REVIEW

CHANGING FORTUNES OF THE METHOD OF HYPOTHESIS

A review of Larry Laudan, *Science and Hypothesis* (Western Ontario Series), D. Reidel, Dordrecht, 1981, pp. x + 258, pbk $14.95, hbk $34.95.

Philosophers, if they think about the history of scientific methodology at all, usually think of it in terms of a gradual rise in the fortunes of hypotheses. Scientists are seen as having become more favourably disposed towards speculation as they realized the impossibility of eliciting hypotheses from sensory experience. The history of methodology, in other words, is largely construed as a matter of the replacement of inductivism by the hypothetico-deductive method. Moreover, this shift in perspective is widely regarded as having come about because of developments in philosophy. According to the standard view, the optimism of earlier methodological conceptions was tempered as the limitations on human inquiry became more fully understood.

In *Science and Hypothesis*, however, Larry Laudan contends that this picture of the development of modern methodology is almost entirely wrong. He argues that the fortunes of the method of hypothesis have varied more than is usually thought. The method did not simply arise when the shortcomings of inductivism were finally realized. It rather blossomed in the seventeenth century, went into decline in the following century and re-emerged as a well-entrenched feature of scientific inquiry only in the nineteenth century. Furthermore, Laudan insists that the shifts in how the method was regarded were mostly prompted by changes in science itself. The "purist model of scientific methodology", which takes the theory of scientific method to have been developed by "great philosophers in response to developments in metaphysics and epistemology", is in Laudan's view historically inadequate and even "pernicious" (pp. 6-8).[1]

Briefly, Laudan's argument is that scientists changed their attitude towards the method of hypothesis as they changed their views concerning the cogency of explanations in terms of micro-entities. The method fared

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well when deep-structure theories fared well and poorly when they did not. It waxed in the seventeenth century when corpuscularianism flourished; it waned in the first half of the eighteenth century with the spread of Newtonian ideas; and it revived in the second half of the century when thinkers such as Hartley and Le Sage once again began to defend explanations in terms of unobservables.

This argument entails major revisions in the traditional account of the development of scientific methodology. Galileo's importance is diminished since he contributes little to the problem of the status of hypotheses about micro-entities. Hume's discussion of the confirmation of empirical generalizations no longer appears to be a great step forward, the main effect of his argument having been to deflect attention from the historically more important issue of the confirmation of deep-structure theories. Newton's influence on British philosophy, moreover, must now be put down to Reid's advocacy of inductivism rather than to the influence of the writings of Locke, Berkeley and Hume. And the abandonment of the idea of a logic of discovery, the rise of attempts to base inductive logic on probability theory, and the general acceptance of the thesis that science is a self-correcting endeavour has to be regarded as having been prompted by the re-emergence of the method of hypothesis and the subsequent rejection of infallibilism.

Furthermore, Laudan argues that important figures in the history of methodology who might be thought to cause trouble for his general interpretative scheme actually fit well within it. Thus, Descartes and Locke were in fact more friendly than they were hostile to hypotheses and even Comte and Mach gave hypotheses a reasonable hearing. For, in Laudan's view, Descartes's method was only partially an a priori method, Locke's main point was that hypotheses should be thought of as "probable judgements", Comte had no quarrel with hypotheses about theoretical entities thought of as "logical artifices", and Mach agreed that hypotheses can play a useful, and possibly crucial, role in the discovery of new empirical correlations. On the other hand, Laudan points out that the empirical aspects of Whewell's methodology have been too frequently underestimated, the operation of forging a "consilience of inductions" being one of the most important features of both Whewell's philosophy and his historiography of science.

Science and Hypothesis is thus a provocative book. A radical theme is

vigorously pursued, and the difficult task of marrying history and philosophy of science is confronted head on. Certainly, no one who reads the book carefully will remain unimpressed by the wealth of detail it contains, the broad explanations it offers, or the flair with which the argument is developed. But Laudan's interpretations are unlikely to be accepted without question. The essays are too bold and too challenging not to be subject to sustained critical scrutiny. And besides, as Laudan reminds us, the book is meant to be "exploratory", an authoritative treatment of the issues still not yet being within sight (p.2).

Experts in the history of scientific methodology will undoubtedly want to question Laudan's revisionism, especially his interpretations of Galileo and Descartes. Some will want to reassert the methodological signifiance of Galileo's emphasis on mathematical descriptions, his astronomical realism and his attempt to distinguish primary from secondary qualitites. And some will surely want to object to Laudan's contention that the method of hypothesis played a central role in Descartes's thinking.[2] A more important challenge, however, relates to the scope of Laudan's general thesis about the character and the causes of the development of scientific methodology.

Qualification and elaboration are perhaps most necessary in the case of Laudan's treatment of nineteenth-century methodological thought. While he certainly provides a useful account of methodological opinion during this period concerning the possibility of developing a logic of discovery and of reducing inductive logic to probability theory, his explanations of the decline of "generationalism" and the rise of "probabilism" run counter to one another. (See chapters 11 and 12). According to Laudan, the collapse of infallibilism rendered the logic of discovery "redundant and supernumerary" (p. 190) and "the application of probability to induction was not taken seriously ... largely because there was thought to be no significant element of uncertainty or doubt attached to the conclusions of so-called inductive inference" (p. 192). But this is puzzling since Herschel and Whewell, "who were among the first philosophers of science to stress that theories could be judged independently of a knowledge of their mode of generation", both argued for "forms of inductive inquiry which were ... allegedly infallible" (p. 188 and p. 192). Furthermore, if infallibilism did indeed "crumble" in the 1820s and 1830s (p. 188), why did no one systematically attempt to reduce inductive logic to prob-

ability theory before Jevons and Peirce in the 1870s (p. 192)?

