one could hold (although I shall not) that the ampliative rules of scientific method fully determine theory choice. HUD says nothing whatever about whether ampliative rules of theory appraisal do or do not determine theory choice uniquely. What HUD teaches, and all that it licenses, is that *if one is* prepared to accept only those theories that can be proven to be true, then one is going to have a drastically limited doxastic repertoire.

Mindful of the some of the dire consequences (enumerated above) that several authors have drawn from the thesis of underdetermination, one is inclined to invoke minimal charity by saying that Humean underdetermination must not be quite what they have in mind. And I think we have independent evidence that they do not. I have dwelt on this weak form of underdetermination to start with because, as I shall try to show below, it is the only *general* form of underdetermination that has been incontrovertibly established. Typically, however, advocates of underdetermination have a much stronger thesis in mind. Interestingly, when attacked, they often fall back on the truism of HUD; a safe strategy since HUD is unexceptionable. They generally fail to point out that HUD will support none of the conclusions that they wish to draw from underdetermination. By failing to distinguish between HUD and stronger (and more controversial) forms of underdetermination, advocates of undifferentiated underdetermination thus piggyback their stronger claims on this weaker one. But more of that below.

The Quinean Reformulations of Underdetermination[9]

Like most philosophers, Quine of course accepts the soundness of HUD. But where HUD was silent on the key question of ampliative underdetermination, Quine (along with several other philosophers) was quick to take up the slack. In particular, Quine has propounded two distinct doctrines, both of which have direct bearing on the issues before us. The first, and weaker, of these doctrines I shall call *the nonuniqueness thesis*. It holds that: *for any theory, T, and any given body of evidence supporting T, there is at least one rival (i.e. contrary) to T that is as well supported as T.*[10] In his more ambitious (and more influential) moments, Quine is committed to a much stronger position, which I call *the egalitarian thesis.* It insists that: *every theory is as well supported by the evidence as any of its rivals.*[11] Quine nowhere explicitly expresses the egalitarian thesis in precisely this form. But it will be the burden of the following analysis to show that Quine's numerous pronouncements on the retainability of theories, in the face of virtually any evidence, presuppose the egalitarian thesis, and make no sense without it. What follows is not meant to be an exegesis of Quine's intentions; it is meant, rather, as an exploration of whether Quine's position on this issue will sustain the broad implications that many writers (sometimes including Quine himself) draw from it.

What distinguishes both the nonuniqueness thesis and the egalitarian thesis from HUD is that they concern ampliative rather than deductive underdetermination; that is, they centrally involve the notion of "empirical support," which is after all the central focus of ampliative inference. In this section and the first part of the next, I shall focus on Quine's discussion of these two forms of ampliative underdetermination (especially the egalitarian thesis), and explore some of their implications. The egalitarian thesis is sufficiently extreme—not to say epistemically pernicious—that I want to take some time showing that some versions of Quine's holism are indeed committed to it. I shall thus examine its status in considerable detail before turning in later sections to look at some other prominent accounts of ampliative underdetermination.

Everyone knows that Quine, in his "Two Dogmas of Empiricism," maintained that:

(0) one may hold onto any theory whatever in the face of any evidence whatever.[12]

Crucial here is the sense of "may" involved in this extraordinary claim. If taken as asserting that human beings are psychologically capable of retaining beliefs in the face of overwhelming evidence against them, then it is a wholly uninteresting truism, borne out by every chapter in the saga of human folly. But if Quine's claim is to have any bite, or any philosophical interest, it must be glossed along roughly the following lines:

(1) It is rational to hold onto any theory whatever in the face of any evidence whatever.

I suggest this gloss because I suppose that Quine means to be telling us something about scientific rationality; and it is clear that (0), construed descriptively, has *no* implications for normative epistemology. Combined with Quine's counterpart claim that one is also free to jettison any theory one is minded to, (1) appears to assert the *equirationality* of all rival theoretical systems. Now, what grounds does Quine have for asserting (1)? One might expect that he could establish the plausibility of (1) only in virtue of examining the relevant rules of rational theory choice and showing, if it could be shown, that those rules were always so ambiguous that, confronted with any pair of theories and any body of evidence, they could never yield a decision procedure for making a choice. Such a proof, if forthcoming, would immediately undercut virtually every theory of empirical or scientific rationality. But Quine *nowhere*, neither in "Two Dogmas . . ." nor elsewhere, engages in a general examination of ampliative rules of theory choice.

His specific aim in propounding (0) or (1) is often said to be to exhibit the ambiguity of falsification or of *modus tollens*. The usual reading of Quine here is that he has shown the impotence of negative instances to disprove a theory, just as Hume had earlier showed the impotence of positive instances to prove a theory. Indeed, it is this gloss that establishes the parallel between Quine's form of the thesis of underdetermination and HUD. Between them, they seem to lay to rest any prospect for a purely deductive logic of scientific inference.

But what is the status of (1)? I have already said that Quine nowhere engages in an exhaustive examination of various rules of rational theory choice with a view to showing them impotent to make a choice between all pairs of theories. Instead, he is content to examine a *single* rule of theory choice, what we might call the Popperian gambit. That rule says, in effect, "reject theories that have (known) falsifying instances." Quine's strategy is to show that this particular rule radically underdetermines theory choice. I intend to spend the bulk of this section examining Quine's case for the claim that this particular rule underdetermines theory choice. But the reader should bear in mind that even if Quine were successful in his dissection of this particular rule (which he is not), that would still leave unsettled the question whether other ampliative rules of detachment suffer a similar fate.*

How does he go about exhibiting the underdeterminative character of falsification? Well, Quine's explicit arguments for (1) in "Two Dogmas . . ." are decidedly curious. Confronted, for instance, with an apparent refutation of a claim that "there are brick houses on Elm Street," we can—he says—change the meaning of the terms so that (say) "Elm Street" now refers to Oak Street, which adventitiously happens to have brick houses on it, thereby avoiding the force of the apparent refutation. Now this is surely a Pickwickian sense of "holding onto a theory come what may," since what we are holding onto here is not what the theory asserted, but the (redefined) string of words constituting the theory.[13] Alternatively, says Quine, we can always change the laws of logic if need be. We might, one supposes, abandon *modus tollens*, thus enabling us to maintain a theory in the face of evidence that, under a former logical regime, was falsifying of it; or we could jettison *modus ponens* and thereby preclude the possibility that the theory we are concerned to save is "implicated" in any schema of inference leading to the awkward prediction. If one is loath to abandon such useful logical devices (and Quine is), other resources are

* In inductive logic, rules of detachment are often called *acceptance rules*. They specify when it is permissible to accept a hypothesis as true, thus "detaching" the hypothesis h from the assertion $P(h/e) = r$, that h has probability r on evidence e.

open to us. We could, says Quine, dismiss the threatening evidence "by pleading hallucination."[14]

But are there no constraints on when it is reasonable to abandon selected rules of logic or when to label evidence specious (because the result of hallucination) or when to redefine the terms of our theories? Of course, it is (for all I know) humanly possible to resort to any of these stratagems, as a descriptivist reading of (0) might suggest. But nothing Quine has said thus far gives us any grounds to believe, as (1) asserts, that it will ever, let alone *always*, be rational to do so. Yet his version of the thesis of underdetermination, if he means it to have any implications for normative epistemology, requires him to hold that it is rational to use some such devices.[15] Hence he would appear to be committed to the view that epistemic rationality gives us no grounds for avoiding such maneuvers. (On Quine's view, the only considerations that we could possibly invoke to block such stratagems have to do with pragmatic, not epistemic, rationality.[16]) Thus far, the argument for ampliative underdetermination seems made of pretty trifling stuff.

But there is a fourth, and decidedly nontrivial, stratagem that Quine envisages for showing how our Popperian principle underdetermines theory choice. This is the one that has received virtually all the exegetical attention; quite rightly too, since Quine's arguments on the other three are transparently question begging because they fail to establish the rationality of holding onto any theory in the face of any evidence. Specifically, Quine proposes that a threatened statement or theory can always be immunized from the threat of the recalcitrant evidence by making suitable adjustments in our auxiliary theories. It is here that the familiar "Duhem-Quine thesis" comes to the fore. What confronts experience in any test, according to both Quine and Duhem, is an entire theoretical structure (later dubbed by Quine "a web of belief") consisting inter alia [among other things] of a variety of theories. Predictions, they claim, can never be derived from single theories but only from collectives consisting of multiple theories, statements of initial and boundary conditions, assumptions about instrumentation, and the like. Since (they claim) it is whole systems and whole systems alone that make predictions, when those predictions go awry it is theory complexes, not individual theories, that are indicted via *modus tollens*. But, so the argument continues, we cannot via *modus tollens* deduce the falsity of any component of a complex from the falsity of the complex as a whole. Quine put it this way:

> But the failure [of a prediction] falsifies only a block of theory as a whole, a conjunction of many statements. The failure shows that one or more of those statements is false, but it does not show which.[17]

Systems, complexes or "webs" apparently turn out to be unambiguously falsifiable on Quine's view; but the choice between individual theories or statements making up these systems is, in his view, radically underdetermined.

