KUHN, NORMATIVITY AND HISTORY AND PHILOSOPHY OF SCIENCE

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I. THE RELEVANCE OF HISTORY TO PHILOSOPHY OF SCIENCE

This paper takes its starting-point from the question of the relevance of the history of science to the philosophy of science. In light of the impact on the philosophy of science of Thomas Kuhn’s book, *The Structure of Scientific Revolutions*, it cannot be denied that the history of science is relevant to the philosophy of science. But the nature of the relevance is not immediately apparent.

Kuhn himself insisted that the history and the philosophy of science are separate fields. Though a “new sort of dialogue between these fields is badly needed”, he writes, “it must be inter- not -intra-disciplinary” ([1977], p. 4). It has to be said, however, that Kuhn was not always a successful participant in this dialogue.

*The Structure of Scientific Revolutions* was published as part of the *International Encyclopedia of Unified Science*. The logical positivist, Rudolf Carnap, was one of the editors of the *Encyclopedia*. In his capacity as an editor, Carnap wrote to Kuhn in sympathetic terms. He liked Kuhn’s “emphasis on the new conceptual frameworks which are proposed in revolutions in science”. He found Kuhn’s parallel between scientific change and Darwinian evolution “very illuminating”.[[1]](#footnote-1) But Kuhn did not know what to make of the overtures of this leading member of the Vienna Circle. He later wrote that he was embarrassed by his reaction to Carnap. He took Carnap’s letter “as mere politeness, not as an indication that he and I might usefully talk” (Kuhn [1993], p. 313).

We may never know if Kuhn read Carnap’s 1956 paper, ‘The Methodological Character of Theoretical Concepts’ before writing *Structure*. If he did read the paper, he might have realized that Carnap held views much like his own in at least one area. For in that paper, Carnap presents a view about the meaning of theoretical terms that is similar to Kuhn’s own view that the meaning of scientific terms changes profoundly in the course of scientific revolution.[[2]](#footnote-2)

This leads to the first, rather simple point that needs to be made. A philosopher and a historian of science may arrive at similar results on the basis of different methods. Kuhn’s claims about meaning change were often based on specific cases from the history of science rather than considerations in the theory of meaning (e.g. Newton vs Einstein on mass, Kuhn [1996], p. 102). Carnap’s views about meaning change reflect his account of the partial interpretation of theoretical terms by means of correspondence rules which connect theoretical terms to observational vocabulary. But, on the basis of different approaches, they came to similar views.

II. PHILOSOPHICAL PROBLEMS FROM *STRUCTURE*

*Structure* has had an enormous impact on the philosophy of science. But the Kuhn of *Structure* was no philosopher. Alexander Bird says that *Structure* is “theoretical history of science”, rather than philosophy (Bird [2000], p. 29). Bird is talking about Kuhn’s model of scientific theory change. Kuhn’s claims about normal science that is practiced under a paradigm and broken at intervals by scientific revolution comprise a large-scale theoretical generalization about science, rather than philosophy of science in a strict sense.

Still, *Structure* raised questions about science on which a great deal of philosophical work has been done. Problems about conceptual change, communication failure and incommensurability between paradigms are one example. Questions about scientific realism, discontinuity of reference and convergence on truth are another example. Indeed, Kuhn seems to have inspired the pessimistic induction on the falsity of past scientific theories, which continues to pose one of the major threats to a scientific realist view of the progress of science.

But I wish to focus on another set of philosophical issues raised by Kuhn. These are questions about scientific method, the normativity of methodological rules and epistemic relativism.

III. NORMATIVE ISSUES IN KUHN’S VIEW OF METHOD

Kuhn’s apparent view in *Structure* that there is no fixed scientific method and that the rules of puzzle-solving adequacy vary with paradigm is one of the major influences behind contemporary epistemic relativism. But what Kuhn established is limited.

In later work, Kuhn described a set of cognitive values which he took to be largely constant throughout the sciences ([1977], pp. 321-2). But this was not the picture that emerged from his discussion of puzzle-solving in *Structure* ([1996], pp. 38-42). Puzzle-solving is governed by rules of puzzle-solving adequacy which depend upon the accepted paradigm. These rules range from the basic laws of a paradigm (e.g. Newton’s laws of motion), to proper experimental technique (e.g. the use of fire in chemistry) and general metaphysical views about the fundamental nature of reality (e.g. corpuscularianism). Because such rules of puzzle-solution depend upon currently accepted paradigm, Kuhn took them to be subject to variation in the history of science. As paradigms change, so do the rules of puzzle-solving adequacy. Thus, without any apparent commitment to a fixed method of science, Kuhn’s discussion of puzzles gave the impression that, with respect to the methodology of science, all is in flux.

