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## WHAT CAN THE THEORY OF KNOWLEDGE LEARN FROM THE HISTORY OF KNOWLEDGE?

In recent years, philosophers of science have been increasingly concerned with questions about scientific change, and, in connection with those concerns, to rest their claims more and more on an examination of cases in the history of science. During the 1960s and early 1970s, those concerns tended to revolve around the question of *whether* scientific change, or at least major scientific change, is or is not "rational." It seems to me, as I shall argue in what follows, that that question is misguided in principle, at least as it is usually understood, and that it calls attention away from the most important and potentially most fruitful problems about the nature of scientific change. Furthermore, I believe, and will argue below, that the most fundamental reasons for investigating scientific change, and the sense in which and degree to which it is necessary to base such investigation on an examination of the history of science (as well as of contemporary science) have not been adequately grasped even by many who are sympathetic to the approach. Finally, I do not believe that the difficulties in the way of such an approach have been properly appreciated and taken account of, and in many cases have not been considered at all. In particular, I will examine here five basic objections or types of objections against the view that the philosopher of science, in attempting to understand the nature of science, must examine the rationale of scientific development and innovation, and must base that examination on a study of cases from the history of science (as well as from contemporary science).

My plan will be to begin by considering the first of these objections, as that discussion will place the remainder of the paper in a more general context of twentieth-century philosophy of science, and indeed of philosophy generally. This discussion will lead into a presentation of a view of the philosophy of science in terms of which the remaining four objections can be examined.

### I

The first objection is based on the distinction, made by logical empiricists among others, between "scientific terms"—terms occurring "within science," like 'force', 'mass', 'acceleration', 'catalyst', 'gene',

'superego'—and "metascientific terms"—terms like 'law', 'theory', 'hypothesis', 'explanation', 'confirmation', 'evidence', and 'observation', used in "talking about science," in characterizing scientific terms, statements, and activities. Thus "' $F=ma$ ' is a scientific law" is a statement predicating the metascientific term '(scientific) law' of the scientific statement ' $F=ma$ '. This distinction was frequently employed to define the central (though of course not the only) task of the philosophy of science: to understand science, in the sense in which the philosopher wishes to understand it, would be to understand what is involved in saying that a certain scientific proposition or argument is a law, theory, explanation, etc. More generally, philosophy of science was conceived as being concerned fundamentally with the "meanings" of metascientific terms or concepts.

For present purposes, the most important point about this view is the idea that, although the science of any given period should, insofar as it is science, exemplify (at least fairly closely)<sup>1</sup> the results of this metascientific inquiry, nevertheless *the analysis of the meanings of metascientific concepts does not depend on analysis of the content of science, in the sense that those meanings are not a function of that content, changing or evolving with the changes or evolution of the specific concepts, propositions, and arguments accepted or employed in science.* No matter how our explanatory theories about the world change, and no matter how well confirmed those theories might be, the meanings of such terms as 'explanation', 'theory', and 'confirmation', as well as the totality of other metascientific terms which together constitute the concept of 'science', will remain unaltered. What the philosopher of science is interested in are, for example, the characteristics that make *any* particular body of propositions, at *any* given period, a "(scientific) theory." Philosophy of science is thus concerned with the invariant characteristics of any possible theory, with the very "concept" of a theory (the very meaning of 'theory').

This view of the aim of the philosophy of science was, among logical empiricists, closely associated in practice with another view. Since philosophy of science was concerned with the concept of theory, with the characteristics of any possible theory, and since, according to the influential *Tractatus Logico-Philosophicus*, logic was the study of the total range of possibilities, it followed that the appropriate tool for the analysis of metascientific concepts is logic.<sup>2</sup> Again, the implication was, or was taken to be, that the "content" of science can be safely ignored, and only the "form"—the "logical form"—of scientific reasoning examined.

The logical empiricist conception of the central aim of the philosophy of science, reinforced by the tools of modern logic, thus rested on the assumption that the content of science in no way affects the meanings of metascien-

tific terms; that is, it rested on the sharpness of the science-metascience distinction. And as a corollary of this assumption the historical changes which have taken place in the body of scientific concepts and beliefs were also irrelevant: the meanings which are the objects of the philosopher's scrutiny have an eternal ring about them, independent of the vicissitudes of ongoing science, the comings and goings of particular scientific ideas.

The explosive development of the history of science in the 1950s and 1960s raised serious doubts about this assumption and its corollary. In order to understand these doubts, let us suppose for the moment that the "meaning" of a metascientific term in some way determines the range of possible applications of that term; and let us express this by saying that there is some relation (perhaps of identity) between the meaning of the term and the criteria (not necessarily conscious or explicit) according to which it is applied to determine what can count as, for example, an explanation or a theory. The new discoveries in the history of science then appeared to many to indicate (a) that what counts as a legitimate scientific theory or explanation (etc.) at one stage of the development of science often differs, even radically, from what counts as such at another stage; (b) that the differing sets of criteria of application employed at the different stages could not—any of them, according to the more extreme views—be praised or condemned as more or less "rational" or "correct" than any of the others; and (c) that the criteria accepted at a given stage are intimately linked to the content of scientific belief at that stage. This last allegation seems to have been interpreted by these critics of logical empiricism as a claim that the criteria of what counts as (e.g.) an explanation at a given stage not only mark out the range of *possible* explanations, but even that they imply or strongly suggest the explanations that are *accepted* as correct at that stage.

