KUHN AND THE CONTEXT OF JUSTIFICATION

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In the introduction to *The Structure of Scientific Revolutions*, T. S. Kuhn maintains that the distinction between the contexts of discovery and justification is "extraordinarily problematic" when applied to actual situations in scientific development.[1] Kuhn adds that adoption of this distinction is a commitment to a certain kind of theory which includes "substantive answers to the questions upon which they have been deployed" (p. 9). Kuhn infers that the distinction is not as noncommital as philosophers have thought, and is as questionable as any theoretical claim (p. 9).

I believe that Wesley Salmon is correct in arguing that theses like Kuhn's rest on a confusion, or a misinterpretation of the distinction in question. However, Salmon does not analyze any of Kuhn's arguments, as his interest is in arguing for a Bayesian theory of confirmation.[2] Insofar as Kuhn's position and others like his have gained some currency and depend to some extent on the confusion just mentioned, I intend to criticize Kuhn's views on the context of justification. I shall add that it is this confusion on Kuhn's part that is responsible for a contradiction and other confusions in the book. Specifically, I shall argue that it is responsible for Kuhn's belief that he has replaced the problems of confirmation and disconfirmation with analyses of the history of science.

Unfortunately, I cannot argue for the contentions of this paper straightaway, as Kuhn's thesis must be clarified. To what sort of theory does the distinction between the contexts of discovery and justification supposedly commit us? How is Kuhn's attention to and analysis of the history of science incompatible with the sort of theory to which we are committed by accepting the distinction? What, in the first place, is the distinction between the "context of discovery" and the "context of justification"?

I think that Salmon's characterization suffices as a beginning: "The terms were introduced by Hans Reichenbach to distinguish the social and psychological facts surrounding the discovery of a scientific hypothesis from the evidential considerations relevant to its justification" (p. 68). How, then, does Kuhn think that he has refuted the distinction between evidence for (or against) a theory (or hypothesis) and the circumstances surrounding its discovery?

When he attacks the distinction, Kuhn notes that he may seem to have "violated" it "in the preceding paragraph" (p. 8). The only claim that could be construed in that way is the contention that the study of competition between old and new scientific traditions should *replace* the philosophical discussion of confirmation and falsification (Kuhn, p. 8). In what way does this contention violate the distinction between the contexts of discovery and justification? How does the study of scientific revolutions supplant theses about confirmation and falsification? Kuhn refers us to section 12 for answers to these questions.

There Kuhn argues against various theories of confirmation and falsification. Noting that philosophers no longer demand that theories be *verified*, he turns to "probabilistic verification" (confirmation) theories (p. 145). He rejects those theories on the grounds that they have recourse to a neutral observation language which, according to Kuhn, is impossible (pp. 145–6). The language that a scientist uses is a function of the paradigm he accepts, and there are such forbidding problems of translation from one paradigm language into another that scientists who accept opposing theories always talk somewhat at cross-purposes (Kuhn, pp. 148–9).

Kuhn rejects Popper's emphasis on the role of falsification in science, too, as Popper's thesis implies that scientific practice is not rational. If any and every failure of a theory to fit the evidence were grounds for theory rejection, all theories ought to be rejected at all times (Kuhn, p. 146). For no theory fits all the facts; and, as Kuhn has argued at length, scientists continue to work with a theory despite anamolous results. Surely scientists have not been (nor will they be) irrational in not rejecting a theory when the fit between fact and theory is not perfect. In order to align his theory with scientific practice, then, Popper would have to introduce some criterion of "degrees of falsification" (disconfirmation), such that scientists would not be obliged to reject a theory unless it were highly improbable (Kuhn, p. 147). But then Popper would be subject to the same objections that Kuhn made against probabilistic theories of confirmation (Kuhn, p. 147). These are, presumably, the sorts of theories to which we are committed by accepting the distinction between the two contexts.

These ahistorical approaches to the problems of confirmation and disconfirmation (in terms of the context of justification) fail to fit the history of science. Scientists do not reject theories every time there is an anomalous result. Nor do they have recourse to a neutral "observation language" in which to couch the phenomena to be explained or their theories. When we turn to the facts themselves, we find that scientists who adhere to differing paradigms live in different "worlds"; their paradigms are, in a word, "incommensurate" (Kuhn, p. 150).

Rather than talking about degrees of confirmation or improbability, we must see the competition between paradigms in a different light.

I would argue, rather, that in these matters neither proof nor error is at issue. The transfer of allegiance from paradigm to paradigm is a con-

version experience that cannot be forced. *Lifelong* resistance, particularly from those whose productive careers have committed them to an older tradition of normal science, is *not* a violation of scientific standards but an index to the nature of scientific research itself (Kuhn, p. 151; emphases mine).