But, even if one assumes that Kuhn did provide concrete cases of methodological change in the history of science, this constitutes at most part of an argument for epistemic relativism. The mere fact that the methodological rules actually employed by scientists have changed does not establish the normative point that epistemic justification depends upon and varies with the methodological rules employed by scientists. That is not something that mere empirical evidence about the methods scientists have actually employed can establish. An epistemological case has to be made that epistemic justification depends upon the rules of method that scientists employ. Only after that epistemological point is established can the conclusion of relativism be drawn from the empirical claim that the rules scientists have actually used have undergone change in the history of science.

Kuhn made at least three attempts to deal with the question of epistemic normativity. In *Structure*, he presented justification as a matter of mere social acceptance within a community of scientists ([1996], p. 94). Later, in response to Feyerabend, he sketched an inductive argument from the success of past science that scientists should do what his model of theory-change suggests they actually do ([1970], p. 237). But his mature view was neither sociological nor naturalistic. It was semantic. In a symposium with Hempel and Salmon, Kuhn sought to ground epistemic normativity in semantic considerations about the meaning of the term ‘science’ in a way that is reminiscent of P.F. Strawson’s analytic justification of induction (e.g. [2000] p. 214).

The shortcomings of Kuhn’s attempts to provide a basis for normativity have been documented elsewhere, so my comments may be brief (see Nola and Sankey [2007], pp. 285-97). Kuhn’s initial suggestion that justification reduces to acceptance by scientists problematically overlooks the crucial distinction between being epistemically justified and being adopted as a mere matter of social convention. His later suggestion that normativity is grounded in the success of past science is promisingly naturalistic in spirit, but is presented in too little detail to permit detailed analysis. As for his mature view that normativity is grounded in semantic considerations about the meaning of the word ‘science’, it is altogether unclear how a normative claim about what we are justified in believing may be based in semantic conventions about what our words mean.

In sum, despite several attempts, Kuhn failed to satisfactorily resolve the problem of how methodological rules obtain justification. Given this failure, I will change tack and consider treatment of the issue in the work of Imre Lakatos, Larry Laudan and John Worrall.

IV. HISTORY OF SCIENCE AND THE NORMATIVITY OF METHOD

Imre Lakatos saw a role for the history of science as a way to choose between alternative theories of scientific method (see especially his [1978]). Faced with the choice between inductivism, conventionalism, falsificationism and his own methodology of scientific research programmes, Lakatos appealed to the history of science as a way to determine which theory of method might better account for the rationality of past science. A methodology that revealed the actual rational choices made by scientists *as rational* was better than a methodology that dismissed such choices as irrational.

But how are we to decide which episodes from the history of science should be employed as the basis for choosing between competing theories of method? Here Lakatos appealed to the value judgements of elite scientists ([1978], pp. 124-5, 132-3). Similarly, Larry Laudan suggested that one should appeal to our pre-analytic intuitions about the rationality of past science in determining the historical episodes against which competing theories of method are to be judged ([1977] pp. 160-3).

Of course, this just raises the question of why we should grant any credence to the value judgements of elite scientists or to our own pre-analytic intuitions about the rationality of past episodes in the history of science. Recognizing the problems that arise with respect to such value judgements and intuitions, Laudan later rejected this approach to the rationality of science ([1986]). Instead, he proposed a normative naturalist account, according to which empirical facts about the reliability of methods in realizing cognitive aims serve as the basis for epistemic justification (Laudan [1996]). Laudan proposed that, instead of appeal to intuitions or value judgements, one should employ the history of science to determine whether particular methods had in fact led on a reliable basis to the realization of particular cognitive goals. If so, this would provide empirical evidence for the epistemic justification of the method.

Laudan’s attempt to develop a naturalistic meta-methodology, on the basis of which to evaluate competing epistemic norms or methods, was in turn criticized by John Worrall. For on what basis could empirical evidence of past reliability in leading to an epistemic goal provide a rationale for continued use of a rule or method? Here Laudan appealed to a general inductive principle to the effect that, if use of a method in the past has led more reliably to a goal than use of some other method, then it is more likely that the method will lead to the goal in the future than that the alternative method will. But what, Worrall asks, is the status of the inductive principle that Laudan uses to justify use of a particular rule of method?