The relativism consequent on this interpretation of (c), together with (a) and (b), is by now familiar. It was far too hasty and extreme a reaction to the logical empiricist view. For, even ignoring all the other objections that have been raised against the relativistic view,<sup>3</sup> the revelations of the history of science to which those critics of logical empiricism appealed could equally well have suggested a very different interpretation of (c), namely that *although the meaning of 'explanation' (or the criteria of what can count as an explanation) do, at any given stage, mark out a range of possible explanations, nevertheless the knowledge attained—the set of explanations which come to be accepted from among those possibilities—lead, at least under certain circumstances, to a change in the criteria themselves.* The "meanings" or "criteria of application" are not independent of scientific beliefs (as the logical empiricists held), nor do they imply them (as their relativistic critics maintained); the meanings or criteria of application of the

metascientific terms are connected to the substantive scientific beliefs by what might, for the sake of brevity, be called a rational feedback mechanism (itself perhaps subject to reform in the light of new information which it itself helps reveal). "Meanings" or "criteria of application" determine ranges of possibilities; they themselves are subject to revision in the light of the possibilities which (on the basis of other criteria) come to be accepted as correct.<sup>4</sup>

This alternative view of (c) is compatible with acceptance of (b) and (c), if they are suitably interpreted. As to (a), what counts as a legitimate scientific theory or explanation at one stage could—accepting the alternative view of (c)—often differ, even radically, from what counts as such at another stage, though the notion of "radical difference" would have to be divorced from the infamous "incommensurability" doctrine of the 1960s: for there might now be a chain of reasoning connecting the two different sets of criteria, a chain through which a rational evolution could be traced between the two. And as to (b), we could recognize that, *given* the knowledge and criteria available at a particular time, certain beliefs about possibilities and truth *were* reasonable, even though alteration and improvement might be possible with the acceptance (even on the criteria current at that stage) of new beliefs.

Such a view would involve a denial of such absolute bifurcations as those between meaning and truth, analytic and empirical, science and metascience, method and its application: all would be subject to evolution along with the development of new knowledge. It would affirm that there are *no* concepts or beliefs that are immune to revision in the face of new knowledge: even levels of description as general (even "categorical") as those employing such concepts as "particle" and "entity" would be subject to revision or abandonment. Assumptions underlying much of the philosophical tradition stemming from Plato and Kant would thus be rejected. The view would imply that we learn *what* "knowledge" is *as* we attain knowledge, that we learn *how* to learn in the process of learning.

Such a view would itself, of course, have to contend with a number of objections. The most obvious, perhaps, is that it seems hard to see how criteria of rationality could be said to "evolve rationally" with the developing content of science unless there are higher-level, content-transcending criteria by which changes in criteria of rationality could themselves be said to be "rational"; thus we would be threatened again with collapse into either a "metascientific" or a "relativistic" approach. I do not think this objection is insurmountable; but the purposes of the present paper do not require that the view be defended, or its limitations shown, or its consequences elaborated. The important point, for present purposes, is that it constitutes a possibility

which was not envisaged by either the logical empiricists or their critics (the former having also failed to allow for the possibility of the view presented by the latter). And for now the point of interest is the failure, in this regard, of logical empiricism: both the relativistic view and the alternative to it just outlined reject the logical empiricist conception of the aims of the philosophy of science as having to do with analysis of "metascientific" concepts; both imply that the alleged "eternal," content-transcending character of so-called metascientific concepts is a myth.

Much could have been said in defense of the "metascientific" approach: it could have been admitted, for example (but was not), that criteria of being an explanation (and so forth) vary from period to period; and it could have been agreed that these changes are dependent in some way on the changing content of science. But it could further have been argued that the philosopher of science is concerned with yet a *higher level* of criteria, namely *the criteria by which one identifies what are, in different particular traditions or at different stages of science, criteria for being an explanation (etc.)*. These second-level criteria might now have been said to be invariant, and have been called "meanings," as opposed to the transient and evolving first-level "criteria of application." And so it could have been alleged that the critics had confused meaning and criteria of application, and it could have been reaffirmed that philosophy of science is concerned with meanings, in the sense now proposed, rather than with criteria of application. Indeed, the relativistic critics of logical empiricism might have been saddled with the charge that a claim like "The criteria for being an explanation at stage A are incommensurable with the criteria for being an explanation at stage B" presupposes what those critics seemed to be denying, namely, the existence of content-transcending criteria by which certain statements in the two traditions or stages could be identified by the historian as "criteria for being an explanation."

Numerous other such tacks might have been taken; but though they might have refined the logical empiricist conception of the aims of the philosophy of science, they would not have dealt with the fundamental issue. For example, if the specific defense of logical empiricism just suggested had been taken, then (1) we would still have to ask why philosophy of science should not be concerned with "criteria of application" as well as with "meanings." Indeed, it could be argued even more pointedly that, if we are to understand scientific reasoning, it is particularly the lower-level criteria—the criteria employed by scientists themselves in determining legitimate moves to make in raising and meeting their problems—that should be our central concern, and that attention to the "eternal" (logic, "meanings") was just what

made philosophy of science in the logical empiricist vein seem so irrelevant to real science. But the issues are deeper still; for (2) the further question might have been raised as to whether the "second-level" criteria—the "meanings," on the suggested interpretation—themselves might not alter under certain circumstances with changes in the content of scientific belief and with changes in the ("first level") criteria of what can count as (e.g.) an explanation.<sup>5</sup>

