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November 2015, Eric Palmer epalmer@allegheny.edu

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Lakatos's "Internal History" as Historiography

Eric Palmer
University of Kentucky

Imre Lakatos's conception of the history of science is explicated with the purpose of replying to criticism leveled against it by Thomas Kuhn, Ian Hacking, and others. Kuhn's primary argument is that the historian's internal-external distinction is methodologically superior to Lakatos's distinction because it is "independent" of an analysis of rationality. That distinction, however, appears to be a normative one, harboring an implicit and unarticulated appeal to rationality, despite Kuhn's claims to the contrary. Lakatos's history, by contrast, is clearly the history of a normatively defined discipline; of science and not scientists and their activities. How such history can be written, the historiographic and critical tools available for its construction, and its importance as history are considered in detail. In an afterword, the prevalence of Lakatos's treatment of history in philosophical discussion is indicated: a related approach is shown to arise in social contract theory.

Long Live the Provisional Government! [ON a PLACARD in SERGEI
EISENSTEIN'S *Ten Days That Shook the World*]

Introduction

Imre Lakatos's conception of the history of science, which he dubbed "internal history," has suffered much and varied criticism from philo-

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ophers and historians. Ian Hacking calls it "an imaginative perversion of history," Thomas Kuhn suggests that it is "not history at all but philosophy fabricating examples," and Larry Laudan goes so far as to write that "Lakatos nowhere establishes the necessity (or the desirability) of making a reconstruction of the past which involves an intentional warping of the historical record" (Hacking 1983, p. 252; Kuhn 1971, p. 143; Laudan 1977, p. 170; see also Holton 1978, pp. 106–8). But is Lakatos simply so poor a historian that he does not see virtue in keeping the historical record straight, or is he trying to attain some goal with his "warping of the historical record"? Many authors, such as Laudan, do appear to believe the former (Laudan 1977, p. 168). Some others have attempted to explain Lakatos's historiography and have come up with more or less plausible explanations for why it differs so radically from other historical projects (see esp. Kulka 1977). Hacking presents a particularly interesting and detailed interpretation (Hacking 1979, pp. 392–99) but cannot abide some of Lakatos's more serious departures from historical fact and so, in reference to one of Lakatos's case studies, presents the first remark quoted above!

I do not think Lakatos's historiographic approach need be considered so absurd and perverse, however, and I will argue that internal history can be a clearly defined and useful study of the past, representing one among many valuable historiographic approaches for viewing the history of science. Lakatos's internal history has received rough treatment, by these authors and others, primarily because it is routinely compared with other genres of history that depend upon a somewhat different internal-external distinction among explanations, and is found to be lacking according to the historiographic criteria that are used to judge such history.¹ The projects embodied in Lakatos's internal history and in other internalist approaches, however, have very different goals and standards, and I will suggest that Lakatos argues effectively for a clear demarcation between them. Lakatos's concerns differ from those of intellectual historians, for example, particularly because he is not concerned with interpreting the thoughts and behavior of individual scientists.

I will go on to consider in more detail the character and goals of internal history, for its distinctness is not so apparent without a clear survey of its genuine advantages. Internal history will be shown to be

1. Though Lakatos adopts and alters a preexisting term (for a history of the term, see Shapin 1992), here I will reserve the term "internal history" for Lakatos's conception of the internal history of science alone. Historians of ideas and others also present "internalist approaches" and make different internal-external distinctions, which I will explain presently.

useful in explaining the *retrospective* importance of the history of science to current practitioners; it is noteworthy in that it analyzes history according to an assessment of growth and correct methodology provided by philosophers and scientists together. I will also suggest that other internalist approaches do not provide an ideal position from which to launch methodological criticisms of internal history, since internalism, at least as it is developed by Kuhn and Hacking, retains some fundamental methodological weaknesses of its own. Consequently, narrow criticism of Lakatos's work from the approach of intellectual history, or other approaches that utilize an internal-external distinction markedly different from Lakatos's, is neither more nor less appropriate than criticism from other approaches to history.

I will require significant license to present my interpretation of Lakatos: for example, I will disagree with some of his disparaging remarks regarding sociological explanation. This license I will attempt to point out in footnotes, as Lakatos would have done. Nonetheless, I believe that Lakatos had much the same project in mind.

Internal History versus Other Internalist Conceptions: The Root Difference

Lakatos's internal history differs greatly from other approaches that utilize an internal-external distinction because it is different at its foundation: a clear historiographical (i.e., methodological) demarcation separates the approaches. To construct a satisfactory argument in defense of his project, this demarcation must be exposed.

To begin, we must address the concern that Lakatos is a poor historian, and consider the most prominent areas of disagreement between Lakatos and his critics. To the great discomfort of many, especially historians, Lakatos makes a number of notorious claims, such as, "one way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and to indicate *in the footnotes* how actual history 'misbehaved'" and "history of science is frequently a caricature of its rational reconstructions" (Lakatos 1971a, pp. 120, 138).² He has also produced a small amount of historical writing in the vein of rational reconstruction, committing himself in the practice of writing history to positions complementary to those he expresses concerning historiography: for example, he deliberately attributes a position to Niels Bohr thirteen years before Bohr held it

2. Pages cited for Lakatos 1970, 1971a, 1974, and 1976, and Lakatos and Zahar 1976, refer to reprints of the articles within I. Lakatos, *The Methodology of Scientific Research Programmes: Philosophical Papers*.

and notes the distortion in a footnote (Lakatos 1970, p. 61; see also Lakatos 1971a, p. 119). The very presence of that footnote, however, suggests that Lakatos cannot simply be dismissed as a poor historian on these grounds. The note would appear to be explainable only if Lakatos does have a sense of historical fact and so of history separable from reconstruction; and indeed, Lakatos does speak of the "fact" of Bohr's knowledge as opposed to his own reconstruction (Lakatos 1970, p. 61, n. 1). In reference to another reconstruction, Lakatos writes, "Alas, all this is rational reconstruction rather than actual history" (Lakatos 1970, p. 53, n. 1). At many places in Lakatos's writing a reader can find reference to an "actual history," understandable and distinct, and explicitly opposed to the rational reconstruction that represents internal history (Lakatos 1970, pp. 52–53; Lakatos 1971a, p. 102).³ What is the status, then, of a rational reconstruction?

