LAKATOSIAN PARTICULARISM

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ABSTRACT: This paper explores a particularist element in the theory of method of Imre Lakatos, who appealed to the value-judgements of élite scientists in the appraisal of competing theories of method. The role played by such value-judgements is strongly reminiscent of the epistemological particularism of Roderick Chisholm. Despite the existence of a clear parallel between the particularist approaches of both authors, it is argued that Lakatos's approach is subject to a weakness that does not affect the approach of Chisholm.

KEYWORDS: Imre Lakatos, methodology of scientific research programmes, value-judgements, Roderick Chisholm, particularism

1. Introduction

According to the epistemological particularist, general reflection about the nature of knowledge is subject to the constraint of judgement about specific instances of knowledge. A theory about the nature of knowledge must conform with judgements about specific cases of knowledge which have been identified as such prior to the development of an epistemological theory. On the assumption that particular items of knowledge are positively identified as such, a particularist approach to epistemology is typically anti-sceptical in character. For it is the task of epistemological theory to accord with judgements about particular cases of knowledge rather than to exclude them as failing to be items of knowledge.

Work in the theory of scientific method often proceeds in isolation from general epistemology. But on occasion there is convergence. In the development of his theory of scientific method, Imre Lakatos employed an approach to the meta-methodological appraisal of theories of method that is distinctively particularist in character. In order to adjudicate between competing theories of scientific method, Lakatos proposed that appeal should be made to the value-judgements of élite scientists about past episodes in the history of science. Such judgements about particular episodes in the history of science would serve as touchstones in the evaluation of opposing theories of method.

My aim in this paper is to explore the particularist element that is found in Lakatos's theory of method. In section 2, I will analyse the role played by the value

judgements of the scientific élite in the context of Lakatos' methodology of scientific research programmes. In section 3, I will present the particularist approach to epistemology as proposed by Roderick Chisholm. In section 4, I will draw a parallel between the approaches of Lakatos and Chisholm, and argue that, despite the parallel, Lakatos's approach is subject to a weakness not found in Chisholm's approach.

2. Lakatos and the Value Judgements of the Scientific Élite

T.S. Kuhn's influential book, *The Structure of Scientific Revolutions,* played a key role in the historical turn in the philosophy of science.[1] *Structure* attracted a far greater audience in academic circles and the broader public than is usual in the history and philosophy of science. But within the philosophy of science reaction to the book was decidedly critical.

Philosophers reacted negatively to Kuhn for two main reasons. The first reason was the perceived relativism of Kuhn's account of science due to the variability of methodological standards and lack of neutral observation between incommensurable paradigms. The second was the irrationalism of Kuhn's apparent suggestion that choice between paradigms may not be made on rational grounds, as implied by his talk of religious conversion and gestalt shift.[2]

In response to Kuhn and other advocates of the historical approach, philosophers sought to defend the rationality and objectivity of science in a variety of ways. One form which this response took was the development of alternative models of scientific theory-change which granted a substantive role to method and rationality in the process of theory-choice. As a specific case in point, Lakatos proposed his methodology of scientific research programmes as a model of scientific theory change which would overcome the perceived flaws of Kuhn's model.

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[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 4th ed. (Chicago: University of Chicago Press, 2012).

[2] For examples of the early critical reaction to Kuhn, see Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan E. Musgrave (Cambridge: Cambridge University Press, 1970), 91-196, e.g. 90; Karl Popper, "Normal Science and its Dangers," in *Criticism and the Growth of Knowledge*, 51-58, especially 56; and Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967), 19.

Lakatos understood the choice between paradigms by scientists in a Kuhnian revolution to be an irrational one that may not be based on any "super-paradigmatic" standards. In proposing his own model of theory-change, Lakatos sought to provide an account on which scientists make a rational choice based on a methodological standard. By contrast with Kuhn's idea of a paradigm, Lakatos proposed that scientists adopt research programmes, characterized by a hard core of inviolate theoretical principles within a protective belt of revisable auxiliary hypotheses. Lakatos defined a notion of progress on which each stage of a research programme predicts at least some novel facts ('theoretical progress'), at least some of which are empirically corroborated ('empirical progress'). He then proposed that scientists are rational to choose a progressive over a non-progressive ('degenerating') research programme. In this way, Lakatos provided a methodological criterion, progressiveness, on which a scientist's choice of research programme may be rationally based.