The fundamental issue between the logical empiricist interpretation of the aims of the philosophy of science, on the one hand, and, on the other, the relativism of the 1960s and the alternative thereto which I sketched above, concerns the relations between the alleged "metascientific" concepts (or the criteria of their application) and the content of science. Is it really true that the conditions ("criteria") of being a scientific theory (for example) are independent of the substantive beliefs of science, and so do not change with alterations in those beliefs? Is there *any* level at which there is such independence of criteria from content? If so, what is the source of those criteria? (Traditional arguments for the existence of such a "level" have tended to claim that there *must* be one; but we have learned to be suspicious of such aprioristic arguments.) And for any "levels" which are not independent, but which alter with the evolution of scientific knowledge, what are the rules (if any) and circumstances according to which such changes of criteria occur? (And are those rules and circumstances themselves subject to change, and if so how?) *And the important point is that logical empiricism, simply assuming the sharp bifurcation between the "metascientific" and the "scientific," between the analysis of "meanings" (however interpreted) and the content of science, failed even to raise these questions.* They are questions that should have been raised from the start; but now they have become pressing. Our attention has been called to presumptive evidence that scientific change penetrates deep into the levels of scientific criteria; and a thorough reappraisal of the entire question of the relations between scientific beliefs and the allegedly defining concepts of science, and the ways in which those allegedly defining concepts operate and are operated on by the evolving content of scientific belief, needs to be undertaken. (It is for this reason that a full development and defense of the alternative sketched above is unnecessary here: it need only be recognized as an alternative, not only to logical empiricism but also to the relativism in which its critics were mired.)

On this argument, the necessity of examining scientific change arises from questions about the nature of the relationship between criteria employed in the acquisition of knowledge and the knowledge itself. These questions require an attention, first, to the content of science—both the possibilities which science is willing to contemplate, and the beliefs it is will-

ing to adopt—and second, to the interaction between that content, as it evolves in the development of science, and the criteria, even at the highest levels, employed in arriving at that content. But there is another way of arriving at the same general approach, through reflection on peculiarities and problems about the character of contemporary science itself, rather than out of criticisms of philosophical views about science. I will now outline these peculiarities and problems and the approach they seem to call for.

## II

The picture of the universe offered by science today surpasses all imaginings of earlier thinkers. Space and time are held to be intimately linked, and the space-time is expanding, not in the sense that portions of matter are moving away from one another *in* space, but in the sense that the “curvature” of space-time itself is increasing. The universe is populated with objects, on the level of both the very large and the very small, which common sense and ordinary experience, or even the science of a few decades ago, could only describe as exotic, bizarre, or weird. The very distinction between matter and the space-time in which it is located is blurred and threatened even with obliteration; and at the level of the very small, the very notions of space, time, and matter, as traditionally conceived, may be inadequate. Elementary particles have properties so far from traditional and macroscopic expectations that they fully deserve to be called strange; even the terms ‘elementary’ and ‘particle’ seem not wholly appropriate at best, and the term ‘interaction’ covers cases where only one particle is involved. Distinctions like those between “particle” and “wave,” “discrete” and “continuous,” traditionally taken to be both exclusive and exhaustive of all possibilities, and perhaps even concepts as general as “entity” and “property”, cannot do justice to the world as viewed by contemporary science.

The fact that the conclusions of science are, in many cases, open to specific objections, and that they are in any case subject to revision or abandonment in the light of further developments, does not lessen the problem; on the contrary, it only makes it worse. For many of those beliefs are so extraordinary that even their possibility, their existence as alternatives to be considered, would have been wholly inconceivable, or at best self-contradictory, to even the greatest or most imaginative of earlier thinkers. And at its frontiers—but by no means beyond the limits of legitimate scientific possibility—scientists trying to deal with the conclusions and problems of their subject are willing, and in some cases even compelled, to contemplate alternatives that are still more exotic and bizarre. No amount of reflection,



however ingenious, on the range of logical alternatives could have been expected to anticipate the claims of relativistic cosmology or quantum field theory (to say nothing of the more speculative extensions and alternatives under active consideration) even as possibilities; no amount of examination, however painstaking, of our ordinary experience of the world could have been expected to lead to such conclusions or such possibilities.

Why, then, does science accept such conclusions, and why is it willing to consider alternatives so bizarre? One answer might be that the total body of evidence available makes reasonable a certain range of possibilities, and that in some cases the evidence for one alternative is strong enough to warrant its (tentative) acceptance. But when we examine the evidence put forth, we find that it is itself so unfamiliar, from the standpoint of common sense and ordinary experience, that we are only led to ask in turn why science accepts *that* sort of thing as evidence. Further patient, step-by-step explanation would then be required to delineate a series of successive departures, each made on the basis of putative reasons, from our expectations (whether founded in "common sense," ordinary experience, or whatever else), and finally evenuating in the necessity of accepting the evidence, alternatives, and beliefs of science. It would be as though we had a set of expectations or claims, with a delineation, for each expectation or claim, of a series of arguments (a "reasoning-series") leading at last to the assertions and alternatives of contemporary science. The philosopher of science would then be interested in the patterns of reasoning by which the successive departures from those expectations or claims proceed, with a view to generalizing and, if possible, systematizing those patterns.

Such reasoning-series would not necessarily make explicit reference to any views actually held in the history of science. In actuality, however, there would be considerable overlap; and where there is, the relevant historical cases almost invariably provide subtleties of reasoning that are unlikely to be found in the currently-provided reasoning-series; and those subtleties are of vast importance to the philosopher in his search for reasoning-patterns. Far from narrowing our insight by dealing only with "actual" rather than with "possible" cases, the history of science—as has been amply demonstrated by historians in the past several decades—exposes far more possibilities than we are able to generate otherwise.

