RATIONAL RECONSTRUCTIONS REVISED

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ABSTRACT: Imre Lakatos' idea that history of science without philosophy of science is blind may still be given a plausible interpretation today, even though his theory of the methodology of scientific research programmes has been rejected. The latter theory captures neither rationality in science nor the sense in which history must be told in a rational fashion. Nonetheless, Lakatos was right in insisting that the discipline of history consists of written rational reconstructions. In this paper, we will examine possible ways to cash out different, philosophically interesting, relationships: between rationality and science, between rationality and philosophy of science and/or epistemology, and, of course, between history and philosophy of science. Our conclusion is that the historian of science may be a philosopher of science as well, but if that philosophy of science is essentially *ahistorical* and dogmatic, it either cannot be used for history or it will deprive history of some of its most interesting and useful categories.

Keywords: rational reconstruction, philosophy of science, selective nature, history of science.

CONTENTS

1. Introduction

Imre Lakatos opens his (1971) with the claim "History of science without philosophy of science is blind"[1], meaning, by that, a proper historian of science must also be a committed and proper [read: "Popper"] philosopher of science. Lakatos meant, by "philosopher of science", a philosopher who shared his views. Such a historian/philosopher would *reconstruct* historical episodes in the history of science, showing how they did or did not "fit" the Methodology of Scientific Research Programmes, which, according to Lakatos, was definitive of scientific rationality itself.

Let's have a closer look at the meaning of *rational reconstruction.* Consider the two following quotations:

> the main problem concerns the possibility of the rational reconstruction of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given. By rational reconstruction is here meant the searching of new definitions for old concepts (...) The new definitions should be superior to the old in clarity and exactness, and above all, should fit into a systematic structure of concepts (Rudolph Carnap 1928/1967, p. v).

> Through this outstanding translation by Matthew Cobb Professor Jean Gayon's masterly rational reconstruction of the history of Darwinism -*Darwin et l'apres Darwin: une histoire de l'hypothese de selection naturelles* (1992)- has become easily accessible to all of the Anglo-American community of scholars (Robert Olby 2000).

The meanings of "rational reconstruction", as exhibited in the two quotations above, is twofold at least. Interestingly, Lakatos intentionally conflated the two. It is the "rational" in "rational reconstruction" that raises problems, and this in two ways.

The first meaning of "rational", as exemplified by the Carnap quotation, charges the philosopher to re-describe, or *reconstruct,* actual science in terms of new concepts that exhibit the intrinsic rationality of science. This injunction was a central tenet of the logical positivist program to reconstruct the language of science into a disambiguated ideal logically transparent language. On this meaning, the focus is on the rationality of science itself. Lakatos extended this injunction to historians of science, regarding their descriptions of the historical episodes in science. His goal for rational reconstruction, similar to Carnap's, was a rewriting of an historical episode, as one could know it, into a form that made clear its true, even if latent, rational nature. This goal, of course, Lakatos took more directly from Karl Popper, his teacher and mentor, but, as we have just seen, it has antecedents in non Popperian tradition. Rational reconstruction was Lakatos' form of stating and answering the demarcation problem[2].

In the second sense, used by Olby, rationality refers to the rationality of the historian pursuing his task of doing history. Let us assert from the beginning that all history is reconstruction (or, more simply, constructive: later we will articulate this claim). The narrative the historian tells, even if it is about irrational actions, must itself be intelligible and therefore, in some sense, rational. Olby is asserting Gayon's book is a rational reconstruction, not one with which he agrees, but masterly, nonetheless. By contrast, Lakatos thought that the historian's tale was most intelligible, and useful to philosophy, when it realized, and accordingly exhibited, Lakatos' own methodology.

In this essay we wish to see what may be saved of Lakatos' claims about rational reconstruction, in spirit if not in word. We will not argue against the specifics of the methodology of scientific research programs. This was done sufficiently in the period following the publication of Lakatos (1970 and 1971)[3]. Yet, we will recall and develop a few criticisms as we make our way towards the positive claims of this paper.

We shall start by very briefly outlining Lakatos' position regarding rational reconstruction and the history of science. We shall then talk about philosophy of science, and the rational, about the nature of science and the nature of rationality. If science is somehow the paradigmatic case of human rational activity, then obviously one job for philosophy of science is to explain how and why science is such. Yet there are different starting points, and different ways to go, in doing this.

We then shall talk about the selective nature of history of science, and history more generally, and examine how, and in what contexts, the historian of science might need philosophy of science. This will involve discussing the nature of historical narratives and the purposes of doing history.

2. Lakatos' theory of rational reconstruction

Lakatos' position, following Popper's, has its starting point in the idea that the fundamental question in philosophy of science is, what are the criteria that an intellectual enterprise must satisfy in order to count as science: the problem of demarcation. For Popper, most simply, the demarcation criterion was falsification[4].