Lakatos recognized that his methodology of scientific research programmes was one of several alternative theories of scientific method. This raised the question of how a theory of scientific method is to be appraised.[3] Lakatos's proposal was that theories of scientific method might be used as the basis for a rational reconstruction of selected episodes in the history of science: "all methodologies function as historiographical (or meta-historical) theories (or research programmes) and can be criticized by criticizing the rational historical reconstructions to which they lead."[4] If a theory of method reveals episodes considered to be rational as rational, while classifying episodes taken not to be rational as non-rational, then that may be taken to count as evidence in support of the theory of method. By contrast, if a theory of method fails to appropriately classify a selected episode, that counts as evidence against the theory of method.

[3] Given the Popperian context in which Lakatos worked, the question of how a theory of method is to be appraised takes on a specific form. Popper held that methods have the status of conventions (see Karl Popper, *The Logic of Scientific Discovery* (London: Unwin Hyman, 1959), 53). But he never made clear how to evaluate a theory of method which has the status of a convention. It is hard to see, for example, how to empirically test a theory of method, given its conventional status. As convention, a theory of method is not a statement of empirical fact, so may not be evaluated as such. (For further discussion, see Robert Nola, "The Status of Popper's Theory of Scientific Method," *British Journal for the Philosophy of Science* 38, 4: 441-480.)

[4] Imre Lakatos, "History of Science and its Rational Reconstructions", in *The Methodology of Scientific Research Programmes: Philosophical Papers Volume I*, eds. John Worrall and Gregory Currie (Cambridge: Cambridge University Press, 1978), 102-138, 122.

More generally, if one theory of method appropriately classifies a greater number of selected episodes than does another theory of method, this constitutes evidence that the former theory of method is superior to the latter. There is, of course, no suggestion that all episodes in the history of science are to be accounted for on a rational basis, "since even the greatest scientists make false steps."[5]

On what basis are the touchstone episodes in the history of science to be selected? It is in answer to this question that Lakatos appeals to the "value judgements of the scientific élite."[6] The episodes which are to be used in the appraisal of theories of method are those which are recognized as instances of good science by leading members of the scientific community. Lakatos introduces this idea by noting that Popper's own criterion of demarcation was designed to accord with the belief that while Newton and Einstein had produced great scientific achievements, astrology, Freudian psychoanalysis and Marxism were pseudoscientific. Rather than start off with a methodological proposal, the starting-point is to be particular cases of good science. The methodological proposal is to be tailored to fit the particular cases. As Lakatos goes on to explain:

> While there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning *single* achievements. While there has been no *general* agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular single step in the game was scientific or crankish, or whether a particular gambit was played correctly or not.[7]

In this way, it is judgements made about specific cases from the history of science that are to serve as guide in the evaluation of methodological proposals. It is

[5] Lakatos, "History of Science," 134. For Lakatos, historical episodes which are unable to be explained on a rational basis by a theory of method are to be explained in external rather than internal terms. Theories of method differ with respect to how much of the history of science is relegated to external factors. One advantage claimed by Lakatos for the methodology of scientific research programmes is that it is able to explain a greater proportion of the history of science in internalist terms than competing theories of method, such as inductivism, conventionalism or falsificationism.

[6] Lakatos's terminology is not perfectly consistent. He speaks variously of "accepted 'basic value judgement' of the scientific élite" ("History of Science," 124), "the 'basic' appraisals of the scientific élite" (125), "particular 'normative basic judgment" (131), "'basic judgments' of leading scientists" (132), as well as employing several variations of these forms of words.

[7] Lakatos, "History of Science," 124.

judgements to the effect that one or another past scientific theory or achievement constituted an instance of good (or bad) science that are to be used as evidence in the evaluation of competing theories of scientific method.

