Philosophy of Science

March, 1977

MORE THAN A MARRIAGE OF CONVENIENCE: ON THE INEXTRICABILITY OF HISTORY AND PHILOSOPHY OF SCIENCE*

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History of science, it has been argued, has benefited philosophers of science primarily by forcing them into greater contact with "real science." In this paper I argue that additional major benefits arise from the importance of specifically historical considerations within philosophy of science. Loci for specifically historical investigations include: (1) making and evaluating rational reconstructions of particular theories and explanations, (2) estimating the degree of support earned by particular theories and theoretical claims, and (3) evaluating proposed philosophical norms for the evaluation of the degree of support for theories and the worth of explanations. More generally, I argue that theories develop and change structure with time, that (like biological species) they are historical entities. Accordingly, both the identification and the evaluation of theories are essentially historical in character.

> . . . the selective theory of evolution, as Darwin himself had stated it, required the discovery of Mendelian genetics, which of course was made. This is an example, and a most important one, of what is meant by the content of a theory, the content of an idea. . . . [A] good theory or a good idea will be much wider and much richer than even the inventor of the idea may know at his time. The theory may be judged precisely on this type of

> development, when more and more falls into its lap, even
> though it was not predictable that so much would come
> of it. ([39], pp. 15-16)

Historians and philosophers of science have recently devoted con-
siderable attention to the interplay of their two disciplines.[1] A major
stimulus for the concern with this topic is the realization that philosophy
of science has been radically reshaped in the last ten or fifteen years
due, in good part, to the pressure of historical research and to the
philosophically significant morals purportedly (though not always
simply or directly) based on that research.

There has, however, still been little explicit consideration of the
precise ways in which—and the degree to which—historical studies
ought to influence philosophers of science. This problem is the central
concern of the present article. In it I argue for three theses, two
methodological and one metamethodological: first, that in order to
correctly determine the degree of support for theoretical claims one
must often employ as input information regarding the temporal order
in which hypotheses were propounded, theories developed, and exper-
iments performed; second, that one must also employ additional,
specifically historical inputs regarding the background knowledge
against which these developments took place; and third, that historical
studies ought to play an essential role in the evaluation and revision
of current philosophical views about the logic of support. Much of
the support offered for the third thesis in the present paper grows
out of the discussion of the first two theses.[2] Accordingly, they will
occupy our attention for most of the paper.

The paper begins with an examination of the so-called "historicist"
and "logicist" reactions to the recent preoccupation with historical
materials. After clarifying certain epistemological and methodological
issues, I review the variety of factors considered by philosophers
to be relevant to the "support," "justification," or "confirmation"
of theories and theoretical claims. The details of the logic of support
are not at issue; the concern is, rather, to delineate the types of
information considered relevant in estimating the evidential support
for theories and theoretical claims. Even so, I shall show that

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[1]Cf., e.g., [60]; the symposium "History of Science and its Rational Reconstructions"
in [3]; [17]; and [36]. Much of the work by and about Feyerabend, Hanson, Kuhn,
Lakatos, and Toulmin is significant for the present topic.
   [2]My metamethodological thesis is a corollary of a stronger one for which Dudley
Shapere has argued in [56] (cf. also [42]), to wit, that the logic of support itself
changes as a function of the changes in theoretical knowledge. My argument, however,
does not depend on the correctness of Shapere's controversial claim.

specifically historical considerations affect the determination of the degree of support for a given theory at a given time and that historical studies are of considerable importance in evaluating philosophical claims about the logic of support.

1. Logicism and Historicism: Two Opposed Ideal Types of Philosophical Reaction to Historical Considerations. In [2], S. Brush reviews the manner in which recent studies of scientific history tend to subvert traditional methodological pieties concerning the objectivity and impartiality of scientific methods and institutions. He shows how the history of science may be used to challenge the supposedly truth-seeking character of science. In particular, Brush notes widespread challenges to the impartiality and objectivity of individual scientists and scientific institutions, to the logical rigor of (historically) decisive reasoning in science, to the degree of control exercised by experiment and observation on the choice, verification, falsification, and assessment of scientific theories (e.g., [2], p. 1169), etc. In short, he suggests that effective theoretical and explanatory reasoning does not conform at all well to familiar models of scientific reasoning such as the hypothetico-deductive model,[3] and that experimental verification and falsification in real historical cases is seldom so straightforward and decisive as popular philosophical and scientific mythologies would have it. He illustrates his claims with a number of examples from the development of physics.[4]

Brush's claims represent a significant, though controversial, tendency in the recent work of professional historians of science. And his claims certainly represent historical views which have had considerable influence on some philosophers of science. It is not, however, his historical results which are of immediate interest here—it is the

[3] Brush's contention that his cases do not conform to a straightforward hypothetico-deductive methodology is, no doubt, accurate. Whether they depart from philosophically acceptable methodologies remains an open question however. In [48], Salmon argues for a Bayesian methodology with which these cases should be compared. And many other alternatives are available. Salmon [48], Agassi [1], Grünbaum [18], and Lakatos [30] all suggest that much of the history which has sparked the controversy is bad history, bad because it is based on philosophically naive demarcations of science from non-science and philosophically unsound accounts of scientific methodology. So far as possible, I shall avoid controversies about the role of sound philosophy within history of science; my concern to understand the proper role of sound history within philosophy of science is but little affected by the adequacy of the particular historical claims which first raised the issue.

[4] An instructive example of the differences which tend to arise between philosophers and historians in dealing with the same case history is provided by a comparison of Sir Karl Popper's treatment of the relation of Kepler's planetary laws to Newtonian mechanics in [44] and I. B. Cohen's treatment of this case (and of Popper's view of it) in [6].

variety of philosophical reactions to such results.

Some philosophers have maintained that the historical claims alluded to, supposing them all to be factually correct, are of virtually no methodological or epistemological significance. They argue that such results merely show the irrationality (or imperfect rationality) of some scientists and some scientific institutions, and that even if all of actual science were full of such foibles, no change in *philosophically* sound methodologies and epistemologies would be called for.

Other philosophers maintain that the historians have illustrated the falsity of traditional methodological claims and that historical studies may be used to undercut the claim that there is any universally valid or applicable standard or method by means of which to assess scientific theories and explanations. We have here the germ of two ideal types, the poles of "logicism" and "historicism" between which most philosophers of science operate. I shall amplify the ideal types a bit, perhaps caricaturing them a little, in order to draw out their strengths and weaknesses and to enliven the dialectic between them.

(i) Logicism. An account of the logicist stance might well begin with the familiar distinction between the context of discovery and the context of justification. I shall not tarry on the distinction here, though it is worth noting that it is often drawn too sharply: considerations which would *justify* a theoretical claim may lead one to *consider* it in the first place and may, therefore, belong to both contexts. Rather, I begin by emphasizing the point of the distinction. Logicists are primarily concerned with the evaluation of theories and explanations *conceived as finished products*. The evaluation concerns selected logical, epistemic, and methodological features, and the degree to which these features meet certain philosophically desirable standards or criteria. Logicism is thus what McMullin [37] has called an "externalist" philosophy of science; it evaluates scientific materials (or, perhaps, "rationally reconstructed" scientific materials) according to standards whose source of authority is, in the first instance, extrascientific.[5]

These restrictions of logicist concern stand in the service of lofty goals. The logicist may begin, like Locke's underlaborer, by clearing the underbrush—i.e., by rationally reconstructing theories, explanations, arguments, etc. But his aim in clearing away historical, semantical, and logical difficulties is not merely to achieve parklike tidiness;

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[5] Scientific findings can, of course, be incorporated into these standards. For example, one may require on the basis of relativity theory that an epistemologically satisfactory theory not depend on knowledge of distant events when that knowledge could be obtained only from signals travelling faster than the speed of light.

he seeks, *inter alia,* to determine the precise cognitive content of scientific claims, to facilitate rigorous criticism of scientific reasoning and explanations, to measure the degree of support for theoretical claims and factual predictions (preferably by exhibiting their logical relations to well established evidential claims), to develop explicit norms governing the justified acceptance of scientific claims, and to criticize, clarify, and evaluate major theories and explanations by use of these norms and this apparatus.

The explanations and theories which are the subject of philosophical investigation are produced by scientists, not philosophers. So too are the (often very special) experiences, observations, and experiments which, according to the logicist, are the primary source of justification for explanatory and theoretical claims. The philosopher must be fair to these sources of his enterprise in carrying out his work. But it is the philosopher who first reveals theoretical structure perspicuously, who clarifies theories and the cognitive standards by which they are judged, and who assesses the logical consequences of theoretical claims. Or so, at least, the logicist maintains.

To epitomize those features of ideal-typical logicism of interest to us, let me encapsulate the position by stating the logicist thesis, (L):

> (L)   (*i*) There are universally valid methodological and epis-
> temological standards by means of which both science
> in general and the special sciences may be philosophically
> evaluated.
> (*ii*) The sole inputs needed for evaluating a theory (expla-
> nation, law claim, etc.) as of a given time are (a) knowledge
> of the formal (logical, syntactic and semantic) structure
> of theories (explanations, law claims, etc.), and (b) prop-
> erly parsed statements of the total relevant evidence
> available at that time together with a properly parsed
> statement of the theory (explanation, law claim, etc.).

As will be seen later, this encapsulation of the position needs to be altered, but it will serve us well as a vehicle for sharpening the contrast between historicism and logicism.

When an externalist philosopher is presented with historical cases which do not conform to his avowed standards, he may claim to be in a strong position. Thus, in our case, the logicist may respond to such criticism in the following vein:

> It would be a mistake to require that my reconstruction of, say,
> the justification for accepting a certain theory imitate the evaluation

of that theory by working scientists. Scientists are often irrational
in the beliefs they accept—and in the acceptance or evaluation
of scientific theories there are factors, such as the messiness
of the evidence, the logical inadequacies of the formulation of
the theory and the statements of the evidence, etc., which make
it especially difficult to reach a sound judgement.[6] Precisely the
virtue of a purely logical and epistemological basis for the standards
of justification in science is that such a basis enables philosophers
and scientists to show how things *ought* to be, to develop a vehicle
for *criticism* of specific theories, explanations, and justifications.
It is, perhaps, more surprising that science comes as close to
rationality as it does than it is that your case histories depart
from the logico-epistemological ideals established by philosophers.

