Methodology's Prospects

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1. Introduction.

Ever since Aristotle, the philosophy of science has had two clearly delineated components: (a) conceptual foundations and (b) methodology. These rather closely correspond to what one might call the metaphysical and the epistemic bases of scientific knowledge. Both branches of philosophy of science have co-existed for a long time. If anything, methodology has usually tended to predominate over foundations.

Recently, however, methodology has come onto hard times and, since the 1960s especially, it has come in for considerable abuse. Some of that abuse is doubtless well-deserved. Much of traditional methodology made assumptions about the nature of scientific inference which simply cannot be sustained. (For instance, (a) that there was a total 'unity of method' between the various sciences, (b) that the methods of science were fixed and unchanging, (c) that methodological rules could be justified wholly *a priori*, and (d) that methodological rules, once enunciated, would allow a fully algorithmic decision-procedure for uniquely picking out the true theories.) But it is one thing to note that traditional methodology over-reached itself; it is quite another thing to assert, as we now find influential voices asserting, that the whole methodological enterprise and much of epistemology with it is misconceived. In part, methodology is suffering from the general anti-foundationalist Angst which characterizes our time. But many of the attacks I have in mind are aimed specifically at methodology rather than foundationalist epistemology *per se*. If I seem to exaggerate the seriousness of the charges which have been made against methodology, remind yourselves of some of the broadsides which have surfaced in the last two decades. Consider, as the obvious first exhibit, the work of Paul Feyerabend (1978); the upshot of most of his writing in the 1960s and 1970s was that methodological rules are of no use whatever--indeed they are worse than useless since they positively impede the course of inquiry. "Anything goes" is the only methodological rule Feyerabend will allow us. Moving from the ridiculous to the sublime, Quine's recent turn in the direction of so-called naturalistic epistemology has (in Quine's eyes) the effect of rendering methodology a descriptive enterprise, a branch of empirical psychology which can describe how we come to hold the beliefs we do, but is powerless to mount any normative critique of those techniques. At other points of the compass, we find different but equally acute reservations being voiced about methodology. Kuhn and his followers, for instance, hold that many of the

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shared rules or standards of scientific research are so ambiguous that they can be (and are) differently interpreted by every different researcher (Kuhn, 1977). In so far as Kuhn grants that there are non-ambiguous rules, he holds those to be paradigm-specific and thus as powerless to adjudicate--in the manner in which methodology classically expected its rules to--between the theoretical claims of rival camps.

Michael Polanyi was skeptical about methodology because he believed that scientific research was governed by an inarticulate and inarticulable form of 'know-how', which must remain forever tacit. Further out in left field are a host of followers of Wittgenstein who hold that the search for the rules governing any practice, scientific or otherwise, is self-defeating, since one needs rules telling one how to follow rules, and still higher-order rules for how to follow those rules, *ad infinitum*. Coming at these issues from a different direction are Popper and Lakatos, both earnest methodologists, who nonetheless leave the rest of us gasping as they repeatedly insist that methods and methodological rules are mere conventions, rules for playing the game of science which reflect nothing about the facts of the matter. (Popper maintains, for instance, that the pronouncements of methodology are "for the most part conventions of a fairly obvious kind." Popper, 1959) One has conventions about what to call a basic statement, conventions about what counts as a falsifying instance, conventional stipulations about what is and what is not under test. Indeed, as Popper and Lakatos see it, there are no relevant facts of the matter where the theory of inquiry is concerned. It's all just a matter of which conventions we decide to adopt. Hesse and the British school of radical sociologists of science who call themselves her disciples have argued that methodological rules are just social artifacts, usually invented after all the real work is over as a device for rationalizing certain theory choices which were initially made because of personal and professional interests. Beyond that, many philosophers--from Polanyi to Hacking--have persuaded themselves that science is an 'expert system' or 'practice' whose underlying rationale cannot possibly be captured in any precise set of explicit rules of appraisal. The general situation is aptly summed up by noting that a recent prominent sociologist, having carefully read through the philosophy of science of the last two decades, can write an influential book, pulling together for his colleagues in sociology the upshot of recent work in philosophy of science; it is called *Abandoning Method* (Phillips 1973).