The reason behind Lakatos's curious project lies in a difference between the theoretical foundations of his internal history and other internalist approaches. Intellectual history, as one example of an approach that maintains an internal-external distinction, might be conceived of as an attempt to detect and explain the historical ordering, development, dissemination, and effects of ideas and intellectual traditions. Because the focus of the history is placed upon the roles of ideas and beliefs in history, historians conceive of an internal-external distinction for explaining the historical change of ideas, in order to discern intellectual, as opposed to nonintellectual or extradisciplinary, foundations of change.

As a prelude to criticizing Lakatos's conception of history and his internal-external distinction, Kuhn lays out a fair description of such a division:

In standard usage among historians, internal history is the sort that focuses primarily or exclusively on the professional activities of the members of a particular scientific community: What theories do they hold? What experiments do they perform? How do the two interact to produce novelty? External history, on the other hand, considers the relations between such scientific communities and the larger culture. The role of changing religious or economic traditions in scientific development thus belongs to external history. [Kuhn 1971, p. 140]

3. Lakatos 1976 deals with this subject in great detail. Note that whether or not the writing of "actual history" could possibly be a realizable enterprise—a subdiscipline of history—is left undecided here. I am inclined to believe that it stands as something of

Though Kuhn does not represent the mainstream of intellectual history in some of his work—for example, "paradigms" internal to Kuhn's approach may often be models or concrete instantiations, not ideas—the distinction broadly sketched in this quote is intended to apply to the "standard usage among historians." In its lack of specificity, it fairly enough represents a variety of internalist approaches to history, including intellectual history.⁴

Lakatos's conception of internal history, however, is not at all that of other internalists, such as the intellectual historian, as he explicitly points out (Lakatos 1971a, p. 102, n. 1). Rather than basing the distinction in the professional activities of scientists, Lakatos wishes to ground it instead in an understanding of what science *is*: that is, according to a philosophical methodology (also called a theory of science, logic of scientific discovery, theory of the growth of knowledge, or theory of the rationality of scientific progress). Lakatos is engaged in what he considers to be history of *science*, "science" being a philosophical, normative category, which may be opposed to "ideas." "History of *science* is a history of events which are selected and interpreted in a normative way. This being so, the hitherto neglected problem of appraising rival logics of scientific discovery and, hence, rival reconstructions of history, acquires paramount importance" (Lakatos 1971a, p. 121). "Theories of the rationality of scientific progress . . . [provide] a theoretical framework for the rational reconstruction of the history of science" (Lakatos 1971a, p. 118). Lakatos maintains that, because science is a category defined according to a theory of science, the content of a written history of science must be intimately affected as a result of which theory one chooses. Kuhn has sketched a distinction between internal and external history that may be applied to history of ideas; Lakatos's internal history is a history of the scientific, and this difference turns out to greatly affect the way in which history is written, and the purpose, and use, of what is written.

Internal History as a Coherent Discipline: Writing Internal History

What does this imply for historical writing, then? That which is internal to the history of science, on Lakatos's scheme, can only be what is considered scientific, and so a theory of science and what it is to be

a regulative ideal for Lakatos, who maintained that "*history without some theoretical 'bias' is impossible*" (Lakatos 1971a, p. 120).

4. Thanks go to an anonymous reviewer at *Perspectives on Science* and Steve Fuller for clarifying the need to distinguish Kuhn's internalist approach from intellectual history. For a detailed treatment of the internal-external distinction in history and sociology, though not in Lakatos's writing, see Shapin 1992 (N.B. pp. 336, 349–51).

scientific is necessary to get internal history off the ground. Therefore, the choice of a theory of science is of prime importance to the internal historian, because it provides the criterion of demarcation and tells the historian what is and what is not science, what does and what does not belong in internal history. Different theories of science may provide different accounts of what is scientific and present different historical developments as representative of scientific growth. Thus, Lakatos claims, conventionalists find the Copernican revolution to be a paradigmatic case of rational growth, because it represents a triumph for simplicity among otherwise closely matched rivals; inductivists, on the other hand, find Kepler's discovery of the elliptical orbit from Tycho Brahe's observations particularly representative of growth. Each methodology may provide a history that finds the other development to belong to external history, however.⁵ The varied theories provide differing reconstructions of the history of scientific growth and differing accounts of the contribution of historical events to that growth; it is not surprising, then, that Lakatos has as a major purpose of the historiography of science the evaluation of histories for the purpose of determining the relative adequacy of different theories of science. How the adequacy of a reconstruction may be judged will be considered below, along with the rationale behind such an effort; assessing the relative merits of theories of science from the historical cases is beyond the scope of this article.

