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THEODORE ARABATZIS

RATIONAL VERSUS SOCIOLOGICAL REDUCTIONISM: IMRE LAKATOS AND THE EDINBURGH SCHOOL

I. INTRODUCTION

The publication of Thomas Kuhn's *The Structure of Scientific Revolutions* initiated a new era in history, philosophy, and sociology of science. Its influence on history of science, though pervasive, has been indirect. The model of scientific development expounded in *Structure* has never been fully applied (not even by Kuhn himself) for elucidating a past scientific episode.[1] On the other hand, by indicating that the very content of scientific knowledge is amenable to sociological analysis, it had a significant effect on sociology of science and thus indirectly on history of science. Given the well-known and profound transformation that Kuhn's work effected in our philosophical understanding of the nature of scientific knowledge and its considerable effect on sociology of science, its indirect influence on history of science should not be underestimated. Two recent historiographical research programs, Imre Lakatos's *Methodology of Scientific Research Programs* and the 'strong program in the sociology of science' associated with a group of sociologists in the University of Edinburgh, emerged in an attempt to respond to or develop certain aspects of *The Structure*. Their aim was to reconstruct past scientific episodes either, as in Lakatos's case, in the light of a philosophical theory of scientific rationality, or, as in the strong program's, in the light of a sociological theory of scientific practice. Since both programs were considerably influenced by Kuhn and, as I will argue below, were reductionist in that they aimed at reducing historical explanation to a rational or sociological core, it is instructive to make a comparative evaluation of them.

The goal of this paper is to offer a concise critical exposition of these two historiographical approaches, to highlight their differences and common aspects, and to discuss their relevance for contemporary historiography.[2] The core of my argument will be that there are some striking parallels between rational and sociological reconstructions of the history of science. In particular, both are fundamentally ahistorical in that they dismiss the explanatory value of the historical actors' own reasons for upholding their beliefs and pursuing their actions.

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II. HISTORY OF SCIENCE AND ITS RATIONAL RECONSTRUCTIONS

II.1. *Rationality and Methodology*

Lakatos's *Methodology of Scientific Research Programs (MSRP)*[3] was developed to meet the challenge that Kuhn's work posed for scientific rationality. According to Lakatos, Kuhn portrayed paradigm replacement as a fundamentally irrational process. Given the then prevailing notion of rationality, Lakatos was, of course, right. This notion had its origins in Reichenbach's codification of the two contexts within which scientific activity takes place, the context of discovery and the context of justification. The former consists in the processes of discovery of scientific hypotheses and theories; the latter in their testing and validation. In Reichenbach's view the context of discovery was the province of historians, psychologists, and sociologists and was not susceptible to logical analysis. On the other hand, the context of justification was an area which could be rigorously explored and formalized and thus fell within the province of logic and philosophy.[4] Standards of rationality applied only to the context of justification. The rules which governed the process of justification defined the canons of scientific rationality. Thus, in the logical positivist view of science 'rational' meant rule-governed. Kuhn, on the other hand, denied that rigid and precise principles of rationality unambiguously determine paradigm-choice. So, if we adhere to the positivist notion of rationality we are led to the conclusion that paradigm replacement is an irrational process.

Kuhn, however, did not develop an alternative theory of scientific rationality, appealing instead to 'socio-psychological' factors to explain paradigm-change. Lakatos, on the other hand, proposed his *MSRP* as a theory which captured the essence of scientific rationality; a theory which could be employed to explain the development of science over the last four centuries in predominantly 'rational' terms. For the purposes of this essay it is not necessary to present in detail Lakatos's methodology, since my assessment of its value for historiography will be relatively independent of its particular features and its philosophical merits.[5] Suffice it to say here that for Lakatos

> The basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a '*research programme*', with a conventionally accepted (and thus by provisional decision 'irrefutable') '*hard core*' and with a '*positive heuristic*' which defines problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them victoriously into examples, all according to a preconceived plan.[6]

It is the machinery of research programs that is to be employed to evaluate the rationality of past scientific episodes. However, one is tempted to ask in what sense Lakatos's methodology would explain those episodes. But before discussing this question we need to elaborate on Lakatosian historiography.