Popper's major contributions to falsificationism, Lakatos argues at great length, are his claim that no theory could be actually "falsified" except with respect to a better theory, and the introduction of the notion of "empirical content" as a way to decide between theories[5]. In short, the scientist is bound to accept T' over T when T' has excess empirical content over T, and at least part of the excess of empirical content is corroborated. The discovery of *novel facts* is the standard by which theories compete with one another[6].

For Lakatos, as for Popper, scientific change is, or should be, rational. Lakatos credits Popper with adding the historical dimension to falsificationism by shifting from a logical relation between a theory and its empirical basis to a complex relationship between competing theories[7]. It is this relationship that Lakatos' own methodology of research programmes was designed to cash out, thereby exhibiting the rationality of science as it may be seen in the history of science.

"Philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and therefore provides a rational explanation of the growth of scientific knowledge" (Lakatos 1971, p. 91). As we pointed out in the beginning, that history without some normative philosophy of science is blind (Lakatos 1971, p. 107) is the boldest of Lakatos' claims about the historiography of science. However, Lakatos' problematic attitude with respect to rationality (as opposed to rational reconstruction) is revealed when he says:

> Rational reconstruction of science (...) cannot be comprehensive since human beings are not completely rational animals; and even when they *act rationally* they may have a false theory of their own rational actions (Lakatos 1971, p. 102).

So human beings, including scientists, can and do act irrationally, and, even if they act rationally, they may not really know why what they do is rational. An action is rational if and only if it satisfies the norms for the "growth of objective knowledge", as specified by philosophy of science (in Lakatos' case, by the methodology of research programmes). Those actions not conforming to the norms (including misconceptions about rationality) must be explained by *external* history (including, but not limited to, psychological, political, economical factors). From this position, it follows that rational reconstruction refers to the historian's use of the philosophical criterion of demarcation in constructing the historical narrative. This, in its turn, requires drawing a distinction between internal-normative and external-empirical history[8].

Lakatos' idea of "growth of objective knowledge" is derived from the Popperian idea of empirical content. As he writes, "progress (in science) is measured by the degree to which a shift is progressive, by the degree to which the series of theories leads us to the discovery of new facts" (Lakatos 1970, p. 124). The "discovery of new facts" works as a test for competing theories, in terms of which one has "excess empirical content" over the others. What explains the growth of objective knowledge, therefore, are choices based on the differential empirical content of theories. By stipulation, choices favoring excess empirical content are rational.

Now, a major internal problem with this as a criterion is that neither Popper nor Lakatos ever made good on a coherent measure of empirical content[9]. This problem, though, while devastating to Lakatos' theory of the methodology of scientific research progammes, is *not* where we wish to focus our attention.

The ability to allow for internal history is the very goal of rational reconstruction. So much so that the sign of a "good" criterion of demarcation is that it will allow the reconstruction of a massive part of history as "rational". It will allow most, if not all, of scientific progress to be explained by scientists' choices that may be reconstructed as perfectly rational choices between competing theories or programmes. External history is an unavoidable part of history of science, Lakatos argues, because humans are not always rational. Yet, the job of the historian is to follow the rational actions and, particularly, rational choices; even if that means "reconstructing" these choices by narrating events that did not take place, relegating what actually happened, together with its "external" explanation, to the footnotes[10].

One important criticism of Lakatos' rational reconstruction was raised by Thomas Kuhn, in his (1971). The first part of Lakatos' Kantian slogan, "philosophy of science without history of science is empty", meant that there was need to "test" the demarcation criterion (which Lakatos seems to consider the hard core of philosophy of science) by means of cases from history of science (i.e., as we said, by means of reconstructing more and more of history as rational). Kuhn points out that data from such cases that are selected and interpreted in the fashion suggested by Lakatos will never be able to "react" (Kuhn's expression) against the methodology that (reconstructed" them in the first place. In short, Lakatos' rational reconstruction makes testing the philosophical theory hopelessly circular (and unfalsifiable).

Lakatos himself is aware of a different, but not less serious problem, concerning the nature of scientific rationality. As he writes,

> the criterion of heuristic power strongly depends on how we construe *factual novelty*. Until now we have assumed that it is immediately ascertainable whether a new theory predicts a novel fact or not. But *the novelty of a factual proposition can frequently be seen only after a long period have elapsed* (Lakatos 1970, p. 155, author's italics).

Lakatos then provides an example[11], and suggests that the requirement about new facts should be applied leniently to new research programs. Since there is no such thing as "the hard fact", at the pure, non-theory-laden level, novel facts can be only reinterpretations of already discovered facts and these are possible only *ex post facto*.