According to its own self image, then, logicist philosophy can supply
the standards of *cognitive value* in terms of which the "moves" or
"positions" in the "game" of science should be judged. This is
compatible with the claims that real science is a complex, many-faceted,
impurely cognitive enterprise, that goals other than cognitive ones
affect the professional decisions and behavior of scientists, and that,
in any case, scientists are imperfectly rational. Accordingly, the ideal
evaluations (and prescriptions) of logicist programs need not bear
any simple relation to historical science. In particular, no set of case
histories, by itself, can show that a particular logicist proposal regarding
the ideals, standards, and criteria of rationality is mistaken.

This logicist way of dealing with difficult cases is not uncommon
in the literature. The reader who wishes to see how easily it allows
one to deal with specific case histories will enjoy working out a
response along these lines to Brush's treatment of J. C. Maxwell
and the development of the kinetic theory of gases ([2], pp. 1168–
1169).[7]

   (ii) Three Difficulties for Logicism. (1) The technique for dealing

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[6]Furthermore, as historians and sociologists of science have emphasized, there are
many noncognitive factors relevant to theory acceptance in real life. These include
such diverse matters as the difficulty of the mathematical symbolism in which a theory
is couched, the availability of reliable measuring apparatus, the prestige and authority
of those favoring opposing theories, political factors affecting the institutions making
research grants, etc.

[7]It is important to see that the dismissal of history as irrelevant because of its
irrationality is not the only logicist response available to Brush's case history. In
particular, the line taken by Salmon in [48] (see note 3 above) suggests that he would
treat Brush's case as reflecting defects in hypothetico-deductive methodology which
were known to exist on strictly philosophical grounds. Salmon would presumably argue
that his Bayesian methodology provides the grounds for understanding both the defeccts
in H-D methodology and the rationality of Maxwell's procedure.

with hard cases just described is, however, too easy. It makes real science *totally* irrelevant to the evaluation of whatever cognitive standards a philosopher might propose. This irrelevance is the price of the impregnability to historically based attacks. Consider: suppose two philosophers propose conflicting evaluative standards for explanations. Suppose further that the informal judgements which scientists in fact make in a large range of cases fit fairly well with one of these standards and very badly with the other. Suppose yet further that the scientists who become aware of explicit statements of these standards uniformly hold that the one is sound, the other not, and that they seek to improve the few explanations departing from the favored standard which they had hitherto accepted. Such a situation would not be flatly decisive—there might yet be adequate arguments to show that the disfavored standard was nonetheless more appropriate for, say, achieving the truth. Surely, however, the logicist argument being considered goes too far in dismissing the reaction of the scientific community as utterly irrelevant.

(2) If a logicist is to capture the standard of evaluation appropriate to science (or to some science), he must capture the formal structure of the metatheoretic notions (e.g., "confirmation," "explanation," "theory") relevant to the practice of the scientist. But the continued debate among philosophers about the formal properties of such notions shows that there is more than one plausible formal reconstruction of such notions. Unless a successful *a priori*(!) proof can be constructed to show that only one of these formal reconstructions is relevant to all cases of confirmation, to all explanations, or to all theories, a case by case consideration of the appropriateness of one rather than another reconstruction will be required. Furthermore, the development of science is relevant; not only would one have to show that theories, explanations, and confirmations all have the same logical form in all sciences, but also in all stages of the development of science. That this can be *proved,* or even argued abstractly, is most implausible.[8]

(3) It has been argued recently that the reconstructions encountered in the vast majority of the logicist literature are not of the right type to employ if the enterprise is to succeed, for they exclude essential

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[8]Considerations of this sort lead Mary Hesse, who believes that the "norms or criteria of 'good science' . . . are not timeless," to suggest that "there will be no simple process of testing a proposed logic against historical examples—the relation of logic and cases will rather be one of mutual comparison and correction" ([22], pp. 6–7).. This process of mutual correction will, itself, prove extremely delicate, especially if the concerns mentioned in passing in note 3 about the influence of philosophical methodologies on the outcome of case studies are taken seriously.

"inputs" which are required if one's evaluation of scientific theories is to be sound. Lakatos is perhaps the most important of a large number of philosophers[9] who have argued that the career of a theory is, at least sometimes, more important than the formal relations between evidence claims and theoretical postulates at any stage of the theory's history. Relevant factors here include, arguably, the theory's ability to accommodate auxiliary hypotheses, its capacity to reshape itself under competitive pressure or in the face of experimental difficulties, its success in predicting facts before they are discovered, etc. These factors are not included in traditional models of theory evaluation—and their inclusion requires a significant departure from the spirit of logicism. This departure is necessitated by the fact that a theory, considered as a product, does not always reflect the intricacies of its career. To understand its career, one must compare materials taken from different stages of its development, materials not included in the finished structure of the theory nor in the totality of evidential claims. Yet, by studying the career of a theory one may learn that it has hidden strengths, that it promises to overcome the difficulties revealed in a logistic evaluation of its present state and its present fit with the evidence. The significance of such cross-temporal considerations will be examined below.

For the moment, it seems fair to claim that these difficulties show that a "pure" logicism will not do as it stands. Whatever else the historically rooted criticisms of philosophy of science have accomplished, they have forced philosophers of science to bring their abstract methodologies closer to real cases and to scientific practice. They have increased the burden of proof on the philosopher to show that his "rational reconstructions" of the epistemological problems faced by scientists are relevant and applicable to real theories. As McMullin puts it, "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 the experience, the more he will have to rely on the help of descriptivist techniques" ([36], typescript p. 5).

(iii) *Historicism.* There is no single historicist position. The simplest way to state what is common to the great variety of historicist positions is to restate their polar opposite in oversimplified form. Formulate logicism as the view that scientific materials may properly be evaluated by reference to their formal structure and the evidence available at

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[9]Cf. [29] and [30]. Among the many articles relevant to this position are[11], [35], [36], [37], [38], [62], [63], [64], [65], and [67]. I am grateful to Professor McMullin for advance copies of [35] and [36].

a given time *in abstraction from further historical considerations.* Then, to a first approximation, historicists hold that logicist tenets either become irrelevant to real science or result in seriously mistaken evaluations of it precisely because they proceed "in abstraction from further historical considerations." The minimal historicist claim may thus be formulated as follows:

> (MH)   The evaluation of a theory (explanation, etc.) as of a given time cannot be accomplished solely by reference to the formal structure and content of the theory and the relevant evidence available at that time.

It will soon be clear that (MH) is compatible with a great variety of positions, some of which are quite logicist in spirit. (One reason for discussing below the variety of factors which have been considered relevant to assessing the degree of support for theoretical claims is to give some flesh to the claim that (MH) encompasses a great variety of positions.) But before the argument turns in this direction, it will be useful to consider a stronger version of historicism, to discuss the relevance of the study of the history of science to the historicist-logicist dispute, and to clarify certain methodological questions.

I take strong historicism to amount to the denial of (Li), i.e., to come to the following:

> (SH)   There are no universally valid methodological and epistemological standards by means of which both science in general and the special sciences may be evaluated.[10]

Paul Feyerabend, in [12], advocates a strong historicism. He maintains that all codified methodologies and all standards of evaluation for theories (etc.) should be taken, at best, as rules of thumb. He argues, in part from cases, that all traditional methodological rules have been broken by scientists *in the course of doing good science* ([12], pp. 21 ff.), and that all of the traditional standards of evaluation have led to substantive misevaluations of sound theories. Furthermore, he argues, the attempt to enforce any codified standard or methodology will block or inhibit the steps crucial to major theoretical advance.[11]

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[10]This denial sometimes rests on the denial of (Liia), i.e., on the denial of the claim that there is a unique formal structure for theories, explanations, etc. Among those who would deny (Liia) are Hesse [22], Naess [41], Shapere [56], and Toulmin [62].

[11]Feyerabend argues that we must often use theories with smaller empirical content than their available competitors and protect them from known empirical refutations by use of *ad hoc* strategems in order to be able to discover the facts crucial to theory revision (cf. the treatment of Brownian motion in [13], pp. 175–176) and to obtain evidence against currently accepted theories (cf. the treatment of the impact

His position, then, is that no universal standards have in fact governed science and that, in principle, none ought to.

It has often been noted that while justification and evaluation are at the center of logicist concern, description is often at the center of historicist concern. It is typical for the historicist to devote considerable effort to determining and describing both the professed and the actual norms and standards employed in actual science(s). One significant point of such descriptions is to show that a variety of norms, methodologies, values, and goals have operated in science and to use this "plain truth" in an attempt to show the incorrectness of any proposed set or hierarchy of norms, methodologies, values, or goals.

There is a familiar difficulty with this position, recently articulated again by McMullin ([36]). Neither the professions nor the practices of actual scientists establish strong historicism. What is needed *in addition* to the knowledge of the professed and the practiced methodologies and standards of scientists is some *independent* means of showing these methodologies and standards to be appropriate and correct, at least within the context in which they are employed, or some means of establishing *in general* that the highest standards applicable to a science are those set "internally" by scientists acting in their professional capacities. And neither course will prove easy.

First of all, even the strong historicist must admit that there have been episodes in the history of science in which not only individual scientists, but also scientific institutions and communities have flown in the face of sound scientific method. (The Lysenko era in Russian genetics and agronomy will presumably suffice as an example.) But this admission requires the use of some "external" standard, not derived directly from the practice or professions of the institution or community in question.[12] Knowledge of the professed and of the

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of the general theory of relativity on the evaluation of the notorious 43 seconds of arc per century by which the perihelion of Mercury advances beyond Newtonian predictions in [15], note 7, p. 253). (Regarding this second case, cf. also [12], pp. 40 ff. and [14], pp. 300–301.) Cf. also the treatment of Einstein vs. Lorentz in [16].

[12] In [62], Toulmin proposes a novel historicist solution to this problem. He attempts to isolate a scientific *discipline* as an entity with certain features independent of the individuals, institutions and communities practicing that discipline. Disciplines can be characterized, *inter alia*, by the problems and explanatory ideals they recognize, the approved techniques, instruments and theories to be used in dealing with their problems, and the population of concepts employed in and crucial to the discipline. The discipline, then, defines a series of goals (the solution of certain problems) and the techniques, concepts, and styles of theorizing appropriate to their solution. The gap between the scientific actualities and the disciplinary goals sets the "internal" rather than "external" standard by which, say, the Lysenkoites should be judged to have taken a drastic backward step.

actual methods, standards, and goals of scientists, scientific institu-
tions, and scientific communities does not free the historicist from
the need for "externalist" or "critical" argument, transcending con-
siderations limited to the methodologies of the day. Pure historicism,
conceived as a descriptivist position, is no more viable than pure
logicism. We must be concerned, therefore, to locate the soundest
position between these poles and to occupy it.