Obviously, this approach is rather more interesting than Quine's other techniques for saving threatened theories, for here we need not abandon logic, redefine the terms in our theories in patently ad hoc fashion, nor plead hallucinations. The thesis of underdetermination in this particular guise, which I shall call Quinean underdetermination (QUD), can be formulated as follows:

QUD Any theory can be reconciled with any recalcitrant evidence by making suitable adjustments in our other assumptions about nature.

Before we comment on the credentials of QUD, we need to further disambiguate it. We especially need to focus on the troublesome phrase "can be reconciled with." On a weak interpretation, this would be glossed as "can be made logically compatible with the formerly recalcitrant evidence." I shall call this the "*compatibilist version of QUD*." On a stronger interpretation, it might be glossed as "can be made to function significantly in a complex that entails" the previously threatening evidence. Let us call this the "*entailment version of QUD*." To repeat, the compatibilist version says that any theory can be made *logically compatible* with any formerly threatening evidential report; the entailment interpretation insists further that any theory can be made to function essentially in a *logical derivation* of the erstwhile refuting instance.

The compatibilist version of QUD can be trivially proven. All we need do, given any web of belief and a suspect theory that is part of it, is to remove *(without replacement)* any of those ancillary statements within the web needed to derive the recalcitrant prediction from the theory. Of course, we may well lose enormous explanatory power thereby, and the web may lose much of its pragmatic utility thereby, but there is nothing in deductive logic that would preclude any of that.

The entailment version of QUD, by contrast, insists that there is always a set of auxiliary assumptions that can replace others formerly present, and that will allow the *derivation*, not of the wrongly predicted result, but of precisely what we have observed. As Grünbaum, Quinn, Laudan and others have shown,[18] neither Quine nor anyone else has ever produced a general existence proof concerning the availability either in principle or in practice of suitable (i.e., nontrivial) theory-saving auxiliaries. Hence the entailment version of QUD is without apparent warrant. For a time (circa 1962), Quine himself conceded as much.[19] That is by now a familiar result. But what I think needs much greater emphasis than it has received is the fact that, *even if nontrivial auxiliaries existed that would satisfy the demands of the entailment version of QUD, no one has ever shown that it would be rational to prefer a web that included them and the threatened* *theory to a rival web that dispensed with the theory in question.* Indeed, as I shall show in detail, what undermines *both* versions of QUD is that neither logical *compatibility* with the evidence nor logical *derivability* of the evidence is sufficient to establish that a theory exhibiting such empirical compatibility and derivability is rationally acceptable.

It will prove helpful to distinguish four different positive relations in which a theory (or the system in which a theory is embedded) can stand to the evidence. Specifically, a theory (or larger system of which it is a part) may:

- be logically compatible with the evidence;
- logically entail the evidence;
- explain the evidence;
- be empirically supported by the evidence.

Arguably, none of these relations reduces to any of the others; despite that, Quine's analysis runs all four together. But what is especially important for our purposes is the realization that *satisfaction of either the compatibility relation or the entailment relation fails to establish either an explanatory relation or a relation of empirical support.* For instance, theories may entail statements that they nonetheless do not explain; self-entailment being the most obvious example. Equally, theories may entail evidence statements, yet not be empirically supported by them (e.g., if the theory was generated by the algorithmic manipulation of the "evidence" in question).

So, when QUD tells us that any theory can be "reconciled" with any bit of recalcitrant evidence, we are going to have to attend with some care to what that reconciliation consists in. Is Quine claiming, for instance, that any theory can—by suitable modifications elsewhere—continue to function as part of an *explanation* of a formerly recalcitrant fact? Or is he claiming, even more ambitiously, that any formerly recalcitrant instance for a theory can be transformed into a *confirming instance* for it?

As we have seen, the only form of QUD that has been firmly established is compatibilist Quinean underdetermination (an interpretation that says a theory can always be rendered logically compatible with any evidence, provided we are prepared to give up enough of our other beliefs); so I shall begin my discussion there. Saving a prized, but threatened, theory by abandoning the auxiliary assumptions once needed to link it with recalcitrant evidence clearly comes at a price. Assuming that we give up those beliefs without replacement (and recall that this is the only case that has been made plausible), we not only abandon an ability to say anything whatever about the phenomena that produced the recalcitrant experience; we also now give up the ability to explain all the other things which those now-rejected auxiliaries enabled us to give an account of—

with no guarantee whatever that we can find alternatives to them that will match their explanatory scope.

But further and deeper troubles lurk for Quine just around the corner. For it is not just explanatory scope that is lost; it is also *evidential support*. Many of those phenomena that our web of belief could once give an account of (and which presumably provided part of the good reasons for accepting the web with its constituent theories) are now beyond the resources of the web to explain and predict. That is another way of saying that the revised web, stripped of those statements formerly linking the theory in question with the mistaken prediction, now has substantially less empirical support than it once did; assuming, of course, that the jettisoned statements formerly functioned to do more work for us than just producing the discredited prediction.[20] Which clearly takes things from bad to worse. For now Quine's claim about the salvageability of a threatened theory turns out to make sense just in case the only criterion of theory appraisal is logical compatibility with observation. If we are concerned with issues like explanatory scope or empirical support, Quine's QUD in its compatibilist version cuts no ice whatsoever.

Clearly, what is wrong with QUD, and why it fails to capture the spirit of (1), is that it has dropped out any reference to the *rationality* of theory choices, and specifically theory rejections. It doubtless is possible for us to jettison a whole load of auxiliaries in order to save a threatened theory (where "save" now means specifically "to make it logically compatible with the evidence"), but Quine nowhere establishes the reasonableness or the rationality of doing so. And if it is plausible, as I believe it is, to hold that scientists are aiming (among other things) at producing theories with broad explanatory scope and impressive empirical credentials, then it has to be said that Quine has given us no arguments to suppose that any theory we like can be doctored up so as to win high marks on those scores.

This point underscores the fact that too many of the discussions of underdetermination in the last quarter century have proceeded in an evaluative vacuum. They imagine that if a course of action is logically possible, then one need not attend to the question of its rationality. But if QUD is to carry any epistemic force, it needs to be formulated in terms of the rationality of preserving threatened theories. One might therefore suggest the following substitute for QUD (which was itself a clarification of (1)):

(2) any theory can be rationally retained in the face of any recalcitrant evidence.

Absent strong arguments for (2) or its functional equivalents, Quinean holism, the Duhem-Quine thesis and the (non-Humean) forms of underdetermination appear to pose no threat in principle for an account of scientific methodology or rationality. The key question is whether Quine, or any of the other influential advocates of the methodological significance of underdetermination, have such arguments to make.

Before we attempt to answer that question, a bit more clarification is called for, since the notion of retainment, let alone rational retainment, is still less than transparent. I propose that we understand that phrase to mean something along these lines: to say that a theory can be rationally retained is to say that reasons can be given for holding that theory, or the system of which it is a part, as true (or empirically adequate) that are (preferably stronger than but) as least as strong as the reasons that can be given for holding as true (or empirically adequate) any of its *known* rivals. Some would wish to give this phrase a more demanding gloss; they would want to insist that a theory can be rationally held only if we can show that the reasons in its behalf are stronger than those for all its *possible* rivals, both extant and those yet-to-be-conceived. That stronger gloss, which I shall resist subscribing to, would have the effect of making it even harder for Quine to establish (2) than my weaker interpretation does. Because I believe that theory choice is generally a matter of comparative choice among extant alternatives, I see no reason why we should saddle Quine and his followers with having to defend (2) on its logically stronger construal. More to the point, if I can show that the arguments on behalf of the weaker construal fail, that indeed the weaker construal is false, it follows that its stronger counterpart fails as well, since the stronger entails the weaker. I therefore propose emending (2) as follows:

(2*) any theory can be shown to be as well supported by any evidence as any of its known rivals.

Quine never formulates this thesis as such, but I have tried to show that defending a thesis of this sort is incumbent on anyone who holds, as Quine does, that any theory can be held true, come what may. Duly considered, (2*) is quite a remarkable thesis, entailing as it does that all the known contraries to every known theory are equally well supported. Moreover, (2*) is our old friend, the egalitarian thesis. If correct, (2*) entails (for instance) that the flat-earth hypothesis is as sound as the oblate-spheroid hypothesis[21]; that it is as reasonable to believe in fairies at the bottom of my garden as not. But, for all its counter-intuitiveness, this is precisely the doctrine to which authors like Quine, Kuhn, and Hesse are committed.[22] (In saying that Quine is committed to this position, I do not mean that he would avow it if put to him directly; I doubt that very much. My claim rather is (a), that Quine's argument in "Two Dogmas . . ." commits him to such a thesis, and (b), that those strong relativists who look to Quine as having espoused and established the egalitarian thesis are exactly half right. I prefer to leave it to Quine exegetes to decide whether the positions of the *later* Quine allow him to be exonerated of the charge that his more recent writing run afoul of the same problem.)