This insistence on hindsight shows the problem. Lakatos' demarcation criterion allegedly works only on "ready, articulated theories". These theories, and, more importantly, their rational reconstructions, exist "in Plato's and Popper's third world" (Lakatos 1970, p. 180); a world that is independent of the nature of the knowing and acting subjects (except *qua* idealized rational subjects). The problem is that Lakatos' "internal history", at the most, might describe, counterfactually, growth in the third world of *disembodied* knowledge. As Kuhn points out, Lakatos' history is just "philosophy fabricating examples" about what a disembodied rational scientist could do in appraising idealized reconstructed theories[12]. But there is no interesting sense in which this is history. If we want to preserve the relationship between history and philosophy of science so correctly underlined by Lakatos himself, we need a concept of rationality that applies to the scientific enterprise *as it is and was.*

3. *Philosophy of science,* and *science, as rational*

The presupposition behind the Popper-Lakatos' approach is that science is paradigmatically (not in Kuhn's sense) *the* rational human endeavor. Science as paradigmatic rationality, according to the demarcation criterion, is shown by contrast with pseudo-sciences. But if our intuition is that science is rational, this intuition must arise from science as actually practiced. We cannot warrant our intuition that science is rational by counterfactually reconstructing science according to some "third world" objectivist idealized norms of rationality. This would undercut the real world basis of the intuition, and it is real world science, as it really exists, that we want to qualify as rational, and compare it with (existing) pseudo-science.

A Lakatos-type position must hold that our ideas about rationality do not come from real science, but are somehow constructed into an objective idealization of the rational. Such a third world idealization would need to be argued for, on grounds that have nothing to do with existing scientific practice or, more strongly, argued for on some basis completely independent of what people actually did, i.e. independent of history. The attempt to find some logical measure of empirical content as a measure for scientific progress, and so for rationality, is exactly such an attempt.

Put baldly, the problem is that, on a Lakatos-Popper type view, either scientific progress, as measured by increasing empirical content[13], is descriptive of what happens in science, and so shows, by its description of it, how science is a rational endeavor, or it is a normative claim, independent of historical happenings. If the latter, then an argument is needed to prove that rationality is in fact constituted by theory choice that increases empirical content. Such an argument would have to be analytic, *apriori*, transcendental, or somehow based on premises that are detached from history's actual happenings, separated from any empirical basis. For, on this view, appeal to the latter could only provide us with descriptive, not normative content, and inductive, not *apriori* support. Neither of these implications seems to fit the Lakatos-Popper program.

For Lakatos, the rationality of science was exhibited and tested by the reconstructed internal history of science, by the research programmes and their relation to empirical content. This in an internalist way, since theories and their changes, theories and their relation to evidence (or facts), and changes in evidence (or facts) are all supposed to be describable in purely quasi-logical terms. To be sure, Lakatos does loosen up the relations, allowing inconsistencies or anomalies to be tolerated for a while[14], but none of these loose parameters is essential to defining scientific progress or rationality. The essential relations are purely syntactic or formal relations that do not depend upon historical actors or events. These internal relations, at any given moment, will either hold or not hold, quite independently of what any scientists think, or what work they have done. It is only through comparisons among abstract structures bearing these formal relations, that they may be judged to be rational or not. Whatever is not part of this internal structure is external, and does not belong to the sphere of the rational. It is but an external historical fact that real people were involved in developing these theories or programs. This position should remind one of the cognitive/non-cognitive distinction as used by the positivists and logical empiricists[15]. It is their heir[16].

Yet, these internal models of rationality, based on empirical content increase, do not work for equally internal reasons. As we noted, there is no way yet shown to elaborate a notion of empirical content that can do what Lakatos (and Popper) wanted. But even if empirical content could be made clear, the Lakatos-type internalist theory of rationality would still be a theory of the same form as one that held that *only people who reason deductively* are rational. In the latter case we have a clear explication of what it is to reason deductively. Yet, what could make such a stipulation, equating deductive reason with human rationality, at all plausible, let alone *apriori* warranted? The very fact that such a stipulation would leave out most of human reasoning should provide reason to reject any such *a priori* equation between rationality and deductive reasoning. So it is also with Lakatos' theory. Human history of science, in fact, simply does not fit the Lakatosian methodology (even if the methodology were coherent). This lack of fit is why the history has to be massively reconstructed, through what Kuhn derogatorily called "fabrications"[17].