Well, I beg to differ. Although I shall not try to convince you that there is something called the scientific method, I do want to suggest that the methodological program, as envisaged by philosophers as diverse as Whewell, Mill, Duhem, Peirce, Mach, and the logical empiricists has been written off prematurely. More than that, I want to show that many of the familiar arguments which have been mounted against methodology fall flat when carefully scrutinized.

Indeed, as my title suggests, I want to take a step back from our debates about the specifics of this or that method in order to make some observations about methodology as a whole. Faced with a litany like the one recited above of wholesale dismissals of scientific methodology, some sort of omnibus reply is called for. Because the issues at stake are so large and my time is so short, I shall have to sketch my response in broad brush strokes. If my style tends towards more towards the aphorismatic than I would like, let me stress that I have sought elsewhere to sketch out the arguments in considerably more detail than I can do here (Laudan 1984, 1987). The case may ultimately turn out to be non-persuasive, but there are arguments to be considered.

Thus, I shall temporarily put to one side these sundry broadsides against methodology in order to sketch briefly a positive account of what methodology is and should be. In the later sections of my remarks, I will show how this sketch gives us the machinery to counter many, if not most, of these familiar dismissals. One final preliminary is in order. It will prove helpful to our general effort to divide the critiques of methodology into two distinct classes. Some criticisms boil down to saying that the activity we call science does not involve anything like methodological rules. Others grant that scientists use rules or maxims for designing experiments and appraising theories but argue that these rules are without epistemic warrant or merit. Thus, the approach of Polanyi or Feyerabend is clearly of the first sort: as they see it, they are no rules which uniformly govern scientific activity. On the other hand, Popper's insistence that methodological rules are conventions or Hesse's claim that such rules merely reflect local social and cultural exigencies and Quine's naturalism represent attacks on the warrant for methodology. I shall return to deal with both sorts of criticisms in the latter part of my remarks.

2. Outlines of a General Theory of Inquiry.

Let us begin with basics. Science is a form of inquiry, not the only form to be sure, but probably its most impressive. Methodology is the study of how to conduct inquiry effectively. Methodology is thus both a form of inquiry and the study of inquiry. There is an obvious self-reflexivity there, but not of the vicious sort. The methodology of science is the study of how to conduct scientific inquiry. Inquiry--whether scientific or otherwise--begins, to but it in the simplest possible way, by raising questions or posing problems. It carries on by proposing answers to those questions, or solutions to those problems. Inquiry terminates, at least *pro temporem*, by the provision of *satisfactory* answers or problem solutions.

But what counts as a satisfactory answer or problem solution? That depends on why we are asking the question or why the problem is held to be a problem. In short, whether a given piece of inquiry is successful depends upon whether its end product (conceived as an answer or a solution) has those attributes which further the ends of inquiry. But that is just to say that inquiry is a goal-directed activity whose success depends upon whether our proposed answers or solutions exemplify the goals which prompted the question in the first place. Our goals, both practical and cognitive, thus act as constraints on what we will accept as a satisfactory answer or solution.

So conceived, inquiry exemplifies the general notion of practical reason and practical action. The conduct of a given inquiry will be *rational* just insofar as we have grounds to believe that this inquiry process will be likely to realize our ends, i.e., to produce answers or solutions which exemplify our goals or standards. Methodology, on this conception, is thus a *theory* (in the quite literal sense) about how to *conduct inquiry rationally*. That is to say, a methodology is a theory about how to conduct inquiry so as to maximize the likelihood that the answers and solutions we produce will satisfy our ends (practical and cognitive). To come right out with it, methodological rules, maxims and principles specify *means* to follow for the realization of the *ends* of inquiry.[1]