One looks in vain in "Two Dogmas . . ." for even the whiff of an argument that would make the egalitarian thesis plausible. As we have seen, Quine's only marginally relevant points there are his suppositions (1) that any theory can be made logically compatible with any evidence (statement) and (2) that any theory can function in a network of statements that will entail any particular evidence statement.[23] But what serious epistemologist has ever held either (a) that bare logical compatibility with the evidence constituted adequate reason to accept a scientific theory,[24] or (b) that logical entailment of the evidence by a theory constituted adequate grounds for accepting a theory? One might guess otherwise. One might imagine that some brash hypothetico-deductivist would say that any theory that logically entailed the known evidence was acceptable. If one conjoins this doctrine with Quine's claim (albeit one that Quine has never made out) that every theory can be made to logically entail any evidence, then one has the makings of the egalitarian thesis. But such musings cut little ice, since no serious twentieth-century methodologist has ever espoused, without crucial qualifications, logical compatibility with the evidence or logical derivability of the evidence as a sufficient condition for detachment of a theory.[25]

Consider some familiar theories of evidence to see that this is so. Within Popper's epistemology, two theories, T_1 and T_2, that thus far have the same positive instances, e, may nonetheless be differentially supported by e. For instance, if T_1 predicted e before e was determined to be true, whereas T_2 is produced after e is known, then e (according to Popper) constitutes a good test of T_1 but no test of T_2. Bayesians too insist that rival (but nonequivalent) theories sharing the same known positive instances are not necessarily equally well confirmed by those instances. Indeed, if two theories begin with different prior probabilities, then their posterior probabilities must be different, *given the same positive instances*.[26] But that is just to say that even if two theories enjoy precisely the same set of known confirming instances, *it does not follow that they should be regarded as equally well confirmed by those instances*. All of which is to say that showing that rival theories enjoy the same "empirical support"—in any sense of that term countenanced by (2*)—requires more than that those rivals are compatible with, or capable of entailing, the same "supporting" evidence. (2*) turns out centrally to be a claim in the theory of evidence and, since Quine does not address the evidence relation in "Two Dogmas . . . ," one will not find further clarification of this issue there.[27]

Of course, "Two Dogmas . . ." was not Quine's last effort to grapple with these issues. Some of these themes recur prominently in *Word and Object*, and it is worth examining some of Quine's arguments about underdetermination to be found there. In that work, Quine explicitly if briefly addresses the question, already implicit in "Two Dogmas . . . ," whether ampliative rules of theory choice underdetermine theory choice.[28] Quine begins his discussion there by making the relatively mild claim that scientific methodology, along with any imaginable body of evidence, *might possibly* underdetermine theory choice. As he wrote:

> *conceivably* the truths about molecules are only partially determined by any ideal organon of scientific method plus all the truths that can be said in common sense terms about ordinary things.[29]

Literally, the remark in this passage in unexceptionable. Since we do not yet know what the final "organon of scientific method" will look like, it surely is "conceivable" that the truth status of claims about molecular structure might be underdetermined by such an organon. Three sentences later, however, this claim about the conceivability of ampliative underdetermination becomes a more ambitious assertion about the *likelihood* of such underdetermination:

> The incompleteness of determination of molecular behavior by the behavior of ordinary things . . . remains true even if we include all past, present and future irritations of all the far-flung surfaces of mankind, and probably *even if we throw in [i.e., take for granted] an in fact achieved organon of scientific method besides.*[30]

As it stands, and as it remains in Quine's text, this is no argument at all, but a bare assertion. But it is one to which Quine returns still later:

> we have no reason to suppose that man's surface irritations even unto eternity admit of any systematization that is scientifically better or simpler than all possible others. It seems *likelier*, if only on account of symmetries or dualities, that countless alternative theories would be tied for first place.[31]

Quite how Quine thinks he can justify this claim of "likelihood" for ampliative underdetermination is left opaque. Neither here nor elsewhere does he show that *any* specific ampliative rules of scientific method[32] actually underdetermine theory choice—let alone that the rules of a "final methodology" will similarly do so. Instead, on the strength of the notorious ambiguities of simplicity (and by some hand-waving assertions that other principles of method may "plausibly be subsumed under the demand for simplicity"[33]—a claim that is anything but plausible), Quine asserts "in principle," that there is "probably" no theory that can uniquely satisfy the "canons of any ideal organon of scientific method."[34] In sum, Quine fails to show that theory choice is ampliatively underdetermined even by *existing* codifications of scientific methodology (all of which go considerably beyond the principle of simplicity), let alone by all possible such codifications.[35]

More important for our purposes, even if Quine were right that no ideal organon of methodology could ever pick out any theory as uniquely satisfying its demands, we should note—in the version of underdetermination contained in the last passage from Quine—how drastically he has apparently weakened his claims from those of "Two Dogmas. . . ." That essay, you recall, had espoused the egalitarian thesis that *any* theory can be reconciled with any evidence. We noted how much stronger that thesis was than the nonuniqueness thesis to the effect that there will always be some rival theories reconcilable with any finite body of evidence. But in *Word and Object*, as the passages I have cited vividly illustrate, *Quine is no longer arguing that any theory can be reconciled with any evidence*;[36] he is maintaining rather that, no matter what our evidence and no matter what our rules of appraisal, there will always remain the possibility (or the likelihood) that the choice will not be uniquely determined. But that is simply to say that there will (probably) always be at least one contrary to any given theory that fits the data equally well—a far cry from the claim, associated with QUD and (2*), that *all* the contraries to a given theory will fit the data equally well. In a sense, therefore, Quine appears in *Word and Object* to have abandoned the egalitarian thesis for the nonuniqueness thesis, since the latter asserts not the epistemic equality of all theories but only the epistemic equality of certain theories.[37] That surmise aside, it is fair to say that *Word and Object* does nothing to further the case for Quine's egalitarian view that "any theory can be held true come what may."

Some terminological codification might be useful before we proceed, since we have reached a natural breaking point in the argument. As we have seen, one can distinguish between (a) *descriptive* (0) and (b) *normative* (1, 2, 2*) forms of underdetermination, depending upon whether one is making a claim about what people are capable of doing or what the rules of scientific rationality allow.[38] One can also distinguish between (c) *deductive* and (d) *ampliative* underdetermination, depending upon whether it is the rules of deductive logic (HUD) or of a broadly inductive logic or theory of rationality that are alleged to underdetermine choice (QUD). Further, we can distinguish between the claims that theories can be reconciled with recalcitrant evidence via establishing (e) *compatibility* between the two or (f) a one-way *entailment* between the theory and the recalcitrant evidence or (g) equivalence of support between rival theories. Finally, one can distinguish between (h) the doctrine that choice is underdetermined between at least one of the contraries of a theory and that theory (*nonuniqueness*) and (i) the doctrine that theory choice is underdetermined between every contrary of a theory and that theory ("cognitive *egalitarianism*").

Using this terminology, we can summarize such conclusions as we have reached to this point: In "Two Dogmas . . . ," Quine propounded a thesis of normative, ampliative, egalitarian underdetermination. Whether we construe that thesis in its compatibilist or entailment versions, it is clear that Quine has said nothing that makes plausible the idea that every prima facie refuted theory can be embedded in a rationally acceptable (i.e., empirically well-supported) network of beliefs. Moreover, "Two Dogmas . . ." developed an argument for underdetermination for only one rationality principle among many, what I have been calling the Popperian gambit. This left completely untouched the question whether other rules of theory choice suffered from the same defects that Quine thought Popper's did. Perhaps with a view to remedying that deficiency, Quine argued—or, rather, alleged without argument—in *Word and Object* that *any* codification of scientific method would underdetermine theory choice. Unfortunately, *Word and Object* nowhere delivers on its claim about underdetermination.

But suppose, just for a moment, that Quine had been able to show what he claimed in *Word and Object*, to wit, the nonuniqueness thesis. At best, that result would establish that for any well-confirmed theory, there is in principle at least one other theory that will be equally well-confirmed by the same evidence. That is an interesting thesis to be sure, and possibly a true one, although Quine has given us no reason to think so. (Shortly, we shall examine arguments of other authors that seem to provide some ammunition for this doctrine.) But even if true, the nonuniqueness thesis will not sustain the critiques of methodology that have been mounted in the name of underdetermination. Those critiques are all based, implicitly or explicitly, on the strong, egalitarian reading of underdetermination. They amount to saying that the project of developing a methodology of science is a waste of time since, no matter what rules of evidence we eventually produce, those rules will do nothing to delimit choice between rival theories. The charge that methodology is toothless pivots essentially on the viability of QUD in its ampliative, egalitarian version. Nonuniqueness versions of the thesis of ampliative underdetermination at best establish that methodology will not allow us to pick out a theory as uniquely true, no matter how strong its evidential support. (*Word and Object*'s weak ampliative thesis of underdetermination, even if sound, would provide no grounds for espousing the strong underdeterminationist thesis implied by the "any theory can be held come what may" dogma.[39])

Theory choice may or may not be ampliatively underdetermined in the sense of the nonuniqueness thesis; that is an open question. But however that issue is resolved, that form of underdetermination poses no challenge to the methodological enterprise. What would be threatening to, indeed debilitating for, the methodological enterprise is if QUD in its egalitarian version were once established. Even though Quine offers no persuasive arguments in favor of normative, egalitarian, ampliative underdetermination, there are several other philosophers who appear to have taken up the cudgels on behalf of precisely such a doctrine. It is time I turned to their arguments.

