

## HISTORY AND PHILOSOPHY OF SCIENCE: A MARRIAGE OF CONVENIENCE?

In a recent article, Ronald Giere has argued that the currently fashionable union of history and philosophy of science is no more than a marriage of convenience, justified like so many youthful marriages by unhappiness with the parental homes – history and philosophy – rather than by compatibility and mutual need. He allows that it *is*, for the moment at least, a convenience from the institutional standpoint to encourage the two to pair off together, but insists that this pairing has no “strong conceptual rationale.”<sup>1</sup> In a session devoted to analyzing the directions in which philosophy of science is developing, this topic would seem to merit an important place, for it can hardly be gainsaid that one of the two most obvious shifts in the philosophy of science over the past fifteen years has been its growing involvement with the history of science; the other shift is, of course, the shift away from the empiricist orthodoxies inherited from the Vienna Circle. And the two are not unconnected. If the former ought (as Dr. Giere suggests) be construed as a growing sensitivity to the realities of actual scientific procedure (whether contemporary or in the past) rather than a call upon the history of science as such, the consequences for the directing of our future efforts would be quite different from what many historicist philosophers of science have been urging upon us.

I want to argue, as I have done before,<sup>2</sup> that the relation between the two fields, is an intimate and a complex one, one that must inevitably affect the future development of both. This is not to say that all (or even most) problems in the philosophy of science require explicit reference to the history of science for their solution; nor is it to say that history of science and philosophy of science in the university context are best carried on in a single department or program. Philosophy of science is, first and foremost, part of philosophy. And what it is studying is science. It sounds as though I am agreeing with Giere’s assertion that “the primary relationships for philosophy of science are with philosophy and science,”<sup>3</sup> but I can accept this only if “science” be enlarged beyond the consideration of present theories and practice (i.e. what would be taught in a “science”

course) to include a knowledge of their historical roots as well. To put this even more concretely, my position is that some quite central issues in philosophy of science could not be properly handled solely on the basis of the sort of knowledge that a good science course would give. Among these issues, I would judge the principal ones to be: (1) theory-assessment; (2) scientific growth; (3) the ontology of theoretical entities.

In this paper, I will restrict myself, since time is short, to the first of these.<sup>4</sup> Underlying all three of them, of course, is the question of scientific rationality and how it is to be determined. If it can be shown that an adequate treatment even of the most “logical” of the three questions, i.e. that of theory-assessment, cannot avoid reference to the history of science, it seems safe to conclude that the afore-mentioned marriage of the history and philosophy of science is not just one of convenience. My strategy will be to examine the different strands in theory-assessment and show why it is that the history of science has assumed an increasing importance in their determination.

#### I. LOGICIST VERSUS HISTORICIST

But first, let me try to separate two quite different sorts of consideration that might be brought in support of a theory of science. It might rest on a series of logical intuitions in regard to the epistemic terms used, terms like ‘confirm’, ‘explain’, ‘falsify’, and so forth. Thus for example, someone might say that it is intuitively evident that if  $E$  “confirms”  $H$  (as the term ‘confirms’ is normally understood),  $E$  also “confirms”  $H'$  where  $H'$  is logically equivalent to  $H$ . It seems reasonable, they will add, that if a piece of evidence confirms an hypothesis, the same evidence will confirm any other hypothesis which is, to all intents and purposes, identical with the first. Notice the pattern here. One appeals to an intuition based on a shared language: “anyone who knows what ‘confirms’ means would say that ...” The only experience involved is the unspecific experience required to allow one to come to use the term, ‘confirms’, correctly. This need not be an experience with the technicalities of scientific method, since the term has a much wider scope. To construct a “logic of confirmation”, one has to articulate a set of intuitive principles of this kind and weave them into a single deductive system, paying close attention to the choice of logical connectives to represent such apparently innocuous terms as

'equivalent' and 'implies'. The "logician" approach (as it is often called) thus relies on our ability to articulate with certainty some very general principles governing what would count for us as, for example, confirmation. It does not require any references to instances, any induction over (say) various types of confirming case. It is assumed that these would bear out theorems derived from the intuitive first principles; if they did not, we would simply conclude that they were being incorrectly applied to the instances in question, not that the principles were wrong.

