

PHILOSOPHY OF SCIENCE

The Road Since Structure. Philosophical Essays, 1970–1993, with an Autobiographical Interview

By THOMAS S. KUHN

Edited by James Conant and John Haugeland

Chicago University Press, 2000. viii + 336 pp. £16.00

This collection of Kuhn's philosophical essays from 1970 to 1993 will serve both to temper and to whet appetites for his book detailing his taxonomic account of scientific progress—a book that he promised for many years but was unable to finish during his foreshortened life. The editors of the volume under review, working from guidelines they had discussed at some length with Kuhn himself, have chosen only philosophically significant essays from the 1970s through the 1990s, while abjuring any essays that were early drafts of material for the promised book. This leaves what Kuhn himself thought to be his most significant late work for that book, which this volume's editors are currently editing. There is, nonetheless, ample useful material in this volume, some of which has heretofore been hard to obtain.

The volume begins with a section of stand-alone essays that demonstrate the acuteness of Kuhn's philosophical mind and the catholicity of his interests. As one might expect from essays written across twenty years, the philosophical contexts of Kuhn's discussions change considerably from one essay to the next, but throughout one sees how Kuhn's characteristic concerns are importantly marshalled to shed light on philosophical topics and problems. The philosophical doctrines and problems discussed most prominently in these essays are in philosophy of language: the causal theory of reference, questions of translation and communication, and occasional forays in the direction of Kuhn's taxonomic approach to theory dynamics. His historically-informed discussions of the limitations of the causal theory of reference, possible world semantics, and certain philosophical doctrines regarding translation are deep and elegant, leaving the reader assured of the need for something more in understanding language change in science than analytic philosophers of language have achieved. Indeed, all questions of Kuhn's account of language and theory change in science are crucially illuminated by two essays in this collection: his 1982 Philosophy of Science Association essay, 'Commensurability, Comparability, Communicability,' and his 1986 Nobel Symposium paper, 'Possible Worlds in History of Science'.

An interview from 1995 concludes the volume and provides key hints to Kuhn's intellectual biography and self-understanding. One of the most interesting aspects of the interview is Kuhn's conservative self-image as historian of science. He calls himself an historian of "analytic ideas" (p. 319) and claims never to have done external history (p. 288). These claims, which might seem odd to philosophers who view Kuhn as having destroyed proper history of scientific ideas and ushered in the era of sociology of

science, are taken up in more detail in perhaps the most important and most frustrating essay in the volume, Kuhn's 1991 Rothschild lecture, 'The Trouble with Historical Philosophy of Science'. In this essay, Kuhn makes the famous remark that the strong program in sociology of science is "an example of deconstruction gone mad" (p. 110). What is frustrating about the essay is that Kuhn's own ultimate position in 1991—that a proper sociology of scientific knowledge does not "replac[e] evidence and reason by power and interest" (p. 116)—is very much the same position the strong programmers had reached by 1991 also. Indeed, current sociology of science involves no replacement of knowledge or reason by interest or power; rather it insists that one and the same social process can exemplify both power and reason, interest and evidence. Nonetheless, it is true that Kuhn's own main interests are 'internalist' in the sense he meant—that is, concerned with conceptual puzzles that arise from within scientific theories. Kuhn's 'internalism' is beautifully illustrated by his examples from Volta's illustrations of electrical cells and Planck's use of statistical reasoning (pp. 20–28) in the elegant first essay, 'What are Scientific Revolutions?'.

Despite the importance of the essays contained in this volume, it will be of limited value as a coherent introduction to Kuhn's late thought. Beyond the intellectual problem of the relative absence of his taxonomic writings (for reasons mentioned above), there is the additional problem that the second section of the book, comprising fully half the essays, is given over to Kuhn's comments on the work of others. Unfortunately, the editors did not provide even brief summaries of the positions that Kuhn discusses in those essays, leaving the reader to fill in the context. Indeed, the editorial apparatus is slight throughout, amounting to little more than a relatively uninformative nine page introduction and a reprinted bibliography of Kuhn's publications. There are no editorial footnotes explaining or cross-referencing points in the essays and there is no index (the latter is an editorial oversight that one might have thought that the University of Chicago Press would not be eager to repeat in a book by Kuhn). One only hopes that the light editorial touch in this volume is due to the Herculean efforts that the editors must be making in order to produce a credible edition of Kuhn's major posthumous book detailing his taxonomic account of scientific progress.

THE UNIVERSITY OF BRITISH COLUMBIA

ALAN RICHARDSON

Scientific Realism: How Science Tracks Truth

By STATHIS PSILLOS

Routledge, 1999. xxvi + 342 pp. £27.99 cloth, £16.99 paper

This book defends scientific realism and traces the development of the realism-antirealism debate from about the turn of the nineteenth century. The usual cast of characters is well represented: Mach, Duhem, Poincaré, Carnap, Laudan, van Fraassen, Worrall, Fine, the mouse in the wainscoting, Craig's

theorem, Perrin's 13 ways to infer Avogadro's number, universal forces, the pessimistic induction, underdetermination, miracle arguments, verisimilitude, and so forth. Psillos does a nice job of disentangling the various issues in dispute, distinguishing various antirealist views, and setting out the standard arguments. He is generally reliable and fair in characterising views opposing his own.