This difficulty aside, there remains the problem of explaining the demise of infallibilism. Had fallibilism emerged along with the method of hypothesis, Laudan could reasonably claim that it was prompted by the turn from phenomenological to deep-structure theories. But however plausible it is to couple fallibilism with the method of hypothesis, infallibilism in fact only declined much after hypotheticalism had re-established itself: initially, hypotheses were not held to be epistemologically inferior to the conclusions obtained by inductive methods. Indeed, as Laudan himself observes, the return to hypotheticalism was accompanied not by an appreciation of the fallibility of hypotheses, but by a search for self-correcting logics of discovery (p. 187). Thus, the decline of infallibilism can hardly be explained simply by observing that scientists at the end of the eighteenth century were once again becoming more sympathetic to deep-structure theories.

In addition to these specific difficulties, there is a more general problem about Laudan's division of the history of methodology into three major periods. Laudan suggests that the method of hypothesis was in eclipse in all areas of science during the eighteenth century and that inductivism was generally taken to have been thoroughly discredited by the end of the first quarter of the nineteenth century. But this claim is too sweeping. To retain Laudan's divisions we must qualify them to allow for the fact that Buffon, Le Duc, Maupertius, Nollet, Scheele and other important eighteenth-century scientists speculated as boldly as most nineteenth-century scientists. And we must bring Laudan's description of methodology in the nineteenth century into line with the fact that much of the science of the time (e.g. Agassiz's studies of the movement of glaciers, Koch's work on the anthrax bacillus, Pasteur's analysis of fermentation and Semmelweiss's research on childbed fever) was as narrowly empirical as that of any eighteenth-century inductivist.

But even granting that many many eighteenth-century natural philosophers subscribed to inductivism and that many nineteenth-century scientists explicitly advocated the method of hypothesis, the question of whether developments in scientific methodology reflected developments in actual scientific research still remains. What needs to be shown is that the methodological statements of scientists during this period summarized their practice and were not merely part of the prevailing ideology

of science.[3] Why think that professions of methodological faith were more closely linked in the eighteenth and nineteenth centuries than they are today? Might it not be that inductivism and hypotheticalism simply exaggerate the conflicting scientific desiderata of empirical caution and theoretical audacity?

The argument of *Science and Hypothesis* thus appears to be subject to much the same qualification as traditional history of methodology. Laudan, no less than the traditional historian, concentrates more on philosophical statements about method than on concrete methodological principles embodied in scientific research; he too provides us with a history of explicit methodological pronouncements and programmatic statements. Thus, while Laudan is surely justified in complaining about the attention lavished on figures who have "enjoyed greater reputations as philosophers than as scientists" (p. 7), we must remember that in confining his attention to the work of individuals like Hartley, Le Sage, Herschel and Senebier, Laudan has chosen to examine the work of thinkers who were especially concerned with philosophical issues. (Interestingly, Laudan himself on occasion refers to Hartley and Le Sage as "philosophers". See p. 230.)

Finally, there are questions to be raised about the significance Laudan sees in his findings for the history of the philosophy of science. Were the philosophy of science exclusively concerned with "the conceptual foundations of science" and "the theory of scientific methodology" (p. 3), it would indeed be implausible to treat it separately from science. But philosophers of science have always studied issues of a more generally epistemological character as well. Consider the problem of the epistemological status of unobservables, or the issue of the relationship of science to metaphysics, or the question of whether science alone produces knowledge. With regard to such issues, we do better to look to philosophers like Hume, Kant, Carnap and Wittgenstein than to scientists like Hartley, Le Sage, Priestley and Whewell. Certainly, the history of methodology should not be regarded, still less written, as though it were a branch of the history of epistemology, but neither should the philosophy of science be reduced to the theory of scientific method.

Laudan modestly describes the essays of *Science and Hypothesis* as being more of "an attempt at forest management than a taxonomy of tree-types" (p. 2). But – even after all the qualifications and reservations have

been noted – Laudan can justifiably claim to have done much more than simply put a little order into the history of scientific methodology. Not only does he clarify our understanding of the major players in the story, he retrieves the ideas of figures like Senebier and Prevost, who are all too often forgotten. He establishes the importance of detailed examination of the vicissitudes of the method of hypothesis for the history of methodology, while providing a general overview of the subject which future historians can expand, refine and revise. But best of all, he shows how philosophy can improve historical research and how history can be philosophically important.

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NOTES

[1] This argument continues the argument of Laudan's bibliographical review, 'Theories of Method from Plato to Mach', *History of Science* **7** (1968), 1-63. In this essay, Laudan maintains that there is a "fundamental flaw in letting history of epistemology call the tune for the history of methodology" (p. 4).

[2] Laudan's interpretation is challenged in G.A.J. Rogers, 'Descartes and the Method of English Science', *Annals of Science* **29** (1972), 73-104. While it is understandable that Laudan has not attempted to revise the essays of *Science and Hypothesis* in the light of recent scholarship, it would have been interesting to have had his response to Rogers and other critics.

[3] This task is admittedly difficult. The archival material leaves much to be desired, and apart from a few notable exceptions, the bearing on the history of methodology of what is available still remains to be studied. Nevertheless, a careful examination of the scientific details of published works – as opposed to scientists' explicit methodological pronouncements – is likely to throw important light on the ways in which scientists actually proceed. In addition, the related issue of the influence of scientific ideology on scientific practice requires much more investigation.

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