So, either there are internal "logical" relations, tying a problem to its solution, or what counts as a solution needs some sort of external criteria[18]. This suggests to us that the deep problem with Lakatos' model lies far deeper than in the failure of the internal coherence of his major rational concept, i.e. empirical content. Lakatos and the other philosophers who dealt with theory change are not alone in trying to find internalist idealized "logical" canons for rationality. For years, in many disciplines, theorists have looked to various logical systems to provide the basic structures for rationality. Logical inference, statistical inference, decision theory, game theory, inductive logic, and confirmation theories, have been but some of the idealized rational models that have been proffered. Indeed, for some purposes, using these systems as models of rational human action may be appropriate. But models such as these only sometimes, and in specified contexts, may claim to carry normative force for human actions. An argument in each case must be given for their applicability and for their necessity.

Whatever may be the norm for human rationality, it does not lie in some formal logical principle. Rationality (in a major sense) is more effectively construed as a selection and goal problem-solving process[19]. So, in prudential judgments, or problem solving tasks, the problem or goal is specified partially by something external to the scientist. For example, one could argue that part of the problem is set, or the solution is constrained, by the external world. The "rationality" aspect consists in finding the *proper* means to achieve the goal. Rational decisions are then those choices of "steps" toward the goal that actually will lead to its satisfaction. Satisfaction is judged by a combination of different types of *external* criteria. Here are two important ones:

(1) How each step and the end relate to the world (the *reality principle*); for the solution must work and the goal must be achieved in reality and not just in imagination.

(2) What the participants in the problem solving episode, and the community around them, consider satisfactory or acceptable (the *socialization principle*). For these are the people who have to accept the inferences drawn by the problem solvers, and inference drawing and its norms are social by nature[20].

Another way to get to see how the external must be involved in accounts of rationality is by looking at contemporary epistemology. Many contemporary epistemic theories, and certainly the ones we think are good theories, still try to take account of the empirical bases for knowledge claims, but do so by means of theories of warrant and/or reliability[21]. These are the current naturalistic favorites in the field of theories of knowledge. Now, presumably knowledge and the ability of gain such knowledge are a mark, if not *the* mark, of rational. At least if science is held to be paradigmatically rational in some sense, then it is the knowledge that science brings us, and the procedures that sciences uses to gain knowledge, that make it such a rational endeavor. In this paper, we will not argue in favor of a particular theory of knowledge, though we do believe that warrant and reliability will be central concepts in any such theory (and that an adequate theory will be more along Dretske's line than Plantinga's[22] or Goldman's, and will take account of how perception and memory really work, rather than just *assuming* that such systems somehow reliably deliver more true beliefs than false ones). Despite our lack of defense of this claim, what is important to notice, is that the most viable reliabilist or warrant theories are all externalist. That is, they all have as a central tenet how people in fact interact with the world. Sticking tight to our pattern of making claims without argument, we would go further and say that any cognitive theory of knowledge must have an account of the mutual interaction of the humans and the world, or, what amounts to the same, must have as the central guiding principle that humans and their social milieu are interactive parts of the natural world. This is what we tried to indicate above by our proposed principles of reality and socialization. One consequence of such a view is that there is neither need, nor place, in such epistemological theories for any analogue of an idealized disembodied schema for the rational (or even any Tarski-like semantic criterion for truth.) But we will not solve any serious epistemological problems in this essay, nor will we sketch how rationality would described and yet be normative in such a scheme. Rather, we shall turn to history.

4. The selective nature of the history of science, and of history in general

It is almost too truistic to tell that all history is selective[23]. Historians need to be able to pick out the important bits from the large and sometimes overwhelming morass of data that they have or even that they could have. They have to decide what kinds of things to look for before they can decide what to select from what they have collected. If selection is not to be haphazard, or due to whim, then there must be some, however vague or implicit, principle of selection. One way of conceiving of selection principles is that they function like categorical schemata. Selective principles guide the historian as to what kinds of things to present in the written history. Further, a selective principle includes or presupposes that there are connections within and among the bits of history selected as important. The alternative would be a mere list of historical facts, or more correctly, a mere compilation of historical "finds" (in the broad sense). These bits and their connections are the structuring elements that are used intentionally to write and, then, are represented in the written history. In our categorical terminology, categorical schemata are not just ways to break up the world into entities, using nouns, they also must include ways of describing activities that characterize and connect the nouns, namely by using verbs.

It is as such a schema that Lakatos' methodology of scientific research programmes (as philosophy of science) constituted a selective principle for doing history of science. It was by use of this principle that, Lakatos claimed, the historian would be able to avoid blindness. The principle required the historian to re-construct the historical episodes in terms that exhibited the "logical" structure of increasing empirical content that derived from the rational choices scientists made among research programmes. The entities and activities picked out had to fit into the categorical scheme provided by the substantive terms of Lakatos' methodology. In other words, one needed to find, in history of science, referents for terms such as "hard core", "direct anomalies", "to choose a programme", "novel fact", etc[24]. He called this *internal* history because the structure of the formal relation among programmes is quasi-logically expressible (assuming one had an adequate measure of empirical content). In consequence a comparison of the two programmes would allow one to answer the question as to which was progressing and which degenerating.