Chapter 1 reviews arguments against operationism, verificationism, and similar attempts to regard each fact expressible in a theoretical language as having truth-makers expressible in an observational language. Psillos gives an interesting account of how Carnap eventually concluded that there is no sharp distinction between these languages. In Chapter 3, Psillos cites passages from Carnap's late letters to explain how Carnap tried to use Ramsey sentences to formulate a position neutral between realism and antirealism. Psillos seems to me correct to press the objection that the Ramsified theory (if empirically adequate) is trivially true if logico-mathematical entities can count as realising it, whereas if the realisers must be physical unobservables, then Carnap's view becomes a kind of "structural realism".

Chapter 2 examines instrumentalism and offers the familiar argument that "persistent novel and successful predictions cannot be seriously attributed to mere chance" (p. 35). For example, "it would be a great coincidence if atoms did not exist and yet all experimental findings were exactly those predicted by atomic theory" (p. 22). An antirealist would surely object that in view of under-determination, a plethora of false theories must make the same predictions, so such "staggering coincidences" must occur. In Chapters 5 and 6, Psillos argues that those false theories that mature sciences once considered to be empirically quite successful did not come by their success by mere coincidence. Rather, they were accurate in the theoretical laws and mechanisms that "made essential contributions" (p. 109) to those empirical successes; in these respects, they agree with the currently accepted theories. This is Psillos's reply to the pessimistic induction. (Psillos gives sufficient conditions for a posit's playing an indispensable role in a theory's empirical successes, though these are somewhat vague and—so far as I could tell—nowhere deemed necessary.) Even if correct, this argument fails to address the anti-realists' claim that a great many false theories agree in all tested respects with accepted theories but differ in the posited unobservables making "essential contributions" to those predictions. However, Psillos argues in Chapter 8 that these other theories lack some of the non-empirical virtues of the accepted theory, such as "coherence with other established theories, consilience, completeness, unifying power, lack of ad hoc features and capacity to generate novel predictions" (p. 171).

Psillos's argument that successor theories usually retain the unobservable posits having "an indispensable role" (p. 110) in the empirical successes of earlier theories relies on his close reviews (in Chapter 6) of the development of the caloric theory of heat and nineteenth-century dynamical theories of the luminiferous aether. Admittedly, Maxwell (for instance) had very different attitudes toward different parts of his aether theories, regarding

some as very probably correct, others as merely showing that a mechanical model was possible, and still others as enabling a mathematical analogy to be drawn between electromagnetic and other processes. However, it is unclear to me that Psillos is correct in thinking that those bits of the theory that played the most crucial role in deriving observations, and so were taken to be best supported by observations, were usually retained by successor theories. For example, Maxwell regarded the identification of the magnetic and electric field energies with the aether's kinetic and potential energies as "very probable" (on p. 564 of 'A Dynamical Theory of the Electromagnetic Field', *The Scientific Papers of James Clerk Maxwell*, ed. W.D. Niven, vol. 1 (Cambridge University Press, 1890), pp. 526–97) even though Psillos would consider the aether to be unnecessary scaffolding within an essentially dynamical theory. It is not the case (contrary to Psillos on p. 137) that Maxwell and his followers took the electromagnetic field as having physical reality independent of its embodiment in the aether. This came with Einstein (or perhaps Lorentz). Moreover, even if we regard the aether as dispensable to the theory's capacity to generate accurate predictions, and instead take energy to be the key actor, we must recognise that many followers of Maxwell (such as Heaviside and Hertz) had grave doubts regarding energy's ontological status, considering the arbitrariness of its zero and the non-uniqueness of the Poynting vector for energy flow (since the vector is definite only up to the addition of an arbitrary divergence-free field). Of course, energy flow is not Lorentz invariant, so it was not retained as the key actor in the successor theory, even while Maxwell's equations were retained (under a new interpretation).

Psillos regards a theory's non-empirical virtues, such as its unifying power, as evidence of its truth. In line with an approach he calls "methodological naturalism", he argues in Chapter 8 that a non-empirical virtue is known to be truth-conducive on inductive grounds: the search for unity, for example, has in the past often led to the truth. Of course, without more history of science, this argument remains gestural, especially since the non-empirical virtues are not carefully explicated here. A further problem is that this inductive argument appeals to the truth (not merely the empirical adequacy) of certain past theories. But a belief that they (rather than empirically equivalent alternatives) are true must be based on their possessing the same non-empirical virtues that we were originally trying to justify regarding as truth-conducive. As Psillos says, "These background theories have themselves been accepted because they enjoyed evidential support and displayed similar theoretical virtues" (p. 172). So circularity looms.

Psillos devotes Chapter 9 to van Fraassen's constructive empiricism, persistently seeking van Fraassen's basis for treating inferences to unobserved observables differently from inferences to unobservables. Psillos argues in Chapter 10 that Fine's "natural ontological attitude" is ambiguous and that Fine's reasons for regarding a metaphysical analysis of truth as impossible or pointless would, if generalised, prohibit giving an epistemology of science at all. In the concluding chapters, Psillos aims to "refill the realist toolbox"

with verisimilitude and a causal theory of the reference of theoretical terms. In sum, Psillos offers us an up-to-date, comprehensive defence of scientific realism and a thorough introduction to this vexing topic.

THE UNIVERSITY OF WASHINGTON, SEATTLE

MARC LANGE