1. INTRODUCTION

For centuries, philosophers of science have regarded their primary role as that of identifying and justifying the methodological rules which inform and shape the learning and testing techniques of the sciences. A science for which no rules could be given, a science without a methodology of inquiry and testing, seemed unthinkable to many of our forebears. But not so to many of our contemporaries. However plausible the identification of rules of assessment with scientific rationality may once have seemed (and, for some of us, still seems), several recent writers have criticized — and in some cases thoroughly repudiated — the enterprise of identifying and clarifying the methodological procedures or rules utilized in scientific assessment. A growing chorus of voices insists that the identification and analysis of the rules of 'scientific method' is not the route to a philosophical understanding of science.

Paul Feyerabend, for instance, preaches that scientific methodology is dead; that all methods of inquiry are flawed; that "anything goes". Coming to the same conclusion from a different direction, numerous followers of Wittgenstein insist that communal life generally, and the life of the scientific community in particular, is mis-characterised if one imagines it to be grounded in clear rules of assessment or evaluation. This repudiation of methodology is not restricted to philosophers. New-wave sociologists of knowledge, rebelling against Merton and his focus on the 'norms' of scientific behavior, see the rules of scientific rationality proposed by philosophers as little more than post hoc rationalizations for beliefs and actions which are grounded, not in an abstract, objective and disinterested 'scientific method', but rather in the subjective and professional self-interest of individual researchers. Michael Polanyi has argued that the 'rules' of science typically cannot even be articulated, and that much of science consists of 'tacit knowledge' which defies explicit formulation.
I think that this whole approach is fundamentally wrong-headed. But it is much too ambitious a task to attempt to take on in omnibus fashion the entire range of arguments which have been directed against the possibility of methodology as a normative and descriptive enterprise. I shall focus instead on one of the most influential critics of classical conceptions of scientific methodology, namely, Thomas Kuhn. I shall seek to show in this paper that his specific arguments about the unavailingness of appeals to scientific methods and standards will not stand up to sustained analysis. (I shall leave it to another occasion to examine the arguments of several of Kuhn's fellow travelers!)

I have chosen to deal with Kuhn's critique of methodology because it strikes me as possessing a great deal more prima facie plausibility, and to be more closely based on how science actually works, than the discussions of most of the other authors in this tradition. In brief, Kuhn's view is this: if we examine situations where scientists are required to make a choice between the handful of paradigms which confront them at any time, we discover that the relevant evidence and appropriate methodological standards fail to pick out one contender as unequivocally superior to its extant rival(s). I shall call such situations cases of 'local' underdetermination, by way of contrasting them with the more global and more familiar forms of underdetermination associated with Quine and Duhem (which say, in effect, that the rules are insufficient to pick out any theory as being uniquely supported by any data). Kuhn offers four distinct arguments for local underdetermination. Each is designed to show that, although methodological rules and standards do constrain and delimit to some degree a scientist's choices or options, those rules and standards are never sufficient to compel or unequivocally to warrant the choice of one paradigm over another.

2. 'THE AMBIGUITY OF SHARED STANDARDS' ARGUMENT

Kuhn's first argument for local underdetermination rests on the purported ambiguity of the methodological rules or standards which are shared by advocates of rival paradigms. The argument first appeared in the Structure of Scientific Revolutions (1962) and has been expanded considerably in The Essential Tension (1972). As he put it in