If lifelong resistance *cannot* be called unscientific, then we can no longer speak in terms of confirmation and improbability, for that is simply not at issue. Rather, we must describe those features of new paradigms that in fact *persuade* scientists to accept it. In this way, according to Kuhn, the study of the history of science supplants the traditional problems of how a theory is confirmed or shown to be improbable. But how does this thesis violate the distinction between the contexts of discovery and justification?

In part, I believe that the thesis does violate the distinction. I think that this is a result of confusion on Kuhn's part. But in one place, Kuhn, I think, believes that he is rejecting the distinction when he is not. [To be fair to Kuhn, I should note that he never says explicitly that he is rejecting the distinction in question by criticizing Popper. What he says in the introduction, however, suggests that he thinks this.] I shall spell out why I think he mistakenly believes he has rejected the distinction first.

Let us examine his rejection of Popper's thesis on the grounds that it does not square with the historical data. This could be an instance of rejecting the distinction between the contexts of discovery and justification (as Kuhn maintains on page 8) only if one believes that the distinction "marks the boundaries between history of science and philosophy of science" (Salmon, p. 68). I think that Salmon's arguments show that it does not.

Because Reichenbach "described the transition from the context of discovery to the context of justification in terms of a *rational reconstruction*," some philosophers have inferred that philosophers are not concerned with the process by which hypotheses and theories are discovered (Salmon, p. 69). The nonrational or irrational process by which the scientist arrives at his hypothesis are psychologically relevant, but of no interest to a philosopher, who is interested solely in the relationship between the evidence and the conclusion (Salmon, p. 70). He is interested in the logical (and thus, atemporal) relationship between the evidence and hypothesis, which presupposes that the process has been reconstructed rationally. Thus, the study of the history of science and rationally reconstructed processes are mutually exclusive.

Like Salmon, I "believe that the distinction between the context of discovery and the context of justification is viable, significant, and fundamental to the philosophy of science. I do not believe, however, that this view commits me to an intellectual divorce from my historical colleagues . . ." (p. 70). Scientists do not, after all, think wholly irrationally in discovering hypotheses. A scientist may observe and gather evidence in arriving at a hypothesis; he finds that evidence persuasive. Here, one and the same thing is both psychologically and logically relevant to the hypothesis. We must distinguish this factor from, say, certain religious beliefs that may have persuaded a scientist to accept an hypothesis. That falls squarely within the context of discovery only, and is not logically relevant. But there is no reason for saying that one and the same thing cannot be both psychologically and logically relevant (Salmon, p. 73). So the contexts of discovery and justification are not distinguished in terms of subject matter. Scientists themselves produce finished research reports, which constitute *their* rational reconstruction of the process of discovery (Salmon, p. 71). Scientists make logical inferences in arriving at their hypotheses, which is of interest to philosophers (Salmon, p. 71).

Despite the fact that the contexts of discovery and justification overlap, it is important to keep the two distinct. However, as Salmon notes, "even if the argument seems compelling to an entire scientific community, it may still be logically faulty. The convincing character of an argument is quite distinct from its validity; the former is a psychological characteristic, the latter is *logical*" (p. 73). So the two contexts intersect. We can adopt the distinction without ignoring the history of science.

How does this help us to assess Kuhn's arguments? His criticism of Popper can be accepted without rejecting the distinction between the contexts of discovery and justification. If Kuhn's criticism of Popper's theory of falsification is sound, it implies that (on Popper's view) scientists have not been rational throughout the centuries (Kuhn, p. 146). This, however, is quite similar to a hypothetical case that Salmon discusses: "If a philosopher expounds a theory of the logical structure of science according to which almost all of modern physical science is methodologically unsound, it would be far more reasonable to conclude that the philosophical reasoning has gone astray than to suppose that modern science is logically misconceived" (p. 73). This does not mean rejecting the rule of inference *modus tollens*. Rather, it means that the process of disconfirmation of theories and hypotheses is not as simple as the relationship between antecedent and consequent.

This defense of the distinction between the contexts of discovery and justification allows us to say that Kuhn has made a mistake in another argument. There Kuhn infers that "scientists who resist for a lifetime are not violating scientific standards" from the fact that "a transfer of allegiance to another paradigm is a conversion experience that cannot be forced" (p. 151). But that is a non sequitur that violates the distinction between the contexts of discovery and justification. The fact that someone is not con-

vinced does *not* imply that he is *entitled* to be unconvinced. The fact that someone is not convinced is a psychological factor. Whether he is *entitled* to be so is a logical one. Thus it is not the case that we cannot say that someone who resists throughout his life is being unreasonable, provided that we have good reasons for saying so. Kuhn's thesis violates the distinction in the following way. If we must limit ourselves to what persuades a scientific community and cannot say that the holdouts (or advocates of the new paradigm, for that matter) are mistaken (Kuhn, p. 159), then either there is no context of justification or the contexts of discovery and justification are identical.