2. The Place of History of Science: A Negative Preliminary Assessment.
But what role is the history of science to play in tempering logicism?
It is not yet established that there is a significant role for it to play.
R. Giere has forcefully maintained in [17] that arguments like those
so far employed show at most that the need was to bring the formal
apparatus of the philosopher into contact with real science, not with
history of science. In particular, Giere holds that an analysis of the
concept of confirmation employed in contemporary science reveals
that the validity of certain tests requires that the outcome be predicted
before the experiment was undertaken. Accordingly, the evaluation
of a theory by means of such a test rests on information about the
temporal order in which hypotheses were propounded and experiments
performed. (Giere thus accepts the first methodological thesis of the
present paper, though he denies the second.) But since Giere claims
to obtain this result from an analysis of confirmation and not from

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Toulmin's apparatus, however, makes it impossible to establish the relevant standard,
or to apply it to cases, except *ex post facto*. First, disciplines evolve internally and
split up into subdisciplines with independent lives. Episodes like the Lysenko episode
could be part of such changes in or of discipline, thus compromising the relevance
of *prior* disciplinary standards. Second, it is not obvious that one can separate the
"internal" from the "external" as clearly as is needed. Can we separate the discipline
clearly enough from the community and institutions practicing it to allow the requisite
independence of disciplinary standards and methods from all community standards
and methods? (On what grounds would the standards of some particular discipline—or
even broader inter-community standards—pertain to the Lysenkoites? For each proposed
answer it is open to challenge whether the Lysenkoites were practioners of, or owed
allegiance to, that discipline.) Finally, supposing these difficulties overcome, since
Toulmin's view allows disciplines to evolve by the introduction of conceptual variants
most of which will fail and only a few of which will succeed—success and failure
being established by the survival of the variants and their disciplines through time—it
will still not be possible to rule out definitively procedures, theories, and explanations
like those implicit in Lysenko's "vernalization" except *ex post facto*. Toulmin embraces
this last claim without qualms (cf. [65], p. 400); he insists that definite judgments
of this kind can be made only after the fact and that all that is available in advance
is a "rational bet."
    Toulmin's criteria exclude the "external" political support for Lysenko as extra-
scientific, but they still do not allow an adequate and timely internal evaluation of
Lysenko's demerits. Their fatal flaw is in refusing (or seeming to) any authority to
supposedly external, i.e., *extradisciplinary*, standards.

the study of historical cases, he holds that nothing essential is gained by using historical studies and non-contemporary cases. The chief philosophical benefit of the recent attention to the history of science, as he sees it, is that it has forced philosophers to deal with real science; the remaining benefits are primarily pedagogical, and stem from the ease of exposition and the relatively lower complexity of historical cases as compared with contemporary ones. His central claim is that the argument to date has failed to show that philosophy of science is significantly dependent on specifically historical considerations, that it will take a stronger argument to establish my metamethodological thesis.[13]

The matter may be put more broadly. Many historicist philosophers are seeking a *dynamics* of science; they accuse the logicists of missing essential phenomena by restricting themselves to a *statics* of science. But, supposing this complaint to be well-founded, it is still not obvious that scrupulous attention to the history of science is required. After all, the development and justification of Newton's dynamics was not dependent on scrupulous attention to specifically historical studies of the physical systems to which it applied. (This is not to deny the need to determine the values of a variety of parameters at different times in order to test Newtonian theory; it is to deny a significant place to specifically historical techniques and considerations.)

Could not the same hold true for a philosophical study of the growth and development of science, of the changing evaluations of theories and explanations? That is, could not the study of current science (together with occasional checks of historical material) suffice in the development of an adequate philosophical understanding of science as a changing phenomenon? Since philosophical understanding here amounts to an understanding of such matters as the appropriate norms and standards governing rational acceptance of claims, theories, and explanations, it is not immediately clear what knowledge of the norms and the behavior of historical as opposed to contemporary scientists has to contribute.

It is still an open question, then, what role(s) the history of science should play within the philosophy of science.

3. Ambiguities of "Rationality": A Tool in Delimiting the Logicist Enterprise. The present section is intended to dispel a number of

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[13] [36] is a direct attempt to rebut Giere's minimization of the role of history. But in it McMullin does little to spell out the role(s) which history of science ought to play within philosophy of science.

confusions that affect the way in which logicism is usually conceived and which, if left unchecked, would undermine my argument.

(i) "Rationality": An Accordion Term. Much current debate focuses on problems concerning the rationality of holding certain claims or theories, the rationality of changing one's beliefs or theories, and so on. The expansion and contraction of the accordion term "rationality" and its cognates mars much of the debate. In the present subsection I will note one dimension of ambiguity in the use of the term, in the next another. I will also illustrate the consequences of ignoring or mishandling these ambiguities.

To begin, however, note that all of the uses of terms like "rational" considered below are end-relative. That is, whether an action, policy, decision, or whatever, is considered rational will depend on the end or goal in view with respect to which the action (etc.) is assessed. (Not all uses of the term are end-relative in this way.) I will ultimately distinguish four cases of primary interest for the sequel. They involve four different but interlocking ends in terms of which actions (e.g., accepting a theory) or policies (of, e.g., theory evaluation) may be assessed. Because of the interlocking character of the ends in question, it becomes very difficult to determine whether or not there is a univocal and general account of rationality (or of the rationality of, say, theory acceptance) in science. (But cf. below, note 15.) I shall not face this problem in the present paper, but I shall argue that we must have recourse to the history of science if we are to resolve it properly.

Among the many uses of the term "rational," two are common—and, alas, commonly conflated—in philosophy of science. The first use is broader and, loosely speaking, methodological; the second narrower and primarily epistemological. In the broad use, an action is rational, roughly speaking, when *all things considered* it will or ought to bring about a desired end. Rationality in this sense is largely a matter of tactics and strategy. Thus if we suppose that science is primarily a truth-seeking enterprise, it will be rational for scientists to do that which will probably yield the greatest degree of truth.

This is, of course, no simple matter. Suppose, for example, that at a given moment scientists know of two competing theories T and T' that the net truth content of T is greater than that of T'. Suppose that they also know (a) that whether they accept either T or T' it is likely that they will soon be led to some T'' with an even greater truth content, and (b) that due to the psychological and calculational difficulty of working with T they are likely to arrive at such a richer T'' sooner if they work with T' rather than with T. *In the current sense of "rationality," it may prove more rational to accept T', the*

"less true" theory, *rather than T.*[14] (Note that there are now serious difficulties regarding the univocity of "accept" and "reject" in this paragraph.)

However, the term "rational" is also used in just such discussions in a sense according to which it is never rational for a truth seeker to accept a claim (or theory) known to have less truth content than an available competitor. In this narrower sense of the term, all things may not be considered; one may only take account of the degree to which the desired goal has been achieved and the extent of the shortfall from that goal. This is the sense of the term which has been of central interest to logicists. Indeed, speaking generally, this is the sense of the term relevant to most of the recent attempts to formalize notions like those of epistemological justification or warrant, epistemological acceptability, rational belief sets, and so on.[15]

It is important to realize that the term "rational" and its cognates are commonly used in both the broad and the narrow ways characterized above. The literature is filled with discussions that fail to make their point because they overlook or mishandle such ambiguities in terminology. I believe, for example, that Toulmin's arguments that logicists have conflated rationality with logicality are invalidated by their failure to take this ambiguity properly into account.[16] He holds (mistakenly, as I shall argue shortly) that the narrowly epistemological sense of rationality is restricted to a measure of the logicality of a system

[14] I owe this suggestion to Jon Adler. In [23], N. Koertge also discusses cases in which it may be rational (in the present sense) to protect "weak" theories from their currently stronger competitors.

[15] Professor P. Quinn (Brown University, personal communication) has suggested that the sense of "rationality" has not changed in these two assessments, just the end-in-view with respect to which rationality is assessed. Thus, in the situation sketched in the text, if one's aim were to maximize truth content at each stage of inquiry, it would be rational so far forth to adopt *T*, while if one's aim were to maximize the rate at which truth content increases, it would be rational to adopt *T'*. (Incidentally, cf. [19] regarding the difficulty of formalizing the notion of truth content which is required here.)

Little depends on whether one states the point in Quinn's way or mine: in assessing the rationality of a means, one's calculation depends utterly on the end-in-view. One advantage of my formulation, though, is the rarity of pure assessments of rationality-as-means. Where two ends, applied to a given case, dictate incompatible actions, it may be rational to act on one rather than the other because of *further* ends-in-view or because of the undesirable side effects which acting on one of the ends is likely to have. Sometimes, of course, it will be possible to have separate research groups pursuing separate *aims* and separate programs of investigation. But whether this is even possible depends on the problem, and whether it is feasible (given the limitations on available resources and so on) depends on further circumstances. Thus, a full assessment of rationality involves a weighing of ends as well as means, a feature my formulation is calculated to bring out.

[16] Cf. [65], [63], and Chapter I of [62]. McMullin's criticism of Toulmin on this issue in [38] has been helpful to me.

(plus, presumably, the epistemological acceptability of certain "observation claims"). His argument is that this sense of rationality is irrelevant to the (true) rationality of science which is concerned with the cross-temporal strategies for self-correction and error elimination. ("Surely, the rationality of science has less to do with the logical systematicity, or supposedly unique authority, of any *one* body of ideas or propositions than the manner in which, and the considerations in the light of which, men are prepared to *give up* one body of scientific ideas or concepts in favor of another" ([65], p. 405).)

But Toulmin misuses the invidious contrast implicit in this claim. (1) He often suggests that the manner in which scientists alter their beliefs or concepts is largely independent of considerations of logicality. But this does not follow from the distinction between logicality and rationality. Indeed, (2) the manner in which such alterations in beliefs and concepts are carried out should itself be assessed in part in the light of the logicality and of the epistemological merit (rationality in the narrow sense) of the belief systems scientists produce. Finally, as McMullin has argued ([38], pp. 424–425), (3) Toulmin's contrast between logicality and temporality is specious because logical assessments of theories, though they are made at a certain time, may take into account not only the latest formulation of the theory as a closed system and the current status of the evidence, but also considerations concerning the career of the theory and the development of the evidence through time. Points (1) and (2) undercut Toulmin's claim that a logicist conflation of rationality and logicality is fatal to the logicist program; they show that his requirement that the broad sense of rationality be largely independent of logicality is based on a confusion. Point (3) (which will be supported below) rests on a disagreement over the proper analysis of rationality in the narrow sense. As pursued below, it will ultimately lead to a revision in tenet (Lii) of the logicist program. Point (3) suggests that the history of science can play a key role in clarifying the interplay between rationality in the broad and the narrow senses and in clarifying the role of temporal considerations in assessing rationality in the narrow sense.

