BOOK REVIEWS


Starting from a three-level model of disagreement and consensus formation in science, Laudan criticizes in a variety of sensible ways Kuhn, Feyerabend, and others, who have stressed theory incommensurability and underdetermination. The simple hierarchical model fixes disagreement and subsequent consensus at one of three levels: the factual, the methodological, and the axiological. The author is particularly concerned to argue that disagreements over the aims or cognitive values of science can be rationally argued about and adjudicated. He emphasizes throughout the book that the interaction among the three levels is not a simple unidirectional one, but is a continual, complex process of mutual adjustment and mutual justification. His main focus of criticism in the first four chapters is Thomas Kuhn, and in the final and fifth chapter on scientific realism, Hilary Putnam.

In chapter 1 he challenges the view that current accounts of the development of science are the intellectual resources to explain both the existence of consensus in science and the nature of disagreement, when it exists. In the second chapter he proposes a hierarchical structure of scientific debates in terms of the three-level model already mentioned. Chapter 3 really comes to the core of the book in its focus on resolving disagreements about cognitive values. The chapter is devoted chiefly to a discussion of consensus formation mechanisms at the level of cognitive values. In spite of his concentration on cognitive values, the author does not really set forth anywhere his systematic view of what such values are. He does give one-liner examples in a number of places (for instance, on page 35 he refers to "such familiar cognitive goals as truth, coherence, simplicity, and predictive fertility . . ."). His most important view of cognitive values is expressed in chapter 5 (p. 106): "At its core, realism is a normative doctrine about what the aims or values of science ought to be. Specifically, the realist maintains that the goal of science is to find ever truer theories about the natural world." To avoid any misunderstanding, Laudan does not hold the absolute view that realism is only a normative doctrine. He also gives proper place to the descriptive component of realism.

In chapter 4 he dissects the holistic picture of scientific change, with a number of excellent criticisms of Kuhn and those of similar views. He examines with particular care Kuhn's several arguments for the underdetermination of theory. In chapter 5, in many ways the most systematic in the book, Laudan undertakes a detailed critique of scientific realism as formulated in the writings of Hilary Putnam, Wilfrid Sellars, Richard Boyd, William Newton-Smith, and some other contemporary philosophers. In the spirit of what he is mainly trying to do in this book, the discussion of realism is from an axiological standpoint; that is, Laudan undertakes a detailed critique of the view, already quoted, that scientific realism principally expresses a goal of science. In the process of his analysis, however, he also has a good deal to say about the descriptive aspects of realism.

As the tone of my remarks should suggest already, I am sympathetic with much that Laudan has to say. In this respect, Laudan reminds me of Dewey, whom Laudan also cites in several places as a sympathetic predecessor. Laudan reminds me of Dewey because of the abstractness and generality of much of the argument. Both are preachers of some considerable skill; and, others of like persuasion, can listen to their sermons with approval and satisfaction, but Laudan’s prose is livelier and more engaging. A striking feature of Dewey’s philosophical writings is the absence of almost any specific facts or details about any subject at all. Above all, in his many writings on the scientific method there are no substantive nontrivial examples. Only in his early writings on psychology can one find any definite scientific descriptions or judgments. Laudan is different, but he has, all the same, a similar failing. His book is full of brief references to many specific examples in the history of science. For instance (p. 56), “Franklin’s fluid theory of electricity, Boerhaave’s vibratory theory of heat, the Buffonian theory of organic molecules, and phlogiston
chemistry constitute a typical sample of the growing set of Enlightenment theories that hypothesized unobservable entities in order to explain observable processes." Lists of a similar sort, all interesting, are found throughout this short book. I certainly prefer Laudan's lists to Dewey's vague abstractions, but neither will really wash as serious history or philosophy of science.