Furthermore, the claims we might make in the name of "common sense" or "our ordinary experience" constitute only a small proportion of such claims that have been made. Departing from what we today might think of as common sense possibilities cannot begin to do justice to the bizarre character of contemporary science; for our "common sense" is undoubtedly

much closer to those scientific conclusions and possibilities than anything earlier thinkers might have conceived. Yet the views of those earlier thinkers, at the time of their proposal, appeared plausible or convincing to their adherents, while the beliefs constituting our "common sense" claims often would not have. Arrival at *our* expectations (however based) from such beliefs is as much a part of the knowledge-attaining process as is the transition from our expectations to the claims of contemporary science. And the philosopher interested in developing a general theory of the knowledge-acquiring process must consider the reasoning-patterns involved in the former sorts of transitions also.

Finally, such consideration becomes absolutely essential when we ask, as we must, why what seemed so convincing to earlier thinkers differs so radically from what we take to be acceptable (on either a common sensical or scientific level) today. Have we simply learned more—while the rules of learning themselves, the reasoning-patterns involved in knowledge-acquisition, have remained constant? Or have we removed obstacles and misunderstandings in the way of applying rules of learning which, once grasped, will remain henceforth immutable? Or have the reasoning-patterns embodied in the acceptance or rejection of claims about the possible and the true evolved with the development of the structure of our knowledge-claims, so that we may expect similar evolution in the future? These questions—in particular the last—require us to attend to scientific change, as manifested in the history of science.

We have thus come full circle back to the point made in Section I above: by considering the peculiarities of contemporary science and the requisites for understanding them, we have arrived at the same conclusion as we found earlier by considering certain deficiencies in recent philosophy of science. The question of why science today believes the peculiar things it does about the universe, and why it is willing to consider the alternatives it does, requires attention to the question of how science has come to think in those ways.

### III

Against the background of this discussion of the methodology of the philosophy of science, we may now return to the consideration, promised earlier, of five objections against the idea that the philosopher of science, in trying to understand the nature of science, must examine the rationale of scientific development and innovation, and must base that examination on a study of cases from the history of science.

(1) The first of these five objections, that the philosopher of science, since he is concerned with the nature of science in general, cannot concern himself with the ephemeral content of science at all, has been discussed in

Part I, above. However, we will encounter variations on the theme of that objection in the context of our examination of some of the objections to come.

(2) The second objection is a familiar one: that there is no such thing as a "logic of discovery"; there is only a "logic of justification." The processes by which ideas are arrived at in science are not "rational"; only when such ideas are already presented can canons of rational test be applied to them. The introduction of ideas in science is a matter of genius, of inspiration, of imagination, of creative originality, a matter for study by the psychologist or sociologist or historian, but not for the philosopher. Reasoning is to be equated with testing, not with creativity. A concern with the development of science can therefore be of no use to the philosopher of science.

In the past several years, a number of important distinctions have been made regarding the question of a "logic of discovery." The problem has been reformulated so as to recognize that the "reasons" involved in the introduction of new developments in science need not be conclusive ones, and that the "discoveries" involved need not be discoveries of *acceptable* ideas, but only of *plausible* ones. Given these reinterpretations, it is now clear that, while the question of why some specific person thought of an idea may not in general be a subject for philosophical investigation (though under some circumstances it might be), it is often possible to understand, against the background of the ideas and techniques available at a certain stage, why a certain idea was or would have been a reasonable one to suppose, and, further, that that idea was not only possible, but one worthy of serious consideration. That is, reasons are involved in the introduction (in the preceding sense) and serious consideration, as well as in the justification, of such new ideas in science. (Whether the reasons employed in all three of these contexts are, in all cases, of the same type, is a further question which need not be considered here.)

(3) A third major objection—one which has not been given the attention it requires—maintains that cases from past history of science, or general criteria extracted therefrom, cannot be used as supporting or counterevidence to theses about the nature of science in general, or about the nature of science as it exists today. The objection may be considered as consisting of two steps: (a) that even if consideration of cases from the history of science were necessary for the understanding of science in general or as it exists today, appeal to such cases would not be sufficient to show their relevance to such understanding; and (b) that consideration of past cases is not necessary for such understanding.

(a) Scientific ideas in the past—so the first step in this objection goes—may well have been influenced by religious, economic, social, or psychological factors, or by presuppositions which, while we would recognize

them as unambiguously "scientific," were nevertheless *employed* then in an "unscientific" way, dogmatically or even unconsciously. It may well be, for instance, that Galileo was not particularly interested in experiment, or that Kepler's "scientific" work was "inextricably intertwined" with elements that we today consider "mystical," or that Newton's interpretation of his "crucial experiment" concerning light and color presupposed, in an uncritical way that blinded him to alternatives, a particle interpretation of light. But even accepting these claims about historical cases, it does not follow that science does today, or always will, or must or should, proceed in similar ways. For it may be that we have *learned* the importance of experiment, and to distinguish "external" from genuinely "scientific" factors, and that we have become progressively more conscious and critical about fundamental assumptions. It is therefore necessary to establish not only that such claims about past science are correct, but also that they are relevant to the understanding of science in general, or that similar things hold about science today. That something was done by past science, or by past scientists, is by itself insufficient to establish that science works that way in general or today.

(b) It is also unnecessary to examine such past cases. If we want to know whether present science makes presuppositions of the sorts mentioned above, the way to find out is to examine present science; and if we wish to know what "science in general" is like, we are much more apt to find out by examining up-to-date science than by looking at cases from earlier stages in which we ought to *expect* that irrelevant features would not yet have been sorted out.