Properly parsed, methodological rules are thus hypothetical imperatives, of the form: "If one's goal is x, one ought to do y." The fact that methodological rules have such a normative form has been the occasion for much wringing of hands about the matter of their warrant. Specifically, philosophers who hold that 'ought'-statements cannot be derived from 'is'-statements have been led to believe that no conceivable facts of the matter could have any bearing on methodology. (That, incidentally, is the chief reason why most philosophers of science still hold that the history of science has nothing really important to say to the philosophy of science. It is also why Popper holds that methodological rules, being normative, are conventions; and it is why Quine thinks that descriptive epistemology must sever its pretensions to normative import.) But a moment's reflection will show that, however one stands on whether the naturalistic fallacy is a fallacy, methodological rules--provided they have the conditional form I have just indicated--are generally contingent claims about matters of fact. Whether a particular form of action or conduct will promote a particular aim or goal depends on the features of the world in which we find ourselves. There may be some methodological rules which apply in all possible worlds, although I doubt it. But it is transparently clear that many methodological rules reflect, and rightly so, features or our particular world or features of us as inquiring agents. Inquirers with sensory and neurological apparatus very different from ours would almost certainly need different methods than we do, even if we can conceive them (*per impossibile*) as having goals identical to ours.

This claim of mine about the contingency of methodology appears to trouble many philosophers. They would like to believe that methodological rules are derivable purely *a priori* and that they enjoy the status of logical necessities. As I have already said, however, the view that methodological rules can be derived *a priori* or that they would be true in all possible worlds is wholly implausible. Inquirers with sensory capacities different from ours, inquirers with neurophysiologies different from ours, and inquirers just like us but in a world constituted differently from the way this one is would all be well advised to use means for realizing the aims of inquiry other than those which we find efficacious.

But if the contingency of methodology makes our task more complex, in that we need more than our *a priori* intuitions to do it (see Laudan 1986), that very contingency points the direction to solving the problem of the warrant for methodological rules. Specifically, I hold that the correctness of a methodological rule (of the form "if one's goal is x, one ought to do y") presupposes[2] the truth of the claim that "doing y can realize x, or bring one closer to the realization of x". More than that, the acceptability of a methodological rule rests on our having grounds for believing that "doing y is more likely to realize x than doing any alternative course of action open to us". And that means that the acceptability of a methodological rule depends on our having in hand relevant empirical evidence or theoretical arguments concerning the relative frequency with which doings of y (and its known alternatives) lead to the realization of x.

This way of thinking about methodological rules has several interesting and important implications. It shows, first, that--contrary to Popper and Lakatos--the acceptance of a methodological rule is misleadingly characterized if described as the adoption of a convention. Methodological rules are theories about the relations between ends and means and their adoption or rejection should not be one whit more conventional than the adoption or rejection of any other sort of theory. Secondly, this analysis shows that the old 'is'/'ought' bogeyman is not to be feared, at least not here. For we can see that if we conceive of methodological rules as prudential hypothetical imperatives, then we can both have our cake and eat it. Such rules retain all the normative force associated with any prudential rule of conduct, yet they derive their warrant from empirical information about how this particular world is constituted. One can thus "naturalize" methodology (thereby avoiding the twin perils of treating methodology as either *a priori* or conventional), without being forced (with Quine) to believe that making it empirical and descriptive robs it of its normative force.

I have talked thus far about inquiry in general, and that is appropriate since I hold that methodology writ large deals with inquiry in its broadest sense. It is time, however, to turn to the question of science and its methodology. As the foregoing discussion makes clear, our first concern must obviously be with the question of the ends of scientific inquiry, for it is in light of those ends that methods must be appraised. It will come as no surprise to those who know my earlier work when I say that I do not believe that there is any single set of aims or goals which have always been constitutive of science. It is individual scientists who have aims and goals, and virtually everything we know about the history of the enterprise called science is that its aims have shifted drastically through time. But one needn't spend long hours reading its history to see that science doesn't speak with one voice about its aims. In virtually any contemporary science, one can find instrumentalists and realists, unified theory seekers and particularists, reductionists and anti-reductionists. Each of these familiar divides represents *axiological* diversity. There is, I believe, substantial axiological heterogeneity among practicing scientists, and thus I find it highly misleading to speak (with Popper) of "*the* aims of scientific inquiry." For closely related reasons, I find it unseemly to speak about "the method of science." If 'scientists' have different ends, then it would be most extraordinary (although not impossible) if the same methods promoted all of those ends.

