

After Popper, Kuhn and Feyerabend: Recent Issues in Theories of Scientific Method

Robert Nola and Howard Sankey (eds)

Australasian Studies in History and Philosophy of Science vol. 15

Dordrecht: Kluwer Academic Publishers, 2000.

Pages xix plus 256

Price: US\$156

For some years the received wisdom has been that there is no such thing as *the* scientific method. The emergence of a mature historiography of science has shown that at best there are many methods. There may be universal principles (such as non-contradiction) common to all enquiry worthy of the name, but these alone cannot explain scientific success (though they may explain some failures). At worst, we must abandon the whole project of explaining the emergence and succession of scientific orthodoxies as growth in knowledge. That project can only produce a whiggish meta-narrative with which right-thinking post-modern persons can have no truck. The motivating conviction of this book is that this apocalyptic extreme does not hold. Post-modernism and ‘strong’ sociology notwithstanding, there remains respectable work for philosophical theorists of scientific method to do. This at least is the common conviction of its editors and authors.

In a sixty-page introduction, the editors argue this claim by offering a partial overview of recent decades in the philosophy of scientific method. They begin with detailed accounts of the work of Popper, Kuhn and Feyerabend. These show that Popper was not a naïve falsificationist, that Kuhn’s view is not merely an appeal to mob psychology and that Feyerabend was not a quixotic champion of unreasonableness. Though brief, these sketches correct many popular misconceptions and dispose of the crude readings of these philosophers on which much of the *fin de méthode* movement depends. For this reason alone this long introduction might be valuable reading for graduate students or advanced undergraduates trying to move beyond the familiar slogans associated with these writers. However, the doctrine that there is no worthwhile function for methodologists has other sources, in addition to popular mis-readings of Kuhn and Feyerabend. One is the ‘Strong Programme in the Sociology of Knowledge’. Over five pages the editors do a succinct demolition job, including the familiar points that the Strong Programme has a problem with normativity and in any case has failed to produce any convincing or insightful case studies. The Strong Programme *requires* that there be a scientific method since this is the advantage claimed by sociology over philosophy—that sociology uses the scientific method while philosophy does not. But there can be no argument in favour of this method, since arguing about method is precisely the activity that the sociology of science is intended to replace. So much for the Strong Programme.

The editors mention a third source for the End of Method doctrine: post-modernism. Here the treatment is less satisfactory and indicates a wider weakness. The only post-modernist text canvassed is Lyotard’s *The Postmodern Condition: A Report on Knowledge*. This soft target is easily disposed of in a page and a half, using the standard anti-sceptical argument that scepticism itself commits one to a knowledge claim. Hence, scepticism is hoist by its own petard. The editors have an easy time mocking Lyotard’s suggestion that the “dialectics of Spirit” or “the hermeneutics of meaning” might once have legitimated natural science. However, they do nothing to establish that there is a single doctrine called ‘post-modernism’ or

that Lyotard is its best representative. One would expect a discussion of post-modernism in the philosophy of science to include Foucault, but there is no mention of him here. This is especially odd as one of the editors, Robert Nola, has recently edited and contributed to a collection of papers on Foucault. Moreover, the editors make no effort to understand why intelligent, serious-minded people might find some version of post-modernism convincing, even if erroneously. Without such a diagnosis the editors have no reason to think that they have understood the object of their criticism. This brief treatment (one and a half pages!) will not do given that post-modernism is supposed to be one of the principal sources of End of Method thesis and thus one of the major targets of the editors' argument.

This failure to engage seriously with post-modernism exemplifies a weakness in the whole book: it is altogether innocent of modern European philosophy. This is serious because all three of Popper, Feyerabend and Kuhn have some 'European' aspect to their thought. The first two were educated in the German-language intellectual cultures of their respective youths. Feyerabend's conviction that there is no universal method is instantly recognisable to anyone familiar with Hegel, who throughout his works railed against what he called 'formalism'. For Hegel, a common method necessarily dictates a set of logical boxes into which diverse phenomena must be forced. These boxes require the phenomena to be trimmed and beaten into appropriate shapes. Thus a common method necessarily distorts or ignores aspects of the phenomena to which it is applied. This is not quite Feyerabend's thesis, but it is a plausible ancestor given that Feyerabend praised Hegel in his correspondence with Lakatos. Such Germanic connections render Feyerabend's work less radical than it might otherwise have seemed (as we can see him standing in a tradition of resistance to 'formalism'), but they also draw attention to its metaphysical side. The connection between method and metaphysics is part of the 'unfinished business' left over from Feyerabend, but it receives scant attention here. Again, this is odd from Nola, who (according to the notes on contributors) has written on Marx and Nietzsche. Moreover, a contrast with German-language philosophical culture might have encouraged the editors and contributors to consider the unacknowledged *a priori* commitments of their own species of philosophy of science. Specifically, the historical spirit of much German-language thought can illuminate the tendency of anglophone philosophy of science to fall into *scientism*. Hence, this book's innocence regarding European philosophy seems on the evidence here to permit a further innocence regarding the book's own background assumptions. In other words, what does he know of Athens who only Athens knows?