We saw earlier (and will return to the point in objection (5), below) that there is a sense in which the science of any given period can be examined "in its own terms," independently of any appeal to the science of other periods. In particular, it is possible to examine current scientific theories and uncover the reasoning involved therein. But to focus only on contemporary science would be to ignore some of the most profound problems about the nature of human knowledge. Suppose it is true that we have "learned how to do science." What would have been involved in such learning? A progressive freeing of science from hindrances in the form of external influences and unexamined assumptions, gradually exposing a scientific method which, once understood, governs the further development of science without itself needing further elaboration? Or an evolution of method itself in conjunction with an evolution of the knowledge found by that method? And in either case, what is the character of the reasoning by which such changes are brought about? *An understanding of the criteria which are employed in contemporary science will not by itself reveal answers to these questions, any more than the consideration of the science of any other particular stage will necessarily provide conclusions applicable to science at other stages or in general.* To consider

science at a particular stage, even the contemporary, is only to expose the criteria employed at that stage; but can it also give us an understanding of why we accept those criteria, and whether and how we can expect them to alter in the future? It certainly cannot if criteria evolve with the content of science—if we learn how to learn in the process of learning. And it is important to recognize that *the objection with which we are now concerned against the relevance of history of science relies on the very possibility that this may be true*: it rejects the relevance of past cases on the ground that what counts as scientific may have changed since the case in question. As I have said earlier, the present paper is not the place to defend a claim that this is in fact true, or to examine the extent to which it is true. For as long as it remains a possibility, our understanding of the knowledge-acquiring process is severely restricted by failure to investigate its possibility—that is, by limiting ourselves to criteria currently employed in that enterprise, and failing to examine the rationale by which those criteria may have evolved, and may be in the process of evolving.

In short, then, part (a) of this objection, to the effect that examination of any specific case in the history of science is an insufficient basis for more general conclusions about science, is valid. But on the one hand, the objection also holds, for the reasons given, against sole reliance on investigations of the reasoning involved in contemporary science. And on the other hand, the objection is misdirected. It is not our purpose to make generalizations about science on the basis of examination of single, isolated cases, or even, necessarily, on the basis of characteristics possessed in common by a number of cases. This is not merely because generalizations on the basis of single or few cases are always dangerous, but because the generalizations we seek are of a different sort. They have to do with the dynamics of rational change, which may make characteristics discernible at any one period of science altered or obsolete at a later stage, and therefore not suitable as bases of generalization. This is not to say that there cannot be or have not been any general characteristics of science, operating at all stages in the past; but whether there are any such, and if so the reasons why they have operated so far, and whether and why they can be expected to continue to operate, are questions that can be settled only by an investigation of science.<sup>6</sup>

Part (b) of the objection—that an investigation of the history of science is unnecessary—is valid only if we restrict ourselves to a very limited range of questions, the answers to which would leave us with a very limited understanding of the nature of science and of human knowledge. It is, as I have argued, possible to examine the science or specific areas of science of any particular period, to study its axiomatic foundations, the criteria employed in that area, at that period, in determining what are proper or fruitful areas for study,

legitimate or important problems, promising lines of research, possible and acceptable answers to those problems, and so forth.<sup>7</sup> Indeed, the possibility of such examination is presupposed by any effort to study the dynamics of rational change in science. And there are many interesting and important problems, of philosophical concern, which can be illuminated by such investigations. But there are larger questions, having to do with the status of those criteria themselves—why they are adopted, and whether they are themselves subject to change, and if so why—which require attention if we are to understand the nature of human knowledge and its acquisition, questions which require an examination of scientific development.

(4) A fourth objection is the following: any attempt to understand science on the basis of actual cases (whether historical or contemporary) must be limited to description. But a description of what *has* happened in science cannot provide a basis for any claims about what might or will or ought to happen in a future science, nor about what could have or ought to have happened in the past. But an understanding of science is necessarily concerned also with such questions.

It is true that philosophy of science must be concerned not only with what has occurred or does occur in science, but also with what could happen, and even with what should happen, in science. If philosophy of science were limited to "mere description" (assuming for the sake of argument that a sense might be given to that notion which would be appropriate in the present context), it would be little different, except perhaps for a greater penchant for generalization, from the history of science. Nevertheless, in the senses of "could" and "should" which are relevant to the kind of investigation of scientific change outlined in this paper, it is not necessary to appeal to criteria of "possibility," or to "prescriptive principles," which have no grounding in actual facts. On the contrary, *there are senses of the expressions "E could have happened" (or "E could happen") and "E should (ought to) happen (have happened)" (a) which are the ones of crucial relevance for the understanding of the rationale of scientific change, and (b) the truth-conditions for which are to be found in what has in fact happened.* In other words, a description of what actually occurs or has occurred in science can serve as the basis for an account of epistemologically relevant alternatives and prescriptions. (Conversely, there are senses of these expressions which are correspondingly irrelevant to an understanding of the rationale of scientific development: contrast, for example, the sense in which study of recombinant DNA "can" and "ought" to be pursued because biology, as a scientific field, is, so to speak, "ready" to pursue its study and the problem is an important one for the further progress of the field, with the sense in which it "ought not" be pursued because of possible dangers to society.) In defending these claims, I will in-

cidentally note that the historian of science, far from being concerned only with "mere description," is and cannot avoid being interested in questions about what "could" and "should" have happened in science, in the same senses as the philosopher of science. I will also make some observations (by no means exhaustive) about the differences between the history and the philosophy of science.

Let us first consider "E could have happened"; some of the most obvious interpretations of such statements are the following:

(i) In one sense, Thales "could have" devised the general theory of relativity, complete with all the mathematics and physics necessary for its formulation. This broad sense—call it "logical possibility," though that notion is not without its problems—has not proved very helpful in understanding the workings of science. For example, the fact that it sheds no light on the question of why general relativity was (and could have been) devised under the circumstances in which it was (and not, for example, by Thales) is a mark of its failure to come to grips with real scientific reasoning.