It may be objected that I have misrepresented Kuhn here, emphasizing a subjectivistic reading. Granted. Kuhn contradicts himself: "Counter-arguments are, in any case, always available, and no rules prescribe how the balance must be struck. Nevertheless, as argument piles on argument and as challenge after challenge is successfully met, *only blind stubbornness* can at the end account for continued resistance" (p. 204, emphasis mine). If the scientists who hold out for their entire lives in spite of the evidence are "blindly stubborn" in continuing to resist, then they *are* violating the rational standards of science. This, however, does not square with what Kuhn said earlier (p. 151).

Far from being "extraordinarily problematic" in its application to historical situations, the distinction between the context of discovery and the context of justification helps to elucidate it. Kuhn, in fact, uses it as such. It allows him (and us) to distinguish between the fact that a few scientists are not persuaded and whether they are justified in not being persuaded. Generally, it allows us to distinguish between nonevidential factors that may be operative and evidence for the theory.

> In dealing with any significant case, say the replacement of an old theory by a new hypothesis, the historian will be deeply interested in such questions as whether, to what extent, and in what manner the old theory has been disconfirmed; and similarly, what evidence is offered in support of the new hypothesis, and how adequate it is. . . . Since science aspires to provide objective knowledge of the world, it cannot be understood historically without taking seriously the role of evidence in scientific development and change. Such historical judgements—whether a particular historical development was or was not rationally justified on the basis of the evidence available at the time—depend crucially upon the historian's understanding of the logic of confirmation and disconfirmation (Salmon, pp. 74–5).

In fact, Kuhn makes these sorts of judgements. When he turns to the history of science, we find Kuhn saying that "it makes a great deal of

sense to ask which of two actual and competing theories fits the facts *better*" (p. 147). But this reintroduces the concept of degrees of confirmation and disconfirmation. Thus, Kuhn apparently did not mean to object to *all* probabilistic theories of confirmation and disconfirmation (p. 145).

Evidently, saying that different paradigms are "incommensurate" is insufficient. Despite the problems in translation to which Kuhn draws our attention, a good deal of the indeterminacy must be overcome in order to judge which theory is better on rational grounds. So a traditional problem in the philosophy of science arises: "How can one translate from one theory into another in order to be able to judge rationally which theory is better?"

Kuhn's criticism of the concept of degrees of falsification is odd, particularly since his own schematic presupposes it. A paradigm in a state of crisis is, after all, a theory which is taken to be highly improbable. A philosopher wants to know what the grounds are for saying that a theory is improbable, whether they are good grounds, and whether the proposed theory of disconfirmation does, in fact, fit accepted scientific practice.

One final point. When Kuhn begins to describe the general features of paradigm shifts, he mentions such things as the ability of the new paradigm to solve (or dissolve) the old paradigm's problems and greater quantitative precision in the new paradigm. Here the contexts of discovery and justification overlap once again. A philosopher can rationally reconstruct these historical incidents and ask questions about the rationality of the scientific enterprise. This is, as I have shown, also the sort of question that a historian of science wants to and does ask.

Several conclusions emerge from these arguments. First, Kuhn's challenge of the distinction between the context of discovery and the context of justification fails because it rests on the mistaken notion that the distinction marks the boundaries between history of science and philosophy of science. Quite the contrary, we have seen that philosophies about scientific practice can be tested by historical cases without violating the distinction. Second, Kuhn's criticisms of all theories of confirmation and disconfirmation are inconsistent with his own program because his analysis presupposes some such position. It is unfortunate that Kuhn merely "presupposes" the concepts of confirmation and disconfirmation. The theorists whom he criticizes have the advantage of trying, at least, to obtain a clear understanding of confirmation and disconfirmation. I have argued, to some extent, that some of the problems posed by "reconstructionist" philosophers of science are legitimate. Adopting the distinction between the contexts of discovery and justification does not, of course, commit us to any particular theory of confirmation or disconfirmation. Rather, it commits us only to certain problems in the philosophy of science. So the distinction *is*

an "elementary methodological distinction," despite Kuhn's contrary belief (p. 9).

If I were to characterize Kuhn's intended position on the legitimacy of trying to theorize about confirmation and disconfirmation, and the usefulness of the distinction between the contexts of discovery and justification, I would say that it is basically along the lines of Carnap's interpretation. Here as elsewhere, however, Kuhn is given to subjectivistic excesses at points. I have criticized him for a couple of his subjectivistic statements.

NOTES

1. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970), p. 9. Henceforth cited in the text.

2. Wesley C. Salmon. "Bayes's Theorem and the History of Science." *Minnesota Studies in the Philosophy of Science*, V (Minneapolis: University of Minnesota Press, 1970), pp. 68–86. Henceforth cited in the text.

3. Salmon, p. 74. I say "allows us," not "guarantees."