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DISCUSSION:

AN ALTERNATIVE TO THE TRADITIONAL MODEL? LAUDAN ON DISAGREEMENT AND CONSENSUS IN SCIENCE*

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Larry Laudan's primary aim in his latest book, *Science and Values,* is to account for the high degree of agreement and the ubiquity of disagreement in science. Arguing that earlier philosophers have either concentrated on agreement and ignored disagreement or highlighted disagreement at the expense of agreement, he sets out to provide "a single, unified theory of rationality which promises to be able to explain both these striking features of science" (p. 3).[1] However, while recognizing that Laudan has done much to clarify this issue and to bring traditional thinking about science into line with scientific practice, I shall argue that the basic problem remains to be solved. To do justice to agreement and disagreement, we must pursue a more radical course than the one Laudan charts.

Problems with the Traditional Theory. As Laudan remarks, philosophers have tended to think of scientific agreement in terms of the theory of instrumental rationality (p. 23). On this theory, which Laudan refers to as "the hierarchical model of justification," lower-level disagreements are resolved on the basis of higher-level agreements. Disagreements at the "factual level" are held to be resolvable at the level of shared methodological rules, disagreements about methodology to be eliminable at the level of shared aims and goals, and axiological disagreements to be either "nonexistent" (because "scientists are presumed to share the same goals") or "irresolvable" (because "goals are at the top of the justificatory lad-

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[1]Here I summarize Laudan's own view of how his ideas relate to earlier work (see chapter 1). It is an interesting question whether "the central problem" of philosophy and sociology of science in the 1940s and 1950s was to explain agreement. And it is also debatable whether "the central intellectual puzzle" in the 1960s and 1970s was to explain disagreement. (See especially p. 2.)

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der") (pp. 26 and 42). In Laudan's view, this familiar model has much to recommend it but it also falls short in important respects.

Laudan's first major criticism of the model is that it overestimates the degree to which higher-level considerations determine lower-level choices. (See chapter 2.) He does not deny that scientists may be able to determine that one theory (or method) is better than a second by moving to the level of methods (or goals)—indeed he insists that factual and methodological consensus is largely achieved in this way. However, he rejects the contention that scientists can always decide which theory (or method) should be adopted simply by moving up a level in the hierarchy. It is, he argues, as much a mistake to suppose that methodological rules always "mandate a preference" (p. 28) as it is to think that there is "only one set of rules for realizing any specific set of cognitive aims" (p. 35).

Secondly, Laudan objects to the way in which proponents of the hierarchical model treat disagreements about goals. (See chapter 3.) In his view, scientists often disagree about the goals of the enterprise, yet are frequently able to resolve their differences in a rational manner. We should not, he tells us, simply assume that "a rational choice between alternative sets of internally consistent sets of cognitive goals is always impossible" (p. 50); the adoption of a goal need not be a "subjective and emotive matter" but may instead be "rationally negotiated" (p. 47). More specifically, Laudan argues that a goal may be criticized on the grounds that it is "utopian or unrealizable" or because it fails to accord with "the values implicit in the communal practices and judgments we endorse" (p. 50).[2]

According to Laudan, then, proponents of the traditional view both exaggerate the scope of agreement in science and misjudge the frequency with which scientists are able to settle their differences. On the one hand, he argues that since methods underdetermine the choice of theories and aims underdetermine the choice of methods, factual disagreement can arise even when scientists agree at the methodological level and methodological disagreement can occur even when they share the same goals. Admitting that such situations are "nonstandard or highly unusual" (p. 42), he quite rightly points out that they should not be ruled out from the start. On the other hand, Laudan insists that rival aims can often be rationally adjudicated so that while disagreement at the axiological level is always possible, subsequent consensus at this level is also possible and even likely.

[2]Without doubt, goals should be realizable, but the issue is perhaps more comlex than Laudan allows. In particular, can one show that an aim is "not realizable" simply by showing that there is "no known method" for achieving it (p. 62)? Moreover, notice that the proponent of the hierarchical model might argue in response to Laudan that the twin requirements of realizability and coherence with actual scientific practice on which he lays so much stress function as "metagoals" that may be used to assess lower-level goals.