Internal history differs from other forms such as the history of ideas, then, in that it is a history of the growth of scientific knowledge itself, entirely pure of adulteration, according to the theory of science used in the history's construction. Lakatos makes his distinction between the scientific and the nonscientific most evident by referring to a "third world" to which this history is tied and in which knowledge resides independently of the knower: "*the—rationally reconstructed—growth of science takes place essentially in the world of ideas, in Plato's and Popper's 'third world,' in the world of articulated knowledge which is independent of knowing subjects*" (Lakatos 1970, p. 92; see also Lakatos 1971a, p. 110). This third world is to be contrasted with the first (of material objects) and the second (of psychology, beliefs, and ideas). It is "independent" of the others in some sense; precisely what it consists of and how it is independent will be considered shortly. In the light of Laka-

5. Lakatos (1971a, p. 116) shows a clear example of the way in which methodologies will produce different internal histories. Different theories of science will also affect the writing of external history according to Lakatos, and not merely insofar as they will categorize internal and external explanation differently: they will affect the substance of explanations as well; see Lakatos 1976 and n. 20 below.

tos's distinction among worlds, internal history is an attempt to present the history of the development of this third world, in third world terms: the real subject matter of internal history is the historical growth of scientific knowledge according to a methodology, and not the activity of people at all. Lakatos is constructing a history of science itself, in a "world" perceived through the lenses of a theory of science (itself within that world), populated only with knowledge, not people, and not their ideas nor their actions. History in *that* world is the history of objective growth, and so the difference between that history and the history of ideas, which is focused more upon the history of the second world, is understandable. Actual history and intellectual history follow time order, for example, whereas Lakatos suggests that objective growth need not, so it is not surprising that the histories of the second and third worlds may not look the same.

This interpretation explains why Lakatos is willing to discount the character of history in the second world, the world of consciousness: he may forsake the ordering of beliefs as they arise in the second world on occasion in order that he may write a history of the logical growth of knowledge in the third world. Niels Bohr, then, does not exist in internal history—he and others are replaced by the Bohrian scientific research program, according to Lakatos's preferred methodology—and what the person believed and why he believed it are matters external to the history of the growth of science; they are matters of psychology and situation, that is, matters of the second world. In this light, Lakatos's explanation of why he has written his history in this fashion should not appear odd at all:

Internal history is not just a *selection* of methodologically interpreted facts: it may be, on occasion, their *radically improved version*. One may illustrate this using the Bohrian programme. Bohr, in 1913, may not have even thought of the possibility of electron spin. He had more than enough on his hands without the spin. Nevertheless, the historian, describing with hindsight the Bohrian programme, should include electron spin in it, since electron spin fits naturally in the original outline of the programme. Bohr might have referred to it in 1913. Why Bohr did not do so, is an interesting problem which deserves to be indicated in a footnote. [Lakatos 1971a, p. 119]⁶

6. In his 1976 article, Lakatos retracts the position embodied in this passage, referring to it as "a rather unsuccessful joke . . . such parodies may be written, and may even be instructive; but I never said that this is the way in which history actually ought to be written." Presumably, he also dissembles the earlier discussion of Bohr (Lakatos 1970,

I have attempted to sketch a coherent disciplinary structure for internal history. It might pass muster, making further pursuit of the details of its methodology and purpose worthwhile, provided sense can be made of the claim that objective growth does not strictly follow time order, yet still may be considered historical. If we take Lakatos to intend that internal history is about the third world, about knowledge, and not about psychology and individuals, then his efforts should not be construed as an attempt to brand scientists as irrational; they should rather be taken as an attempt to elucidate and explain the rational structure of scientific knowledge and the rationality of scientific growth, taken impersonally. Lakatos disrupts the time order of the acquisition of beliefs by individuals, then, because of this goal, and within his historiography he maintains that certain developments in the objective growth of knowledge may have occurred in the wrong order in actual history.

Perhaps more is needed to explain this extravagance with historical ordering, however. Lakatos appears to suggest that science has a logical structure that history might on occasion have filled in out of order, the disorder perhaps arising from sociological factors. In a metaphor, a few bricks were missing in the original construction project and were later tapped into place. Who tapped them in, and when, is less important for Lakatos's history than the fact that they *were* placed there, and the building was completed. Can anything more be said concerning the "logical structure" of science, beyond metaphor? Lakatos refers to both Plato and Popper as sources of his conception of a third world in which this structure resides, but the ideal worlds of the two authors differ, particularly in that Popper's world is populated with objective knowledge—conjectures, theories, and arguments—which is an emergent human product, and may be objective without being true (Popper 1972, chap. 3; esp. pp. 112, 119–23). Taking up Popper's conception, we find something by way of explanation of this structure: "The third world has grown far beyond the grasp not only of any man, but even of all men (as shown by the existence of insoluble problems). Its action upon us has become more important for our growth, and even for its own growth, than our creative action upon it" (Popper 1972, p. 161).

Popper particularly points to unsolved problems within theories, such as Goldbach's conjecture, and well-formed but unanswerable

questions (see, e.g., p. 161, n. 11), as examples indicating the structure and mind-independence of the third world. Popper maintains that the "objective logical content" of a theory may be greater than its development in human minds in the second world, providing a property of the third world that distinguishes it from the second (Popper 1972, pp. 155–56). Second world history, or psychohistory, follows time order, or, alternatively, it is "causal" (Popper 1972, p. 114); third world history, as a "radically improved version," might not. The extent to which it might diverge from time order, and the extent to which divergence weakens an internal history, are indicated by the methodological constraints on writing internal history and its uses, which will be discussed below.

Defense of Internal History against Criticism of Kuhn and Hacking

I have attempted to convey what it is I think Lakatos is after in his internal history; but a question still remains as to why one would *want* to pursue history in the confines of the third world: the question raised by Kuhn's review of Lakatos's article. Kuhn, like most critics of Lakatos, does not consider him to be pursuing history at all, because Lakatos's internal-external distinction does not allow many of historians' tried and true methods of explanation, such as appeals to aspects of individual psychology, to count internally as explanation (for sentiments essentially parallel to Kuhn's, see also Hacking 1983, p. 122). But is Lakatos's project nonhistorical? And are other internalist approaches remarkably more robust?