Lakatos is thinking of specific judgements about particular cases. The judgements are evaluative in nature. They involve an assessment of whether a particular instance of science is an example of good or bad science. For this reason, Lakatos speaks of the judgements as "value judgements," "normative judgements," and as "appraisals." The objects of appraisal are particular instances of science. In the main, Lakatos's examples are theories such as Einstein's or Newton's physical theories. However, it does seem clear that he could easily have taken experiments or specific choices of theory on the basis of evidence as examples as well. As for whose judgement is to count, Lakatos does not go into this in detail, though he speaks of "élite scientists" as well as "leading scientists."

There is, as Larry Laudan has noted, a potential circularity with this approach.[8] For how, exactly, is one to determine who is a leading or an élite scientist? The problem is not so much how to distinguish élite from run-of-the-mill scientists, but of how to identify scientists in the first place. On the assumption that scientists employ scientific methods, one might seek to identify the scientists by identifying those who employ scientific methods. But surely one must be able to identify scientists without drawing on methodological considerations in making the identification. It would be inappropriate to identify scientists by determining which individuals employ scientific methods, and then identifying them as scientists because of their use of such methods. It would be inappropriate because the whole point of the exercise is to develop a theory of method based on an independent selection of cases of good (and bad) science. The theory of method is to fit cases of good science and exclude cases of bad science where these have been independently classified as such by the élite scientists. But if one appeals to the methods of science in the identification of scientists, then one already has a grip on the methods of science prior to the identification of the scientists. If this were the case, then selection by scientists of cases of exemplary science could not serve the function of independently identifying cases of good science prior to the development of a theory of scientific method.

[8] Larry Laudan, "Some Problems Facing Intuitionist Meta-Methodologies," *Synthese* 67, 1 (1986): 115-129, 117.

3. Chisholm's Epistemological Particularism

The emphasis by Lakatos on particular cases of good science is strongly reminiscent of the particularist approach to epistemology famously associated with Roderick Chisholm.[9] Chisholm develops his approach in relation to a problem of circularity that is closely analogous to the problem that we have just seen to arise for Lakatos with respect to the identification of scientists. For Chisholm, the problem is basically the problem of the criterion bequeathed to us by the ancient Pyrrhonian sceptics.[10] How does one arrive at an epistemic criterion which may be employed to identify items of knowledge? To determine whether a proposed criterion correctly picks out items of knowledge, one must be able to determine whether the purported items of knowledge selected by the criterion are indeed items of knowledge. If one were able to identify items of knowledge in advance of arriving at an epistemic criterion, then one might evaluate a proposed criterion by determining whether it correctly identifies the items of knowledge as such. But how does one identify items of knowledge prior to having an epistemic criterion? If one already has an epistemic criterion, then one might use the criterion to identify the items of knowledge as items of knowledge. But if one does not already have an epistemic criterion, then it is not clear how to identify a purported item of knowledge as an item of knowledge. The problem is how to arrive at an epistemic criterion without already being able to identify items of knowledge prior to adopting an epistemic criterion.

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[9] See Roderick Chisholm, *The Problem of the Criterion: The Aquinas Lecture 1973* (Milwaukee: Marquette University Press, 1973). For the most part, Lakatos's discussion is strongly particularist. However, in the final pages of his "History of Science" (136-137), Lakatos contrasts the statute law of the philosopher with the case law of the scientist, suggesting that there may be circumstances in which the statute law may take precedence over the case law. Laudan notes that it is hard to reconcile this with the role that Lakatos accords to scientists' judgements about particular cases ("Some Problems," 124). I find the remarks too compressed to determine whether they constitute a significant departure from Lakatos's more explicit reliance on a particularist approach.

[10] The problem of the criterion is usually put in terms of the choice between infinite regress, circularity and dogmatic halting-point that arises when one attempts to justify any proposed epistemic criterion. Chisholm employs an alternative formulation of the problem, the *diallelus* or wheel, which involves the reciprocal relationship between epistemic criteria and actual items of knowledge.