Some philosophers (e.g., Feyerabend, 1978 and Fine, 1986), noting that different scientists have different ends, have suggested that there can therefore be no methodology of science. But this is too hasty. The absence of a single over-arching methodology for science only complicates the task of the methodologist, it does not undermine it. (If we can learn to live, as most of us have, with a plethora of different logics, why should we regard the existence of a variety of different methodologies as hopelessly distressing?) The task of the methodologist of science, as I see it, is to formulate methodological rules for the realization of the various plausible ends of scientific inquiry which one finds in place in the scientific community. (Of which, more below.) Once formulated, those methodological maxims-- conditional on specific ends--have to be evaluated. That evaluation, if my earlier analysis is correct, will involve the appraisal of proposed ends/means connections. Those which pass the relevant tests acquire thereby a normative force, binding on any rational actor who is pursuing the aims in question. Because these methodological rules with be parasitic on particular constellations of aims, the methodologist's tool kit will have to be rather more extensive than it used to be when folks imagined that science had a single guiding aim or set of aims. But the complexity introduced in no way changes the general character of the enterprise.[3]

So soon as one begins to descend to the details of this general framework for understanding methodology, a whole host of tricky problems have to be grappled with. Among them, I should mention at least three:
i). Goals are not simply held co-equal, but are typically weighted in some fashion or other. Because that is so, the methods we advocate should be sensitive to the fact that some ends are more deeply entrenched than others.
ii). Precisely how one decides which of two or more methods, both of which can realize an end, is optimal *appears* to bring in considerations of value into the determination of methodological adequacy in a manner quite distinct from the explicit role of values in the antecedents of hypothetical imperatives.
iii). Showing how one decides in a non-circular fashion when there is good evidence for the soundness of a methodological rule, when those rules themselves seem to constitute our concept of evidence is no simple task.

But these are all worries for which there are at least *prima facie* plausible solutions. Rather than dwell today on the fine-grained details of this approach, which I have discussed at length in print, I hope rather to direct your attention to the forceful arguments which this view provides for meeting head-on a number of fashionable recent dismissals of the methodological enterprise. However, before turning to that task, one further remark is in order concerning the aims of science.

I have so far suggested that methodology simply takes certain ends as given, and proceeds to explore the best means for realizing those ends. If this is all one had to say on the topic, it would invite the response that one is giving away too much to relativism by acting as if aims themselves are immune from scrutiny or analysis. But I believe nothing of the sort. The methodology of inquiry has to be supplemented by the axiology of inquiry; by that latter phrase, I mean the study of the legitimacy of propounded aims or goals. By and large, philosophers of science have largely ignored this question. Either (like Reichenbach) they treat ends as uncriticizable matters of taste or (like Popper) they hold that aims can be faulted only if found to be internally consistent or else (like perhaps most philosophers of science and epistemologists) they imagine that the aims of science (e.g., finding true theories) are self-justifying.

Neither approach has been very illuminating. I believe that axiology, like methodology, can be suitably naturalized, and thus provided with a non-arbitrary decision procedure. Rationally holding an aim of goal, whether scientific or otherwise, requires one to have grounds for believing that the goal is either realizable or is such that one can move closer to its realization. To discover that the state of affairs specified in one's goals is in principle unachievable and that moreover there is nothing that can be done to bring one closer to its realization is to learn that that goal can no longer serve as a goal of inquiry. It can- not do so because the character of a goal is precisely that one is committed to engaging in actions designed to bring it about. To specify a goal for inquiry is to specify certain features that our answers and solutions are supposed to have. If we have reason to believe that no answers or solutions can possibly have those features, then the reten-tion of that goal literally puts an end to inquiry and, as Peirce once said, "do not block inquiry". But how do we discover that certain ends are unrealizable? Again, by a combined process of conceptual analysis and empirical research. These tools may not, indeed I believe that they will not, produce a unique set of aims which alone are appropriate to and realizable by scientific inquiry. But these tools can, indeed through history they already have, work to exhibit the inappropriateness of widely-held and once plausibly advocated ends.