First, let us note more precisely Kuhn's criticism of Lakatos's project. Kuhn claims to detect a simple circularity and suggests that internal history is a pointless endeavor: "If 'internal' [as Lakatos uses it] were an independent term unequivocally applied, as it is for the historian, then one could hope to learn something about rational methodology from the study of internal history. But if 'internal history' is simply the rational part of history, then the philosopher can learn from it about scientific method only what he puts in" (Kuhn 1971, p. 141). Kuhn claims here that Lakatos's position, due to a fatal circularity, lacks the critical power available on his own approach. From the quotation of Kuhn's discussed in the second section of this article, it appears that the shortcoming relates to the lack of attention to what Kuhn calls "the professional activities" of scientists, which include (real) experiments and the psychological idiosyncrasies of participants. The more traditional historian, then, can learn about methodology by studying scientists' professional activities; Lakatos's historian cannot. Such are Kuhn's doubts.

pp. 61 ff.), noted above in this article. This surprising comment, in the context of the criticism from Kuhn that elicited it, will be considered in the following section of the article and its footnotes; I close this section with an attempt to characterize what Lakatos saw as the useful positive historiographic program that underlies the "joke," producing it as a natural, if spurious, outgrowth.

I doubt that Kuhn has characterized Lakatos's position correctly. The argument from the beginning of the next section through the rest of the article will provide a general response regarding the critical power and practical usefulness of internal history. Before proceeding with that task, however, Kuhn's internal-external distinction should also be scrutinized, for Lakatos suggests that the independence that Kuhn claims for the methodology of the history of ideas is available to *no* historian (Lakatos 1971a, p. 120; Lakatos 1976, p. 192).

What, then, is the basis of Kuhn's distinction, which he has claimed is "independent" of any analysis of rationality? Kuhn lists features of internalist intellectual history—experiments, theories, intellectual idiosyncrasies, and so on—and external features—religion, technology, and so on—and claims that "the internal-external distinction is not always hard and fast, but there is wide consensus in its application among historians" (Kuhn 1971, p. 140). He does not present a detailed account of a reasonable grounding for that consensus, however, or even a purpose for the distinction; so what is the basis of the distinction? For lack of an explanation from Kuhn, I move to speculation and suggest that the distinction is maintained—albeit "not always hard and fast"—because the "historians" harbor a lurking intuition that experiments and theories are more appropriately relevant to the development of ideas than religion and technology. But what is this relevance relation? Religious belief has greatly affected the activities of scientists and the history of science: it would be considered an internal feature in some historical cases if Kuhn were to adopt the actors' categories,⁷ and it is a sort of intellectual activity—so why is it ruled external on Kuhn's demarcation of internalist approaches? Might it be that Kuhn and others have a deep-seated, but unexamined, conception of rational historical change in science and irrational sources of change, represented in the internal-external distinction?

Kuhn does not really enlighten us regarding the foundations of his internal-external distinction; and Hacking, in a similar critique of Lakatos, does no better a job. He writes:

Lakatos begins with an "unorthodox, new demarcation between 'internal' and 'external' history" ([Lakatos 1971a] p. 102), but it is not very clear what is going on. External history commonly deals

7. Few, I believe, would doubt that Newton was forthright in his assurance that, "when I wrote my treatise [*Principia*] about our Systeme I had an eye upon such Principles as might work with considering men for the beleife of a Deity & nothing can rejoyce me more then to find it usefull for that purpose" (Newton to Richard Bentley, 10 December 1692, in Newton 1959–77, vol. 3, p. 233).

in economic, social and technological factors that are not directly involved in the content of a science, but which are deemed to influence or explain some events in the history of knowledge. . . . Internal history is usually the history of ideas germane to the science and attends to the motivations of research workers, their patterns of communication and lines of intellectual filiation. The distinction is not very clear. . . . But roughly speaking the distinction is clear enough. [Hacking 1979, p. 394]

On this conception, aren't religious beliefs "ideas germane to science"? What patterns of communication and lines of filiation does Hacking wish to include: do threats to tenure and nepotism fit here? They matter in history—if they do not fit, might it not be because they are germane to the history of science, but germane in the wrong way, according to a particular preconception of the "intellectual," as a normative category? If this is the case, then it would appear to compromise Kuhn's explicit claim that his approach is independent of a theory of rationality, as well as his implicit claim that his approach, unlike Lakatos's, will allow back more in insight about rational methodology than "what he puts in."

Given that they all choose to exclude some historical events as "external," the disagreement between Lakatos and these other authors comes down to the issue of how such exclusion is to be accomplished. Lakatos appears to have the advantage here, at least in that he acknowledges the issue and is not willing to leave his historiography with the claim that "roughly speaking the distinction is clear enough." Lakatos criticizes Kuhn for his claim regarding independence from a theory of rationality and uses his own theory of historiography and the division of the three worlds to make the criticism explicit: "All historians of science who distinguish between progress and degeneration, science and pseudoscience, are bound to use a 'third world' premise of appraisal in explaining scientific change. *It is the use of such a premise in explanatory schemas describing scientific change that I called 'rational reconstruction of the history of science'*" (Lakatos 1976, p. 191). This precise demarcation of rational reconstruction is the product, first, of Lakatos's more general assertion concerning the theory-ladenness of all empirical science, including explanation of historical change,⁸ and, second, his view that history of science is normative and that Kuhn is also masking such a normative distinction. Lakatos allows for a clear

8. See Lakatos's discussions, beginning with the claims that "history without some theoretical 'bias' is impossible" and "all histories of science are *always* philosophy fabricating examples" (Lakatos 1971a, p. 120; 1976, p. 192).

distinction between internal and external explanations of historical change for his internal history by distinguishing between second world psychological claims about why actors maintain the positions that they do with respect to contemporary theories and third world claims about the objective scientific virtues of theories (where "objective virtues" are, of course, virtues as seen from the point of view of particular historiographic research programs).⁹

Lakatos's rational reconstruction, then, is clear at least insofar as philosophical methodology, historical reconstruction, and the events of actual history are distinguishable. Methodology (a theory) is present in the third world, though it may impinge upon actors in the second world. Since second world events that do not contribute to objective growth by the lights of the theory have no appropriate third world correlates, they are external, and do not belong within a rational reconstruction; hence, a reconstruction will exclude some events of history. Internal history, a form of rational reconstruction, is the linking of a philosophical theory, or a logic of scientific discovery, to the second world in an account of rational growth in the third world (Lakatos 1971a, p. 118; 1976, pp. 190–91).¹⁰ What second world history should be excluded, and how much that is included may be reorganized in its time order, will be considered in the following section.