Contrasted with this is the descriptive approach to the theory of science: one begins from a survey of the actual practice of the scientist, the ways in which he tests his theories, for instance, or the degree of importance he attaches to crucial experiments. One seeks to formulate "laws", just as one does in inductive inference at first level, except that these will now be laws of method rather than laws of nature. There is, of course, one crucial difference: scientists can make mistakes, so that some instances of method may have to be discounted. Practice cannot therefore, be taken as *altogether* determinative, an immediate problem for the descriptivist program. One may be content to scrutinize contemporary practice to test one's generalizations. Or one may turn to the history of science, either as a richer source of the sort of information the contemporary scene still could in principle reveal, or perhaps as a source of information that a limited epoch (whether the present or any other moment) could never yield. This last is the "historicism" emphasis; not only must we look to the practice of science, but we have to look to it in the detail of its historical manifestation. Historicism and logicism are clearly at the opposite poles in epistemological inquiry such as ours: the historicist will be content with an account which is tentative, descriptive, subject to change; whereas the logicist will hope to enunciate principles that are exact, normative and immutable. The historicist's work is empirical; it is controlled by what he finds in the records. The logicist can lay down norms in what almost seems an *a priori* way; I say "almost" because the logicist's explication *does* depend upon experience, albeit the very general experience that may be required in order to allow him to use terms like 'confirm' and 'falsify' with assurance and exactness.

I have delayed on this distinction, familiar though it is, in order to make sure that my use later on of the terms 'logician' and 'historicism' will be unambiguous. They refer to two different, and apparently complemen-

tary, ways of warranting claims about scientific method. When they are taken to extremes, that is when the all-sufficiency of the intuitive explication or of the historical review are over-emphasized, I will speak of "logicism" or "historicism". It is clear that the logicist must make sure that his intuitions are anchored in the experience which the term he is explicating is intended to articulate; the more complex that experience is, the more he will have to rely on the help of descriptivist techniques. It is also clear that the historicist must accept the general principles of logic, and must admit too that scientists make mistakes of method on occasion in their work. "Pure" logicism or "pure" historicism in the context of the theory of science, are thus plainly untenable and of no practical concern. What we *are* interested in is the spread of the spectrum in between.

## II. THE ASSESSMENT OF SCIENTIFIC THEORY

How are scientific theories validated? How are the relative worths of two competing theories to be assessed? When scientists say of a theory that it is "confirmed" by all the available evidence, what does this amount to? There have been a variety of answers to these questions. Let me recall the three most obviously "logicist" types of response that have been given, each based on a perfectly sound logical intuition but one which in each case is manifestly far too simple to serve as sole guide in the search for a theory of theory-assessment.

1. *Deductivist*. In the classic Greek account, the warrant of a scientific statement consisted in its deductive derivability from first principles, themselves intuitive in nature and not strictly part of the science. This is the most secure of all warrants, the simplest of all modes of assessment. One finds it still glimmering like a will o' the wisp in the seventeenth century, beckoning scientists like Descartes on to fresh but unavailing efforts. The ideal it held up was unattainable, but it took centuries of unsuccessful attempts to discover the intuitive first principles, on which the entire program depended, to convince philosophers – and scientists – of this. Aristotle's analysis of an axiomatic science was not at fault; what proved wrong was his (empirical) assumption that human intuition was sufficiently powerful and nature sufficiently transparent to allow the discovery of the principles his science needed.

2. *Inductivist*. The second attempt fared a little better. Since science is the most general and best-warranted knowledge of nature, it clearly has to work from the singulars of experience to universals or laws, by means of a process of induction or generalization. From a finite set of points we get a smooth curve linking two variables in a lawlike manner. Later experiment may show a fine-structure in the curve, or may extend the curve in either direction, but within its original limits of accuracy and specification of variables, the law is a permanent acquisition, and its assessment a straightforward affair. Or so it was thought.

The main problem, of course, with this is that the element of *theory* is overlooked. If science really *did* consist of empirical laws, making use of unproblematic operationally-defined concepts, the inductivist account might suffice, although it would still have to face Hume's challenge to the logicist attempt to infer from the particular to the general. But what of the explanatory dimension of science? its hypothetical structures? its theoretical concepts? Some other mode of validation must be found for these. Once again, a single-strand logicist theory of assessment has failed.