Ampliative Underdetermination

With this preliminary spade work behind us, we are now in a position to see that the central question about underdetermination, at least so far as the philosophy of science is concerned, is the issue of ampliative underdetermination. Moreover, as we have seen, the threat to the epistemological project comes, not from the nonuniqueness version of underdetermination, but from the egalitarian version. (That version states that any theory can be embedded in a system that will be as strongly supported by the evidence as any rival is supported by the same evidence.) The question is whether anyone has stronger arguments than Quine's for the methodological underdetermination of theory choice. Two plausible contenders for that title are Nelson Goodman and Thomas Kuhn. I shall deal briefly with them in turn.

Goodman's *Fact, Fiction and Forecast* is notorious for posing a particularly vivid form of ampliative underdetermination, in the form of the grue/green, and related, paradoxes of induction.* Goodman is concerned there to deliver what Quine had elsewhere merely promised, namely, a proof that the inductive rules of scientific method underdetermine theory choice in the face of any conceivable evidence. The general structure of Goodman's argument is too familiar to need any summary here. But it is important to characterize carefully what Goodman's result shows. I shall do so utilizing terminology we have already been working with. Goodman shows that one specific rule of ampliative inference (actually a whole family of rules bearing structural similarities to the straight rule of induction) suffers from this defect: Given any pair (or n-tuple) of properties that have previously always occurred together in our experience, it is possible to construct an indefinitely large variety of contrary theories, all of which are compatible with the inductive rule: "If, for a large body of instances, the ratio of the successful instances of a hypothesis is very high compared to its failures, then assume that the hypothesis will continue to enjoy high

* Goodman defines the predicate *grue* in the following way: an object is grue if and only if it is observed before time T and is green, or else it is not observed before time T and is blue. Suppose we examine a large number of emeralds and find that all emeralds we have observed are green. We might reasonably infer that the hypothesis "All emeralds are green" is probably true (because it is confirmed by its many instances) and then use that hypothesis to predict that emeralds observed in the future (including those observed after T) will be green, too. But if our inspection has taken place before time T (say, the year 2050), then "All emeralds are grue" is also confirmed by the observed instances, and we could use the grue hypothesis to predict that any emerald observed after the year 2050 will be blue (and *not* green). Thus, we seem to have rival hypotheses, supporting incompatible predictions, both confirmed equally well by our evidence. For more on the grue problem and attempts to solve it, see "Goodman's Gruesome New Riddle of Induction" in the commentary on chapter 3.

success in the future." All these contraries will (along with suitable initial conditions) entail all the relevant past observations of the pairings of the properties in question. Thus, in one of Goodman's best-known examples, the straight rule will not yield an algorithm for choosing between "All emeralds are green" and "All emeralds are grue"; it awards them equally good marks.

There is some monumental question begging going on in Goodman's setting up of his examples. He supposes without argument that—since the contrary inductive extrapolations all have the same positive instances (to date)—the inductive logician must assume that the extrapolations from each of these hypotheses are all rendered equally likely by those instances. Yet we have already had occasion to remark that "possessing the same positive instances" and "being equally well confirmed" boil down to the same thing only in the logician's never-never land. (It was Whewell, Peirce and Popper who taught us all that theories sharing the same positive instances need not be regarded as equally well tested or equally beliefworthy.) But Goodman does have a point when he directs our attention to the fact that the straight rule of induction, as often stated, offers no grounds for distinguishing between the kind of empirical support enjoyed by the green hypothesis and that garnered by the grue hypothesis.

Goodman himself believes, of course, that this paradox of induction can be overcome by an account of the entrenchment of predicates. Regardless whether one accepts Goodman's approach to that issue, it should be said that strictly he does not hold that theory choice is underdetermined; on his view, such ampliative underdetermination obtains only if we limit our organon of scientific methodology to some version of the straight rule of induction.

But, for purposes of this paper, we can ignore the finer nuances of Goodman's argument since, even if a theory of entrenchment offered no way out of the paradox, and even if the slide from "possessing the same positive instances" to "being equally well confirmed" was greased by some plausible arguments, Goodman's arguments can provide scant comfort to the relativist's general repudiation of methodology. Recall that the relativist is committed, as we have seen, to arguing an egalitarian version of the thesis of ampliative underdetermination, i.e., he must show that all rival theories are equally well supported by any conceivable evidence. But there is nothing whatever in Goodman's analysis—even if we grant *all* its controversial premises—that could possibly sustain such an egalitarian conclusion. Goodman's argument, after all, does not even claim to show apropos of the straight rule that it will provide support for any and every hypothesis: his concern, rather, is to show that there will always be a family of contrary hypotheses between which it will provide no grounds for rational choice. The difference is crucial. If I propound the hypothesis that "All emeralds are red" and if my evidence base happens to be that all previously examined emeralds are green, then the straight rule is unambiguous in its insistence that my hypothesis be rejected. The alleged inability of the straight rule to distinguish between green- and grue-style hypotheses provides no ammunition for the claim that such a rule can make no epistemic distinctions whatever between rival hypotheses. If we are confronted with a choice between (say) the hypotheses that all emeralds are red and that all are green, then the straight rule gives us entirely unambiguous advice concerning which is better supported by the relevant evidence. Goodmanian underdetermination is thus of the nonuniqueness sort. When one combines that with a recognition that Goodman has examined but one among a wide variety of ampliative principles that arguably play a role in scientific decision making, it becomes clear that no global conclusions whatever can be drawn from Goodman's analysis concerning the general inability of the rules of scientific methodology for strongly delimiting theory choice.

But we do not have to look very far afield to find someone who does propound a strong (viz., egalitarian) thesis of ampliative underdetermination, one which, if sound, would imply that the rules of methodology were never adequate to enable one to choose between any rival theories, regardless of the relevant evidence. I refer, of course, to Thomas Kuhn's assertion in *The Essential Tension* to the effect that the shared rules and standards of the scientific community *always* underdetermine theory choice.[46] Kuhn there argues that science is guided by the use of several methods (or, as he prefers to call them, "standards"). These include the demand for empirical adequacy, consistency, simplicity, and the like. What Kuhn says about these standards is quite remarkable. He is not making the point that the later Quine and Goodman made about the methods of science; namely, that for any theory picked out by those methods, there will be indefinitely many contraries to it that are equally compatible with the standards. On the contrary, Kuhn is explicitly pushing the same line that the early Quine was implicitly committed to, viz., that the methods of science are inadequate ever to indicate that any theory is better than any rival, regardless of the available evidence. In the language of this essay, it is the egalitarian form of underdetermination that Kuhn is here proposing.

Kuhn, of course, does not use that language, but a brief rehearsal of Kuhn's general scheme will show that egalitarian underdetermination is one of its central underpinnings. Kuhn believes that there are divergent paradigms within the scientific community. Each paradigm comes to be associated with a particular set of practices and beliefs. Once a theory has been accepted within an ongoing scientific practice, Kuhn tells us, there is nothing that the shared standards of science can do to dislodge it. If paradigms do change, and Kuhn certainly believes that they do, this must be the result of "individual" and "subjective" decisions by individual researchers, not because there is anything about the methods or standards scientists share that ever requires the abandonment of those paradigms and their associated theories. In a different vein, Kuhn tells us that a paradigm always looks good by its own standards and weak by the standards of its rivals and that there never comes a point at which adherence to an old paradigm or resistance to a new one ever becomes "unscientific."[41] In effect, then, Kuhn is offering a paraphrase of the early Quine, but giving it a Wittgensteinean twist: "once a theory/paradigm has been established within a practice, it can be held on to, come what may." The shared standards of the scientific community are allegedly impotent ever to force the abandonment of a paradigm, and the specific standards associated with any paradigm will always give it the nod.

If this seems extreme, I should let Kuhn speak for himself. "Every individual choice between competing theories," he tells us, "depends on a mixture of objective and subjective factors, or of shared and individual criteria."[42] It is, in Kuhn's view, no accident that individual or subjective criteria are used alongside the objective or shared criteria, for the latter "are not by themselves sufficient to determine the decisions of individual scientists."[43] Each individual scientist "must complete the objective criteria [with 'subjective considerations'] before any computations can be done."[44] Kuhn is saying here that the shared methods or standards of scientific research are *always* insufficient to justify the choice of one theory over another.[45] That could only be so if (2*) or one of its functional equivalents were true of those shared methods.