(ii) Prescription vs. Evaluation. A second dimension of ambiguity in the use of terms like "rationality" arises from the differences between "decision contexts" (D-contexts) and "evaluation contexts" (E-contexts).[17] In a D-context, one is concerned with forward-looking

or prospective questions. (Should we test T_1 or T_2? Should I advocate a change of priorities for our research team? Should we treat T_2 as background knowledge in planning new experiments?) When decisions are difficult, when the question faced is a hard one, it is often necessary to act (and in such cases inaction is also an act) in spite of substantial ignorance and with little or no opportunity to gain new information before the decision must be made. The forward-looking character of D-context questions, the possibility of unexpected side effects accompanying any decision, the variety of "external" constraints typically affecting an agent in D-contexts, these all make the standard of rationality in such contexts far less stringent than it is in E-contexts. It is, for example, often rational to expend considerable effort inquiring further into theories which it is by no means rational to accept on the available evidence.

In E-contexts, where the issues to be considered are retrospective, one often evaluates the action, policy, or theory of concern with respect to a single value (e.g., truth content) while treating the available evidence as if it were closed and complete. Such evaluations often concern a value or an issue not considered by, or not of central importance to, the agents who employed the policy or theory being evaluated. The outcome of the evaluation is not, in general, affected by the degree to which the agents were concerned with this value or by their ignorance of crucial information. In short, all of the following factors contribute to the stringency of evaluations in E-contexts: the (pretended) closure of the evidence which allows the evaluator to ignore potential side effects, the freedom to ignore considerations irrelevant to the value in terms of which the evaluation is carried out, and the freedom to abstract from the "external" constraints and motivations of the agents involved.

One particular complication must be mentioned here concerning double perspective cases. Consider, for example, Galileo's support of heliocentrism in the *Dialogue Concerning the Two Chief World Systems. Our* questions about this support typically involve a compounding of Galileo's perspective with our own. The distinction between D- and E-contexts is significant here. Notice, for example, the differences between two questions which Galileo might have asked himself: "Should I treat the unresolved dynamical problems facing heliocentrism as if they will not provide insuperable obstacles for that theory?" (a D-context question) and "Is heliocentrism better supported on the available evidence than geocentrism?" (an E-context question). Now if *we* ask whether heliocentrism or geocentrism was better supported when Galileo wrote his *Dialogue,* we must separate the answers we get using *our* criteria of support and using criteria

available to him. And when we *evaluate* Galileo's decision to treat the dynamical obstacles to heliocentrism as if they would, one and all, be overcome, we must appreciate the fact that we are working in an E-context; the evaluation may proceed either by *our* criteria or by our reconstruction of (a limited subset of) the criteria available to Galileo and his contemporaries.

It should be clear that evaluative considerations are relevant to Galileo's decision—e.g., *his* estimate of the degree of support enjoyed by the heliocentric theory. It should also be clear that one may restrict one's concern in the context of decision to a certain range of issues. If, for example, one wishes to base a decision as to how to treat the dynamical difficulties of heliocentrism solely on epistemological considerations, one might well base the decision on his best estimate of the way(s) in which the remaining dynamical difficulties might be overcome and of the degree of support enjoyed by the theory. But in any case, because this is a D-context problem, we must not hold the agent responsible for *information* or *criteria of choice* available only after his decision. The character of D-context issues requires that they be faced from the perspective of a suitably placed agent employing the theoretical and calculational apparatus available at the time of decision and facing the then pertinent uncertainties. Failure to take the agent's perspective into account in this way means failure to evaluate the decision of concern.

The distinction between D- and E-contexts is often more helpful than the traditional distinction between the contexts of discovery and justification. For example, as the discussion of Galileo just showed, evaluations affect decisions, including those leading to scientific discoveries. It is also obvious that decisions (such as those regarding which of two theories to test) affect evaluations. Thus the common temptation to treat considerations belonging to the context of justification as irrelevant and useless in the context of discovery[18] does not carry over to D- and E-contexts. Indeed, by clarifying the differences in circumstance and purpose between evaluation and decision making, a philosopher employing the latter distinction can offer an account of the reasons for which, in particular cases, considerations employed

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[18]There are still many authors who treat epistemic evaluation as a game, depending on honesty and good record keeping, but irrelevant to real life beliefs, actions, and policies. Lakatos is sometimes guilty of this as Smart points out in [58], p. 269. Lakatos himself recommends a "whiff of inductivism" to Popper ([31], pp. 256 ff.), but what is needed is not a whiff, but a heady draught if we are to learn from experience. The point is central to an appreciation of the intense interplay among the senses of "rationality" under discussion.

in one context do not enter into, or are weighted differently, in the other.

This discussion has highlighted the importance of distinguishing between prescriptive norms (to be applied in D-contexts) and evaluative norms (to be applied in E-contexts). When this distinction is crossed with that drawn above between the methodological and the epistemological senses of "rationality," one obtains a group of four complexly interrelated contexts in which philosophical evaluation is pertinent. These are set forth in the following table:

	Epistemology	Methodology
Evaluative (E-context)	*Evaluative Epistemology* (*EE*): The study and application of criteria for evaluating the rational credibility of theories, explanations, etc., in the light of their track record and the available evidence.	*Evaluative Methodology* (*EM*): The study and application of criteria for evaluating the utility (all things considered) with respect to scientific purposes of theories, explanations, etc., in the light of their track record and the available evidence.
Prescriptive (D-context)	*Prescriptive Epistemology* (*PE*): The study and application of criteria of choice for actions, policies, etc., affecting the development, testing, and application of theories and explanations, said criteria aimed at maximizing the rational credibility of future beliefs, explanations, theories, etc.	*Prescriptive Methodology* (*PM*): The study and application of criteria of choice for actions, policies, etc., affecting the development, testing, and application of theories and explanations, said criteria aimed at maximizing the utility (all things considered) with respect to scientific purposes of future beliefs, explanations, theories, etc.

It is worth noting that the different enterprises listed in this table are often conflated with one another in the philosophical literature. One reason for this, I believe, is the use of such Janus-faced terms as "justified belief" and "rational acceptability." [19] Justification, after all, is retrospective, for it is largely a function of evidential history. Belief, on the other hand, involves a strongly prospective component, for it serves as the antecedent ground of intentional action. But,

[19] I owe this point to Phil Quinn. For the purposes of the following discussion it should be observed that the question whether or not a belief is justified, whether or not it is rationally acceptable, is quite independent of the subjective belief states of scientists. The problem is not which subjective belief states scientists ought to be in (a matter which is seldom under voluntary control), but which premises they ought to employ in planning their actions, evaluating their theories, and so on. The standards of belief being explored pertain to communities, not merely to individuals.

as I have argued, assessments of rationality in D-contexts often differ markedly from those in E-contexts; if one is to understand the deep connections between evaluation and prescription properly, one must separate the retrospective and the prospective considerations implicit in concepts like that of *justified belief*.[20]

Against this background it will be useful to highlight the differences among *EE, EM, PE,* and *PM* by noting that each of these enterprises has different aims. Very roughly, these are, in order: to estimate the truth content of claims as revealed by the available evidence, to estimate the scientific utility of claims and procedures as revealed by their track record in use, to determine how to maximize the rate at which the truth content of our knowledge increases, and to determine how to maximize the rate at which the scientific utility of our procedures, theories, etc. increases. Accordingly, each enterprise has its own criteria of rationality—criteria which interlock, interact, and adjust to each other in a variety of ways. It follows that in appraising the "rational acceptability" of a theory, one must consider different questions according to which of these four enterprises is at stake. I illustrate the point by citing one set of questions from each enterprise.

> *EE*: Given the evidence available up to time t_0, what is the status of theory *T* at t_0 according to the current criteria of rational credibility? (For example, what is its degree of confirmation or corroboration?) Would use of the criteria available at t_0 yield a different result? Where alternative measures of *T*'s status are available, can we determine which among them are appropriate, which most nearly reflect *T*'s actual epistemic status?
>
> *EM*: Given the track record of *T* up to time t_0, how well has *T* fared according to current criteria and standards of scientific utility? (For example, how has it compared with its competitors for purposes of calculation—e.g., with respect to accuracy, ease of use, reliability and suitability for typical (or unusual) problems? How does its heuristic power compare with that of its competitors? How sound, how straightforward is its physical interpretation? And so on.) Would the criteria available at t_0 yield different results? Which uses of the theory and which criteria are, in balance, most important for the assessment of its utility?
>
> *PE*: Given the evidence available up to t_0, what should one do with *T* in order to test it or in order to maximize the rational credibility of one's ensuing beliefs? (E.g., should *T* be treated as beyond reasonable doubt and incorporated into background

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[20]Compare McMullin's [35] on proven and unproven fertility of theories.

knowledge? If it is to be tested, how accurate must the test(s) be to discriminate between T and its competitors? Is T a reliable basis for interpreting experimental results? Or should it be dismissed as unworthy of further consideration?) Do current standards yield the same results as those available at t_0? If not, how can the matter be adjudicated?

PM: Given the evidence available up to t_0, what uses ought one make of T in the attempt to maximize the scientific utility of one's beliefs and procedures? (E.g., should one use T as a calculational device? If T is known to be unreliable in certain domains or circumstances, what restrictions should be placed on its use? In the light of resource limitations, is it worthwhile testing T against its competitors? How much effort should be put into improving test accuracy? And so on.) How can one adjudicate between competing benefits and risks and between competing measures and conceptions of scientific utility in deciding how to use T and its competitors?

An answer to any one of these questions may depend on substantial evidential and theoretical considerations; in justifying a favorable answer to any one of them, a philosopher or scientist may well claim to have shown that it is (was) rational to accept T, albeit provisionally. Yet, clearly, the claim that it is rational to accept T comes to something different in each case. It is of great importance to make these differences clear in any full-length discussion of the ways in which theories are supported in science and in any discussion of the nature of scientific rationality.[21]

(iii) Difficulties in the Ongoing Debate. Logicists are primarily concerned with *EE*. Traditionally, most logicists have worked within an empiricist epistemological tradition according to which the genesis of a theory (or explanation) is irrelevant to the epistemic evaluation of the theory-as-product. (Note that the same is *not* true for observation

claims!) Again, the tradition holds that only logical and semantic relations among evidence statements and theoretical statements carry epistemic weight so that, e.g., whether or not a bit of evidence is in hand *before* the theory is stated or only *after* it has been predicted by use of the theory makes no epistemic difference.