We must not, he argues, overlook the fact that scientists possess a "wide range of critical tools" for making "rational assessments" (p. 50).

Disagreement in Practice. But how plausible is Laudan's view as a theory of how scientific disagreement is resolved? Here what counts is not so much the differences between Laudan and his opponents as the points on which they agree. Do scientists attempt to resolve their disagreements by moving up the "hierarchy of justification" and re-evaluating their initial views in the light of their "shared agreements"? And when they are unable to forge agreement on the basis of the information they share, do they typically "agree to disagree" pending the acquisition of information that tips the balance one way or the other? The view that they do is certainly plausible in theory, but it is much less clear as an account of how scientists actually proceed.

In the first place, scientists rarely move to a higher level to resolve their differences. They do not as a rule turn from an examination of factual issues to an examination of methodological ones, still less from methodological issues to axiological ones. Quite the contrary: they usually defend their views against attack and isolate the shortcomings of their opponents' views on the basis of what they themselves take to be independently tenable. Of course, when scientists disagree about "lower-level issues," they do sometimes make reference to higher-level considerations. But even then they do not normally first attempt to get clear on the higher-level issues with an eye to applying them in a neutral way to the lower-level ones. In general, higher-level considerations are taken into account only insofar as they apply to particular situations.

Nor do scientists often attempt to resolve their differences by engineering a show-down between their views. "Crucial experiments" between methodological principles (or axiologies) are at least as uncommon as crucial experiments between theories. In the normal course of events, scientists do not resolve their disagreements at one fell swoop; they rather eliminate them by gradually reducing the distance between themselves in response to criticism and new information. Usually, they engage in a complex process of argument, debate, negotiation and compromise in which they root out the weak points of their opponents' views and attempt to incorporate the strengths of these views into their own position. As William Whewell puts the point, disagreements are mostly resolved by scientists "transforming" their hypotheses until these "pass into one another" (1851, p. 139).

Significantly, the view that disagreements are like informal debates can also be discerned in many of Laudan's own descriptions of actual scientific research. He too portrays scientists as disagreeing because they embark on their deliberations with different background beliefs and as

resolving their disagreements by arguing back and forth. Thus, in an extended discussion of the dispute between Newtonians and Cartesians, he stresses their differing theories, methods, and goals, and he speaks of Cartesianism as having "largely run out of steam" by the 1740s as a result of the Cartesians' "growing recognition" of the difficulties attending their views (pp. 60–61). Indeed, Laudan even refers to Whewell's account of how the Cartesians gradually capitulated to the Newtonians as being "remarkably insightful" (p. 80).

Constraint and Justification. At this juncture, it might be argued on Laudan's behalf that he actually rejects the hierarchical model in favor of the view that theories, methods and goals function at the "same level." After all, he takes himself to have "closed the evaluative circle"; he explicitly states that "there is a complex process of mutual adjustment and justification going on among all three levels of scientific commitment"; and he even insists that "the pecking order implicit in the hierarchical approach must give way to a kind of leveling principle that emphasizes the patterns of mutual dependence between [the various] levels" (pp. 62–63). In light of these remarks, Laudan would seem to be interpreted more plausibly as advocating the "Whewellian view" that disagreements are resolved by a process of balancing factual, methodological and axiological claims against one another.

However, the matter is not so straightforward as it may seem. Laudan does hold that "justification flows upwards as well as downwards in the hierarchy" (p. 62), but he also thinks of upward-flowing and downward-flowing justification in quite different ways. When he speaks of justification flowing upwards, he is reacting to the view that scientists begin by selecting an aim, then methods that realize it, then theories that satisfy the methods. His main contention is that scientists select methods that are factually sound and aims that are realizable given what they know about the world and how it is best investigated. But this point is compatible with the claim of proponents of the hierarchical model that scientists justify their choice of theories with reference to methods and justify their aims with reference to goals. It does not establish that justification flows upwards but rather (as Laudan himself puts it) that what is below may "constrain" and "shape" what is above (see especially p. 63).