What arguments does Kuhn muster for this egalitarian claim? Well, he asserts that all the standards that scientists use are ambiguous and that "individuals may legitimately differ about their application to concrete cases."[46] "Simplicity, scope, fruitfulness and even accuracy can be judged differently . . . by different people."[47] He is surely right about some of this. Notoriously, one man's simplicity is another's complexity; one may think a new approach fruitful, while a second may see it as sterile. But such fuzziness of conception is precisely why most methodologists have avoided falling back on these hazy notions for talking about the empirical warrant for theories. Consider a different set of standards, one arguably more familiar to philosophers of science:

- prefer theories that are internally consistent;
- prefer theories that correctly make some predictions that are surprising given our background assumptions;
- prefer theories that have been tested against a diverse range of kinds of phenomena to those that have been tested only against very similar sorts of phenomena.

Even standards such as these have some fuzziness around the edges, but can anyone believe that, confronted with *any* pair of theories and *any* body of evidence, these standards are so rough-hewn that they could be used indifferently to justify choosing either element of the pair? Do we really believe that Aristotle's physics correctly made the sorts of surprising predictions that Newton's physics did? Is there any doubt that Cartesian optics, with its dual insistence on the instantaneous propagation of light and that light traveled faster in denser media than in rarer ones, violated the canon of internal consistency?

Like the early Quine, Kuhn's wholesale holism commits him to the view that, consistently with the shared canons of rational acceptance, any theory or paradigm can be preserved in the face of any evidence. As it turns out, however, Kuhn no more has plausible arguments for this position than Quine had. In each case, the idea that the choice between changing or retaining a theory/paradigm is ultimately and always a matter of personal preference turns out to be an unargued dogma. In each case, if one takes away that dogma, much of the surrounding edifice collapses.

Of course, none of what I have said should be taken to deny that all forms of underdetermination are bogus. They manifestly are not. Indeed, there are several types of situations in which theory choice is indeed underdetermined by the relevant evidence and rules. Consider a few:

a) We can show that for some rules, and for certain theory pairs, theory choice is underdetermined for certain sorts of evidence. Consider the well-known case of the choice between the astronomical systems of Ptolemy and Copernicus. If the only sort of evidence available to us involves reports of line-of-sight positions of planetary position, and if our methodological rule is something like "Save the phenomena," then it is easy to prove that any line-of-sight observation that supports Copernican astronomy also supports Ptolemy's.[48] (It is crucial to add, of course, that if we consider other forms of evidence besides line-of-sight planetary position, this choice is not strongly underdetermined.)

b) We can show that for some rules and for some local situations, theory choice is underdetermined, regardless of the sorts of evidence available. Suppose our only rule of appraisal says, "Accept that theory with the largest set of confirming instances," and that we are confronted with two rival theories that have the same known confirming instances. Under these special circumstances, the choice is indeterminate.[49]

What is the significance of such limited forms of ampliative underdetermination as these? They represent interesting cases to be sure, but none of them—taken either singly or in combination—establishes the soundness of strong ampliative underdetermination as a general doctrine. Absent sound arguments for global egalitarian underdetermination (i.e., afflicting every theory on every body of evidence), the recent dismissals of scientific methodology turn out to be nothing more than hollow, anti-intellectual sloganeering.

I have thus far been concerned to show that the case for strong ampliative underdetermination has not been convincingly made out. But we can more directly challenge it by showing its falsity in specific concrete cases. To show that it is ill conceived (as opposed to merely unproved), we need to exhibit a methodological rule, or set of rules, a body of evidence, and a local theory choice context in which the rules and the evidence would *unambiguously* determine the theory preference. At the formal level it is of course child's play to produce a trivial rule that will unambiguously choose between a pair of theories. (Consider the rule: "Always prefer the later theory.") But, unlike the underdeterminationists,[50] I would prefer real examples, so as not to take refuge behind contrived cases.

The history of science presents us with a plethora of such cases. But I shall refer to only one example in detail, since that is all that is required to make the case. It involves the testing of the Newtonian celestial mechanics by measurements of the "bulging" of the earth.[51] The Newtonian theory predicted that the rotation of the earth on its axis would cause a radical protrusion along the equator and a constriction at the poles—such that the earth's actual shape would be that of an oblate spheroid, rather than (as natural philosophers from Aristotle through Descartes had maintained) that of a uniform sphere or a sphere elongated along the polar axis. By the early eighteenth century, there were well-established geodesic techniques for ascertaining the shape and size of the earth (to which all parties agreed). These techniques involved the collection of precise measurements of distance from selected portions of the earth's surface. (To put it oversimply, these techniques generally involved comparing measurements of chordal segments of the earth's polar and equatorial circumferences.[52]) Advocates of the two major cosmogonies of the day, the Cartesians and the Newtonians, looked to such measurements as providing decisive evidence for choosing between the systems of Descartes and Newton.[53] At great expense, the Paris Académie des Sciences organized a series of elaborate expeditions to Peru and Lapland to collect the appropriate data. The evidence was assembled by scientists generally sympathetic to the Cartesian/Cassini hypothesis. Nonetheless, it was *their* interpretation, as well as everyone else's, that the evidence indicated that the diameter of the earth at its equator was significantly larger than along its polar axis. This result, in turn, was regarded as decisive evidence showing the superiority of Newtonian over Cartesian celestial mechanics. The operative methodological rule in the situation seems to have been something like this:

> when two rival theories, T_1 and T_2, make conflicting predictions that can be tested in a manner that presupposes neither T_1 nor T_2, then one should accept whichever theory makes the correct prediction and reject its rival.

(I shall call this rule R_1.) We need not concern ourselves here with whether R_1 is methodologically sound. The only issue is whether it underdetermines a choice between these rival cosmogonies. It clearly does not. Everyone in the case in hand agreed that the measuring techniques were uncontroversial; everyone agreed that Descartes's cosmogony required an earth that did not bulge at the equator and that Newtonian cosmogony required an oblately spheroidal earth.

Had scientists been prepared to make Quine-like maneuvers, abandoning (say) *modus ponens*, they obviously could have held on to Cartesian physics "come what may." But that is beside the point, for if one suspends the rules of inference, then there are obviously no inferences to be made. What those who hold that underdetermination undermines methodology must show is that methodological rules, even when scrupulously adhered to, fail to sustain the drawing of any clear preferences. As this historical case makes clear, the rule cited and the relevant evidence required a choice in favor of Newtonian mechanics.

Let me not be misunderstood. I am not claiming that Newtonian mechanics was "proved" by the experiments of the Académie des Sciences, still less that Cartesian mechanics was "refuted" by those experiments. Nor would I suggest for a moment that the rule in question (R_1) excluded all possible rivals to Newtonian mechanics. What is being claimed, rather, is that this case involves a certain plausible rule of theory preference that, when applied to a specific body of evidence and a specific theory choice situation, yielded (in conjunction with familiar rules of deductive logic and of evidential assessment) *unambiguous* advice to the effect that one theory of the pair under consideration should be *rejected*. That complex of rules and evidence *determined* the choice between the two systems of mechanics, for anyone who accepted the rule(s) in question.

Underdetermination and the "Sociologizing of Epistemology"

If (as we saw in the first section) some scholars have been too quick in drawing ampliative morals from QUD, others have seen in such Duhem-Quine-style underdetermination a rationale for the claim that science is, at least in large measure, the result of social processes of "negotiation" and the pursuit of personal interest and prestige. Specifically, writers like Hesse and Bloor have argued that, because theories are deductively underdetermined (HUD), it is reasonable to expect that the adoption by scientists of various ampliative criteria of theory evaluation is the result of various social, "extra-scientific" forces acting on them. Such arguments are as misleading as they are commonplace.[34]

The most serious mistake they make is that of supposing that *any* of the normative forms of underdetermination (whether deductive or ampliative, weak or strong) entails anything whatever about what *causes* scientists to adopt the theories or the ampliative rules that they do. Consider, for instance, Hesse's treatment of underdetermination in her recent *Revolutions and Reconstructions in the Philosophy of Science*. She there argues that, since Quine has shown that theories are deductively underdetermined by the data, it follows that theory choice must be based, at least in part, on certain "non-logical," "extra-empirical" criteria for what counts as a good theory.[55] Quine himself would probably agree with that much. But Hesse then goes on to say that:

> it is only a short step from this philosophy of science to the suggestion that adoption of such [non-logical, extra-empirical] criteria, that can be seen to be different for different groups and at different periods, should be explicable by social rather than logical factors.

The thesis being propounded by these writers is that since the rules of deductive logic by themselves underdetermine theory choice, it is only natural to believe that the choice of ampliative criteria of theory evaluation (with which a scientist supplements the rules of deductive logic) are to be explained by "social rather than logical factors." It is not very clear from Hesse's discussion precisely what counts as a "social factor"; but she evidently seems to think—for her argument presupposes—that everything is either deductive logic or sociology. To the extent that a scientist's beliefs go beyond what is deductively justified, Hesse seems to insist, to that degree is it an artifact of the scientist's social environment. (Once again, we find ourselves running up against the belief—against which Duhem inveighs in the opening quotation—that formal logic exhausts the realm of the "rational.")