3. The Critique of Methodology Revisited.

The approach to methodology sketched earlier enables us to see how to secure a warrant both for the methods we propose and for the ends to which they are directed. More than that, this approach puts us in a position to see how wide of the mark are many of the currently fashionable dismissals of methodology. Recall a few of the more familiar criticisms.

Paul Feyerabend's claim that anything goes can now be seen to boil down to the assertion that every possible action is a viable means to any possible cognitive end. Put in that form, it is transparently false. Certain putative means to our ends fail to be means to those ends at all; as such they do not qualify as legitimate methods. Methodological anarchism presupposes that, where inquiry is concerned, there is no way the world is. And that strong metaphysical thesis has never been rendered even plausible, let alone inevitable.

Or recall Michael Polanyi's claim that the methods which scientists use in their research cannot ever be made fully explicit and thus that there will always remain an ineliminable tacit dimension to theory evaluation and testing. That view, too, appears to be unargued hogwash. If there are methods or standards which scientists use (and Polanyi does not deny this much), then there is always the in-principle possibility of describing those methods in linguistic form. To deny this, as Polanyi is committed to, is equivalent to asserting that certain facts of the matter will forever elude linguistic characterization. Since Polanyi does not hold that there are generally matters of fact about the natural world which can never in principle be described, why should we accept without further ado his claim that our methodological actions fall in this category? Methodologies, recall, are nothing but theories, and it is a bit unseemly to hold that our theories about the world can be made explicit, whereas our theories about how to interact with that world defy all verbal description.

Or consider the claim, associated most especially with Popper and Lakatos but also endorsed by Quine, that the acceptance of methodological rules is best understood as the adoption of conventions. As I have already suggested, there is no reason why it is any more conventional to accept a certain methodological rule than it is to assert a certain scientific theory. We presumably adopt the rules we do because we believe that they will conduce to the realization of our ends. That belief, because it is about matters of fact, is either true or false. Moreover, we have in principle at least as much epistemic access to its truth status as we do about most of the theories which make up the natural sciences. If, like Popper and Quine, we are tempted to treat methodological rules as conventions that can only be because we believe that there is no fact of the matter (or at least no ascertainable fact of the matter). Yet neither Popper nor Quine have given us any grounds for believing that there is no fact of the matter where ends/means statement in general, and methodological rules in particular, are concerned.

Of course, the methodological enterprise--as described here--will differ in significant respects from its earlier conceptualization at the hands of positivists and empiricists. Gone, for instance, is the idea that methods can generally be warranted wholly by conceptual analysis, or that methodological rules have the same a priori status as the principles of formal logic are presumed to enjoy. Gone, too, is the notion that methodology remains fixed once and for all. Precisely because the aims of inquiry slowly evolve, methods and standards which are appropriate to one axiology of inquiry may prove to be inappropriate for its successors. And even if our aims never shifted, we should still expect our methods to shift, since our judgments about the appropriateness of a method depend on our background beliefs and theories, which are themselves radically subject to change. The character of rational methodology will change as surely as, if perhaps less frequently than, the other theories of the sciences do. Possibly gone as well is the idea that all sciences do or should use the same methods or the same standards for their very diverse investigations. We have to accept the possibility that we may discover that some methods are appropriate for certain forms of inquiry and inappropriate for others. The unity of method--that favorite dogma of the Vienna Circle--has shown itself to be grossly insensitive to the very real methodological divergences among the sciences.

But what is retained from the traditional conception is the idea that there are significant things to be said, descriptively and normatively, about how scientific inquiry is and should be conducted.

Notes

[1]For ease of exegesis, my formulations in this early section of the paper will assume that all inquirers have the same ends. That is manifestly not true. However, the argument can readily be reformulated so as to apply to a community of researchers with quite diverse ends, as shall show below.

[2]I am not suggesting that this is *all* that methodological rules presuppose; only that they do presuppose this much.

[3]It is probably important to stress that, although I believe there is a fairly wide axiological diversity in science through time and across the various sciences, my general account of methodology would still apply.

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