3. *Falsificationist*. Another attractive possibility suggests itself when we reflect on the logical relationship of hypothesis and evidence. Because of its finite character, the evidence can never be sufficiently strong to prove the hypothesis. But surely it can *disprove* it? And if disproof operates in a definitive way then science can progress by successive attempted refutations and theory can be assessed in terms of its potential refutability and survival after successive severe tests. Crucial experiment then becomes the central feature of scientific method.

Of course, we know that it is nothing of the sort. But even without consulting the historian, it is possible to see why falsificationism cannot work as a general account of theory-assessment. One reason, as we all know, is that theories are not single indivisible entities; they are networks of assumptions, postulates, generalizations, many of which could be separately modified while still leaving the main theory intact. Theory refutation is not the simple *modus tollens* of the logician, then; it involves decisions about whether the modifications that would save the theory are *ad hoc* or not, whether the rival theory does not do better in setting up verified predictions, and so forth.

Furthermore, the proposed falsifying fact is never in practice quite as

“hard”, as unproblematic, as the logicist model demands. One has to take seriously the possibility that it too may have to be modified or even abandoned. But these modifications take us far away from the original insight of falsificationism;<sup>5</sup> to retain the old label, as Lakatos does, may indeed be filial but it runs the risk of making his program look an “epi-cyclic” one (as a recent critic has charged) where one complication is piled upon another in an effort to maintain an older view.<sup>6</sup> Or to use his own language, does this steadfast adherence to the falsificationist label indicate that we have here a “degenerating problem-shift”? I think not, but the way in which his methodology of research programs was originally proposed might easily lead one to suppose so.

Here then are three “pure” strategies, the deductivist, the inductivist and the falsificationist. To show that they are of themselves separately inadequate for the assessment of theories may seem a simple matter today, since it requires only the smallest familiarity with contemporary science, but it is worth recalling how hard-earned an insight it was in the seventeenth century. Indeed, one still sometimes finds scientists proposing with undiminished enthusiasm one or other of these strategies as an attractively simple account of what they are doing and what all other scientists ought to be doing.<sup>7</sup> And there is no gainsaying that scientists *do* make use of crucial experiments on occasion, and *do* accumulate thousands of detailed empirical laws. There is nothing wrong with the inductivist or the falsificationist accounts, provided that they are seen as part of a wider and much more complex scheme.

There are two other elements in this scheme that have not been mentioned so far, the use of criteria *internal* to a theory to assess its worth, and the so-called HD mode of validation. Each can be considered as a general strategy of assessment in its own right; each has, in fact, been proposed at some time or another as the main strategy for use in the assessment of scientific theories.

4. *Coherentist*. One might look at the *intrinsic* properties of the theory, such properties as coherence, elegance, simplicity, convenience. Scientists are influenced by such considerations. But why *should* they be? One can see why an inductivist or a falsificationist strategy might lead one to the truth about the world. But why should a “coherentist” one, if I may use the term, do so? Is there any reason why a simple theory is more likely

to be correct than a more complex one? Can one use words like 'correct' in the context of this sort of strategy at all? It is this sort of question that leads this sort of strategy to be called a "conventionalist" one, though, be it noted, it need not be so. If one can give reasons why a particular intrinsic feature of a theory is likely to be an index of its adequacy as an account of the real world, then one is not a conventionalist. If of two competing theories, one is more coherent than the other, in some definable sense of that term, one might well argue that the more coherent is to be preferred as more likely to prove out in the long run.

Conventionalism is not, then, a strategy of assessment, strictly speaking. Rather, it is an evaluation of all the available strategies and an assertion that in the end a partially arbitrary choice will have to be made. The conventionalist is not saying that the adoption of a scientific theory is *entirely* arbitrary; he recognizes the constraints of evidence. But he will argue that the decision as to which part of a theory a damaging fact ought lead us to modify, or which curve among the infinitely many available to us one ought to choose to represent an inductively-based law, will ultimately have to rest on some criterion internal to the theory itself, like simplicity or convenience. And this criterion cannot in turn be justified in truth-terms; it is no more than a convention, arbitrarily chosen only because *some* convention is needed in order that a theory may be formulated and defended.