Turning from Sunday's sermon to Monday's work, let me now try to say what I find unsatisfactory in Laudan's approach to cognitive values in science. As I have already indicated, it is not his general viewpoint that bothers me. It is his method of approach. Laudan emphasizes at several points that "the appraisal of proposed cognitive methods and aims requires extensive empirical research" (p. 39, note 14). In many respects, the critique of scientific realism in the last chapter of the book is meant to serve as a model of how Laudan thinks such appraisals should be conducted. But his analysis in this chapter is very much in the spirit of traditional philosophical analysis. Opponents' claims are summarized and then each of the claims is criticized. I emphasize that much of what Laudan has to say in this chapter is sound and at the right level for constraining unsupported general claims put forth by various philosophers. But these discussions represent in one sense a backsliding in precision and clarity of philosophical analysis. Laudan mentions at the end of the chapter that the arguments against scientific realism are certainly as good as those that have been presented in the past against positivism, classical empiricism, and other philosophical isms. What he does not emphasize, and I make an explicit point of it here, is that the writings of Carnap, Reichenbach, and Nagel possess a level of clarity and detail not characteristic of much of the writing on realism. In this respect, Carnap is perhaps the best example. A large number of his views have been shown to be explicitly wrong but they have the great virtue of being formulated with the kind of precision that made refutation possible. In criticizing contemporary philosophers of scientific realism, Laudan uses a logical apparatus that is extremely elementary and informal. This is not necessarily a bad thing, but I do want to contrast, for reasons that will come out in a moment, this level of discussion with the kind of detailed analyses of various views of Carnap's that have been given over the last several decades.

Now, it certainly can be said that Laudan would not be expected to give logically sophisticated arguments when operating in an arena that has not used such methods. A specific contrast is the very detailed critical discussion by many philosophers of Carnap's views on inductive logic. But surprisingly, richer methods that would be available for various points in Laudan's discussion are not even mentioned. I have in mind the powerful probabilistic and statistical methods that are now so widespread in science itself and also in discussions of scientific methodology. There are a number of points in Laudan's polemics where some reference to Bayesian views, pro or con, seems needed. It seems surprising and almost quaint to have the rules of inductive inference associated with Bacon, Hume, and Newton mentioned (p. 81) with no indication whatsoever of the much more powerful methods that originated with Bayes and especially Laplace. However these are caveats and not, I think, very serious ones.

I now come to what I think is essentially wrong with Laudan's approach. He wants to convert from being a good preacher to being a good empirical scientist but he seems confused about what direction to follow. His many short lists of specific scientific discoveries, results, etc., suggest that he would like to move in the direction of the kind of quantitative methodology Derek Price undertook for different purposes in his 1961 book, Science Since Babylon, and in a number of articles. Price was concerned to study a number of phenomena of growth in science: the number of journals, the number of physics abstracts since 1900, the growth of the number of papers in a given field of science, and the growth of the number of scientists. He was, moreover, concerned to see to what extent simple mathematical functions would fit the data. Laudan's proposal in earlier publications that progress might be measured by counting problems solved indicates that he would like to move in this direction too. His many lists in the present book reinforce this idea, but a finer reading of what he has to say suggests that he does not really have the taste for the detailed and tedious analysis of data required to pursue with thoroughness any quantitative program of analysis.

There are other directions, however, he can follow, in the footsteps of other historians
of science, and present in magnificent detail individual case studies of the role of cognitive values in scientific research. I have in mind, as an example of such detail, although dealing with other historical problems, the excellent volumes in the Springer studies in the history of mathematics and physical sciences, especially the recent book by Swerdlow and Neugebauer on mathematical astronomy in Copernicus' *De Revolutionibus*. The specific level of analysis offered by Laudan is about as far from the level in the Swerdlow and Neugebauer volume as could be imagined.

Like Dewey, Laudan is a great advocate of empirical methods, but his own methodology is still primitive and undeveloped in character. What he has written in the present book is a polemic against other views on the nature of science and the place of cognitive values in scientific research. Although Laudan's polemic is often on the side of the angels—mine, anyway—I hope he will now turn to the detailed and demanding empirical work he properly thinks is required to appraise the cognitive values considered important in any particular area of science. *Patrick Suppes, Stanford University.*