These two epistemic claims are currently being challenged. (Cf. [40].) This complicates the ongoing debate in a number of ways:

(1) It means that the "rational reconstructions" of case histories hitherto employed will be disputed. For a philosopher subscribing to the above epistemological tenets, the task of *EE* will be greatly simplified by rationally reconstructing any case studies of interest so as to exclude "irrelevant" genetic and temporal considerations. Indeed, perspicuous formal representation of a case for the purpose of epistemic evaluation will require the use of a symbolism which treats theories and evidence as wholes "considered either at particular times or apart from time" ([65], p. 404). For a philosopher who disputes the above epistemological tenets, the very rational reconstructions from which the case study starts will exclude crucial and relevant information. The whole enterprise will therefore be suspect *ab initio.*

(2) The resultant debate will likely be flawed by confusion over the sources of the objections to the reconstructions of historical cases. These objections may rest on a variety of differences regarding the proper reading of the historical record, the information relevant to epistemological evaluation, and the uses to which the reconstruction is to be put. Only in unusual cases will it be clear whether a challenge to the reconstruction of a case history rests on differences in epistemological doctrine rather than on disagreements about the historical substance of the case or on differences in the uses to which the case is to be put. Confusion of this sort is especially likely since the logicist will tend to reject all attacks on his views that draw on the temporal order of events as misconceiving his enterprise.

(3) Challenges to particular logicist positions may be (but need not be) allied to an all-out attack on logicism in general. But even when it is clear that such an all-out attack is involved, matters are complicated by the need to separate other enterprises (e.g., *PE*, *EM* and *PM*) from *EE*, the logicist's true home. It is surely legitimate for the logicist to limit his concern to *EE* without giving these other undertakings much consideration. On the other hand, because the enterprises do impinge on each other, specific criticism of specific logicist positions as yielding untenable consequences for the other enterprises is possible. In this delicate situation, few philosophers have been clear enough in delimiting the purpose of their arguments.

4. On What Does Theory Support Depend? In the present section, I shall take a very brief glance at the contemporary debate over confirmation, corroboration, and support. My concern is to exhibit some of the great variety of independent variables on which theory support has plausibly been alleged to depend. I shall not, therefore, consider any details of the logic of support; indeed my primary sources for this discussion are the (often logically naive) writings of historically oriented philosophers rather than those of inductive logicians. However, the crude botanization of views achieved here will prove useful in advancing the discussion of the place of history of science within philosophy of science.

All of the confusions discussed in the preceding section infect the literature on theory support. Most discussions are quite unclear about the criteria by which proposed support functions should be judged. As we shall see, a number of proposals introduce a ''personal factor'' into support functions. An alert logicist, in response to these proposals, might argue that the very introduction of subjective or perspectival factors into the proposed support functions shows that they are serving the ends of *PM* or *EM* or some other undertaking rather than those of *EE*, and that the criteria by which they should be judged are thus not the same as the criteria by which a logicist support function should be judged.[22] The sequel will show that this argument is mistaken, both in its rigid separation of the criteria relevant to *EM*, *PM* and *PE* from the criteria relevant to *EE* and in its *a priori* rejection of all subjective and perspectival considerations from evaluative epistemology.

To facilitate the discussion of theory support, the following notation will be used: '$S_{T,e,t}$' will stand for the degree of support for theory T on evidence e at time t, and '$S_{T,e,t} = S(x,y,...,z)$' will stand for the claim that $S_{T,e,t}$ is a function of the variables $x,y,...$, and z. It will be noticed that I often write 'T' as '$T(t)$', i.e., I treat T as time-dependent. This is in order not to beg the question whether theories have a fixed formal structure and/or a fixed set of empirical consequences. If they do, then there is a T_0 such that (t) $(T(t) = T_0)$. But if not—if, for example, the career of a theory is relevant to its epistemic evaluation—then the notation remains adequate.

Using this notation, traditional empiricist logicism holds that the

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[22]Though, as Ron Giere has kindly reminded me, Bayesian accounts of theory evaluation like those in Salmon's [49] and Shimony's [57] are logicist in character even though they employ ''subjective'' input in the form of prior probabilities.

form of (quantitative and qualitative) measures of theoretical support is

(1) $S_{T,e,t} = S(T(t),e(t))$.

Perhaps the most radical challenge to this view comes from Feyerabend who holds that all evidence is radically theory-laden *in a way which depends on the choices made by the person evaluating the theory.* Thus Feyerabend says that "a theory may be inconsistent with the evidence, not because it is incorrect, but because the evidence [like all evidence for Feyerabend!] is contaminated" ([12], p. 46). On Feyerabend's view, the seriousness of the inconsistency between theory and evidence is a function of how seriously we take certain auxiliary theories and hypotheses, i.e., of the way in which we conceive or treat the evidence. It follows that no evidence is objectively and interpersonally fixed; all evidence is interpreted and weighted, and both the interpretation and the weighting depend upon the evaluator.[23] Accordingly, Feyerabend holds

(2) $S_{T,e,t} = S_P(T(t),e(P,t))$

where P is the person evaluating the theory.

It should be noted that where two different persons, P and Q have different support functions, i.e., where

(3) $S_P \neq S_Q$

no decision procedure as to the preferability or correctness of one of these functions rather than the other is possible; indeed, in general there is not even a procedure which makes it likely that S_P and S_Q can be brought into closer accord at a later time.

The views expressed in Thomas Kuhn's well-known *Structure of Scientific Revolutions,* though they are obscured by his recent back-tracking,[24] make theory support paradigm-relative in much the way that Feyerabend makes it person-relative. According to Kuhn, theories and explanations are evaluated from a perspective established in its essentials by a supratheoretical unit of belief called a "paradigm." Paradigms are metaphysical belief systems; they establish what Kuhn now calls a "disciplinary matrix" within which scientific problems are set and formulated and techniques for dealing with them are

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[23] Note that there is a significant difference between Feyerabend's approach and the treatment of "subjective factors" implicit in, e.g., Carnap's credibility functions. Credibility functions leave free *only* the weighting, not the interpretation or the relevance of the evidence.
[24] Cf. [24], [25], [26], and [27].

established and evaluated. The standards by which the adequacy of solutions to scientific problems are judged, like the standards determining the evidential relevance of one claim to another, are paradigm-dependent.

On Kuhn's familiar account, theory assessment in "normal science" is intraparadigmatic and proceeds, more or less, in logicist style. In "scientific revolutions," however, one paradigm is replaced by another, with dramatic effect: persons operating within different paradigms necessarily work on different problems, apply different standards of adequacy for problem solutions, and disagree on the relevance of specific observations and experimental results to their explanatory theories and claims. In short, theories belonging to different paradigms, even when they compete with one another, are "incommensurable." Scientists and philosophers, when they evaluate theories, have no extraparadigmatic purchase for stating or interpreting the evidence. (Cf. [5].) Therefore, where there is no common paradigm, there can be no agreement on what the evidence is, on how to parse it, or on the relevance relations obtaining between the evidence and the incommensurable theories. (If such agreement were possible, then the theories would not be incommensurable!) Person-relativity is here replaced by paradigm-relativity, and in spite of the intersubjectivity of intraparadigmatic assessment, we have, once again

(4) $S_{T,e,t} = S_P(T(t),e(P,t))$,

though this time 'P' stands for a paradigm, not a person. Again, where

(5) $S_P \neq S_Q$,

no decision procedure is possible; no procedure guarantees, and perhaps none even makes it likely, that S_P and S_Q can be brought into closer accord at a later time.[25]

E. Zahar, developing some themes from Lakatos in his controversial treatment of the competition between Einstein's and Lorentz's research programs ([67]), has argued that the degree of support which a theory is awarded should depend on its success in accounting for "novel facts." Without debating the merits of this proposal, note that there are two ways (expounded in [40]) of understanding the proposal. On the first, a theory is constructed by a scientist or scientists at

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[25]One difference between person-relativity and paradigm-relativity: it is not clear, on Kuhn's account, that paradigms can coexist except during scientific revolutions. Persons can. Accordingly, in the current case we may be forced to compare $S_P(t)$ with $S_Q(t')$ for $t \neq t'$. This significantly complicates the handling of $e(P,t)$ and $e(Q,t)$.

least partly in the light of, and to account for, certain facts, say
$f_1...f_m$. If, after the theory is developed, it accounts for
$f_1...f_m, f_{m+1}...f_n$, the facts $f_{m+1}...f_n$ count as novel facts and support
the theory. (There may also be facts $f_{n+1}...f_o$, believed relevant,
for which the theory has no account, and facts $f_{o+1}...f_p$ which, *prima
facie*, falsify the theory. Then $e(t) = f_1,...,f_m,...,f_n,...,f_o,...,f_p$.) On
the present account, since the relevant factor in determining novelty
is the knowledge and the intentions of the inventor(s) of the theory,
*novelty can only be measured by reference to the knowledge and purposes
of the inventors of the theory.* Accordingly, we have here a special
case of the person-relative historicist view, one which makes the
degree of support enjoyed by a theory a function of the knowledge
of its inventors.

According to the second reading of Zahar, what counts as background
knowledge is not determined by the accidents affecting the personal
knowledge of the inventors of the theory, but by the relevant intersub-
jectively available knowledge, i.e., the knowledge which could be
located by all investigators in the field were it not for practical obstacles.
(It is, of course, historically difficult, but not impossible to estimate
what belongs to background knowledge in this sense.) This means
that the relevant background knowledge becomes a simple function
of time. If we symbolize it by '$b(t)$', Zahar's proposal comes to

(6) $S_{T,e,t} = S(T(t), b(t), e(t))$.[26]

Musgrave, in arguing against each of the positions just ascribed
to Zahar, maintains that the relevant measure of the novelty of a
fact which a certain theory accounts for is whether or not the best
of the hitherto available theories could account for it. (This measure
of novelty would explain the increasing difficulty of major theoretical
innovation as our theoretical knowledge expands.) If we let '$T^*(t)$'
stand for the best of the competing theories available at t, this proposal
comes to

(7) $S_{T,e,t} = S(T(t), T^*(t), e(t))$.

Many logicists would find this proposal congenial, for it allows a
strictly formal development of the support function $S_{T,e,t}$.[27] The sole
historical input required to determine the degree of support for T,
other than the evidence $e(t)$, is supplied by determining which theory

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[26]Levi [33] and Cohen [7] may be read as treating support in this way, though
the former's system seems more suited for the purposes of *PM* and the latter's more
suited to those of *EE*.

[27](7) is preferable to (6) on this score because $T^*(t)$ is more readily formalizable
than the inchoate and often incoherent "background knowledge," $b(t)$.

to "plug in" to S as the best available alternative to T. Not surprisingly, a number of current theories of induction, e.g. Lehrer's and Harman's, have this form. (Cf. also [11].)