I shall come back briefly to conventionalism later; as a sceptical attitude in regard to the effectiveness of the available strategies of theory-assessment, it can be overcome only by showing that these strategies are in fact able to accomplish what they purport to do. As a theory of theory-truth, it is allied with instrumentalism and positivism, and opposed to realism. I have introduced it here, let me repeat again, only because it so often crops up, in a rather confusing way, in the discussion of internal criteria of theory-assessment. A conventionalist will always fall back on such criteria, interpreted as conventions. But the use of such criteria by a scientist or theorist of science in no way implies a commitment to conventionalism.

Can these criteria be illuminated by the history of science? Yes, in several ways. First, notions like simplicity or elegance are notoriously difficult to analyze; logicist attempts to explicate them have met with very little success. To get a grip on them, there seems to be no other resource

than case-studies drawn not just from the present but from the past of science; the widest possible variety of contexts would have to be scrutinized to discover, what, for instance, the criterion of "simplicity" has amounted to in the work of men like Newton or Darwin or Einstein.

There are further questions that could also be asked. In what sorts of situation did scientists fall back on this criterion? To what extent was it a successful one? This last is the historicist question, *par excellence*. That it has not been particularly emphasized either by historians or philosophers suggests that neither party feel that the answer is likely to be affirmative. Or perhaps, the general lack of historical interest in internal criteria of theory-choice may derive from the difficulty in determining just what internal-criteria *did* affect a particular scientific research program; it will not be enough to discover the scientist using words like 'simplicity' or 'beauty'. And there will be a suspicion that these criteria are in any event secondary to those involved in successful prediction, and are justified only insofar as they reduce to the latter. So to these we now finally turn, having first investigated the deductivist, the inductivist, the falsificationist, and the coherentist strategies of assessment.

5. *Hypothetico-deductivist*. If what we are assessing are hypotheses, surely the best way to do so is to see how successful we are in deriving verified predictions from them. This advice is common in scientific literature from the time of Descartes onwards. And recent attempts to construct a logic of confirmation have taken it as their starting-point. But it is not as simple as it seems. I would like to focus on one particular difficulty, one that has come in for a lot of attention of late.<sup>8</sup> It amounts to this, that deduction and prediction are *not* equivalent. Successful prediction is a deduction which when tested observationally proves to be correct. But the notion of *test* here demands that the result not be known in advance. There is a world of difference, from the point of view of confirmation, between a theory which simply accounts for all the data in the light of which it was originally formulated, and one which predicts novel results, which when tested prove to be the case. We are much more impressed by the latter than the former. Yet from the purely logical point of view, there is no way of distinguishing them: in both cases the data are deduced from the theory, and that is all there is to be said. In his theory of confirmation, Carnap asked: to what degree is the hypothesis, *H*, confirmed by



the evidence, *e*, but he made no distinction between "evidence" known in advance (i.e. the data the theory was intended to explain) and verified predictions of novel facts, which clearly serve as "evidence" in a quite different and much stronger sense. The logical relation of deducibility cannot of itself convey this difference; a temporal analysis of some kind is needed.

### III. THEORY-ASSESSMENT AND THE HISTORY OF SCIENCE

Is this where history of science makes its appearance?<sup>9</sup> Yes and no. Once again, no particular expertise in history of science is needed to make the general point I have just made about the manner in which the verification of a prediction of something new differs from the mere deduction of something already utilized in the construction of the theory. The fact that a *temporal* relation now enters in does not, of itself, mean that history of science need be called on in some special way. Part of the problem lies with the notorious double meaning of the word, 'history'. The history of science is a body of information about the past of science; it is also that past itself. The reference to before and after which we have seen to be essential to an understanding of confirmation forces us to take confirmation to be a temporal process, one where we have to wait for the results to come in. It is thus a *historical* process too, though ordinarily not one which is spread out over a long period. But it may be sufficiently simple to allow us to formulate some more or less intuitive principles governing it; in that event, even though it is *historical*, we would not need to have recourse to the *history of science* in order to formalize it.