Perhaps surprisingly though, reconstructing such logical relations is not what historians mean by "internal history". Internalist historians try to reconstruct the actual thought patterns that scientists used to come up with their discoveries or to solve their problems. Typical internalist projects were such as: Reconstruct the thought processes that Kepler went through in finding the orbit of Mars; or, reconstruct the changes that Galileo made to his theory of local motion as outlined in *De Motu* (1590) in order to come up with his final theory of free fall, parabolic motion of projectiles and bodies moving on an inclined plane (1609, published in 1638).

On this view of internal history, the assumption that doing science is rational amounts to the claim that at least some (or, maybe, most) of the mental maneuvers of these scientists were rational. This may be, and often was, combined, with the claim that those thought processes were clearly rational in that they provided the means for achieving the "right" (more accurately, better) solutions to the problems they were trying to solve. Internal histories, as exhibited in our examples, are elucidations about individual persons and their thought processes as they deal with the content of science. However, internal history need not be only about the thought processes of individuals or geniuses. An internal historian could write, and some have written, about background theories, accepted myths or guiding metaphors, about community beliefs, intellectual traditions and climates of thoughts. The first point to notice is that, though much of what is written from an internalist point of view is about individuals, internalist narratives may be written at many levels of generality and abstractness. The second point is that the main content of an internalist historical discourse is about the *ideas* or *concepts* or *constraints* that constitute the science.

All these internalist histories may be grouped together because they are about mental processes at some level of abstraction, or, in an older phrase, they are intellectual histories, histories of *ideas*. But what is important to notice, for the purpose of this essay, is that there being about ideas does not entail, or even suggest, that there is anything *purely rational* here. Some intellectual moves can be smart, ingenious or well considered; yet others can be hasty, fallacious or even downright failures (even in terms of the actor's categories). Kepler was unsatisfied with many of the moves he made along the way to figuring out Mars' elliptical orbit. The assumption of the rationality of science amounts to saying that these guys were sometimes *thinking properly* as they solved their problems. What counts as rational, then, has to be abstracted from what are judged to be the proper thought patterns they used. The identification of proper thought patterns presupposes some sort of criterion of acceptable or adequate problem solving. This abstract characterization along with its attendant criterion then may be taken as a model of at least one style of rationality.

This is, e.g., what N. R. Hanson (1958) tried to do with Kepler, and what Drake (1978) and Hooper (1996) tried to do with Galileo. Other historians or philosophers may argue with the specifics of these interpretations or reconstructions. One more general type of argument may go like this: Hanson tried too hard to fit Kepler into his (Hanson's) abductive inference model, but the fit is not really good. At this point, the critic must show where and how Kepler's reasoning departs from the abductive model. So, she would prove that Hanson's is a bad reconstruction; or (by different means) that Drake tried too hard to make Galileo's theory of causes and the continuous too much like modern dynamics, to make it too *acausal*.

In the Hanson case, using an abductive model as the selection principle turns out to be unsatisfactory. It does not guide the historian in a way that clearly fits the account we have, from Kepler's documents, of what was going on. The same type of claim may be made about Drake: there is just too much of Galileo's writing that does not clearly fit his picture of what Galileo was doing[25]. In a Lakatosian framework, we would have to reject the scientific procedure (Galileo's or Kepler's) as irrational, if it did not fit the philosopher's scheme. This seems to get things backward. It amounts to saying, that, whatever Kepler or Galileo did, if they did not follow the philosopher's criterion, then they were irrational, no matter the outcome. If Hanson had used his model of abduction in such a way, he would have had a "quasi logical" rational reconstruction theory like Lakatos'. Hanson's claim would then have been that scientists must use abductive models, for only by use of such a model science can actually succeed, and the rationality of science is determined by using this model. In addition, he would have claimed that it is the task of the historian to reconstruct abductive reasonings when there were none, and to have recourse to external history (in the footnotes!) to explain why the scientist actually behaved irrationally.

The big difference between the abstractive and the logical approach seems to lie in the foundations which are used to claim that the model is rational. A model of the "logical" type is *definitive* of rationality and so, as we have discussed, must be grounded on the presupposition that it can be shown to be necessary *apriori*. Another difference may be seen in the fact that in the abstractive model, as our cases show. we are dealing with the realm of ideas; the realm of how people think and how thoughts have influenced other people. This latter is not what Lakatos wanted, nor, on his view, would attention to how people really think illuminate where rationality lay. This is not just because Lakatos' *rationality* judgments are *ex post facto*, and so cannot be used to guide the scientist (as observed earlier). His view seems to miss the point that even *acceptability* judgments of theories or research programmes are based on many factors that are not internal.