Lakatos's solution, of course, is not the only one that may be offered. Many historians of late have suspected that the internal-external dis-

inction as it is found in the history of ideas, and the intellectual-nonintellectual distinction generally, are at root normative divisions. Attempts to abandon these normative intellectual categories in the historical explanation of culture, led by Michel Foucault and Clifford Geertz, and more recently by the historians Roger Chartier and Carlo Ginzburg, have blossomed in the 1980s and 1990s in the New Cultural History and other historiographies.¹¹ As their efforts testify, the problem of rationally grounding the internal-external distinction for the intellectual historian remains a serious one. What can be said for Lakatos's approach on this score?

The Critical Methodology of Internal History

Kuhn claims in the quotation above that Lakatos has thrown out so very many often-used historians' tools in his redefinition of internal history that criticism of a theory of rationality—one of the main purposes of Lakatos's endeavor—has become impossible. And indeed, the observation that Lakatos has expelled many methods is correct: So upon what may criticism be based? What can be accomplished in internal history, and what that could not be done better along Kuhn's approach, or in intellectual history?

Internal history and its method of criticism, I believe, lead to a very interesting conception of the growth of knowledge, quite different from any that might be available from other internalist approaches. Lakatos presents two clear criteria for discerning the relative merits of reconstructions. First, a reconstruction is looked upon favorably if it reconstructs relatively more of the history of science internally. That is, a reconstruction is superior if it includes more activity in history within the development of, for example, research programs or falsifiable theories or . . . (choose your methodology), and so is within the edifice of knowledge. This is essentially a normative constraint of maximal content on reconstructions: it is a virtue if a rational reconstruction can convert more "value-impregnated" history into the currency

11. Historians of ideas who *do* attempt to explain the distinction show its lurking normative nature even more clearly. Thus, Gilbert (1971, pp. 93–94) indicates that intellectual history is seen by practitioners as in competition with social history in its explanations, and he wrestles with vague remnants of an isolated sphere of historical influence for ideas. Toews (1987) suggests a recent trend among historians toward the abandonment of the history of ideas due to problems pertaining to the view of the intellectual as separable from the social. Intellectual history is to be replaced with a history of meanings that does not countenance the traditional internal-external distinction; instead, history is the analysis of semiological and nonsemiological forces affecting meanings generally (p. 882).

9. Note that internal history as we have developed it here is a more restricted class than "rational reconstruction": internal history is the history of the growth of knowledge, whereas any sort of history that appeals to the third world in its explanations—such as Kuhn's, or intellectual history—is rational reconstruction. Lakatos's internal history might be considered the "image" as seen from the third world of the second world history that Popper suggests in chap. 4 of his 1972 book, which is also rational reconstruction. Popper presents an example of psychohistory in which the third world bears upon Galileo's thought (pp. 170–75), and also writes: "I suggest that one day we will have to revolutionize psychology by looking at the human mind as an organ for interacting with the objects of the third world" (p. 156).

10. On this accounting, I think, we can see what Lakatos is up to in his retraction of his earlier "joke" that the misbehavior of actual history belongs in the footnotes of internal history (see n. 6). If "subjective factors are of no interest for any internal history" (Lakatos 1971a, p. 118), then rational reconstruction only concerns the second world insofar as the second world connects with the third, and footnotes pointing to "misbehavior" are spurious. Such footnotes do point to instructive historical possibilities not achieved, perhaps because of sociological factors, but this should not be read as criticism of "missteps" of historical actors, as the discussion of the next section should indicate. Popper's psychohistory, therefore, should be considered a more appropriate treatment of historical actors on Lakatos's scheme (see previous footnote).

of the third world (Lakatos 1971a, pp. 132–33).¹² This criterion for criticism is to be balanced with a second, that one methodology is to be judged superior to another if it agrees to a greater extent with the opinion of the current scientific elite regarding the "best gambits" of scientific growth (Lakatos 1971a, p. 124).¹³ Lakatos maintains that, though there is not widespread agreement among scientists on what constitutes good methodology or progress, there is much agreement regarding which were a few of the greatest achievements of a field. A methodology should largely cohere with the opinion of the scientific elite, and one is superior to the extent that it coheres better than others. This is a constraint of fidelity to the opinions of the elite.

Kuhn appears to have mischaracterized Lakatos's critical resources, then, for he claims that Lakatos can learn nothing from internal history. Strictly speaking, he is correct: a *single* rational reconstruction is nothing more than a history constructed along the lines of a methodology. But Kuhn neglects that the internal historian may, in assessing reconstructions, utilize all of (1) the objective suitability of boundaries of internal and external history, (2) the relative merits of two or more internal histories with respect to time order, and (3) the opinions of scientists. All of these are available critical tools, consequences of the division of worlds and criteria of fidelity and maximal content. This "pluralistic system of authority" (Lakatos 1971a, p. 137) would appear to provide a good deal of critical power.¹⁴

12. Note that this should not be taken simply as a license to find *all* of the putative history of science—or all of history, for that matter—rational, for the constraint of fidelity to the opinions of the elite (see below) militates against it. In his 1976 postscript, Lakatos presents another, simpler account, voicing the constraint of maximal content, and leaving out the check provided by the constraint of fidelity (p. 192). This seems to be a mistake, or at best an abbreviation, since it accords with neither the solution in 1971a, discussed here, nor the solution in Lakatos and Zahar 1976, to which Lakatos 1976 is supposed to be a postscript. The problem of putting the brakes on internalization is one that Garber 1986 points to: see n. 16 for a plausible solution.