(ii) We also speak of what "could have happened" given the instrumentation, mathematical techniques, and physical ideas (whether accepted or not) available at the time in question. There are sometimes difficulties in applying this notion in specific cases: for example, it is sometimes not perfectly clear that, or to what extent, a certain idea was "available" at a certain time, or to a certain person or group at that time. On the other hand, there are clear cases in which such questions can be decided, or in which arguments can be given which are clearly relevant (whether decisive or not) to the question. And whatever the difficulties in formulating general criteria free of all loopholes (a fantasy in most cases), the existence of clear cases in which decisions can be made or relevant arguments presented without, or with only minimal, ambiguity, is sufficient to show that this concept has an application. And in those cases, the state of science at the time—the actual techniques and ideas available—determine a range of possibilities that "could have occurred." In this sense, we often speak of something that could have happened but did not, as having been the "natural" (expectable) thing to have occurred; at other times, we might speak of an event which, though it could have occurred, was not the reasonable or natural thing to have occurred. The historian of science is often interested in these sorts of cases (especially the former), and deals with them successfully and in an enlightening way. The philosopher of science, concerned with the reasoning which determines ranges of possible alternatives and selections from among those alternatives, is concerned even more crucially with such situations (especially the latter) than is the historian.

(iii) The preceding sense fades gradually into another, when we say that

something "could have happened" at a given stage of science in the sense that the ideas and techniques concerned could have been developed with relative ease at that time. Again, the truth-conditions of such claims lie in the actual state of science at the time.

(iv) A further sense, important in the interpretation of science, is that in which certain ideas and techniques *were not* "available" at the time, and which "could" have been developed only later, and without which a certain idea could not be defended or even expressed clearly (or at all). Such is the sense in which the Cartesian "geometrical" program for science was not a "possible" one in the seventeenth century: it "could" not even have been stated clearly (and was not by Descartes) without the later developments of Gauss and Riemann. Such also is the sense in which Newtonian cosmology was shown to be "possible" only with the work of Milne and McCrea (1934). The historian of science, though he must often be aware of this sense, in general must not allow it to interfere with his judgment of what was "possible" (senses [ii] and [iii]) at the time with which he is concerned. For the philosopher of science, interested as he is in the development of criteria of possibility, this sense is of great importance: the philosopher of science must be concerned to understand the science of a particular period "in its own terms"; but he must also be sensitive to the limitations and errors in the approach of that period, limitations and errors which might not have been perceivable except in the light of later developments. As with senses (ii) and (iii), however, employment of this sense is firmly grounded in an understanding of the actual content of science at the stages concerned.

As to "what should have happened" or "what should happen," there is again a very broad sense, of no use whatever in understanding the nature and process of knowledge-acquisition in science (whatever *other* interests might lead to raising such questions), in which the concern is with certain moralistic presuppositions in terms of which judgment of an episode, or of science as a whole, is made.<sup>8</sup> There are, however, other senses that are useful to the historian and the philosopher, senses which are tied closely to the details of actual scientific procedure. For both the historian and the philosopher, to different degrees in different circumstances, and often for different purposes, are concerned with reporting and judging, for example, all of the following: weaknesses in the reasoning by which a particular scientist or group of scientists conceived their subject matter, raised their problems, argued in favor of certain lines of research as "promising," constructed alternative possible answers to their problems, chose acceptable solutions from among those alternatives, etc. (that is, we are interested in what they "could" have said or done—in any of senses (ii)–(iv) above—had they been more accurate or careful or aware, that is, if they had been doing their business—dealing with



their own problems—well, as they, or in other cases as later scientists, conceived that business); vaguenesses or inaccuracies in their statements of premises or conclusions or arguments; presuppositions of their approaches, whether they were (or could have been) aware of them at the time; irrelevancies or other unnecessary ingredients in their arguments; consequences of their approaches which they may not have seen (and, if not, we are interested in why not). In all such judgments, formal rules of deductive or inductive logic are insufficient as criteria of what the person or group “should” have said or done; such rules must be coupled with a knowledge of the scientific situation at the time and what “could” have happened, especially in senses (ii) and (iii) above.

(5) The final objection that I will discuss here alleges that historical investigation always presupposes some point of view in the selection and interpretation of what happened in the past. Specifically, in the attempt to construct a philosophical interpretation of an episode in the history of science, the need to distinguish the “genuinely scientific” from the scientifically irrelevant in the episode requires that criteria of selection and interpretation be brought to bear. It should then occasion no surprise if the case, as thus reconstructed, appears to support philosophies of science in which those criteria are embedded. Other criteria of selection and interpretation would, however, produce a very different view of the case. Hence, because a philosophical interpretation of science must always be *presupposed*, it is impossible to *arrive at* such an interpretation through an investigation of historical cases.

Another version of this objection is that, in attempting to distinguish the “reasons” involved in an historical case from (for example) the psychological, sociological, economic, political, or religious factors involved, we cannot avoid bringing to bear on the case a conception of what is involved in something’s being a reason, even though the very purpose of our investigation is purportedly to *arrive at* (but therefore definitely not to presuppose) such a conception.