Would the formalization constitute a formal logic in such a case? It would be a formal system but its principles would be of a more material sort than those of deductive logic. That is, they would be limited to a very special sort of epistemic context; they would have a lesser degree of assurance than the ordinary rules of logic in the sense that it might not be quite certain how well they actually applied to the empirical process we call theory-assessment, or confirmation, in empirical science. The fact that the relation of confirmation has a temporal dimension is, of itself, no barrier to formalization or to logicity (unless one restricts the term 'logicality' to general deductive logic). Logicality and temporality are not incompatible, as Giere quite rightly points out.<sup>10</sup>

However, the issue is perhaps, not quite as open and shut as it may seem. After all, Toulmin rests much of the argument of his book, *Human Understanding*, on the claim that the temporal dimension of science (which he takes to define the rationality peculiar to science) cannot be analyzed in terms of formal structures at all; one must fall back on a sort of ecological and developmental analysis. One may well ask whether this analysis will not involve some formal structures akin to a logic; the selection among alternative conceptual-changes will certainly require a systematizable notion of assessment. One has to be careful not to fall into a category error as old as Plato and assume that formal structures cannot in principle be applied exactly to changing realities.

But suppose (as Toulmin appears to be assuming) the formal structures *themselves* are changing, i.e. suppose that the notion of theory-assessment itself has changed between Descartes' time and ours, what then? Presumably, one would no longer be able to formulate an intuitive set of principles governing such assessment, and one would have to rely not only on a descriptive account of how assessments are actually made in contemporary science but also on at least some historical detail regarding the kinds of change that have occurred in scientific modes of hypothesis-assessment and why these *did* occur. To the logicist such a suggestion is, of course, anathema. His view is that there is an unchanging set of epistemological principles underlying proof in science, as elsewhere; it is possible that these principles are not well-understood at a given time and are incorrectly applied on occasion. But insofar as they are accepted *as* principles, it is not (in his view) because they have been successfully used in the past, but because they carry intuitive weight in their own right, once they are properly understood. One is right back to Aristotle, and the theory of science of the *Posterior Analytics*. And the issue is clearly joined between logicist and historicist; to the one, the testimony of history is irrelevant, to the other, it is indispensable. If the issue be taken at *this* level, i.e. at the level of changing theories of science and not just at the level of changes in science itself, then Toulmin may have a point in his opposing of logicity and temporality. But this is not because of any inherent tension between them, only that *his* theory of science is going to come perforce from an empirical inspection of history, not from an intuitively-warranted logical system. The contrast thus reduces to the accepted one between historicism and logicism as two basically different modes of justifying theories of science.

But have we any reason to believe that notions of HD-validation *have* altered? And even if they have, might it not be that they have gradually approximated to a norm that can be more directly revealed by careful analysis of intuitive principles? Do we really want to say that our theory of theory-assessment rests on the contingency of the historical record? Let me leave these questions aside for the moment to focus on a more tractable one.

Is our intuition, informed presumably by some rather general experience with the validation of hypotheses, able to come up with a set of principles that everyone could agree on? The main problem is going to be with the word 'novel'; what constitutes a *novel* fact and how much more should its prediction count than the assimilation of previously-known facts? A novel fact is not necessarily one in the future; it is one unknown to the person formulating the theory. But suppose it is already known to others, though not to him? Or suppose he did know about it, but did not take it into account? And surely what we need is a novel *kind* of fact, not just a novel fact? Musgrave formulates a whole series of puzzles of this sort that forcefully bring home the fallibility of our intuitions in this complex area of theory-assessment. Of course, one can easily set up a postulational system of plausible principles of evidence. But will it apply to what actually goes on in science? And if it does not, which is to yield? Are our intuitions, or even the intuitions of the skilled scientist, sufficiently articulable in this domain to allow us to set forth a system that in no way needs to rely on the testimony of actual practice, contemporary or historical? I think not. One has only to see the wide divergences in the published accounts of method over the last century, ranging from Mill and Keynes, who see no virtue whatever in the claim that the prediction of a novel fact should count for more than the deduction of a fact already known on the one hand, to Popper and Lakatos at the other extreme who would hold that a theory that can claim no novel facts to its credit has no sort of confirmation (corroboration) at all.<sup>11</sup>