Hesse's contrast, of course, is doubly bogus. On the one side, it presupposes that there is nothing social about the laws of logic. But since those laws are formulated in a language made by humans and are themselves human artifacts fashioned to enable us to find our way around the world, one could hold that the laws of logic are at least in part the result of social factors. But if one holds, with Hesse, that the laws of formal logic are not the result of social factors, then what possible grounds can one have for holding that the practices that constitute ampliative logic or methodology are apt to be primarily sociological in character?

What Hesse wants to do, of course, is to use the fact of logical underdetermination (HUD) as an argument for taking a sociological approach to explaining the growth of scientific knowledge. There may or may not be good arguments for such an approach. But, as I have been at some pains to show in this essay, the underdetermination of theory choice by deductive logic is not among them.

There is another striking feature of her treatment of these issues. I refer to the fact that Hesse thinks that a semantic thesis about the relations between sets of propositions (and such is the character of the thesis of deductive underdetermination) might sustain *any* causal claim whatever about the factors that lead scientists to adopt the theoretical beliefs they do. Surely, whatever the causes of a scientist's acceptance of a particular (ampliative) criterion of theory evaluation may be (whether sociological or otherwise), the thesis of deductive underdetermination entails nothing whatever about the character of those causes. The Duhem-Quine thesis is, in all of its many versions, a thesis about the logical relations between certain statements; it is not about, nor does it directly entail anything about, the causal interconnections going on in the heads of scientists who believe those statements. Short of a proof that the causal linkages between propositional attitudes mirror the formal logical relations between propositions, theses about logical underdetermination and about causal underdetermination would appear to be wholly distinct from one another. Whether theories are deductively determined by the data, or radically underdetermined by that data; in neither case does *anything* follow concerning the contingent processes whereby scientists are caused to utilize extralogical criteria for theory evaluation.

The point is that normative matters of logic and methodology need to be sharply distinguished from empirical questions about the causes of scientific belief. None of the various forms of normative underdetermination that we have discussed in this essay entails anything whatever about the causal factors responsible for scientists adopting the beliefs that they do. Confusion of the idiom of good reasons and the idiom of causal production of beliefs can only make our task of understanding either of them more difficult.[56] And there is certainly no good reason to think (with Hesse and Bloor) that, because theories are deductively underdetermined, the adoption by scientists of ampliative criteria 'should be explicable by social rather than logical factors.' It may be true, of course, that a sociological account can be given for why scientists believe what they do; but the viability of that program has nothing to do with normative underdetermination. The slide from normative to causal underdetermination is every bit as egregious as the slide (discussed earlier) from deductive to ampliative underdetermination. The wonder is that some authors (e.g., Hesse) make the one mistake as readily as the other.

David Bloor, a follower of Hesse in these matters, produces an interesting variant on the argument from underdetermination. He correctly notes two facts about the history of science: sometimes a group of scientists changes its "system of belief," even though there is "no change whatsoever in their evidential basis."[57] "Conversely," says Bloor, "systems of belief can be and have been held stable in the face of rapidly changing and highly problematic inputs from experience."[58] Both claims are surely right; scientists do not necessarily require new evidence to change their theoretical commitments, nor does new evidence—even prima facie refuting evidence—always cause them to change their theories. But the conclusion that Bloor draws from these two commonplaces about belief change and belief maintenance in science comes as quite a surprise. For he thinks these facts show that *reasonable scientists are free to believe what they like, independently of the evidence.* Just as Quine had earlier asserted that scientists can hold any doctrine immune from refutation or, alternatively, they can abandon any deeply entrenched belief, so does Bloor hold that there is virtually no connection between beliefs and evidence. He writes: "So [sic] the stability of a system of belief [including science] is the prerogative of its users."[59] Here would seem to be underdetermination with a vengeance! But once the confident rhetoric is stripped away, this emerges—like the parallel Quinean holism on which it is modeled—as a clumsy non sequitur. The fact that scientists sometimes give up a theory in the absence of anomalies to it, or sometimes hold on to a theory in the face of prima facie anomalies for it, provides no license whatever for the claim that scientists can rationally hold on to any system of belief they like, just so long as they choose to do so.

Why do I say that Bloor's examples about scientific belief fail to sustain the general morals he draws from them? Quite simply because his argument confuses necessary with sufficient conditions. Let us accept without challenge the desiderata Bloor invokes: scientists sometimes change their mind in the absence of evidence that would seem to force them to, and scientists sometimes hang on to theories even when those theories are confronted by (what might appear to be) disquieting new evidence. What the first case shows, and all that it shows, is that the theoretical preferences of scientists are influenced by factors other than purely empirical ones. But that can scarcely come as a surprise to anyone. For instance, even the most ardent empiricists grant that considerations of simplicity, economy and coherence play a role in theory appraisal. Hence, a scientist who changes his mind in the absence of new evidence *may* simply be guided in his preferences by those of his standards that concern the nonempirical features of theory. Bloor's second case shows that new evidence is not necessarily sufficient to cause scientists to change their minds even when that evidence is prima facie damaging to their beliefs. Well, to a generation of philosophers of science raised to believe that theories proceed in a sea of anomalies, this is not exactly news either.

What is novel is Bloor's suggestion that one can derive from the conjunction of these home truths the thesis that scientists—quite independent of the evidence—can reasonably decide when to change their beliefs and when not to, irrespective of what they are coming to learn about the world. But note where the argument goes astray: it claims that because certain types of evidence are neither necessary nor sufficient to occasion changes of belief, it follows that no evidence can ever compel a rational scientist to change his beliefs. This is exactly akin to saying that, because surgery is not always necessary to cure gall stones, nor always sufficient to cure them, it follows that surgery is never the appropriate treatment of choice for gall stones. In the same way, Bloor argues that because beliefs sometimes change reasonably in the absence of new evidence and sometimes do not change in the face of new evidence, it follows that we are always rationally free to let our social interests shape our beliefs.

Conclusion

We can draw together the strands of this essay by stating a range of conclusions that seem to flow from the analysis:

- The fact that a theory is deductively underdetermined (relative to certain evidence) does *not* warrant the claim that it is ampliatively underdetermined (relative to the same evidence).
- Even if we can show in principle the nonuniqueness of a certain theory with respect to certain rules and evidence (i.e., even if theory choice is weakly underdetermined by those rules), it does not follow that that theory cannot be rationally judged to be better than its extant rivals (viz., that the choice is strongly underdetermined).
- The *normative* underdetermination of a theory (given certain rules and evidence) does not entail that a scientist's belief in that theory is causally underdetermined by the same rules and evidence, and vice versa.
- The fact that *certain* ampliative rules or standards (e.g., simplicity) may strongly underdetermine theory choice does not warrant the blanket (Quinean/Kuhnian) claim that all rules similarly underdetermine theory choice.

None of this involves a denial (a) that theory choice is always deductively underdetermined (HUD) or (b) that the nonuniqueness thesis may be correct. But one may grant all that and still conclude from the foregoing that no one has yet shown that established forms of underdetermination do anything to undermine scientific methodology as a venture, in either its normative or its descriptive aspect. The relativist critique of epistemology and methodology, insofar as it is based on arguments from underdetermination, has produced much heat but no light whatever.

Appendix

In the main body of the paper, I have (for ease of exposition) ignored the more *holistic* features of Quine's treatment of underdetermination. Thus, I have spoken about single theories (a) having confirming instances, (b) entailing observation statements, and (c) enjoying given degrees of evidential support. Most of Quine's self-styled advocates engage in similar simplifications. Quine himself, however, at least in most of his moods, denies that single theories exhibit (a), (b), or (c). It is, on his view, only *whole systems* of theories that link up to experience. So if this critique of Quine's treatment of underdetermination is to have the force required, I need to recast it so that a thoroughgoing holist can see its force.

The reformulation of my argument in holistic terms could proceed along the following lines. The nested or systemic version of the nonuniqueness thesis would insist that: *For any theory, T, embedded in a system, S, and any body of evidence, e, there will be at least one other system, S'* (containing a rival to T), *such that S' is as well supported by e as S is.* The stronger, nested egalitarian thesis would read: *For any theory, T, embedded in a system, S, and any body of evidence, e, there will be systems, $S_1, S_2, \ldots, S_n$, each containing a different rival to T, such that each is as well supported by e as S.*

Both these doctrines suffer from the defects already noted afflicting their nonholistic counterparts. Specifically, Quine has not shown that, for any arbitrarily selected rival theories, T_1 and T_2, there are respective nestings for them, S_1 and S_2, that will enjoy equivalent degrees of empirical support. Quine can, with some degree of plausibility, claim that it will be possible to find systemic embeddings for T_1 and T_2 such that S_1 and S_2 will be logically compatible with all the relevant evidence. And it is even remotely possible, I suppose, that he could show that there were nestings for T_1 and T_2 such that S_1 and S_2 respectively entailed all the relevant evidence. But as we have seen, such a claim is a far cry from establishing that S_1 and S_2 exhibit equal degrees of empirical support. Thus, Quine's epistemic egalitarianism is as suspect in its holistic versions as in its atomistic counterpart.