Following Agassi,[7] Lakatos argued that philosophical and normative elements inevitably enter the historian's reconstruction of past scientific practice by influencing her selection and interpretation of historical data. However, Lakatos moved a step further and asserted that historical reconstructions should be explicitly carried out in the light of the best available theory of scientific rationality, namely the theory which portrays most of past scientific episodes as rational. Not surprisingly, Lakatos believed that the best available model of rationality was his *MSRP*. The latter would enable the historian to delineate the domain of internal history, i.e., the domain of past scientific developments which appear as rational in the light of the *MSRP*. When a past scientific episode can be portrayed as rational in the light of the *MSRP* this does not mean that all historical actors who participated in that episode followed the specific methodology proposed by Lakatos. It only means that the methodology would result, in most cases, in the same judgements, decisions, and actions as those of the relevant scientific élite. The actual reasons which led this group to make these judgements and to follow these actions are, according to Lakatos, irrelevant from the point of view of methodology and internal history of science.

One point that should be emphasized for my analysis is that Lakatos's notion of internal history is, as he put it, 'unorthodox' and comprises only a few of the elements which would fall under the customary conception of internal history. Internal history, in Lakatos's sense, not only excludes the institutional, cultural, and socio-economic context of scientific practice but also the "scientists' beliefs, personalities or authority. These subjective factors are of no interest for any internal history." Moreover, it excludes "everything that is irrational in the light of his [the historian's] rationality theory"; its sole concern is the rational 'growth of disembodied knowledge'.[8]

Lakatos recognized that no theory of rationality could ever portray every episode in the history of science as completely rational, either because scientists *qua* humans are not completely rational or because the cultural, political, and social context of scientific activity might unduly influence its rational development. Thus, he held that when the historian focuses on such irrational episodes, i.e., past scientific developments that did not take place as they should have according to the *MSRP*, then she is to explain those aberrant events by resorting to 'external' socio-psychological factors.[9] External history, thus, becomes parasitic on the internal(rational) history of scientific knowledge. The latter determines the subject matter and scope of the former which, incidentally, "is irrelevant for the understanding of science."[10]

II.2 *Lakatos and Historiography*

If Lakatos's aim were only to rationally reconstruct the majority of past scientific developments in the light of his methodology, the completion of his project, if successful, would have provided a rational explanation of the growth of knowledge. That is, it would have shown in what respects victorious research programs in the history of science were superior to their competitors from the point of view of a timeless methodology. But Lakatos had a more ambitious aim, namely to provide a model for historiographical practice.[11] However, if the *MSRP* is to be of some use for the historiographical enterprise, as usually conceived and practiced by historians, that methodology must have been employed, if only implicitly, by the historical actors in the scientific episodes under scrutiny. Only then it could have constrained their decisions and could, thus, function as an explanatory resource for the historian. Otherwise, despite its potential significance for providing a *posthoc* justification of the historical actors' decisions, it could not be of any use for explaining why the actors themselves were led to those decisions.[12]

Thus, to show the universal historiographical applicability of the *MSRP* one needs to demonstrate that it was shared by the majority of scientists who had an impact on the development of scientific knowledge. Such a demonstration cannot be found in Lakatos's writings. Instead, Lakatos seems to have assumed without argument that scientists have been, for the most part, adherents of his methodology. Otherwise, one can not make sense of his insistence on the need for socio-psychological explanations of all those episodes in the history of science which cannot be portrayed as rational in the light of his methodology. Only if the *MSRP* was shared by the majority of scientists and, thus, constrained their decisions, would any deviation from its prescriptions be in need of 'external' explanation. In the absence of such an assumption, those 'deviant' episodes might be explained adequately – without recourse to

socio-psychological factors – by recovering from the historical record the values of appraisal shared by the scientists in question. In that case, the difference between these values and those prescribed by the *MSRP* would account sufficiently for the 'irrational' aspects of those episodes.

The heart of the problem is that Lakatos tended to conflate the justificatory and the explanatory aspects of his methodology. If the triumph of a research program over its competitors could not be justified from the point of view of his methodology he assumed that that victory could only be explained by appealing to socio-psychological factors. If, on the other hand, a past scientific episode took place as it should have according to his methodology he assumed that it was thereby completely explained. The latter assumption pervades most of the historical case studies that have been carried out by Lakatos's followers in accordance with his methodology. But, as Kuhn pointed out, those studies fail to consider "what actually attracted scientists to or repelled them from the various research programmes under study. ... If analysis discloses a philosophically relevant difference between research programmes, then that difference is assumed to have played a role in programme choice."[13] Indeed a successful reconstruction of a particular development would explain why it was a rational episode in the growth of knowledge. What it would not explain, however, is why the actors of that episode adopted certain beliefs and made certain decisions leading to that outcome, which is the historian's question.