My conclusion is now clear: logicism will not work unaided in theories of HD-assessment. Our intuitions are simply not secure enough in this domain. They have to be instructed, and supported, and challenged by the testimony of history. It will not be enough to draw on contemporary practice; a more diverse context is needed, because theories can be of

widely-different sorts and the ways in which they can come to be accepted are quite various. This does not mean that the philosopher will have to be making constant explicit reference to history of science in his work of formal explication. Only when a question arises about the articulation of a particular axiom or principle might such a reference be needed. To the extent that clear principles can be isolated, the work of formal construction can then go on much as the rational mechanics of the eighteenth century went on, without much reference to the messy empirical world of practice.

#### IV. THE UNIT TO BE ASSESSED

So far I have assumed that the main type of appraisal that goes on in science is of *theories*; that it is in terms of theory-change that science grows; that it is in the assessment of rival theories that scientific rationality most clearly manifests itself. But there have been other proposals as to what the unit of assessment should be. One, the inductivist claim that the unit should be an empirical law of nature, we have already rejected. Another more recent one, that of Toulmin, is that variation and assessment should be considered at the level of the individual concept. He needs this assumption for his evolutionary model of scientific change to work; he rejects systematic for what he calls "populational" analysis, where the "populations" are historically-developing populations of concepts in which "mutations" occur, the more advantageous of which are selected for survival.<sup>12</sup> The main problem with this metaphor is that it makes each concept an individual competing entity; in fact, a concept would ordinarily only "compete" with those concepts proposed to fill roles similar to its own. More seriously, it assumes that conceptual variations are more or less independent of one another and can be assessed separately. But this is assuredly not the case. Scientists do not evaluate concepts in isolation from one another; they weigh up models or theories involving interconnected sets of concepts. Furthermore, when one of these concepts is altered, it frequently involves a shift across the entire network.

In short, then, Toulmin's unit is much too narrow. Though we can and should study individual concept-changes in science, it is dangerous to isolate these from the wider theoretical context in which they occur. As-

assessment and change proceed alike on a broader basis than the populational metaphor allows.

At the other end of the scale of generality is Kuhn's "paradigm", so broad and enduring that when it changes, we can talk of revolution. The ambiguity of this concept is well-known, and Kuhn himself (among others) has done something to remedy it.<sup>13</sup> But clearly the work of assessment in science goes on at a finer level than that of paradigm; indeed, the message most people took from the first edition of his *Structure of Scientific Revolutions* was that the change from one paradigm to another is not the work of assessment at all but rather more akin in its immediacy and inarticulable quality to conversion. There are some few things one can say about pressures generated by anomalies, about demands for coherence, and so forth, but, on the whole, paradigm-shift is presented as almost opaque to the methodologist's scrutiny. I am not concerned here with the merits of Kuhn's thesis, nor with the various modifications of it that have been proposed, I merely want to remark that the units to be considered in a theory of assessment in science are clearly not paradigms.

What, then, of the unit we used above, i.e. *theory*? Will not this suffice? It would, except for one thing, and this brings us to one last strand in the tangle of assessment strategies, a sixth if you have been keeping count. What counts, perhaps, most of all in favor of a theory is not just its success in prediction but what might be called its *resilience*, its ability to meet anomaly in a creative and fruitful way. This is *not* a matter of prediction, let it be stressed. It is, rather, a quality of metaphor in the theory which suggests to the scientist how its conceptual structures can be further developed to derive new results or to meet new challenges. Obviously, this is something which manifests itself only gradually over the course of time; one cannot attest to it until the theory has survived many tests and been extended in illuminating new ways. If one looks at the best-established theories of science, the kinetic theory of gases for instance, or the nuclear theory of the atom, one immediately realizes that the confidence we place in them results not merely from their successful predictions of novel facts, but at least as much from their behavior as lead-metaphors in the process of conceptual and model change over a considerable period.