Worrall argues that Laudan’s attempt to justify method empirically fails to be fully naturalistic (Worrall [1999]). It is at least in part *a priori*. Without assuming the inductive meta-methodological principle on an *a priori* basis, Laudan’s own normative naturalism must break down. For otherwise it would be unable to provide normative force for the reliable methods of science. The normative force of a rule of method does not come from the empirical facts about the past reliability of the method. Rather, it comes from the empirical facts of past reliability combined with the *a priori* principle that past reliability is an indicator of future reliability. So, Worrall concludes, it’s not possible for a theory of the normative nature of rules of method to be a completely naturalistic one. It can be empirical in part. But, to the extent that it is normative, it must also be *a priori*.

V. PARTICULARISM, NATURALISM AND THE META-INDUCTIVE PRINCIPLE

In my view, the issue of epistemic normativity may be resolved by drawing upon three key elements of the views of Lakatos, Laudan and Worrall that I have just surveyed.

Lakatos’s value judgements and Laudan’s pre-analytic intuitions reflect the particularist insight that we have a better grasp of concrete cases than we do of general principles (Lakatos [1978], p. 124, Laudan [1977], p. 160). Just think of the example used by G.E. Moore in his proof of the external world. We know that “Here is one hand and here is another”. We know such facts as these even though we may not know what, if any, general epistemological principles underlie such commonsense knowledge of particular facts.

The particularist insight may be combined with Laudan’s normative naturalist view of the warrant of rules of method.[[3]](#footnote-3) Because we are able to have knowledge of particular facts, it is possible to evaluate the reliability of rules of method. On the basis of knowledge of particular facts we are able to tell whether use of a given method of inquiry yields knowledge of such facts. For we are able to determine whether the result of using a given method is indeed an item or items of knowledge. Similarly, it may be determined whether one method leads to knowledge more reliably than another method. In this way, it is possible to avoid epistemic relativism because there is no need to allow that one method is as good as any other from the point of view of epistemic reliability.[[4]](#footnote-4)

Such a naturalistic account of the reliability of methods may well require a meta-inductive principle. For if, as the naturalist suggests, we are to learn from experience how best to conduct inquiry, then surely what we have learned in the past must be applicable to the future. It may well be, as Worrall suggests, that this meta-inductive principle must be adopted on an *a priori* basis. At the very least, it seems clear that we are unable to learn from experience if we cannot apply lessons from the past to the future. If this is an *a priori* point, then so be it. It is no great departure from naturalized epistemology to allow that some principles of inquiry have an *a priori* basis. Still, I would myself be inclined to a pragmatic and hence *a posteriori* justification of the meta-inductive principle that is required as the basis of epistemic normativity.[[5]](#footnote-5) But we may agree that such a principle is required without resolving the issue of how it is to be grounded.

VI. CONCLUDING REMARKS

I will now return to my main theme and state the point that I hope to have illustrated in the above discussion.

The history of science may raise a great many philosophical questions. It can raise questions about the method of science and it is relevant to normative questions about the justification of rules of method. But history of science, by itself, does not answer these questions. Philosophical reflection upon the nature of epistemic justification is necessary in order to extract appropriate epistemological morals from the history of science.

This point is not restricted to the inability to derive normative claims directly from empirical facts about the history of science. In areas which are not obviously normative in character, such as the problem of semantic change and the debate about scientific realism, the history of science has great relevance. But the history of science is unable to resolve the disputes about these problems in the absence of philosophical argument and analysis.

In offering these remarks about the relevance of the history to the philosophy of science, I have been speaking primarily about the area of the philosophy of science that is these days known as general philosophy of science. This is the area of the philosophy of science that addresses general philosophical questions about the nature of science, such as questions about the normative aspects of scientific method, or the debate about incommensurability or scientific realism. But it seems clear that similar remarks would also apply to philosophy of specific sciences (e.g. biology or physics), since such field-specific philosophy of science addresses questions that arise within the context of specific fields of science. Of course, in the philosophy of specific sciences pressure may be felt to come to grips with contemporary research in an area of science, rather than just the history of that science.

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1. The quotes from Carnap’s letter to Kuhn are from Reisch ([1991], pp. 266-7). [↑](#footnote-ref-1)
2. On meaning variance in Carnap [1956], see English ([1978], p. 65) and Reisch ([1991], p. 270). [↑](#footnote-ref-2)
3. For details of how the particularist position may be combined with normative naturalism as antidote to epistemic relativism, see my [2010]. [↑](#footnote-ref-3)
4. An added benefit of such a particularist approach to epistemological matters is that it can be used as the basis for a response to the Pyrrhonian sceptical problem of the criterion, as well as to forms of epistemic relativism based on the problem of the criterion. See my [2010] for further discussion of this approach. [↑](#footnote-ref-4)
5. For an example of a pragmatic justification of such a meta-inductive principle, see Rescher [1977], p. 34. [↑](#footnote-ref-5)