In [51], K. Schaffner argues that both the alternative available theories and the (intersubjectively delimited) available background knowledge are relevant to an estimate of the degree of support earned by a theory. He claims that competing theories ought to be evaluated not only with respect to their ability to account for the known, directly relevant, empirical results, but also for their "fit" with the general theoretical background then extant, i.e., for what he calls their "theoretical context sufficiency."[28] This analysis is extended to a variety of cases in [50] and [52]. In our symbolism, it comes to

(8) $S_{T,e,t} = S(T(t),T^*(t),e(t),b(t))$.

To conclude this partial list of the variables on which support functions have been held to depend, I shall combine some suggestions taken from Schaffner's treatment of the Einstein-Lorentz case ([50]) with the approach implicit in McMullin ([35] and [36]) and Lakatos ([29]). Schaffner stresses the need to consider the relative ability of competing theories to incorporate new hypotheses fruitfully in their attempt to account for recalcitrant evidence. (Cf. also the epigraph to this paper.) There are serious difficulties here in assessing the various ways in which and degrees to which such hypotheses are *ad hoc*. (Cf. [18], [50], and [67].) Fortunately, the resolution of these difficulties lies beyond the scope of this paper. It is worth noting, however, that according to Schaffner these problems should be resolved by examining how such hypotheses fare *vis à vis* the changing background knowledge and further theoretical developments. Two factors are of special importance in this approach to the evaluation of hypotheses and theories: the sorts of adjustment forced on a theory in order to maintain consistency with the new hypotheses and the power of the adjusted theory to cope with the empirical results of

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[28] One of Brush's cases ([2], p. 1168) illustrates Schaffner's position beautifully. According to Brush, the acceptance of a wave (i.e., roughly, a kinetic) over a caloric theory of heat in the 1830's was *not* due to Davy's and Rumford's proofs that friction could generate heat indefinitely (ca. 1800), nor to Joule's experimental determination of the mechanical equivalent of heat (which did not happen until the mid 1840's). Rather it was due to the victory of the wave theory of light over its competitors after Fresnel's brilliant studies of the late 1820's and to the assumed analogy (in the absence of any unifying theory!) between heat and light. The assumed analogy illustrates the role of "theoretical context sufficiency" as a factor in theory evaluation. It depends on background knowledge and not merely on the status of those theories (all theories of heat in the present case) with which the theory being evaluated competes.

attempts to provide independent tests of the hypotheses.[29] This compares interestingly with Lakatos's attempt to evaluate theories comparatively according to the progress or degeneration of the research programs with which they are allied ([29]).

All of this suggests yet another set of variables as relevant to the evaluation of a theory T. Before writing the formula out, it will be useful to introduce some of the notation used in it. We are concerned with the adjustments made in T in the light of recalcitrant evidence and of new empirical results arrived at by use of competing theories and research programs. Suppose that T is adjusted by the introduction of an auxiliary hypothesis h_i. Let us designate by 't_i' a moment of time when all the necessary theoretical adjustments have been made in T to accommodate h_i and the relevant evidence. Then we may take '$\langle T(t_i), h_i \rangle$' to be the ordered pair consisting of the adjusted theory and the adjusting hypothesis, and '$\{\langle T(t_i), h_i \rangle\}$' to be the set of such pairs generated when a series of auxiliary hypotheses are introduced, and appropriate adjustments made in T, to account for recalcitrant empirical results and results "foreign" to T but "natural" to the rival theory T^*. ('$\{\langle T^*(t_j), h^*_j \rangle\}$' is to be interpreted similarly.) Using the index 'k' so that it picks out sequentially, from earliest to latest, all of the times in $\{t_i\}$ and $\{t_j\}$, let '$b(t_k)$' represent the state of the (changing) background knowledge at time t_k; the set $\{b(t_k)\}$ is thus the set of all relevant background knowledge for the comparison of T and T^* as they develop. Using this notation, the proposed functional dependence of theory support may be expressed as follows:

(9) $\quad S_{T,e,t} = S[\{\langle T(t_i), h_i \rangle\}, \{\langle T^*(t_j), h^*_j \rangle\}, \{b(t_k)\}, e(t)]$,

where, for all i and j, t_i and t_j are earlier than t. It should be noted that different theories may play the role of T^* at different times and that T and T^* are in interactive adjustment, for they both face the same background and the same empirical results, and they must each account for the other's success in explaining those empirical results.

5. The Place of History of Science: A Reassessment. It is time to take

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[29]Compare McMullin: "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. . . . The confidence we place [in the best established theories of science] 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" ([36], typescript pp. 18-19).

stock, to deal directly with the challenge that the principal benefit
to be obtained from historical studies is improved contact with actual
science, that no distinctive benefit is obtained from historical studies
which could not be obtained from studies of contemporary science
and its workings. To meet this challenge, it will be useful to consider
three sets of problems, namely those surrounding (1) the use of rational
reconstructions in philosophy (and the relation of such reconstructions
to "real science" and "real history"), (2) the proper weakening of
logicism in the light of our previous results, and (3) the "historical"
and "temporal" character of theories. In dealing with each group
of problems, I will exhibit explicitly a function which historical studies
should serve in improving current philosophical accounts of the logic
of support.

It would be wise to preface these considerations with an explicit
acknowledgement of certain major differences between history and
philosophy of science. Historical study is, in the first instance, concrete
and descriptive. It delves into detail, seeking to understand the concrete
particularity of complex and complexly interrelated individuals and
events. Accordingly, it employs a variety of techniques which are
not useful in abstract philosophical study. Practicing it well requires
total immersion in the wealth of historical detail. Historians begin
by facing a virtually seamless web of entanglements among scientific
tradition, social and intellectual context, personality clashes, religious
and theological considerations, and so on. The demarcation between
"internal" and "external" history of science is by no means obvious;
in Giere's words, "Everything is potentially relevant" ([17], p. 295).
From the abstract, normative point of view in technical philosophy
of science, this universality of potential relevance is anathema.
Philosophers must exclude the "irrelevant" ("accidental," "external")
details which form the substance of many historical studies in order
to achieve abstract accounts of the structure of ideal science and
the allied norms governing it. To do this, they demarcate science
rigorously from the enterprises with which it is historically interlocked.
They cannot, of course, eliminate or exclude the "external" influences
which constantly affect science and scientists, but their formal tech-
niques cannot foresee or take into account the specific influences
relevant to given cases with which historians are concerned.

These differences in the concerns of historians and philosophers
contribute to the difficulty of achieving the interaction which I shall
advocate and to the difficulty of recognizing the need for it.

Problem: Which rational reconstruction(s) of a specific theory,
explanation, or confirmatory argument should a philosopher use?
Partial answer: One constraint, at least, is "that the explication

'resemble' the explicatum'' ([17], p. 291).

Discussion: Philosophers do not use or investigate theories in the same ways, or with the same purposes as scientists. Unlike scientists, they do not often seek to maximize calculational efficiency or to reveal new empirical results. Because their concerns are philosophical, the formulations and reconstructions of interest to philosophers are typically more abstract and further from the empirical and calculational surface than those of interest to scientists. This is as it should be. But it imposes an obligation on philosophers—namely to show that the idealized versions (rational reconstructions) of theories which they employ correspond adequately with the (changing) scientific realities and that they fruitfully capture aspects of real theories relevant to philosophical concerns.[30] Use of the criterion of resemblance by the philosopher, especially when it is combined with certain claims about the nature of theories (cf. below, pp. 34 ff.) requires him to pay serious attention to the actual course of science. Giere, who holds that there is no essential difference between contemporary science and earlier science, holds that historical study is not needed to acquire the requisite understanding of the actual course of science. The philosopher may legitimately restrict his attention to contemporary cases; in dealing with them, it is enough "to pay closer attention [than the logical empiricists did] to actual scientific theories, and to worry more about the nature of philosophical conclusions about science" ([17], p. 291).[31]

Even if Giere is correct (which I do not grant), his concession that the resemblance criterion is appropriate brings with it rather more than he has publicly acknowledged in the way of dependence upon historical study. "Actual scientific theories" are not givens; indeed, an issue lurking at the center of the dispute between historicists and logicists is how to characterize and formulate theories and theory support perspicuously. Consider theories: they are not easily recoverable from textbooks. (Textbooks tailor their formulations to their intended audience—its interests, competences and limitations. They disagree fundamentally in their formulations of what is supposedly a single theory. And they have a well-deserved reputation for historical inaccuracy.) Nor are they easily recoverable from journal articles (which usually state theories in fragmentary and divergent fashions)

[30] On the little explored topics of philosophical idealizations, their differences from scientific idealizations, and their fruitfulness, cf. [34], available from Professor Maull at Yale University. I am grateful to F. Suppe for referring me to this paper and to Professor Maull for providing me with a copy.

[31] Giere also asks for consideration of the flexibility and appropriateness of the formal tools employed by the philosopher.

or from the practice of scientists.[32] Different formulations of a given theory are commonly mutually inconsistent and often yield radically divergent empirical consequences. Even in the best cases, competing, logically distinct entities are alleged to be, or to represent, "the" theory. In short, the "*actual scientific theories*" which philosophers' reconstructions must resemble, *are themselves, inevitably, constructs,* constructs whose correspondence to and bearing on the actual practice, thinking, and writing of scientists require empirical—dare I say historical?—evaluation.

So far I have addressed a decision context question—which rational reconstructions should philosophers use in view of the criterion of resemblance? But when one examines the parallel E-context question—how does one evaluate rational reconstructions in light of the criterion of resemblance?—the need to use or carry out historical case studies becomes yet more apparent. Suppose, for example, that two philosophers who wish to evaluate a certain theory—say the Darwinian theory of evolution (or the Darwinian theory as of 1900)— employ significantly differing rational reconstructions of that theory. It is so far forth an open question whether the differences in their reconstructions stem from purely philosophical (e.g., epistemological) differences, from differences in the purposes of their investigations (cf. the differences among *EE, EM, PE,* and *PM*), from disagreements or differences in emphasis regarding the features of a single explicandum, or from the fact that their reconstructions pertain to different, though closely related, explicanda.[33] The differences in their reconstructions may indicate that they refer to different entities under the label "the Darwinian theory of evolution." Should this be so, any differences in their evaluations would be unsurprising, for one's evaluation of a theory depends on which entity one takes that theory to be.[34]

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[32]Cf. [47], Chap. VI, esp. pp. 199 ff., for a description of the role of standardized formulations (and the loss of content they enforce) in establishing facts in science. Similar effects pertain to theories, especially when they are applied in more than one discipline.