Chisholm proposes his epistemological particularist approach in response to the problem of the criterion. He frames the discussion in terms of two pairs of questions:

> (A) " *What* do we know? What is the *extent* of our knowledge?"
> (B) "How are we to decide *whether* we know? What are the *criteria* of knowledge?"[11]

If one could answer the first pair of questions, then one would be able to arrive at an answer to the second pair by inspecting actual items of knowledge for clues as to how knowledge is arrived at. Conversely, if we had an answer to the second pair of questions, one would be able to arrive at an answer to the first pair by employing a criterion to identify items of knowledge. In this way, an answer to either of the pairs of questions presupposes an answer to the other pair. It is not possible to answer one pair of questions without first answering the other.

There are, according to Chisholm, three distinct ways of responding to the peculiar reciprocal relationship that obtains between the above two pairs of questions.[12] One response is that of the sceptic, "You cannot know what, if anything, you know, and there is no possible way for you to decide in any particular case."[13] But Chisholm notes that the response of the sceptic is not the only possible response. There are two other options apart from scepticism. One option is that of the position that Chisholm describes as the position of "methodism." The methodist response is to answer the second pair of questions first by simply adopting an epistemic criterion. Because the methodist adopts the criterion without any constraint being imposed by existing items of knowledge, the criterion must be chosen in what will ultimately prove to be an arbitrary manner. The remaining option is that of the particularist, who answers the first pair of questions first by singling out individual items of knowledge. The particularist only turns to the question of the criteria of knowledge after particular items of

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[11] Chisholm, *Problem of the Criterion*, 12.

[12] Chisholm does not mention a fourth possible approach, that of the Goodman-Rawls model of reflective equilibrium. It is not, however, entirely clear whether the reflective equilibrium is distinct from the particularist approach. Noah Lemos takes particularism to be compatible with the method of reflective equilibrium (see Lemos, *Common Sense: A Contemporary Defense* (Cambridge: Cambridge University Press, 2004), 6), while John Greco argues that there is tension between the two approaches (Greco, "Review of Noah Lemos *Common Sense: A contemporary Defense*", *Notre Dame Philosophical Reviews*, 2005/07/05).

[13] Chisholm, *Problem of the Criterion*, 14

knowledge have first been identified. Based on an inspection of the particular items of knowledge, it is possible to arrive at criteria which tell us "what it is for a belief to be epistemologically respectable."[14]

Of the three possible responses to the problem of the criterion, Chisholm favours the response of the particularist. In effect, his approach amounts to a proposal about the correct procedure to follow in doing epistemology. One should start by identifying particular cases of knowledge. Typically, these will be straightforward and uncontroversial items of knowledge, such as G. E. Moore's example of knowing he has two hands. Only once one has identified items of knowledge, should one turn to the theoretical task of developing criteria of knowledge. Epistemic criteria are designed to reflect the epistemically distinctive features of actual items of knowledge which are identified prior to epistemological reflection. A full-blown epistemological theory is to be developed on the basis of sustained consideration of the broad range of knowledge that we actually do possess.

It remains the case, however, that particularism is only one of the three possible responses to the problem of the criterion. Chisholm was well-aware of this. He explicitly considers the problem that arises from the fact that he proposes to develop an epistemological theory on the basis of the prior identification of particular items of knowledge. Both the sceptic and the methodist will take exception to this approach. The sceptic will raise doubts about the epistemic status of individual items of knowledge. The methodist will object to starting with items of knowledge rather than criteria. Here Chisholm's response may seem somewhat disarming. "What few philosophers have had the courage to recognize," he says, "is this: we can deal with the problem only by begging the question."[15] To attempt to reason with the methodist or the sceptic is to step "back on the wheel" (the *diallelus*) again. To avoid this, there is no choice but to simply beg the question, and carry on in particularist fashion.

4. A Parallel Between the Approaches

To return to Lakatos, the question is whether Chisholm's approach may be of assistance in relation to the circularity that threatens Lakatos's appeal to the value judgements of the scientific élite. As we have seen, the problem for Lakatos is how

[14] Chisholm, *Problem of the Criterion*, 24.
[15] Chisholm, *Problem of the Criterion*, 37.

to identify scientists without drawing on methodological considerations in identifying the scientists. If the identification of scientists is informed by methodological considerations, then the appeal to the value judgement of élite scientists would fail to have the independence that is needed for such judgement to serve as independent arbiter in the choice between competing methodological views. The question now is whether Chisholm's point that the particularist must simply beg the question against the sceptic and the methodist might be put to use in support of the Lakatosian attempt to employ scientists' value judgements as an independent court of appeal.