13. That Lakatos is singling out the judgment of the current elite, with their retrospective view of events, and not the views held by the historical actors, will be maintained here; it is discussed below. The problem of determining who the elite are is a knotty one, but one that I must leave aside entirely here, for the sake of brevity. See Lakatos 1971a, pp. 124–25, esp. p. 125, n. 1.

14. For details of the critical relationship between the "statute law" of rational methodology proposed by the philosopher and the "case law" derived from the judgment of the scientific elite concerning momentous achievements, see Lakatos 1971a, pp. 136–37. The historiographic approach of Lakatos and Zahar 1976 suggests a replacement for the constraints of maximal content and fidelity, which we will not deal with here but might call the constraint of rational appraisal: a reconstruction is superior if it accords more truly with current philosophical opinion regarding when in history a position is rationally tenable. So, in the case of the Copernican revolution, which is discussed there, a

But the critical power available to the internal historian is quite unlike that available to the historian of ideas, for example, for it has nothing to do with a grasp of human psychology, or with an intimate understanding of historical figures. The character of the tools will be conveyed to their product: internal history is not adequately outfitted to discuss the historical changes of ideas over time. What is it structured to examine? If current scientists as well as historians have input as to what makes for a good history of the growth of knowledge, as the fidelity criterion requires, then what is this history to look like? How could current practitioners' normative appraisals even be relevant to a history?

One task that internal history is not designed for is the criticism of the activity of actual scientists: if there is any place at all for such criticism in Lakatos's study of science, it is not there. The governing category for internal history is "the scientific," and fundamental to all of Lakatos's philosophy of science is the position that—on balance—scientists know "instinctively" (cf. Popper 1972, p. 176) much better than philosophers what is and what is not scientific: to think otherwise, he states, would be "hubris" (Lakatos 1971a, p. 137). Though methodologists might compare historians' rational reconstructions to the activities of actual scientists, their role is not to preach rules of method to the scientists, nor to make funding and publishing decisions for them: it is to suggest and to make appraisals, and to let the scientists work out the solutions among themselves; and, for this position, Lakatos of course received strong praise from Feyerabend (Lakatos 1971b, p. 174; Feyerabend 1976, p. 186). The only direct action that Lakatos recommends that philosophers take in practical science is to give advice, and "force a methodological debate" when they see a program as degenerating (Lakatos 1971a, p. 137).¹⁵ In abstract methodology, the philosopher reigns; in practical science, the scientific elite reign; in history of

methodology that holds that the Copernican revolution was only rationally tenable in the nineteenth century, as a result of the observation of stellar parallax, would be less adequate than one that succeeded in locating rational belief in Copernicanism sometime around the turn of the seventeenth century (but not before a specific time) (Lakatos and Zahar 1976, p. 189).

15. See also Lakatos 1971a, p. 103, n. 1, and p. 117: Thus I take other recommendations of Lakatos—such as "editors of scientific journals should refuse to publish [papers deemed the products of degenerating programs according to the methodology of scientific research programs]"—to be recommendations from a philosopher touting a particular methodology that scientists and journal editors give his arguments a thought: "should," then, is to be read as an elliptical "should understand the argument and do as they please if they disagree." Given Lakatos's other statements regarding the hubris of philosophers of science, and the importance of not conflating "methodological ap-

science as a normative history, there must be a balance between the two, as well as the skills of the historian, to investigate, tell, and evaluate the tale.

The Purpose of Internal History: Internal History as the "State of the Union" for Science

As I suggested, the tools leave their marks on the product: there are several odd features of internal history—we have already discussed the disregard for time order—that might be properly classified as anti-historical if compared to the standard of actual history that Lakatos alludes to. Another surprising feature is that internal history requires that we take the opinions of elite scientists into account; for clearly this introduces a good deal of play into the system, as different generations of scientists have had different heroes and have found different events exceptional. These historiographical commitments, I think, indicate that Lakatos's internal history is intended to produce stories that reflect and test particular conceptions of rationality, utilizing the abilities of scientists, historians, and philosophers together, to provide the best judgments that we have available concerning the rationality of historical development.

Differences in opinion among differently situated elite groups should not be surprising: Lakatos stresses the importance of hindsight in assessments of research programs, and it seems likely that scientists, like methodologists, use a great deal of hindsight. Though Lakatos holds that "there has been considerable agreement over the last two centuries" (Lakatos 1971a, p. 124) concerning great achievements in science, it should be clear that there is also significant divergence. Few elite physicists of today, I expect, would consider Descartes a central figure in their field, though his inclusion into the highest ranks of the hall of fame would appear to have been more likely in the recent past. This is not surprising because most scientists, including the elite in physics, often *prefer* for their purposes to voice an admittedly distorted view of the past, and provide few clues concerning the extent of their acquaintance with history in any other form. If evidence is wanted, consider Feynman's popular exposition of quantum electrodynamics, *QED*, in which he provides a mock warning for historians: "By the way, what I have just outlined is what I call a 'physicist's history of physics,' which is never correct. What I am telling you is a sort of conventionalized myth-story that the physicists tell to their students,

praisal" and "heuristic advice about what to do," Lakatos can be read as either blatantly inconsistent or deserving of such a charitable reading.

and those students tell to their students, and is not necessarily related to the actual historical development, which I do not really know!" (Feynman 1985, p. 6). In *QED*, Feynman begins the European history of optics with Newton, excluding Descartes, as is the convention.

The elite of a particular period are, then, a source of knowledge somehow related to the history of science; but what, more precisely, can we say they are knowledgeable about? Lakatos provides us little illumination in this area, focusing upon the importance of the opinion of the elite as a check against irrelevance in philosophical theory (Lakatos 1971a, p. 125). The opinions of the elite regarding "best gambits," he argues, must be accorded weight as "case law" against the "statute law" of the philosophers' theories of rationality, keeping the philosophers' speculations concerning rational practice in check; but since they are not expert in the facts of history, what sort of check does this provide?