Thus, if this last type of assessment strategy be accepted, our unit cannot simply be the theory, it has to be the theory as traced over the cre-

ative and competitive phase of its career. This is close to what Lakatos means by a "research program", and it is his merit to have stressed the all-important sort of theory-assessment which makes necessary the extension of our scrutiny from the theory considered as a conceptual structure at a moment of time to the theory considered in terms of its entire historical career. I do not think the term 'research program' quite captures this, though it comes closer than any other proposed so far.

My quarrel with Lakatos' theory of assessment is not over terms, however; it is over the unfortunate falsificationist residues still found in it, which impair its plausibility in numerous ways. He assumes, for instance, that the typical case of assessment occurs when two theories are in competition with one another. But what of the far commoner case when a single theory is in possession: how is *it* to be assessed? If it is not under test, must it be dismissed as *ad hoc*, as he sometimes seems to suggest? What of the well-established theories that no longer produce novel facts, that have been completely explored? Are they now in a "degenerating" stage? So great is the prejudice against inductivism of any sort, against positive confirmation, against the notion of an *established* theory, that the "research program" is still viewed rather more in terms of the rough and tumble of the arena than the creative and convincing development of a single unified metaphor through a historical series of challenges, most of them coming from anomaly or unexplained fact rather than from the pressure of competing full-fledged theories.

In particular, it leads Lakatos to an extraordinary procedure that he terms the "rational reconstruction" of history. This is one way of using the history of science I do *not* recommend.<sup>14</sup> He takes "scientific research programs" of the past and tells us how they *ought* to have been conducted. What results from this technique is not history, nor is it the use of history to warrant a theory of science. In fact, it has very little to do with history, which is being exploited only as a source of problems which can serve to illustrate how his theory of assessment works.

There is a serious ambiguity about this procedure, for the reader who works through the wealth of apparent historical detail in a Lakatos scenario might easily suppose that this detail somehow supports the epistemology he is proposing. And indeed, it often does. But Lakatos cannot acknowledge this without the spectre of justificationism threatening him. If a methodologist can learn from the experience of history, then presum-

ably a scientist can too; and Lakatos remains enough of a Popperian to regard this inference with a shudder. The entire thrust of Lakatos' acute analysis ought lead him to take history more seriously, and embrace at least a quasi-historicist view. But historicism for him is just one more form of inductivism, so that as well as the usual logicist suspicion of appeals to history, he has the additional burden of a falsificationist faith to live with.

#### V. CONCLUSION

The focus of this paper has been a dual one: the nature of theory-assessment in science, and the degree to which we are dependent on the history of science in such an investigation. Let me now summarize my conclusions. The individual strategies of theory-assessment are so complex that the logicist technique of isolating intuitively acceptable principles frequently fails. Disagreement is common, and there is no alternative to invoking the historical record. What is even more serious is that, in practice, the various strategies discussed above have to be combined with one another, and the ensuing problems of what relative weight to give to each are almost intractable, not only to the logicist epistemologist but quite frequently to the working scientist as well. How should one compare a gain in coherence to a loss in predictive power, or the elimination of an anomaly with a successful prediction of some novel facts? There are no hard-and-fast answers here; one can appeal to the intuitive skills of the working scientist or to the testimony of history but it should not be thought that a completely consistent formalism is going to emerge. In that respect, the historical record is likely to retain its importance. No one bothers to chronicle the times when *Modus Ponens* has worked; it would be a rather dull story. But historians spend prodigious energies in determining the occasions when new theories have arisen in science, and the detail of how they were appraised, confirmed, modified, rejected. Their efforts are not likely to lose their point, if what I have been arguing here is correct.

It is worth comparing first-level and second-level options here. At the first level, which is that of science itself, there have been three broadly different views as to how scientific claims about the world are warranted: they are the intuitive-deductive view sometimes called rationalism, the inductive view and the conventionalist view. Likewise, on the second

level when we ask how theories of science themselves are warranted, there are three main alternatives: logicism, historicism, and conventionalism. The balance between them has, however, shifted since intuition obviously works much better in the determination of principles of method than it does in discovering the natures of physical things; logicism is thus much more powerful in the metatheory of science than is rationalism in science.

