Realism and the progress of science. By PETER SMITH. (Cambridge: C.U.P., 1981. Pp. 135. Price £12.50.)

Smith defends a scientific realism. It is a very mild realism. It claims neither that most of what we believe could refer to a reality it was false of, nor that theories *very* distant from ours in conception or content can be true or false of the things ours are true or false of, nor that any particular theory such as Quantum Mechanics concerns entities whose properties are as the theory taken literally claims. His realism is the claim that in the history of science going back not too far we can construe many theories as referring to quantities and entities that we still countenance, and that in general we can expect the history of science to show such continuity of reference, that at any rate there is likely to be more continuity of reference than continuity of content. It is an admirably sane position. In fact, it is important to see that it can be defended independently of the grander and muddier issues about realism, and important to see that for many purposes in the philosophy of science it is only this milder position that is needed.

Smith's defence of his realism does run through the muddier ground, though. The defence amounts to this: theories do not change that much or that fast; there is usually some important part of the assertions made with a predicate in a theory that can be found (perhaps *via* some simple translation) in a theory that succeeds it. The only obstacle to continuity of reference, then, is Quinean indeterminacy of translation, which threatens the significance of these apparent overlaps of theory. But there are not sufficient reasons for believing that real indeterminacy of translation (as opposed to translations simply being underdetermined by the available data) occurs very often in the history of science.

There are evidently two dangerous spots in the non-historical part of this argument. The first is in the warding off of Quinean indeterminacy. The other is the analysis of co-reference in terms of overlap of content, which he does with a modified cluster-of-descriptions theory of reference. I am not convinced by either. Not because I see definite mistakes; I just find the details confusing and inconclusive. This may be only because I did not pay enough attention. In each case he sets out to bring down a formidable philosopher, and in each case he relies on another philosopher's authority at a crucial point. To refute Quine he relies on Evans and to refute Kripke he relies on Dummett. Neither Quine nor Kripke is exactly a push-over, and so inevitably the result is somewhat inconclusive, and one's reaction to it depends to some extent on which side one is on.

The peculiar thing, though, is that he undertakes to attack these figures for this purpose. And more peculiar yet, he does not seem to find his task remarkable. Why does one have to get through all the *gavagai* stuff to say something about phlogiston? And why should one base a defence of realism on an *attack* on causal theories of reference? Both procedures might be more interesting if they were more self-conscious. For *prima facie* the situation of an interpreter of past scientific theory is very different from that of radical translation. One has a cultural continuity giving a presumption in favour of the historically natural translations of many terms; and, most importantly, it is not the logical structure of the language that is at issue — analytical hypotheses do not come into it — as much as the interpretation of certain predicates *given* translations of a great many others. These are only *prima facie* differences, of course, but a consideration of them would be more interesting than another bash at the undetached rabbit parts.

Once the obstacles are removed, Smith can use a very simple picture of translation and of the relation between sense and reference. Then it is an easy matter to establish his mild realism. There is a one-paragraph refutation of Feyerabend; there are sensible discussions of Dalton on 'atom', Georgian chemists on 'phlogiston', and Newton and

Einstein on 'mass'. All these amount roughly to what one might say on commonsense grounds were one unhampered by any very sophisticated views. And certainly often it is a very valuable thing to show where sophistication makes no difference. It is doing this the hard way, though, to show the irrelevance of disputes about translation and reference by arguing for contentious claims about them. The result is not without interest, but frustrating. For these conclusions one does not want to do quite that much work.

University of Bristol ADAM MORTON

The Structure and Development of Science. Edited by GERARD RADNITZKY and GUNNAR ANDERSSON. [Boston Studies in the Philosophy of Science, Volume LIX.] (Dordrecht: Reidel, 1979. Pp. ix+282.)

This is the second of two volumes arising out of a workshop on the philosophy of science held at Kronberg, near Frankfurt, in 1975. The first volume, *Progress and Rationality in Science*, was reviewed in this journal in April 1980. It consisted of a "position paper" by members of the philosophy department at the London School of Economics, and of responses by a number of other philosophers. The second volume lacks such an overall theme. It consists of nine papers on the philosophy of science, together with an introduction by Andersson. The topics covered in the papers are somewhat diverse, and as a collection it is rather disappointing.

Rescher writes on the completeness of science and on limits to scientific knowledge. He discusses Kant, the Dubois-Reymond/Haeckel controversy, and possible approaches to the question 'Why is there anything at all?', but not recent literature relevant to his theme. Max Jammer contributes a paper on the philosophical implications of "the new physics" which, while interesting, is largely historical. Patrick Heelan briefly presents a "lattice" model of the growth of knowledge. His discussion incorporates, without apparent dissension, ideas concerning scientific change which he attributes to Heisenberg. These impose requirements of ontological continuity between theories, and of the non-revisability of knowledge, that are now very widely called into question. Radnitzky develops a Popperian approach to theory selection, but does not break much new ground; and Peter Hodgson, a physicist, contributes a clear, but slight and under-argued piece on presuppositions and limits to science. There are also papers by Stegmüller, Kockelmans, Feyerabend and Howson.

Stegmüller, a one-time Carnapian, has reacted to Putnam's criticisms of Carnap on theoretical terms by adopting the "new" instrumentalism of Sneed's *Logical Structure of Mathematical Physics*. He appears then to have been struck by the fact that if Kuhn's *Structure of Scientific Revolutions* is interpreted from such an instrumentalistic perspective, many points which reviewers of Kuhn (and contributors to *Criticism and the Growth of Knowledge*) found disconcerting or "irrationalistic" become more comprehensible. Stegmüller's piece (previously published in *Theory and Decision*), like his *Structure and Dynamics of Theories*, presents Sneed's views, and shows how they can be used to interpret Kuhn's work. It also contains criticisms of other philosophers of science, especially of some critics of Kuhn. But Stegmüller tends merely to assume the correctness of his own approach, chiding others for their presupposition of a "statement view" of scientific theories, rather than discussing instrumentalism (old or new) and realism as serious competing alternatives.

Kockelmans' "Reflections on Lakatos's Methodology of Scientific Research Programmes" initially endorses Stegmüller's views as against Lakatos', but then proposes a marriage between the former and some ideas from Bachelard. The upshot is a claim that scientific activity presupposes a distinctive frame of reference, the *telos* or goal of