I expect that what the current elite *are* knowledgeable about is the extent to which some scientific theories, and perhaps methods, as far as they know them, have contributed to growth, and growth as seen from the contemporary scientist's standpoint. The elite know their "textbook history"—that is, what passes for history in science textbooks, the sort that Feynman warns about above—but in good detail, and with a critical eye: textbook history is often rewritten, and the elite form their own opinion, from their working knowledge of a science, of what the really significant developments that led up to the current state of the field were. Some might even have the historical facility to do some research of their own and to reconstruct and rehabilitate lost "scientists," bringing them into the textbooks.¹⁶ The changing stories of the elite's textbook history, then, would provide instances of the growth of the field to its present condition, instances that may have *no* direct relation to actual history and historical actors.

With elite textbook history now identified, we can, I think, see our way to a clear purpose for internal history as an interesting via media between the scientists' constructions and other historiographies, such as intellectual history. If the elite can provide information regarding instances of growth, then internal history becomes a retrospective ac-

16. Likewise, some may be thrown out, and some may be marked as infamous. The solution to the problem noted above by Garber (see n. 12) may be addressed in the light of this approach. There should be no more danger of including too much history as rational than of including too little, provided the critical internal historian pays attention to both positive and negative assessments provided by scientists: in using the constraint of fidelity, the historian is equally responsible for excluding the "worst gambits" on the assessment of the elite.

count of the growth and content of scientific knowledge. The elite point out the content of progressive science, and the philosopher tailors a methodology to explain this content: from these sources, an understanding of the nature of science, with respect to both its theoretical accomplishments (or instances of growth) and the process of its growth, retrospectively conceived,¹⁷ is achieved. The methodology presents an analysis of the process of science, and the elite provide a partial list of science's product. The elite's assessment of scientific content presents the necessary historical content that an internal history must account for (or persuasively argue against); and a reconstruction is superior to another if it can otherwise include as internal history more putatively scientific historical activity.

What, then, is the useful contribution that internal history can hope to provide? It is obvious from Lakatos's writing that he intends that internal history serve to reflect and test methodologies, and a rational reconstruction provides an interpretation of history according to a methodology. Another advantage is evident, however. The textbook history of the current elite, when matched to theories of science that present analyses of growth, can provide retrospective histories of the current content and manner of growth of theoretical scientific knowledge.¹⁸ Rational reconstructions, then, provide tests for deciding among general methodologies, and the extent to which they match actual history tells us the extent to which science has been methodological, to the best of our ability to construct theories of method. Likewise, the textbook history of an elite group or individual from the past, coupled with methodologies that were also available or espoused, may provide another closely related and useful field of study for history.¹⁹

17. Such a conception does indeed provide a retrospective or "whiggish" historical story, since events currently out of favor with the elite, or not in keeping with a methodology, are to be considered nonprogressive. Once this is understood, however, it should present no problem, since internal history is not meant to assess the actions of individuals: it is intended to assess contributions to growth retrospectively considered. Thus, charges that Lakatos provides for poor historical explanation in his internal history because he presents criticism according to methodologies not available to actors, and that he allows the "winners" to retrospectively define what is rational action, miss the point of the enterprise: history with a regard for the actors is not his topic.

18. Of course, a fuller theory of the content and growth of science, which would include more aspects of the practice of science, such as experiment, questions explored, and so on, is also conceivable. For fuller conceptions of scientific practice, see Kitcher 1993; Laudan 1984.

19. Garber (1986, pp. 95–96) argues that Lakatos's internal history does not provide the tools necessary for appraisal of historical actors' actions. The position developed here suggests that Garber is correct, but only because that is not the purpose of internal history; however, the related approach mentioned just above does fulfill Garber's re-

Of course, such a retrospective history will not tell us how science has proceeded in the past, nor how it arrived at its current state, nor how it will proceed in practice in the future: it will rather tell us what the current state of science is to the best of our ability to characterize growth in our theories of methodology, and it may partially answer the question of what the history of science means and what it provides for us culturally, by presenting an ideal of science, mediated by the demands of philosophers, scientists, and historians. To the extent that it fails in the first two regards, it is inferior history; to the extent that it succeeds in the last two, it has some claim to being called history nonetheless. Thus it may perform much the same function for science as the "state of the union" address does for politics in the United States: it is a composite insider's view of the condition of the field, its accomplishments, current methods, and, perhaps, its plans for the future. It may serve educational purposes as a tool for explaining to young scientists, and perhaps managers of science policy, the methods and current condition of science. Internal history may also serve significant political purposes, as I will suggest in the afterword, for it might offer the best available response to Feyerabend's demand for a normative demarcation for science, voiced in the eloquent challenge to Lakatos, "*What's so great about science?*" (Feyerabend 1976, p. 203; a more sophisticated challenge is provided in Feyerabend 1987, pp. 297 ff.).

Afterword: Rational Reconstruction and Political Theory

According to the interpretation of internal history that has been presented in this article, such history is not intended to be of use for discerning the actions of past scientists, nor is it of use for providing much in the way of recommendations regarding what scientists should do in the future: the third world of rational appraisal does not reach into the second world, so it says nothing about psychology; and internal history concerns an ideal of growth, not the process of science as it is carried out by scientists.²⁰ Its comparison to a political state of the union address gives some indication of its purpose, and a further com-

quirement and appears to be essentially the same as the project which he suggests, of a normative "internal history grounded in an historical conception of rational scientific procedure."