Despite the efforts of Duhem, Poincaré and many other talented people, conventionalism ultimately fails to convince, both at the first level and especially at the second level of the scientific enterprise. It simply does not account for the history of science; it fails to make sense of it. There are really only two contenders at the second level, therefore, logicism and historicism. Neither one (if we are right) can claim to be all-sufficient. Where a valid logicist explication is available, it is preferred by the philosopher because of its intuitive and (hopefully) coercive character. But where intuitions diverge, there is no option but to turn to the historical record, not only to discover what scientists in fact *have* done, but also to sharpen the philosopher's own intuitions which (let it be said again) are formed not in a vacuum but in the context of very definite experiences of the world and of ways of dealing with the world.

The dialectic between logicist and historicist modes is of especial importance in determining whether and to what degree, the theory of assessment proposed ought be taken as normative. A logicist theory is ordinarily normative, but to the extent that history has served specifically as warrant, the claim may have to be softened. It is perhaps here above all that the issue between logicist and historicist is joined. If the case I have presented is correct, the epistemology of science is likely to continue to require the best efforts of both.

#### NOTES

<sup>1</sup> 'History and Philosophy of Science: Intimate Relationship or Marriage of Convenience', *British Journal for the Philosophy of Science* 24 (1973), 282-297; see p. 296.

<sup>2</sup> 'The History and Philosophy of Science: A Taxonomy', *Minnesota Studies in the Philosophy of Science*, Vol. 4 (ed. by R. Stuewer), pp. 12-67. Giere's essay is a detailed review of this volume, and specifically of this article.

<sup>3</sup> *Loc. cit.*

<sup>4</sup> I have touched briefly on (3) in 'History and Philosophy of Science: A Taxonomy', pp. 63-67.



<sup>5</sup> In a well-known exegesis of the Popperian canon, Lakatos distinguished between three different sorts of falsificationism, "dogmatic" (the straightforward logicist model defined above), "naive methodological" (a strongly conventionalist view), and "sophisticated methodological" (his own view, and one which has justificationist elements). See 'The Methodology of Scientific Research Programmes', *Criticism and the Growth of Knowledge* (ed. by I. Lakatos and A. Musgrave), Cambridge, 1970, pp. 91–195.

<sup>6</sup> Errol Harris, 'Epicyclic Popperism', *BJPS* 23 (1972), 55–67.

<sup>7</sup> See, for instance, J. R. Platt, 'Strong Inference: The New Baconians', *Science* 146 (1964), 347–353; P. Medawar, *The Art of the Soluble*, London 1967.

<sup>8</sup> See A Musgrave, 'Logical versus Historical Theories of Confirmation', *BJPS* 25 (1974), 1–23; E. G. Zahar, 'Why did Einstein's Programme Supersede Lorentz's?', *BJPS* 24 (1973), 95–123; 223–262.

<sup>9</sup> By calling theories of confirmation that take this temporal distinction into account "historical," Musgrave (*op. cit.*) would seem to be suggesting this, but a closer look at his well-reasoned essay shows that the principles of confirmation he discusses do not depend especially heavily on a review of historical instances of confirmation. It would have been better for him to retain his first label, 'logico-historical', because his analysis does not come down more on one side than the other.

<sup>10</sup> One section of my paper: 'Logicity and Rationality: a Comment on Toulmin's Theory of Science' (*Boston Studies* XI (1974), pp. 415–430) is devoted to this issue.

<sup>11</sup> Musgrave, *op. cit.*

<sup>12</sup> Toulmin makes the concept correspond to the individual organism (the entity in which mutations may occur and which competes with other broadly similar entities for survival). He also tends to take a "discipline" ("characterized by its own body of concepts, methods, and fundamental aims") to correspond to a species: "if intellectual disciplines comprise historically-developing populations of concepts, as organic species do of organisms, we may then consider how the interplay of innovative and selective factors maintains their characteristic unity and continuity." (*Human Understanding*, Princeton, 1972, pp. 139–141). Competition occurs both between species, and between individual members of species, thus presumably between disciplines as a whole and between individual concepts in each discipline.

<sup>13</sup> In the *Postscript* to the second edition of the *Structure of Scientific Revolutions*, Chicago, 1970.

<sup>14</sup> See §5 'History of science and some philosophers' of my 'History and Philosophy of Science: A Taxonomy.'