These untenable assumptions, which are due to Lakatos's conflation of the explanatory and the justificatory aspects of his methodology, proved fatal for his historiographical approach. Historians are not concerned to justify in an atemporal and context-independent sense past scientific beliefs, actions, and decisions; rather they attempt to describe and explain these beliefs, actions, etc., as 'reasonable' solutions to the specific problem situation faced by the scientist under consideration. To illustrate my argument consider Newton's belief in the existence of absolute space. In order to justify this belief Newton would have given, among other reasons, certain theological arguments. For a modern secular historian these arguments would not carry much weight. Nevertheless, in trying to explain Newton's belief in the existence of absolute space, our secular historian should certainly appeal to these particular theological arguments, since they were employed by Newton himself as warrant for his belief in absolute space. Historical sensitivity demands that the historian should adopt, to the extent possible, the mindset of the historical figure whose beliefs and actions she tries to explain.[14] In the ideal case the recovered rationality would amount to the specific epistemic reasons which the historical actors themselves would have given as warrant for their beliefs, decisions, and actions. Needless to say, what counted as a reason for a specific historical actor need not count as a reason for the historian herself.[15]

The insensitivity of Lakatos's methodology to the categories and criteria of historical actors renders it unsuitable as a historiographical tool. A similar insensitivity, as we will see, characterizes the strong program in the sociology of science.

III. SOCIOLOGICAL RECONSTRUCTIONS OF THE HISTORY OF SCIENCE

III.1 *Three Theses of the 'Strong Program'*

The Structure of Scientific Revolutions exerted a profound influence on the sociology of science. Pre-Kuhnian sociology of science focused on the study of scientific institutions, the reward system of science, the norms which constitute the ethos of the scientific enterprise, and the social roles of scientific practitioners and abstained from a sociological analysis of the content of scientific knowledge.[16] When Kuhn indicated in *Structure* that certain aspects of the development of scientific knowledge, and especially theory-choice, "are irreducibly sociological",[17] the road was open, so the proponents of the strong program[18] thought, for a total sociological reductionism. It is beyond the scope of this paper to give a satisfactory account of the intellectual origins of the strong program. Suffice it to say that the work of earlier sociologists of knowledge, most notably Durkheim and Mannheim, along with Kuhn's view of scientific knowledge exerted a formative influence on its development.[19]

The strong program is identified with the following theses, "of causality, impartiality, symmetry and reflexivity."[20] I will focus exclusively on the first three, since the issue of reflexivity – the applicability of sociological explanatory models to sociological knowledge itself – though crucial for the viability of the strong program, is without historiographical significance.[21] These tenets assert that the proper sociology of scientific knowledge "would be causal, that is, concerned with the conditions which bring about beliefs or states of knowledge." Furthermore, "It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation." Finally, "It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs."[22]

The thesis of causality is relatively uncontentious, apart from the fact that the philosophically problematic notion of cause is left unexplicated. The thesis of impartiality emphasizes that all beliefs and actions regardless of their epistemic, rationality, and pragmatic status should be candidates for explanation. As for the impartiality principle, it is, again, uncontentious: no one has disputed, to the best of my knowledge, that all beliefs and actions, irrespective of their status, should be candidates for explanation. The symmetry thesis, as one would expect, has been intensely debated.[23] The core of the dispute concerns the range of factors that could be legitimately invoked to explain a historical actors' beliefs. The symmetry thesis entails a very radical perspective on this issue. Since, by their very nature, epistemic factors cannot be employed to explain 'irrationality' or 'unsuccessful' beliefs it follows from this thesis that they should be dispensed with altogether as an explanatory tool. Thus, the historian and the sociologist of science who adhere to this principle would need to exclude epistemic factors, i.e., the reasons that could be invoked to justify the beliefs in question, from their explanatory repertoire. To put it another way, given that epistemic factors do not bear on the explanation of irrational or unsuccessful beliefs and, moreover, that all beliefs, regardless of their status, should be explained 'by the same types of cause' it follows that 'rational' and 'successful' beliefs should also be explained by non-epistemic factors.[24] Notice how radical and one-sided the symmetry thesis is. Even Lakatos, who was obsessed with rationality, had not suggested that social and psychological factors have absolutely no place in legitimate reconstructions of the history of science.