The claim being made here is that a person's reasoning, be it altogether logical (in whatever sense), or more broadly rational, or even irrational, does not determine whether others will *accept* that reasoning. Proper reasoning is but one factor in determining acceptance, or even acceptability. Certainly, finding flaws in someone's reasoning often is a good reason in favor of not accepting their conclusion, but it is often not sufficient. This point is a generalization of Hempel's distinction between the *logic* of confirmation, or the relation holding between a theory and its evidence, and the *reasons* why a theory is accepted (for example: what level, or kind, of evidential support does one require before one accepts a theory (Hempel 1965)). Acceptance, even acceptability, of theories or research programmes, is based on many factors that are not logically internal; nor do all legitimate reasons clearly fall into the realm of epistemic or cognitive reasons. Rational or reasonable acceptance may take us beyond the realm of logic and ideas: indeed, it must[26].

We mentioned above that the internalist type of history that we have sketchily portrayed[27] is valuable. It is more closely tied to good history than rational reconstructions. In fact, it could well and reasonably be argued that internal history, in our sense, is the only history that deals with the content of science. Such internal history describes how certain intellectual activities change the substantive content of what constitutes the science at a time. Yet, in doing such history, *reconstruction*, or maybe just plain *construction*, is involved. The historian has only at best limited access to the thought processes of the scientist. These come mainly from documents (published papers, notes, letters, etc.) But these documents must be interpreted in many ways. The basic idea is to use the documents to come up with a plausible reconstruction of what the possible thoughts processes were, and then to describe these in a way that makes sense to those that will read the history. The very interpretation of source material, and the linking of these elements together into a sensible narrative, are constructive processes on the part of the historian. Resulting narratives may be judged historically along a continuum of possible, plausible (or implausible), actual, where the history of what really happened is an ideal that never can be reached. The data of history will never be sufficient to logically license the historian to say "it *must* have been like this". It will always be underdetermined.

In reconstructing a person's thought processes, and elucidating the sources she drew upon, the internalist historians attempt to explain how the "final" ideas came about. But the externalist's claim is that even ideas do not work in a vacuum. This was also the point of the reliabilist and warrant theories in epistemology. The warrant for a belief that someone holds comes partially from the circumstances and events (context) external to the thinking subject. A belief is warranted, on one version of such a theory, because it leads to reliable action, where that action's reliability is grounded in evolutionary history that made the action salient or in social norms that exist externally and which warrant the person's inferences that are based on that belief.

This external component we saw earlier when we mentioned Hempel's point about acceptability: what we accept will be determined by many things external to the internal logic of our ideas (i.e., in Hempel's case, the logical relations connecting evidence to theory, which corresponds to what we have called above the epistemic or cognitive). What is *epistemically* external are many of the values and interests that a person has and, further, those values and interests that other persons have that are taken to be relevant to assessing the worth of the theory or claim regarding its acceptability. Even more strongly external are the warrants that are claimed to be provided by adaptive evolution[28], or those that are claimed to rest on social norms, e.g. of inference or acceptable behavior[29].

Even if one does not take such a strong externalist stance on epistemic issues, it still seems clear that, in writing an internal history, the historian, as we have said, must make the narrative *plausible*. But what counts as plausible, the standards by which plausibility is judged, cannot depend upon internal structural relations alone. That is, the standards cannot depend solely on the internal logical relations exhibited in the narrative. This would be like having a criterion of plausibility that was purely syntactic. But history, like semantics, depends upon the world. Plausibility is judged, in the one case, by how well the historical narrative fits the "facts" (loosely speaking), and, in the other, by how well the semantics relate words to the world. The point is, plausibility itself is *externally* determined. This is not to claim that internal coherence and choice of connectives are not relevant to the narrative's success. The parallel is that syntax is important both because it provides rules for the internal structure of language and because it is adequate only when an adequate semantics is represented in the syntactic structures.

Another type of criticism of the internal history of ideas will reveal another aspect of historical reconstruction. Recently, it has become fashionable to claim that traditional internal histories of science are *anachronistic*, and therefore do not reflect with any accuracy the happenings of an historical period or the historical actor's categories[30]. We agree with both the intent and substance of some of the charges. Yet, there are formulations of this claim that miss a major reason for doing history. It is claimed that anachronistic ("Whiggish") history takes contemporary science as it currently stands as the end point, and looks at historical episodes as if they were steps towards it. And, the charge is well grounded. There are many cases, where this is exactly what the historian was doing. But one must pay attention when arguing that this is a bad way of doing history[31]. The only thing that could be seriously wrong about this criterion or goal for doing history would be if historians claimed that this was the only proper way to do history: i.e., that the story they tell of how contemporary science came to be as it is, was the only story that could be told or that was worth telling (i.e. ought to be told). But no historian we know would make (or would have made) such a parochial claim. At its extreme, this would be tantamount to claiming that economic history, political history, or even history of art are not worth doing, which would work as a *reductio* for any such position.