The case of Kuhn is rather different, as his productive dislocation resulted from moving between disciplines rather than between languages. He began as a physicist and ended as an historian, influenced by historically-minded European philosophers (principally Koyré). The treatment of Kuhn here is erudite so far as it goes. But it shows no trace of sensitivity to Kuhn's European sources or the fact that historicism is, like anti-'formalism', common in European philosophy. As in the discussion of Feyerabend, European philosophy and historically-minded (i.e. particularist, contextualising, Heraclitan) philosophy are both markedly absent. The lack of any serious philosophy of history is most evident in Laudan's suggestion (discussed in several papers) that philosophers ought to conduct a statistical survey of past science to see what methods worked best historically. This betrays a naïveté about the seamlessness of history. Episodes (e.g. 'an instance of the application of method *M* to phenomenon *P*') are not readily separated from surrounding events, nor are methods easily isolated in the mental lives of their users. The distinctions

necessary to create Laudan's statistical data out of the seamless historical flux are abstract and often arbitrary (arguably, Kuhn's division of the history of science into discrete paradigms is subject to the same criticism).

The failure to appreciate the European roots of Kuhn's historicism is especially unfortunate as Kuhn is the most obstinate bugbear for these methodologists. Strong sociology and post-modernism are easily despatched, and Feyerabend can be rehabilitated. Kevin T. Kelly, in 'Naturalism Logicized', claims that "many of the results of [formal] learning theory can be viewed as *formally* grounded Feyerabendian critiques of particular methodological proposals" (p. 189). In other words, formal learning theory can prove mathematically some of what Feyerabend suggested historically. This is unsurprising as Feyerabend was always a philosopher of science even at his most doctrinally and temperamentally singular. Stripped of his gleeful iconoclasm, much of his view is now accepted by rationalist realists. The historian Kuhn is not so easily accommodated. As Malcolm Forster observes in his essay, "To be a card-carrying philosopher of science it is almost obligatory to reject Kuhn's point of view" (p. 231). It is a pity then that this book includes no reference to the European roots of Kuhn's work. Forster does not ask what this obligation to oppose Kuhn reveals about the *a priori* commitments of the philosophical genre at hand.

Indeed, it may be that much European philosophy (with its 'dialectics of Spirit' and 'hermeneutics of meaning') seems obscure to these philosophers precisely because it tends to be historically- rather than scientifically-minded. Two essays in this volume (by John D. Norton and Andrew Pyle) argue against strong sociology and crude Kuhnianism respectively by deploying case-studies in the traditional HPS manner. However, the collection as a whole tends away from direct engagement with history and towards a more abstract and formal style—this is most marked in Kelly on formal learning theory and the discussions of Bayesianism. Laudan has argued elsewhere (*American Philosophical Quarterly* 24/1 (1989): 19-31) for a divorce between history and normative philosophy of science. There are respectable reasons for a separation. Laudan argues that scientists in the past had different aims and background beliefs to us today, so there is no reason to expect a methodology that serves our ends to represent the behaviour of past scientists as rational. In this collection Kelly argues that history ought not be our only guide because the best methodology may not lie in the historical record. Perhaps no scientist has found it yet. In short, the study of plausibility (as manifested in the historical record) is distinct from the study of validity (undertaken by philosophers).

Nevertheless I suspect that the bare existence of a mature historiography of science makes the philosophical study of scientific method problematic. Immersion in the details of historical and contemporary scientific work supports Hegel's conviction that every phenomenon has its own peculiarities, and that therefore the process of its discovery must be likewise singular. Now, a general philosophical theory, being general, cannot capture the endless variety in detail of actual scientific practice. In order to produce general theories of scientific method, philosophers must homogenise scientific arguments just as any logical theory must reconstruct in abstraction the arguments it studies. Philosophers may follow Laudan and Kelly; they may decide that normative philosophy of scientific method is indeed independent of the history of science. Then methodology will be again like formal logic: clear and rigorous, but separated by several degrees of abstraction from the actual business of rational persuasion among humans. This in itself is not a problem. However, many methodologists wish to make realist or progressivist claims about actual scientific

practice. Then they will be vulnerable to the gibe that philosophers do not address actual scientific practice, but only deal in rational reconstructions.

The papers in this collection are all cogent and well-researched. Norton and Pyle's case-studies offer detailed accounts of the discovery of electrons and the decline of phlogiston chemistry respectively. Kelly's application of formal learning theory to the philosophy of science is that rare thing, a successful piece of interdisciplinary research. Elsewhere, John Worrall compares Kuhn with Bayes; John Fox discusses inductivism; Larry Laudan distinguishes methodology from epistemology; Howard Sankey combines Laudan's normative naturalism with realism; and Malcolm Forster discusses idealisation and commensurability. The authors will probably not be troubled by the scholarly shortcomings identified here because scientific method is their principal object, rather than the works of Popper, Kuhn and Feyerabend directly. Consequently their lack of sympathy for European historicism will not strike them as a vice. Taken together the book successfully demonstrates that there is still respectable work for methodologists, though much of it consists of analysing the works of other methodologists (how much of Kuhn is compatible with Bayesianism? etc.). It also illustrates the uncomfortable position of the philosophy of scientific method, torn between abstract logic and concrete science-studies. Work in the philosophical genre at hand presupposes that this tension can be resolved or at least rendered creative. That question remains open.