There is a clear parallel between Chisholm's epistemological particularism and Lakatos's appeal to scientists' value judgements about particular scientific achievements. In both cases, judgements about particular instances (e.g. particular items of knowledge or scientific achievements) do normative epistemological work. In Chisholm's case, particular items of knowledge are first selected and then inspected to identify epistemically relevant properties which may serve as the basis of epistemic criteria. In Lakatos's case, scientific achievements are employed in the evaluation of competing theories of method. But, despite this substantive commonality, there is an important difference which renders the Chisholm-style response ineffective in the context of the choice between competing methodologies that Lakatos's approach was designed to deal with. The difference relates to the dialectical context within which the two approaches are situated.

As we have seen, Chisholm holds that the particularist has no alternative but to beg the question against the sceptic and the methodist. Rather than engage in argument with the opposing views, the particularist must simply assume that we are in possession of genuine items of knowledge. For Chisholm, proper procedure in epistemology is to identify a range of particular instances of knowledge, and to build an epistemological theory on the basis of an analysis of the epistemically relevant features of the selected items of knowledge. This approach begs the question against the sceptical denial of knowledge, as well as against the methodist for whom epistemic criteria take precedence over particular cases of knowledge. But, despite begging the question against the opposing views, there remains a significant sense in which the fact that the question is begged fails to incur any argumentative disadvantage in the specific context of debate. The reason is that the particularist position is, at base, the epistemological position of common sense. Thus, there is a sense in which the particularist position is the default epistemological position, which ought to be endorsed even though it may beg the

question against the sceptic and the methodist. No doubt there will be some of a sceptical or Cartesian persuasion who may profess to harbor doubts about items of knowledge identified by the particularist. But, equally, there will be those of a naturalistic or Moorean frame of mind for whom any sceptical argument is to be rejected simply because it conflicts with the dictates of ordinary common sense or with the findings of the sciences. As for the methodist, the idea that choice of criteria may be completely unconstrained by particular cases of knowledge gives rise to an objectionable arbitrariness in choice of epistemic criteria. In sum, the fact that the particularist must beg the question is a small price to pay for endorsing the position that common sense recommends.

By contrast, the Lakatosian appeal to the value judgement of élite scientists is situated in a different argumentative context. For Lakatos, the particular scientific achievements or historical episodes selected by the élite scientists are to be employed in comparative appraisal of competing theories about the nature of scientific method. It is entirely possible that proponents of alternative theories of method may disagree about which episodes in the history of science are to be taken as instances of good (or bad) science. For example, an inductivist might point to cases of empirical confirmation of a theory while a falsificationist might be impressed by the dramatic refutation of a theory. A conventionalist might appeal to the simplicity of a theory while the Lakatosian might see the progressiveness of a research programme as the mark of superior science. It is because of this potential for disagreement that it is crucial for Lakatos's meta-methodological project that it be possible to identify the scientists to whose value judgements appeal is made in a way that does not draw upon a theory of scientific method. The identification of scientists must be undertaken in a way that is quite independent of considerations of a methodological nature. Otherwise, the judgements of scientists would be unable to play the role of neutral arbiter which Lakatos's approach requires.

For Lakatos, particular scientific achievements play an adjudicative role in the comparative appraisal of competing methodologies. Because of this, it is simply not possible for the Lakatosian approach to beg the question in the way that the Chisholm-style particularist may do. It is crucial to the Lakatosian project that the scientific achievements selected by élite scientists be able to function in an entirely independent manner in the comparative appraisal of competing theories of scientific method. There is no sense in which one of the competing theories of scientific method can lay claim to having the status of the default position embedded in common sense, and that it ought therefore to be accepted even if

doing so results in begging the question. For if the question is begged in the Lakatosian context, then the selection of touchstone cases from the history of science simply fails to the play the neutral role which it is required to do. In sum, it seems clear that the Lakatosian approach to meta-methodological evaluation of theories of scientific method is irreparably compromised by the problem of how to identify scientists without reliance on methodological considerations.