20. That rational appraisal has *nothing* to do with some (i.e., external) explanation within the second world is, I think, the only major disagreement with Lakatos that this position presents. Lakatos maintains that internal history of science determines the problems of external history of science, and, according to the position presented in this article, that claim is preserved to the extent that internal history and normative methodologies determine what is science and so determine the scope of human history that external history of science should cover. Lakatos, like most others who have wielded an

parison with political theory, I believe, also indicates both its historical precedents and its methodological advantages over them.

Internal history has, I believe, a distinguished line of predecessors in the natural law tradition of political philosophy perhaps best known through the writings of Hobbes. The goal of most natural law arguments for political organization is to show, from an analysis of the characters and desires of people, that social organization, and specifically particular forms of government, provide the best conditions in which those people can live. Hobbes and Locke go about this task by invoking natural laws, which are laws that restrict the behavior of individuals: the individuals obey them because it is in their interest, it is rational, to do so. According to Hobbes, men are, in their natural condition, in a state of universal war, but "reason suggesteth convenient Articles of Peace, upon which men may be drawn to agreement" (Hobbes [1651] 1914, p. 66), and individuals realize that they are better off surrendering some of their freedom to a sovereign, forming covenants, and erecting a commonwealth (Hobbes 1914, p. 87). Hobbes constructs in *Leviathan* a quasi-historical argument for the foundation of the state: he suggests that, perhaps at some time in the past amidst the struggle, men paused to think, laid down their rights to complete liberty, and followed these laws, which would allow them to preserve themselves better than in individual struggle. The argument is quasi-historical because Hobbes's writing seems to suggest that such an event may have actually occurred,²¹ though he has no specific evidence of its occurrence: Hobbes appears to have logical reasons, from his

internal-external distinction (see Shapin 1992), had a much more significant relation in mind, however, as is suggested in his many invectives against sociology of science: Lakatos conceived of internal and external history as somehow opposed, rather than independent types of explanation (e.g., "Fashionable 'sociologists of knowledge'—or 'psychologists of knowledge'—tend to explain positions in purely social or psychological terms when, as a matter of fact, they are determined by rationality principles"; Lakatos 1970, p. 87; see also Lakatos 1971a, pp. 120–21; Lakatos 1976). My disagreement with Lakatos on this count, at least with respect to the historiography of science, is reflected in Kulka 1977, which brought the problem to my attention: "One can sympathise with Lakatos' contention that historians should try to explain as much of the history as possible internally. . . . However, if internal history is to be at all associated with theoretical structures and contrasted with external empirical history, then the ratio of internal to external explanations cannot be used as a test of rival methodologies. For internal history, so conceived, should always be compatible with any number of external empirical explanations. The point is, which internal reconstruction is the most useful for the understanding of science and which external reconstruction is the closest to what has actually happened?" (p. 337).

21. Though the precise extent to which Hobbes wished his argument to be treated as representative of history is quite open to debate, he certainly insisted that the state of

analysis of human motivation, for believing that the event occurred, rather than historical evidence of its occurrence.

Hobbes's primary purpose, of course, was not to tell history, but to convince his contemporaries that reverting to a state of civil war would be irrational action (Hobbes 1914, p. 391): that is, his purpose was to provide a rationally compelling argument for government. Locke, too, attempted to couch his rational argument for the state in quasi-historical terms; not until Kant, according to Otto Gierke, was the historical fiction eliminated in a recognizably similar strain of political theory:

The majority of the natural-law theorists regarded the original contracts which they postulated as *historical facts*, of which, by the mere play of accident, no historical evidence had been preserved. The most they were willing to allow was that sometimes primitive man, instead of making a definite contract, might have made tacit agreements of union. . . . But side by side with these views another began to make itself felt, which Kant was the first to express in clear terms. According to this view, the political contract had not the historical reality of 'fact': it had only the practical reality of 'an idea of reason.' . . . The distinction drawn by Kant disentangled the problem of the historical origin of the State from the natural-law fiction [of its origin in a contract]; but it only did so at the cost of entangling the problem of the philosophical explanation of the State's legal basis (and entangling it more deeply than ever) in the meshes of individualistic fictions. [Gierke 1934, pp. 109–10]²²

The similarities between natural law theory and internal history should be clear. To the extent that Hobbes attempted to construct a history, it had little to do with actual political history. What he was interested in trying to explain was the rational basis of a form of political organization, and in doing so, he found historical fictions handy. It

nature is a genuine historical possibility that has existed in some places and times, and that commonwealth can arise only through covenant (Hobbes 1914, pp. 65, 89).

22. Another Kantian author who can be seen to relate closely to this tradition is John Rawls, in his *A Theory of Justice* (1971), a book published in the same year as Lakatos's historiography article. Rawls's conception of the significance of the original position closely echoes the one presented here for internal history: "It is clear, then, that the original position is a purely hypothetical situation. Nothing resembling it need ever take place, although we can by deliberately following the constraints it expresses simulate the reflections of the parties. The conception of the original position is not intended to explain human conduct except insofar as it tries to account for our moral judgments and helps to explain our having a sense of justice" (Rawls 1971, p. 120).

was left to Kant to demonstrate that there really is no reason for linking the ideal of government with its historical generation, if the ideal *alone* is what is to be understood. So, by analogy, in the philosophical investigation of scientific growth, the assumption is often made that the path by which science has progressed to its current state may be disregarded if an understanding of its current ideal is under investigation. Lakatos's internal history takes the lesson from Kant that actual history may properly be set aside in the pursuit of an understanding of growth; but it also maintains a relation to Hobbes's project, and with good reason, because it presents an attempt to answer another question, to grapple with the historical construction of the accomplishments of growth.²³ Hobbes's history might be looked upon as approximating an internal political history, built according to the methodology of natural laws. It was, however, one constructed with only psychological, anecdotal evidence, and so one lacking the historical benchmarks of rational history's accomplishments and development that Lakatos works very hard to preserve.

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23. In science, the accomplishment is knowledge that has been attained; in politics, past accomplishment is less important than current process.
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