This type of argument might be called a “philosopher’s objection,” designed to demonstrate the impossibility—the “logical” impossibility (based on the “very concept” of an investigation and the role of “criteria” therein)—of all inquiries of a certain sort. In the face of such objections, we might admit that philosophical lessons cannot be gleaned from a study of actual science, historical or otherwise (for such arguments can be raised against attempts to arrive at an understanding not only of historical, but also of contemporary, cases of scientific reasoning). And we might then feel compelled to return to a view that we must formulate our interpretation of scientific reasoning in advance of, and independently of, any investigation of actual

cases of that reasoning. Such a view, a general form of what I have called the "metascientific" approach to the philosophy of science, can only eventuate in either relativism or a kind of Platonism. Such views, as I have already argued, not only ignore the strategies and tactics, the intricate maneuverings of attack and defense, that take place in the thick of the scientific battle; they also decree the impossibility that, with further victories and improvements in weaponry, not only the strategies and tactics may change, but also that corresponding changes might be wrought in what had previously been sanctified as part of the "very concept" of the scientific struggle, the very "meaning" of 'science'.

But the objection can be attacked more directly. It totally ignores the fact that inquiries into the history of science *do* take place, the results of which are clearly objective and successful, or at least more objective and successful than others. For although until fairly recently it *was* the case that most investigations of actual cases in science *did* distort those cases to fit a particular philosophical mold—a particular set of criteria of "demarcation" and interpretation of science—nevertheless in the past several decades the history of science has undergone a profound and far-reaching transformation. It is not merely that a vast quantity of new information about the history of science has been uncovered—information which has served to demonstrate the inadequacies of many earlier historical interpretations, as well as of the philosophical presuppositions or conclusions which were often associated with them. In addition, and perhaps even more importantly for present purposes, the very standards of investigation of the history of science have become far more sophisticated. Historians have become aware of the necessity of seeing scientific relevance not in terms of later science or of preconceived interpretations of science, but as it was seen by the thinker or group under investigation. And they have developed critical standards (usually, of course, implicit and uncoded) for deciding, independently of views of "what science is," whether that aim has been achieved. This is not to say that the aim *is* always achieved; but it does argue the falsity of any claim that there is no such thing as a "better" or a "worse" ("distorted") interpretation of a case in the history of science, and that there is such a thing as an interpretation based on evidence which is objective relative to philosophical theses about the nature of scientific reasoning. And a full systematic account of criteria of historical investigation which shows their objectivity (relative to the kinds of philosophical conclusions we are seeking) need not be provided for us to recognize *that* such investigation is possible.<sup>9</sup>

A number of other arguments could be advanced against the view of "meaning" and its relation to investigation which underlies the "philosopher's objections," but they may be left for another occasion. For

present purposes it is enough to remind ourselves of the healthy skepticism that has arisen in the past quarter century regarding such *a priori* "proofs" of the impossibility of some undertaking, particularly in the face of the fact that we do seem to engage in it.<sup>10</sup> In such cases, it is the philosopher's objection that should be viewed with suspicion, not the enterprise.

We may, therefore, accept, at least tentatively, the possibility of the sort of investigation advocated in this paper: to examine cases in the development of science—including, of course, contemporary science, which, as I have argued, in many respects sets our problem—with a view to arriving, for each case, at a grasp of the scientific foundations of that case, including the interrelations between substantive ideas and criteria of what counts as an area of inquiry in science, what counts as an appropriate description of the items to be examined in that area, what counts as a legitimate and important problem about that area, what counts as a promising line of research, what counts as a possible and as a correct solution to the problems of the area, and related questions. (Even if there turns out to be more than one well-founded interpretation of the case, that in itself would be an important fact to take into account.) Our investigations must, however, take us beyond such analyses, to a comparative examination of the results thereof, with a view to asking what, if anything, there is in common between such cases, or whether (and to what extent) there has been an evolution in the criteria as well as in the substantive claims of science. We will want to know, of any features or criteria which are common to all science or which have been arrived at at some comparatively late stage of science, whether those features or criteria are necessary ingredients of the scientific enterprise (at least when that enterprise has come to be understood), and if so why they are necessary (they *can* be necessary without being in any usual sense *a priori*),<sup>11</sup> or whether they may possibly be altered or abandoned or replaced in the future, and if so then under what circumstances. And finally, we will want to see whether, on the basis of these investigations of scientific cases and scientific change, a more comprehensive and systematic account of the knowledge-acquiring (or at least the rationally justified knowledge-claiming) enterprise can be obtained. In all these investigations, our results will be tentative, and subject to alteration or rejection in the face of examination of further cases or more adequate examination of the same ones.

It is sobering to realize how little has been accomplished as yet toward a general treatment of these issues. The logical empiricist tradition, I have argued, tended to ignore them; their critics of the late 1950s and 1960s tended to react to logical empiricism in ways, described above, which led to relativism, i.e., to a denial that there is any overall rational development in science. Those relativistic views have, I believe, been effectively criticized;

and, standing above all the criticisms, is the point that science is, after all, a paradigm case of the knowledge-acquiring process.<sup>12</sup> To deny that science and its development *can* be rational—a denial that seems to be the conclusion of the relativist position—fails to recognize that the terms “rational” and “knowledge” have a use. It is a condition of the adequacy of any philosophy of science that it show *how* rational change in science is possible, and a philosophy of science which, after asking *whether* scientific change can be rational, denies that it can be, must be rejected.

But in responding to the challenge of relativism, philosophers of science have, to a large extent, deviated from a concentrated attention on the above issues, focussing their attention on the question of whether science is “rational” at the expense of a conscientious examination of what has been considered rational, and of how and whether such considerations have evolved. And it is the latter sort of investigation that is of immediate importance and greatest potential fruitfulness in the attempt to understand human knowledge and human reason.