Laudan is right to criticize advocates of the hierarchical model who hold that the selection of goals is prior to and entirely independent of all factual and methodological considerations. But this criticism by itself hardly undermines the traditional approach. For one thing, proponents of the hierarchical model do not deny that theories are constrained by the data, and it is but a small step to the conclusion that methods can be similarly constrained by theories and goals by methods. Indeed, it hardly seems

reasonable to suppose that there are constraints on theories but none on methods or goals. What makes them so special? From this point of view, Laudan's "reticulated model" is perhaps better described as a sophisticated version of the hierarchical model than as an alternative to it.

In the present context, it is important to bear in mind that constraint is not justification. When Laudan says that scientists are constrained in their selection of methods and goals by what they take to have been established, he is taking the lower-level considerations to have been previously justified. But when he speaks of methods and goals as providing justification, it is these latter that he is assuming to have been already established; he is no longer thinking of them as candidates for acceptance. In other words, Laudan agrees with proponents of the hierarchical model that scientists rely on accepted methods to justify their choice of theories and on accepted goals to justify their choice of methods. He is only at odds with proponents of the model who fail to see that higher-level considerations are constrained by lower-level ones in much the same way as theories are constrained by data.

The Epistemological Background. Nonetheless, a puzzle remains. Laudan explicitly states that no level is "privileged or primary or more fundamental than the others" (p. 63); he discusses consensus formation in a commonsense way as a matter of deliberation and debate (see, e.g., pp. 60–61); and he claims to provide an alternative to the hierarchical model of justification, not merely a new version of it. In view of all this, one might reasonably wonder why he casts so much of his discussion within the general framework of the traditional model. In particular, why does he continue to speak of levels of disagreement and justification? And why does he play down the idea of disagreements as informal debates?

The answer to this is not hard to find. Having adopted the goal of developing a general theory of scientific rationality, Laudan cannot rest content with a few casual references to scientists debating with one another. Given his aims, he cannot avoid assuming that scientific theories, methods and goals exhibit some general order, nor can he avoid regarding substantive differences of opinion as being resolvable on the basis of shared agreements or else merely temporary (pending further investigation). He must, as he himself puts it, provide "critical tools" (p. 50) or "analytic machinery" (p. 138) for assessing arguments and resolving disputes. It is thus unsurprising that he introduces a view of science that falls squarely within the framework of traditional epistemological theory.

My point here is that one cannot both cling to the demand for a "theory of rationality" and insist that disagreements are properly resolved by a complex process of mutual adjustment and mutual justification. If it is true that no level can be regarded as "primary or more fundamental than

any of the others" and that "axiology, methodology, and factual claims are inevitably intertwined in relations of mutual dependence" (p. 63), the possibility of general principles of rationality becomes moot, to say nothing of the possibility of a "model of methodological and axiological critique" (p. 138). To talk of a network of justification or a web of belief which is modified in various ways depending on particular circumstances is not to provide an alternative theory of rationality but to provide an alternative to any such theory. There is no middle ground to be carved out here.

In retrospect, then, Laudan's discussion can be seen as oscillating between two poles. When he turns to a consideration of historical cases, he sets aside the project of developing a theory of rationality and quite reasonably makes use of the commonsense categories of deliberation and debate. But when his epistemological concerns are to the fore, the familiar picture of disagreements as debates falls by the way and some version or other of the traditional model reappears on the scene. The problem, as I see it, is that the variety and complexity of scientific practice makes the prospects for a critical standpoint with respect to which scientific disagreements can be "rationally adjudicated" exceedingly dim. To obtain a clear view of agreement and disagreement in science, we must step outside the framework of traditional epistemology and acknowledge that scientific investigation neither abides by nor needs a general philosophical theory of rationality.

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