[33]Among the differing explicanda which can be readily confused with one another are competing versions of a theory, different stages in the development of the theory, and the theory employed in different (perhaps overlapping) domains. "Actual scientific theories" do not hold still; the logical constructs which philosophers evaluate often correspond to different stages, phases, or versions of actual theories.

[34]Note that the critical matter here is the delimitation of the explicandum since different rational reconstructions can pick out the same explicandum. Note also that theory evaluation is *doubly* dependent on theory delimitation, for in delimiting the theory one also specifies the domain of entities or behaviors for which the theory should account. If one changes what belongs to the theory, one typically also changes the domain of the theory, thereby affecting the shortfall of the theory from complete

Since all of these sources of disagreement (and others as well) may underlie the differences between two apparently competing reconstructions of a given theory, an immense amount of background agreement must be forged before the criterion of resemblance can be called into play to resolve the dispute. Thus use of the criterion of resemblance is not usually sufficient to enforce agreement regarding "the" proper reconstruction of a theory. Worse yet, two philosophers who employ the criterion of resemblance may have chosen as their explicandum a highly unrepresentative version of the theory of concern, a version of little interest to scientists or of no scientific merit. It is at this point that historiographical skill is most important, for it is precisely what is required to determine the extent to which the explicandum is an appropriate representative of the target theory and whether the rational reconstruction captures adequately those features of the explicandum which make it appropriate.

It does not matter whether the case chosen is contemporary or not: when the criterion of resemblance is employed against a background of sufficient agreement in philosophical position and purpose, it can adjudicate between rational reconstructions. Its use, however, forces one to make extensive use of historiographical technique in examining the case(s) of concern. Whether or not the cases have already been studied by the historians of science is, of course, irrelevant. But the result of the requisite case studies will be something like an historian's chronicle against which the philosophical reconstruction(s) should be checked. This is the fruit of the criterion of resemblance.

Moral: To establish contact with real science, the philosopher is obliged to examine the relation between his rational reconstructions and actual scientific materials. Only after such examination can he know whether the idealizations he employs are fruitful as a tool for understanding real science and, if they are, to what ends and with what limitations. Thus, use of the criterion of resemblance is sufficient to force the philosopher into "messy" empirical studies of the complex relationships between rational reconstructions and real theories, explanations, and so on—studies whose outcome typically depends on the resolution of historical questions.

Problem: Can we weaken the logicist stance in the light of the criticisms raised earlier in this paper without opening the floodgates to strong historicism? *Partial answer:* The best hope appears to lie

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success both by changing the set of behaviors which the theory *ought* to explain and by changing the set of behaviors which it *succeeds* in explaining.

in admitting temporal considerations into the logic of theory testing.[35]

Discussion: Where does this leave the logicist? The resultant weakening of logicism undercuts the tendency to evaluate theories, explanations, and so on solely as finished products, solely as closed systems. Thus, in line with Giere's suggestion and in the light of formulae (7), (8) and (9) above, one can weaken clause (Liib) of the logicist thesis (p. 5 above) by including some temporal considerations in the input employed in evaluating theories. The logicist can claim, e.g., that theories are to be evaluated by use of

(Liib′)   properly parsed and dated statements of the total relevant evidence available during the period in which the theory has been tested, together with properly parsed and dated statements of the theory, its best competitor(s), the various adjustments made in them, and the predictions based on them.

This formulation allows a more realistic account of theory testing along the lines proposed by Giere and has the merit of explicitly acknowledging the relevance to theory assessment of a theory's *resilience* and of its *comparative standing*. These virtues, however, come at the cost of complicating the relation between a logistically tidy formulation of the theory *as it stands at any given time* and the process of assessment.

Logicism as thus revised is a form of minimal historicism. I shall argue shortly that it does not go far enough in its response to historicist criticism because it fails to take proper account of the role of background knowledge and because it does not recognize the deeply historical character of theories. But let us assume for the moment that this weakening of logicism will prove adequate for the purposes of *EE*. What would follow regarding the importance of historical study for these purposes? According to clause (Liib′) there are universally valid evaluative techniques which, in their application, require modest historical information. Specifically required is information about the temporal order of empirical discoveries and theoretical derivations and knowledge of the interaction among a theory, its competitors, and recalcitrant data. In contemporary cases, such information will often belong to the "common knowledge" of the

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[35] "It is at least possible that the reason successful predictions (in the temporal and not the logical sense) support the ontological claim [in favor of the existence of theoretical entities] is simply that valid *testing* of a theory requires that the theory not be designed to fit the data. Explicitly predicted results insure that this requirement is met. This, anyway, was Peirce's view of theory testing and it may be supported by some modern views of hypothesis testing as well" ([17], p. 289).

relevant group of specialists. Such knowledge, however, is often mythological in character;[36] if one is to use it in theory evaluation, one should subject it to appropriate historiographical checks. Thus, while it is important to recognize that not all investigations of the temporal order of a set of events require the use of historiographical techniques (cf. cosmological investigations of the sequence of events shortly after the "big bang"), in the present case such techniques are needed to resolve disputes over temporal priority and to vouchsafe the accuracy of the information used in evaluating specific theories and explanations.

The replacement of clause (Liib) with clause (Liib') does not, however, weaken logicism sufficiently. The latter clause does not take full account of the pervasive role which background knowledge does and should play in scientists' reasoning and decision making, and in their evaluation of theories.[37] The recent trend toward anti-foundationalism in the theory of knowledge highlights the role of background knowledge usefully. Antifoundationalists hold that particular knowledge claims receive their support from intersubjectively accepted assumptions and knowledge claims which, though they are well supported and though they command (nearly) universal assent, are never wholly sacrosanct or beyond challenge. In any such theory of knowledge, coherence with accepted background beliefs is a major (though perhaps an overridable) constraint on acceptance of knowledge claims. Indeed, when interpreted strongly, this constraint treats background knowledge as "locally" unproblematic;[38] it treats *change* of

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[36] Mythological for good reasons—it must serve the interests and social needs of the relevant profession. Cf. [47], esp. Parts III and IV, for an account of the function in professional organizations of such mythologies and for some instances of the degree to which such mythologies can distort the truth.

[37] For an analysis of that role, cf. [40]; recall also the example of note 28 which illustrates the importance of factors in the background not taken account of by (Liib').

[38] Background knowledge, of course, can change. Such changes sometimes place old anomalies or empirical oddities in a new light. For example, Fermi conducted an experiment in 1935 in which uranium fissioned, but until the theoretical possibility of *splitting* (as opposed to radioactive decay) of atoms was recognized, no one could make sense of his results or of the peculiar (misidentified) "contaminants" which chemical analysis revealed. It was because of a shift in background knowledge that Hahn and Meitner's later, related experiments served as the vehicle for the discovery of fission. This seems to be a typical example of the role of the close theoretical background.

It would be worth investigating whether changes in background knowledge, perhaps even distant background knowledge, are partially responsible for the well-known but surprisingly high incidence of simultaneous discovery in the history of science. If such a connection could be shown, it would tend to support the claim that background knowledge affects the rational credibility of hypotheses.

belief rather than belief proper as (in general) requiring justification.[39]

Rational acceptability is thus an *ecological* matter. One of the most important, sensitive, and difficult jobs of the innovative scientist is to determine which parts of the background he can ignore as irrelevant, as sufficiently adjustable, or as ultimately overthrowable, and which parts of it must be made to cohere in relatively short order with his hypothesis, theory, or point of view if the latter is to be worthy of further investigation or, ultimately, of belief. To appreciate the delicacy of such considerations, one should realize that major segments of the background knowledge may occasionally be placed in epistemic jeopardy. For example, judged by a rigid standard of coherence with the background knowledge, Galileo's heliocentrism can only count as irrational. Yet, clearly, if our standards of rationality are to be at all acceptable, his rejection of much of the Aristotelian background must be evaluated positively and the criticisms based on the lack of coherence between his theory and that background overriden.

The philosopher probably cannot offer general guidelines or criteria for the adequacy of the solutions to these extremely delicate and highly contextual problems, but he cannot evade the philosophical problems posed by the division of background beliefs into relevant and irrelevant and by the need to weight the coherence with background beliefs against other epistemic desiderata.

Moral: Logicism must be weakened to accomodate minimal historicism. This makes the *comparative standing* and *resilience* (among other properties) of theories relevant to their evaluation. These properties cannot be assessed without essential reliance on historiographical techniques and empirical study. Furthermore, proper evaluation of a theory's rationality is contextual: it requires knowledge of the relevant background of beliefs, problems, theories, experiments, and instruments affecting the application of and support for the theory. Knowledge of this background depends on knowledge of the historical context in which the theory is used.

Final Problem: What is a theory? *Partial answer:* A theory is a

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[39] Nelson Goodman's well-known views have just this structure. The primary ground against accepting claims employing grue-like predicates is the lack of projectability of those predicates. Projectability thus becomes essential to the evaluation of inductive inferences and theoretical claims. But projectability is a nonformal property depending on the history of term usage (and thus, *inter alia,* on the cultural context): though the projectability of a particular term may depend on its logical and definitional relations to other terms, projectability is ultimately determined by the entrenchment of predicates employed extensively by the relevant community.

growing, developing entity,[40] one which cannot be understood as a static structure.

Discussion: What is it then that scientists learn from ahistorical textbooks when they learn, say, the special theory of relativity or the synthetic theory of evolution? "Unless one is willing to claim that most scientists really do not understand the theories in their fields and could not learn them from standard texts, the claim that history necessarily enters consideration of the structure of theories must be rejected" ([17], p. 293). The label "theory" is, perhaps, overloaded here. But what scientists learn from textbooks as, say, "the" special theory of relativity is by no means always the same. Each textbook formulates the theory differently. Some, perhaps, offer different formulations of a single version of the theory, but others formulate different versions. (I take two formulations to be formulations of a single version when they are, at least roughly, logically equivalent. Cf. [4], pp. 10 ff.) Not only are there formulations of different depths, but also formulations with different, often discrepant observational consequences, formulations which employ different (not necessarily equivalent) mathematical apparatus,[41] formulations with different implicit metaphysics, etc. Science textbooks aim at formulating a single version of each major theory on which the student is expected to draw in pursuing his studies in a given discipline. The formulations used in different disciplines, notoriously, need not be concordant.

But a theory is not just a set of formulations with a common formal structure. The theory develops through time to deal with different domains, to utilize various mathematical and experimental techniques,

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[40] "Now some members of the historical school seem to hold that a theory is an historical entity, or at least that one could not really understand a theory without knowing some temporal developments. Thus the structure of a theory cannot be captured by any single logical or mathematical structure" ([17]. p. 293). I support a version of this position in [4].