Yet once more a question arises, threatening us again with a variant of objection (4). For if our investigation is concerned with what have been *considered* to be “reasons” and “rational change,” then—despite all our analysis of the notions of possibility and prescription given in response to objection (4)—will we not, in the end, have given a merely *de facto* account of science after all? Will we not have failed to account for the normative aspect of calling science as a whole a “rational” enterprise? Again the philosophical temptation, in the face of questions like these, has all too frequently been to argue that the prescriptive canons of rationality must be laid down (and themselves justified) in advance. Again I must disagree: not until we can see, in detail, the complex workings and interactions of the variety of criteria employed in science, and the ways in which those different sorts of criteria interact, in various types of circumstances, with one another and with the substantive knowledge-claims advanced in science, and how the scientific enterprise, even in the criteria it employs (and to which many philosophers have been too quick to accord the sacrosanct and inviolable status of “meanings”), might evolve—not until then will it be possible to see what is gained in the scientific enterprise, and why those gains are advantageous. An understanding of the honorific, as well as of the “descriptive,” aspects of the concepts of rationality and knowledge should and must be the results and not the prerequisites of an investigation of the scientific enterprise. I have tried to argue also that they can be.

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## NOTES

1. The logical empiricist view was that their results were "logical reconstructions" of actual science—logical models of science which did not have to correspond exactly to real science.

2. L. Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge and Kegan Paul, 1961).

3. See, for example, I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (New York: Cambridge University Press, 1970); D. Shapere, "The Structure of Scientific Revolutions," *Philosophical Review* 73 (1964): 383–94; D. Shapere, "Meaning and Scientific Change," in *Mind and Cosmos*, ed. R. Colodny (Pittsburgh: University of Pittsburgh Press, 1966), pp. 41–85; and D. Shapere, "The Paradigm Concept," *Science* 172, 14 May 1971, pp. 706–9.

4. In speaking, in the remainder of this paper, of "criteria of application," I wish this concept to be interpreted broadly, so that any characteristics given in an alleged "definition" of a term (and to be attributed to members of its reference-class, i.e., to be possessed or exemplified by anything to which the term can be applied), as well as any characteristics assigned to all members of the reference-class but not as parts of the "meaning" or "definition" of the term, will be considered "criteria of application" of the term. (Even the requirement that the characteristics be possessed or exemplified by *all* members of the reference-class is too strong; however, I will ignore that point here.) I do not mean to imply that these characteristics are (always) appealed to in explicit and conscious tests of whether or not to apply the terms in question. These remarks may be extended to my use of the term 'criterion' in general. Part of the motivation for this broad sense of 'criterion' lies in well-known difficulties about the distinction between what is part of a "meaning" ("analytic")—and therefore, according to a long philosophical tradition, not subject to change on the basis of "factual" considerations—and what is not ("synthetic"). That distinction is not assumed in this paper, and this fact is reflected in my use of the term 'criterion' and its relation to the (or any) concept of "meaning." And the point of not assuming such a distinction is, as will be seen in the sequel, to allow for the possibility that no part of a conceptual structure is intrinsically immune to revision on the basis of reasons.

5. My point is that, even if "meanings" were entirely divorced from any notion of "criteria of application," the question would still remain open as to whether they are immune to revision in the light of changes in accepted scientific belief.

6. Even if certain types of concepts or criteria were found to be common to all science up to the present time, we would still have to ask (a) whether the basis of acceptance of those concepts or criteria ensures that they may not change in the future, and (b) whether their acceptance thus far, and promise of continued acceptance in the future, is guaranteed by considerations of "logical necessity" or by the way we have found the world to be. We must, for example, allow for the possibility that, even if the same rules of logic have been employed in all science up to the present, that need not guarantee that those rules will remain unaltered in the future. Thus, although the issue of "quantum logic" still remains controversial, the possibilities it envisions need occasion no surprise in the light of the considerations advanced in this paper: we must leave open the possibility that a close analysis of some theory (perhaps quantum theory itself) might make reasonable certain changes even in the classical rules of logic in order to accommodate successfully the domain of that theory.

7. Logical empiricism has often been criticized for its reliance on logic and axiomatization, on the ground that those devices (a) require focussing only on a "completed" version of a science, and (b) "freeze" science into a "static" mold. While it is true that logical empiricism relied too exclusively on those devices, these two criticisms are incorrect. Logical treatment and axiomatization need not be restricted to a "completed" science, but can in principle be applied to any particular stage of development; and axiomatization of an area of science at a given stage does not imply that axiomatization of that same area at a later stage might not be very different. Indeed, such axiomatizations can be of help in revealing certain aspects of the cases in question and their evolution. They cannot, however, be relied on exclusively.

8. See the remark made above regarding recombinant DNA.

9. The words "objectivity (relative to the kind of philosophical conclusions we are seeking)" contain the germ of an account of how objectivity in historical inquiry is possible: criteria of selection and interpretation of historical data need not be "loaded" with a philosophical theory of science. This suggestion is an application of the more general analysis of the concept of objectivity in science developed in another paper, "The Concept of Observation in Science and Philosophy" (unpublished).

10. This attitude is due largely to the influence of L. Wittgenstein's *Philosophical Investigations*, 3d ed. (New York: Macmillan, 1968).

11. See note 6, above.

12. Whether science exemplifies *all* the methods of knowledge-acquisition employed in human activities is a question which cannot be dealt with until its knowledge-acquiring processes are fully understood. But the point is that any general epistemological conclusions about the nature of knowledge cannot ignore the kinds of investigations of scientific reasoning and knowledge-claims outlined in this paper. This is why the title of this paper is addressed to the theory of knowledge and not merely to the philosophy of science.