Notes

1. Pierre Duhem, *Aim and Structure of Physical Theory*, 217.
2. Lakatos once put the point this way:

> A brilliant school of scholars (backed by a rich society to finance a few well-planned tests) might succeed in pushing any fantastic programme [however "absurd"] ahead, or, alternatively, if so inclined, in overthrowing any arbitrarily chosen pillar of "established knowledge." (In I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), 187–88.)

3. See especially R. Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence," *Noûs*, 7 (1973): 1–12. W. Newton-Smith goes so far as to entertain (if later to reject) the hypothesis that "given that there can be cases of the underdetermination of theory by data, realism . . . has to be rejected." ("The Underdetermination of Theories by Data," in N. R. Hilpinen, ed., *Rationality in Science* [Dordrecht: Reidel, 1980], 105.) (Compare John Worrall, "Scientific Realism and Scientific Change," *The Philosophical Quarterly*, 32 [1982]: 210–31.)

4. See chap. 2 of Hesse's *Revolutions and Reconstructions in the Philosophy of Science* (Notre Dame: Notre Dame Press, 1980) and D. Bloor's *Knowledge and Social Imagery* (London: Routledge, 1976) and "The Strengths [sic] of the Strong Programme," *Philosophy of the Social Sciences*, 11 (1981): 199–214.

5. See H. Collins's essays in the special number of *Social Studies of Science*, 11 (1981). Among Collins's many fatuous *obiter dicta*, my favorites are these:

- "the natural world in no way constrains what is believed to be" (*ibid.*, 54); and
- "the natural world has a small or nonexistent role in the construction of scientific knowledge" (3).

Collins's capacity for hyperbole is equaled only by his tolerance for inconsistency, since (as I have shown in "Collins's Blend of Relativism and Empiricism," *Social Studies of Science*, 12 [1982], 131–33) he attempts to argue for these conclusions by the use of empirical evidence! Lest it be supposed that Collins's position is idiosyncratic, bear in mind that the self-styled "arch-rationalist," Imre Lakatos, could also write in a similar vein, apropos of underdetermination, that:

> The direction of science is determined primarily by human creative imagination and not by the universe of facts which surrounds us. Creative imagination is likely to find corroborating novel evidence even for the most "absurd" programme, if the search has sufficient drive (Lakatos, *Philosophical Papers* [Cambridge: Cambridge University Press, 1978], vol. 1, 99.)

6. For a discussion of many of the relevant literary texts, see A. Nehamas, "The Postulated Author," *Critical Inquiry*, 8 (1981): 133–49.

7. See, for instance, my "The Pseudo-Science of Science?," *Philosophy of the Social Sciences*, 11 (1981): 173–98; "More on Bloor," *Philosophy of the Social Sciences*, 12 (1982): 71–74; "Kuhn's Critique of Methodology," in J. Pitt, ed., *Change and Progress in Modern Science* (Dordrecht: Reidel, 1985), 283–300; "Explaining the Success of Science: Beyond Epistemic Realism and Relativism," G. Gutting *et al.*, eds., *Science and Reality: Recent Work in the Philosophy of Science* (Notre Dame: Notre Dame Press, 1984), 83–105; "Are All Theories Equally Good?" in R. Nola ed., *Relativism and Realism in Science* (Dordrecht: Reidel, 1988), 117–39; "Cognitive Relativism," in R. Egidi, ed., *La Svolte Relativistica* (Rome: Franco Angeli, 1988) 203–24; "Relativism, Naturalism and Reticulation," *Synthese*, 71 (1987), 114–39; "Methodology's Prospects," in A. Fine and P. Machamer, eds., *PSA 1986*, vol. 2, (East Lansing, Mich.: Philosophy of Science Association) [347–54]; and "For Method: Or Against Feyerabend," in [J. R. Brown and J. Mittelstrass, eds., *An Intimate Relation: Studies in the History and Philosophy of Science: Presented to Robert E. Butts on his Sixtieth Birthday* (Dordrecht, Netherlands: Kluwer, 1989), 299–317].

8. There are, of course, more interesting conceptions of "theory" than this minimal one; but I do not want to beg any questions by imposing a foreign conception of theory on those authors whose work I shall be discussing.

9. Quine has voiced a preference that the view I am attributing to him should be called "the holist thesis," rather than a "thesis of underdetermination." (See especially his "On Empirically Equivalent Systems of the World," *Erkenntnis*, 9

(1975): 313–28.) I am reluctant to accept his terminological recommendation here, both because Quine's holism is often (and rightly) seen as belonging to the family of underdetermination arguments, and because it has become customary to use the term underdetermination to refer to Quine's holist position. I shall be preserving the spirit of Quine's recommendation, however, by insisting that we distinguish between what I call "nonuniqueness" (which is very close to what Quine himself calls "underdetermination") and egalitarianism (which represents one version of Quinean holism). (For a definition of these terms, see below [in text].)

10. It is important to be clear that Quine's nonuniqueness thesis is *not* simply a restatement of HUD, despite certain surface similarities. HUD is entirely a *logico-semantic* thesis about deductive relationships; it says nothing whatever about issues of empirical support. The nonuniqueness thesis, by contrast, is an *epistemic* thesis.

11. Obviously, the egalitarian thesis entails the nonuniqueness thesis, but not conversely.

12. Quine specifically put it this way: "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system [of belief]." ("Two Dogmas of Empiricism," in S. Harding, ed., *Can Theories Be Refuted?* (Dordrecht: Reidel, 1976), 60 [296–97]. I am quoting from the version of Quine's paper in the Harding volume since I will be citing a number of other works included there.)

13. Grünbaum, in his *Philosophical Problems of Space and Time*, 2d ed., (Dordrecht: Reidel 1974), 590–610, has pointed to a number of much more sophisticated, but equally trivial, ways of reconciling an apparently refuted theory with recalcitrant evidence.

14. Quine in Harding (see note 12 above), 60 [297]. In a much later, backtracking essay ("On Empirically Equivalent Systems of the World," *Erkenntnis*, 9 (1975): 313–28), Quine seeks to distance himself from the proposal, implied in "Two Dogmas . . . ," that it is always (rationally) possible to reject 'observation reports'. Specifically, he says that QUD "would be wrong if understood as imposing an equal status on all the statements in a scientific theory and thus denying the strong presumption in favor of the observation statements. It is this [latter] bias which makes science empirical" (*ibid.*, p. 314).

15. In fact, of course, Quine thinks that we generally do (should?) not use such stratagems. But his only argument for avoiding such tricks, at least in "Two Dogmas of Empiricism," is that they make our theories more complex and our belief systems less efficient. On Quine's view, neither of those considerations carries any epistemic freight.

16. Quine, in Harding (see note 12 above), 63. I am not alone in finding Quine's notion of pragmatic rationality to be epistemically sterile. Lakatos, for instance, remarks of Quine's "pragmatic rationality": "I find it irrational to call this 'rational' " (Lakatos, *Philosophical Papers* [Cambridge: Cambridge University Press, 1978], vol. 1, 97n).

17. W. Quine, *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969), 79.

18. See especially A. Grünbaum, *Philosophical Problems of Space and Time* (Dordrecht: Reidel 1974), 585–92 and Larry Laudan, "Grünbaum on 'the Duhemian Argument'," in S. Harding, *Can Theories Be Refuted?* (Dordrecht: Reidel, 1975).

19. In a letter to Grünbaum, published in Harding (see note 12 above), p. 132, Quine granted that "the Duhem-Quine thesis" (a key part of Quine's holism and thus of QUD) "is untenable if taken nontrivially." Quine even goes so far as to say that the thesis is not "an interesting thesis as such." He claims that all he used it for was to motivate his claim that meaning comes in large units, rather than sentence-by-sentence. But just to the extent that Quine's QUD is untenable on any nontrivial reading, then so is his epistemic claim that any theory can rationally be held true come what may. Interestingly, as late as 1975, and despite his concession that the D-Q thesis is untenable in its nontrivial version, Quine was still defending his holistic account of theory testing (see below in text).

20. And if they did not, the web would itself be highly suspect on other epistemic grounds.

21. Or, more strictly, that there is a network of statements that includes the flat-earth hypothesis and that is as well confirmed as any network of statements including the oblate-spheroid hypothesis.