[41] Cf., e.g., Sneed's careful account ([59], pp. 206 ff.) of the interrelations among certain formalizations of certain Newtonian, Lagrangian, and Hamiltonian versions of classical mechanics. Sneed shows that his Lagrangian and Hamiltonian formalizations may, in his technical sense, be treated as theories of mathematical physics (formal structures with certain physical models) *distinct from Newtonian mechanics.* He shows that his Lagrangian mechanics is "effect equivalent" to Newtonian mechanics and that his Hamiltonian mechanics is "effect equivalent" for conservative systems to Newtonian mechanics. (Effect equivalence is defined on pp. 187–188 of Sneed's text; two theories are effect equivalent if, roughly speaking, each theory may be extended in such a way as to yield the same physical claims as the other about the behavior of the entities of certain specified physical domains.) Sneed is unable to prove a stronger form of equivalence, "theoretical equivalence," for his versions of these theories.

to handle a variety of anomalies, to repair foundational flaws, and so on. Some of its formulations are general while others are specialized to fit the needs of some (sub)discipline or the facts in some domain. The general formulations of a theory do not and need not reveal the specialized elaborations which the theory allows.[42]

Specialized elaborations are a (perhaps the) major source of the empirical content of a theory. Furthermore, the specialized versions of a theory can yield anomalies, reveal foundational problems, and suggest more powerful ways of formulating the theory; as a result they occasionally support significant revisions in the unspecialized formulations of the theory. Indeed, one may learn and successfully apply different specialized versions of a theory without any deep understanding of what makes them into versions of the same theory or of the power and flexibility of that theory.[43] On the other hand, as my epigraph from Monod suggests, one may also recognize, evaluate, and exploit the developable powers of a theory which are not exhibited in any of its current formulations.

As theories develop, their plasticity and their capacity for further development change. At *any* stage, there is room for further development, but at different stages, the directions in which development is possible or natural are quite different. Some of these might be incompatible; there is no unique mature form toward which a theory, if it develops at all, is inexorably driven. Very different mature versions can derive from the same intermediate stage in the career of a given theory.

Theories, then, are historical entities which cannot always be identified or fully understood by reference to the formal structures and empirical consequences of their extant versions. Their developmental capacities must be taken into account—a task which typically requires one to learn something about their developmental histories and their intellectual and physical environments. As they develop, their long-term potentialities alter in ways which can be estimated,

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[42] Compare [47] on the different formulations employed by different disciplinary and professional subgroups and on the degradation of specific content required to achieve generally comprehensible cross-disciplinary formulations. Ravetz states the point most succinctly in connection with his account of the evolution of what he calls facts, but the point is a general one.

[43] Of particular importance is the fact that the time scale on which versions of theories develop and new versions of theories are elaborated is frequently much greater than that of a scientific career. Thus an examination of the long-term historical context of a theory version may prove a useful critical tool in the attempt to improve current formulations of the theory, for it may help bring them in line with the spirit of the theoretical program to which they belong or it may result in a better understanding of the available alternatives to current formulations.

but not rigidly determined. Yet estimation of these potentialities, a key element in the evaluation of theories, depends on a study of the history of the relevant theories and of the problem contexts into which they enter.

Let me illustrate some of these points sketchily by reference to that prime example of logicist philosophers, Newtonian mechanics. Newtonian mechanics cannot properly be identified with any of its formulations, not even Newton's.[44] There are many versions of the theory, differing not only in their structure and in their known empirical consequences, but also in their implicit empirical consequences—most obviously in the specialized force laws which they allow and in the range of cases which they comprehend.

The main elaboration of the Newtonian theory took about 200 years at the end of which time Newton, were he alive, might well have had difficulty recognizing it. Indeed, he might have protested that a quite different mature form of the theory would have been truer to the spirit of his intentions. To put the matter strongly, the length of time it takes to develop such a theory and the real possibility of alternative developments together imply that it requires hindsight to judge the relative importance of the various claims in a book like the *Principia*. Some of these claims are central to the theory which Newtonian mechanics has become (though it may still develop in surprising ways!),[45] others must be revised because of the idiosyncracies of Newton's formulation, and still others are errors due to his incomplete knowledge of the structure and consequences of his system.[46]

Traditional logicism mistook theory versions for theories. After analyzing the (fixed) structure of a theory and showing how to

[44]There are many nonequivalent formulations, e.g., Hamilton's, Hertz's, Jacobi's, Lagrange's and Laplace's. (See note 41 above.) In the *Principia*, Newton does not formulate the second law symbolically, but a literal reading of his words yields $\int F dt = \int m dv$; the law is formulated in terminology applying only to collisions and impulsive interactions so that the integral runs over a single brief interaction. It is arguable whether Hertz's formulation, which is remarkable in being force-free, is really a version of Newtonian mechanics.

[45]Compare the problem of determining which character traits of a 20-year-old will prove central to that person's character at age 60 or of determining which traits of a species (if any) will prove essential to its survival. Recent work in neo-Newtonian theories, e.g. by Dicke, illustrates the point nicely.

[46]An example of each: *an error:* Newton's claim that God's intervention is necessary to prevent perturbations of the planetary orbits from disrupting the stability of the solar system. *An artefact:* the impulse case formulation of the second law. *A case of disputed centrality:* The absoluteness of space and time. Even now the precise meaning of "absoluteness" and its necessity to the Newtonian system are being debated—and Newton's own strong views on the topic are not necessarily decisive.

effectively delimit its empirical consequences,[47] it evaluated the version by comparing it with the available evidence. But the confusion between versions and theories is fatal: no such evaluation can tell us the worth of Newtonian mechanics as described above. Indeed, by ignoring the possibilities of alteration and development implicit in the version evaluated, traditional logicism also missed a major component in the epistemic value of the version itself.

Moral: Theories are historical entities; their proper evaluation requires a full appreciation of their historical character and context. Logicist techniques, which are essential but not sufficient for the proper evaluation of theory versions, do not currently include tools for understanding the unity and continuity of a theory throughout its history (though[59] and [66] contain serious attempts in this direction), nor for measuring the plasticity of the theory. Yet the latter is a major component of the theory's power and its fitness and thus should be given significant weight in evaluating the theory. To develop these tools, logicists will have to take account of the history of the theory and its environment, thereby incorporating a moderate historicism into the logicist enterprise.

CONCLUSION

Historical study and historical sensitivity are required if one is to identify properly the problem contexts which scientists face and the entities (law claims, inductive arguments, hypotheses, explanations, theories, theory versions, and so on) with which they work. Since philosophical evaluation of particular scientific arguments, decisions, explanations, procedures, and theories requires such input if it is to accomplish its aim, historical inquiries are of central importance in the philosophy of science. More specifically, the choice among differing rational reconstructions of an explanation or a theory is not an armchair matter. It ought to depend not only on the purposes for which the reconstruction is to be used and the normative commitments of the philosopher, but also on the best available interpretation of the historical record. This is the way to minimize the risk of anachronism and irrelevance, always serious in abstract philosophical treatments of science.

According to one major strand of my argument, if one wishes to assess the degree of support which a theory (or theory version) enjoys,

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[47]Note, however, that the set of consequences which count as empirical is itself a function of the available instrumentation, the status of auxiliary hypotheses and theories, and so on.

one ought to utilize considerable historical information regarding the theoretical background; the status of neighboring and auxiliary theories; the temporal order among experimental discoveries, theoretical derivations and predictions by competing theories; and so on. In particular, it is well known that scientists take account of a wider range of background information in evaluating theories and deciding how to test them than standard philosophical accounts of theory evaluating and testing suggest they should. This poses serious problems regarding the determination of those factors, if any, in the background which philosophers ought to consider relevant—and regarding the correctness of current philosophical theories of support and testing. The resolution of these issues requires serious examination of accurate case studies. But accuracy is, of course, not enough—the case studies must be shaped so as to yield information about the *kinds* of background information considered relevant, the *uses* to which it is put, the *reasoning* by which these uses are justified, and so on.

Again, particular philosophical evaluations of particular theories and theory versions are subject to a weak form of testing on the basis of the historical record. Epistemic evaluations, I have argued, provide modest guidance in D-contexts. In employing or testing a theory, scientists will at least occasionally act as such evaluations (when supplemented by appropriate auxiliary premises) suggest. When competing evaluations suggest different D-context policies, one can compare the outcome of those actions which accord with each policy with the outcome of alternative actions. Consistent success for one policy, paired with consistent failure of the other would tend to support the corresponding evaluation.

When one turns from the consideration of particular explanations, theories, theory versions, and so on to more general doctrines in the philosophy of science, e.g., concerning the logic of support, the results of this study are not quite so clearcut. However, the feedback effects between evaluation and prescription discussed in the preceding paragraph are clearly usable in the attempt to evaluate competing analyses of the logic of support. Historical study of these feedback effects might prove to be an important tool in evaluating philosophical epistemologies and methodologies. (Cf., e.g., Agassi's, Feyerabend's, and Lakatos's diverse arguments against "inductivist methodologies" on the ground that if scientists had actually conformed to such methodologies, the development of Copernicanism and other progressive theories and programs would have encountered insuperable obstacles.) Similarly, the challenge posed by the historical record of finding adequate criteria of relevance governing the proper use of background information suggests an *interactive* approach between

the philosopher and the historian: as the philosopher explores possible criteria of relevance, the historian attempts to apply them to appropriate cases to see how well they fare, whether they clarify what actually occurred or not. When such interaction works, each scholar will modify parts of his account on the basis of the other's results.

Of special interest in this connection is my claim that theories, as opposed to their versions, are historically developing entities. If true, this claim enforces an even deeper symbiosis between history and philosophy of science, for it makes the proper delimitation of theories into an essentially historical task and it requires a complete recasting of the evaluation of theories and theory versions so as to take account of their developmental plasticity. Logicist techniques, though not alone sufficient, will remain central to the evaluation of theory versions in this connection, but their importance to the evaluation of theories is a wide open question. At issue, among other matters, is the very deep and difficult problem of establishing criteria of identity and individuation for growing, developing, changing theories. The closest analogue to this problem which I have been able to locate is the problem of the identity and individuation of biological species. If my account of theories is correct, philosophers will be forced, as biologists are in dealing with species, to delimit theories by historical as well as formal and structural techniques. As a matter of principle, then, analysis of the formal structure and empirical support (as of a given time) for a theory *version*, or for all of a theory's extant versions, will prove inadequate for the epistemic evaluation of the theory proper.

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