In a less extreme form, such a claim would presuppose that "science" really was the same thing (in some deep sense) in all historical periods, and that this deep story is the only one that needs to be told, *vis a vis* the history of science. But this position is that of the *apriori* "logical" philosopher; and indeed, it is what we have ascribed, in one of its forms, to Lakatos. It is the claim that science has only one timeless logic, e.g. of confirmation or of increasing empirical content. But do *historians* normally hold such positions? It seems to us that only philosophers of a certain bent hold such timeless relations to be definitive of the nature of some subject. As we argued above, such a view makes for bad history.

We want to claim that doing history by showing how the past led to the present is not only one legitimate way of writing history but also that it is an extremely important and worthwhile way. It is certainly not the only way. Historians develop selective principles for many reasons. One may try to assess all the different activities that a person was engaged during a specified period, and so, e.g. we might look at Newton's work in astronomy, physics, theology and alchemy, using our contemporary categories to isolate these different activities. However, we could try to reflect in our narrative how it was that Newton himself did not make those disciplinary distinctions and how it was that he saw all these activities as aspects on one enterprise. Such a history would tell us more about Newton himself and his times than if we only looked at what we took to be his physics. But it is a legitimate historical interest to be interested in his physics as it related to physics that came before and after him. This interest may lead to informative and well constructed history, even though in Newton's time, there was no such no such discipline as physics. A classic example is Marshall Clagget's *The Science of Mechanics in the Middle Ages*. It is a most useful book that has been used to train generations of historians of science, even though there was no science of mechanics in the Middle Ages.

Briefly and most generally stated, the reason why this is a good and legitimate way of doing history is as follows. One of the things that history must provide (if it is to be of more than mere antiquarian interest), is some perspective on the *present*. It is by such provision that we can learn from history. Most, if not all, non procedural learning, is *comparatively based*, and so history can help us to learn by providing comparisons of some aspect in the past with a related aspect in the present. The comparisons are what provide the perspectives, options of differences and space for imagining possibilities. As such, we may learn something about (for example) the present concept of mass by comparing it with Newton's, or even with an earlier concept, say, of Galilean specific gravity. This allows us a perspective of the current concept, and, in addition, shows us something about how science has changed; maybe even can help us understand something about how science came to be *science*.

We are not saying that the historian to do her job well must draw out these perspectival implications or make the contrasts explicit. It is hard enough to get the history straight. Moreover, this argument does not claim nor imply that there are not equally legitimate reasons to do history. Knowledge of the past may be pursued for many reasons, and knowledge for knowledge's own sake is one of them too. But the danger with such positions is that they may collapse into antiquarianism, though on behalf of antiquarianism it must be said that such pass-times have provided a lot of raw material for the historian. Yet, when history is not done with the goal of understanding, it is in danger of becoming fatuous.

There is no place here for presenting or arguing either for the details of this learning theory or of this use of history. But if this sketch is at all persuasive, then one will be persuaded too that anachronistic or Whiggish, history is one legitimate way of doing history. If this is not yet persuasive, then maybe we'll be granted another chance in another place. In any event, we hope we have been persuasive regarding the claim that guiding principles are what saves history from blindness, but that there are some guiding principles, including Lakatos', that lead to history that is worse than blind. Such history is timelessly, in Popper's most fearsome phrase, dogmatic.

Notes

1 "Philosophy of science without history of science is empty; history of science without philosophy of science is blind" (Lakatos 1971, p. 91).

2 Popper's most complete statement of the demarcation problem and of his own solution is in his (1959). Lakatos' own solution, analyzed with respect to falsificationism in general, is offered in his (1970), and further elaborated in his (1971).

3 In particular, Lakatos (1971) was published with comments and Lakatos' own replies; Kuhn, Koertge, and Feigl were among the critics. A thorough criticism of Lakatos' Methodology of Research Programmes is in Laudan (1977).

4 Popper (1959). See also his (1963).

5 See the discussion on sophisticated vs. methodological falsificationism in Lakatos (1970, pp. 116-132), and some precisations on "the real Popper" in the Appendix.

6 Lakatos (1970, pp. 116-117). Lakatos criticizes Popper, among other things, on two main issues. On one hand, Popper still insists on "criteria of refutation" which must be presented in advance; on the other hand, Popper claims that, in order to win over its competitor T, T' must explain *all the facts explained by T* plus predict new facts.