22. Since I have already discussed Quine's views on these matters, and will treat Kuhn's in the next section, I will limit my illustration here to a brief treatment of Hesse's extrapolations from the underdetermination thesis. The example comes from Mary Hesse's recent discussion of underdetermination in her *Revolutions and Reconstructions in the Philosophy of Science*. She writes:

> Quine points out that scientific theories are never logically determined by data, and that there are consequently [sic] always in principle alternative theories that fit the data more or less adequately. (See note 4 above, 32–33)

Hesse appears to be arguing that, because theories are deductively underdetermined, it follows that numerous theories will always fit the data "more or less adequately." But this conclusion follows not at all from Quine's arguments, since the notion of "adequacy of fit" between a theory and the data is an epistemic and methodological notion, not a logical or syntactic one. I take it that the claim that a theory fits a given body of data "more or less *adequately*" is meant to be, among other things, an indication that the data lend a certain degree of support to the theory that they "fit." As we have already seen, there may be numerous rival theories that fit the data (say in the sense of entailing them); yet that implies nothing about equivalent degrees of support enjoyed by those rival theories. It would do so only if we subscribed to some theory of evidential support that held that "fitting the data" was merely a matter of entailing it, or approximately entailing it (assuming counterfactually that this latter expression is coherent). Indeed, it is generally true that *no* available theories exactly entail the available data; so sophisticated inductive-statistical theories must be brought to bear to determine which fits the data best. We have seen that Quine's discussion of underdetermination leaves altogether open the question whether there are always multiple theories that "fit the data" equally well, when that phrase is acknowledged as having extra-syntactic import. If one is to establish that numerous alternative theories "fit the data more or less adequately," then one must give arguments for such ampliative underdetermination that goes well beyond HUD and any plausible version of QUD.

23. I remind the reader again that neither Quine nor anyone else has successfully established the cogency of the entailment version of QUD, let alone the explanatory or empirical support versions thereof.

24. If it did, then we should have to say that patently nonempirical hypotheses like "The Absolute is pure becoming" had substantial evidence in their favor.

25. In his initial formulation of the qualitative theory of confirmation, Hempel toyed with the idea of running together the entailment relation and the evidential relation; but he went on firmly to reject it, not least for the numerous paradoxes it exhibits.

26. Consider, for sake of simplicity, the case where two theories each entail a true evidence statement, e. The posterior probability of each theory is a function of the ratio of the prior probability of the theory to the prior probability of e. Hence if the two theories began with different priors, they must end up with different posterior probabilities, *even though supported by precisely the same evidence.*

27. It is generally curious that Quine, who has had such a decisive impact on contemporary epistemology, scarcely ever—in "Two Dogmas . . ." or elsewhere—discussed the rules of ampliative inference. So far as I can see, Quine generally believed that ampliative inference consisted wholly of hypothetico-deduction and a simplicity postulate!

28. As we shall eventually see, the kind of underdetermination advocated in *Word and Object* has no bearing whatever on (2*) or QUD.

29. W. V. Quine, *Word and Object* (Cambridge, Mass.: M.I.T. Press, 1960), 22, my italics.

30. *Ibid.*, p. 22, my italics. There is, of course, this difference between these two passages: The first says that commonsense talk of objects may conceivably underdetermine theory preferences, whereas the second passage is arguing for the probability that sensations underdetermine theory choice. In neither case does Quine give us an argument.

31. *Ibid.*, 23, my italics.

32. Except a vague version of the principle of simplicity.

33. *Ibid.*, 21.

34. *Ibid.*, 22–23.

35. In some of Quine's more recent writings (see especially his "On Empirically Equivalent Systems of the World," *Erkenntnis*, 9 (1975): 313–28), he has tended to soften the force of underdetermination in a variety of ways. As he now puts it, "The more closely we examine the thesis [of underdetermination], the less we seem to be able to claim for it as a 'theoretical thesis' " (*ibid.*, 326).

He does, however, still want to insist that "it retains significance in terms of what is practically feasible" (*ibid.*). Roughly speaking, Quine's distinction between theoretical and practical underdetermination corresponds to the situations we would be in if we had all the available evidence (theoretical underdetermination) and if we had only the sort of evidence we now possess (practical underdetermination). If the considerations that I have offered earlier are right, the thesis of practical Quinean underdetermination is as precarious as the thesis of theoretical underdetermination.

36. Quine does not repudiate the egalitarian thesis in *Word and Object*; it simply does not figure here.

37. In some of Quine's later gyrations (esp. his "On Empirically Equivalent Systems of the World") he appears to waver about the soundness of the nonuniqueness thesis, saying that he does not know whether it is true. However, he still holds on there to the egalitarian thesis, maintaining that it is "plausible" and "less beset with obscurities" than HUD (*ibid.*, 313). He even seems to think that nonuniqueness depends argumentatively on the egalitarian thesis, or at least, as he puts it, that the "holism thesis [egalitarianism] lends credence to the underdetermination theses [nonuniqueness]." (*ibid.*) This is rather like saying that the hypothesis that there are fairies at the bottom of my garden lends credence to the hypothesis that something is eating my carrots.

38. E.g., the difference between Quine's (0) and (1).

39. Quine's repeated failures to turn any of his assertions about normative underdetermination into plausible arguments may explain why, since the mid-1970s, he has been distancing himself from virtually all the strong readings of his early writings on this topic. Thus, in his 1975 paper on the topic, he offers what he calls "my latest tempered version" of the thesis of underdetermination. It amounts to a variant of nonuniqueness thesis. ("The thesis of underdetermination . . . asserts that our system of the world is bound to have empirically equivalent alternatives . . ." *ibid.*, 327.) Significantly, Quine is now not even sure whether he believes this thesis: "This, for me, is [now] an open question" (*ibid.*).

40. What follows is a condensation of a much longer argument, which can be found, with appropriate documentation, in my "Kuhn's Critique of Methodology" (see note 7 above).

41. Apropos the resistance to the introduction of a new paradigm, Kuhn claims that the historian "will not find a point at which resistance becomes illogical or unscientific" (*The Structure of Scientific Revolutions*, Chicago: University of Chicago Press, 1962, 159).

42. Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1970), 325 [106]. My italics.

43. *Ibid.* [106] My italics.

44. *Ibid.*, 329 [109]. My italics.

45. In *Structure of Scientific Revolutions*, Kuhn had maintained that the refusal to accept a theory or paradigm "is not a violation of scientific standards" (159).

46. Kuhn, *The Essential Tension*, 322 [103].

47. *Ibid.*, 322 [103].

48. See, for instance, Derek Price, "Contra-Copernicus," in M. Clagett, ed., *Critical Problems in the History of Science* (Madison, 1959), 197–218.

49. A similar remark can be made about several of Popper's rules about theory choice. Thus, Miller and Tichý have shown that Popper's rule "accept the theory

with greater verisimilitude" underdetermines choice between incomplete theories; and Grünbaum has shown that Popper's rule "prefer the theory with a higher degree of falsifiability" underdetermines choice between mutually incompatible theories. [David Miller, "Popper's Qualitative Theory of Verisimilitude," *British Journal for the Philosophy of Science* 25 (1974): 166–77; Pavel Tichý, "On Popper's Definitions of Verisimilitude," *British Journal for the Philosophy of Science* 25 (1974): 155–60; Adolf Grünbaum, "Is the Method of Bold Conjectures and Attempted Refutations *Justifiably* the Method of Science?" *British Journal for the Philosophy of Science* 27 (1976): 105–36.]

50. Recall Quine's claim that we can hang on to any statement we like by changing the meaning of its terms.

51. See, for instance, I. Todhunter, *History of the Theories of Attraction and the Figure of the Earth* (New York: Dover, 1962).

52. Typically, astronomical measurements of angles subtended at meridian by stipulated stars were used to determine geodetic distances.

53. In fact, the actual choice during the 1730s, when these measurements were carried out, was between a Cassini-emended version of Cartesian cosmogony (which predicted an *oblong* form for the earth) and Newtonian cosmology (which required an *oblate* shape).

54. Indeed, most of so-called radical sociology of knowledge rests on just such confusions about what does and does not follow from underdetermination.

55. M. Hesse (see note 4 above), 33.

56. This is not to say, of course, that there are no contexts in which it is reasonable to speak of reasons as causes of beliefs and actions. But it is to stress that logical relations among statements cannot unproblematically be read off as causal linkages between propositional attitudes.

57. Bloor, "Reply to Buchdahl," *Studies in History and Philosophy of Science*, 13 (1982): 306.

58. *Ibid.*

59. *Ibid.* In his milder moments, Bloor attempts to play down the radicalness of his position by suggesting (in my language) that it is the nonuniqueness version of underdetermination rather than the egalitarian version that he is committed to. Thus, he says at one point that "I am not saying that any alleged law would work in any circumstances" ("Durkheim and Mauss Revisited," *Studies in History and Philosophy of Science*, 13 [1982]: 273). But if indeed Bloor believes that the stability of a system of belief *is* the prerogative of its users, then it seems he must hold that any "alleged law" could be made to work in any conceivable circumstances; otherwise, there would be some systems of belief that it was not at the prerogative of the holder to decide whether to hang on to.