7 "But, of course, if falsification depends on the emergence of better theories (...) then falsification is not simply a relation between a theory and its empirical basis, but a multiple relation between competing theories, the original 'empirical basis', and the empirical growth resulting from the competition. Falsification can thus be said to have a '*historical character*'" (Lakatos 1970, p. 121, author's italics). Later on, we will problematize this "multiple relation".

8 Lakatos (1971, pp. 105-108). In a footnote (123), Lakatos justifies his different use of the terms "internal and external history" as part of his new historiographical research programme.

9 For an extensive discussion of this problem, see Grünbaum (1976). See also Laudan (1977).

10 This attitude caused Kuhn's reaction: "When one's historical narrative demands footnotes that point out fabrications, then the time has come to reconsider one's philosophical position" (Kuhn 1971, p. 143).

11 The example regards Bohr's theory predicting that hydrogen lines would obey the Balmer formula. While it had already been observed that hydrogen lines obey the Balmer formula, Bohr actually predicted that they would. This is in itself, Lakatos argues, a new fact.

12 What Lakatos never addresses is how real scientists can possibly be rational in the first place. If, as Lakatos argues, there is no possible rational appraisal of science *in the making*, then scientists are denied the possibility of making rational choices. But then, scientific method or any such procedure cannot be considered rational.

13 Assuming, for the moment, that this "measuring" is unproblematic.

14 Including inconsistencies and anomalies afflicting his own methodology of research programs. See Lakatos (1971, p. 118).

15 For example, see Ayer (1942), and Carnap (1936).

16 Larry Laudan holds a somewhat similar view. Whether a *research tradition* solves more problems than not is a matter of "internal logic"; for the rational or scientific progress nothing else matters. See Laudan (1977).

17 A similar story could be told about Larry Laudan's problem solving. Laudan never provides any criteria for what counts as a *solved* problem.

18 This is worth noting in this context, for Laudan tried the same reconstructing history ploy to test his method and show that it works better than Kuhn's or Lakatos', see Laudan, Donovan and Laudan (eds.) (1988).

19 The schema we outline here is not unlike Laudan's project (in his 1977): the important difference is that we give up an account of the rational, or the cognitive, or the epistemic that depends on any internal-external distinction.

20 Hempel actually saw this issue, but he remained too attached to the logical model. For Hempel, the relation between theory and evidence is a *logical* one; while what is involved in *accepting* any such relation as being acceptable may involve non cognitive values or other external motivations. See Hempel (1965, pp. 90-93).

21 Among the many contributions to the debate: Dretske (1981, 1995); Goldman (1986); Plantinga (1993a and 1993b).

22 In the conclusion of his (1993b), Plantinga argues forcefully, against Pollock, Millikan and others, that evolutionary theory, in any respectable form, does not provide the "ultimate" warrant for reliable beliefs and so not for knowledge. However, *pace* Plantinga (1993b and 2000), we do not think that naturalistic accounts of knowledge in terms of warrant are doomed due to the impossibility of a naturalistic (i. e., not theistically grounded) account of function. We do not believe that God is a reasonable ground for warrant, either. Theories of development and learning are specific and relevant enough, and have the potential to provide for warranted beliefs. But this is a promissory note that cannot be cashed at this time.

23 Much of what is written here is said in Machamer (1994), though put in a different way.

24 For example, let's take the Lakatosian claim that "it is primarily the positive heuristics of his (the scientist's) programme, not the anomalies, which dictate the choice of his problems" (Lakatos 1971, p. 99). Terms like "dictate programme choice", or "anomalies", find referents in the case-study on Bohr presented in the earlier 1970. So, rather than the *anomaly* (referent: the inconsistency with the Maxwell-Lorentz electrodynamics), it is the *positive heuristics* (referent: the way the five postulates of Bohr's quantum theory of light emission powerfully solve the problem of the stability of Rutherford atoms, which, according to the well-corroborated Maxwell-Lorentz electrodynamics, should collapse) which *dictate* Bohr's choice of the quantum research programme. See case-study in Lakatos (1970, pp. 140-154).

25 We are not by this citation arguing on behalf of the history done by of Hanson and Drake. A much cleaner history of Galileo's work is offered in Wallace Hooper (1996).

26 For example non-cognitive values enter into decisions of acceptability see Douglas (2000). We would argue further that non cognitive values enter into determining the relation of evidence to theory, and, more inconoclastically, that there is no coherent way to actually draw a cognitive (epistemic)/ non-cognitive (non-epistemic) distinc-

tion. But this is a large claim that cannot be argued here. See Machamer (1998); and Machamer (1999).

27 For example, Hanson (1958), and Drake (1978), and better Hooper (1996).

28 For example, Millikan (1984).

29 For example, see Robert Brandom (1994).

30 A typical example of the criticism coming from the "new history" is Margaret J. Osler (2000).

31 See also Baltas (1994), for a revision of the concept of "Whiggish history".

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