Lurking behind the symmetry thesis is the underlying question: Whose evaluation of the status of beliefs cannot be employed to explain why those beliefs were adopted? The historian's and sociologist's retrospective evaluations or the historical actors' own appraisals? As far as I can tell, no answer to this question can be found in Bloor's *Knowledge and Social Imagery*. In a later article, however, Barnes and Bloor assert that "regardless of whether the sociologist evaluates a belief as true or rational, or as false and irrational, he must search for the causes of its credibility."[25] Barnes and Bloor's concern is with the sociologist's evaluative stance towards the beliefs that he is trying to explain. On the other hand, they say nothing about the potential significance of the historical actors' own appraisal of the beliefs in question for the explanatory task of the historian or the sociologist. I will have to say more on this point below, since it will become important for my assessment of the symmetry thesis.

III.2. *The Strong Program and Historiography*

The historiographical implications of the symmetry principle are clear. Historical episodes should not be explained by the reasons that would justify their outcome. Rather, the sole explanatory resources of the historian should be either macro-social parameters, e.g., class membership or micro-social factors, e.g., the professional interests of the participants in the episode under study. The latter are particularly important since, according to Bloor, "Much that goes on in science can be plausibly seen as a result of the desire to maintain or increase the importance, status and scope of the methods and techniques which are the special property of a group."[26] As Thomas McCarthy pointed out, all of these explanatory factors appear to have nothing "in common, except perhaps that they are *not* the sorts of evidencing reasons actors *themselves* would give for their beliefs."[27]

It would be tempting to attempt a sociological reconstruction of Barnes and Bloor's belief in the central principle of the strong program,[28] but for my purposes it is preferable to offer a 'rational reconstruction' of their path to the symmetry thesis. Their central argument goes as follows: There are no context-independent and supracultural norms of rationality. In other words what counts as a reason is highly context-dependent. Thus, in explaining a scientist's belief, decision, or action we cannot appeal to reasons which would constrain any rational agent, because such reasons simply do not exist. The conclusion seems to follow that all beliefs, decisions, etc., should be explained in the same way, i.e., in sociological terms.[29]

There is, however, a crucial flaw in the above argument; a flaw which results from Barnes and Bloor's ahistorical conception of rationality and from their disregard of the historical actors' own appraisals of the beliefs, etc., in question. The fact that there are no transcultural norms of rationality does not imply that the members of a specific community do not share certain values that define and regulate rational behavior. These norms, to the extent that they were shared by the members of the community, would constrain human actions and should thus be taken into account in explaining these actions.

The historiographical implication of the symmetry thesis is that the historian cannot use the historical actors' own appraisal of their beliefs and actions in her explanatory task. In other words, that the reasons that the historical actors would offer in support of their beliefs do not *really* explain why they adopted them and that a genuine explanation should incorporate the social factors which (supposedly) underlie the beliefs in question. I do not deny that cases of 'false consciousness', where an actor's reasons for upholding a belief are merely *posthoc* rationalizations which have nothing to do with what induced him to adopt this belief in the first place, actually exist. However, the ubiquitous presence of false consciousness should not be an *a priori* presupposition of historiographical practice. That false consciousness was operative should be the outcome and not the starting point of a historical reconstruction.

Some proponents of the strong program could grant that contextual reasons constrain and guide scientific behavior and still argue that reasons themselves are not self-explanatory and should, therefore, be explananda of the sociology of knowledge.[30] The premiss of this argument seems correct. The reasons invoked by an actor are intimately connected with the cognitive values of the community to which the actor belongs and, no doubt, one can always ask why a specific community espoused a particular set of values. It does not follow, however, that such an explanation *must* be carried out in sociological terms. It might be the case, for example, that a 'biological' explanation would be more pertinent and that one would be able to explain the dominance of the values in question by showing how they augmented the survival capacity of the community. Furthermore, the historian always has the option to say that the predominance of the values in question is merely a brute fact, and may or may not have an explanation in terms of underlying social, or psychological, or biological factors.[31] Finally, to the extent that the cognitive values associated with the scientific enterprise have been stable throughout the development of science and have been shared by different scientific communities,[32] we have reasons to believe that sociological explanations of their continuing predominance would not be met with success. Explanations of this kind are inevitably tied to local characteristics of a specific community and are, therefore, not applicable to a phenomenon which transcends the boundaries of particular communities, unless one locates sociological factors which are common among different communities and demonstrates that those factors brought about the phenomenon in question – an admittedly daunting task.

If proponents of the strong program were to acknowledge the significance of reasons that historical actors would give for their beliefs, choices, etc. for the historian's explanatory task, they could not be charged with being ahistorical. To the extent, however, that they have chosen to stay close to the original spirit of their program and adhere to the symmetry principle they are led to the same ahistorical predicament that characterized Lakatosian historiography. Remember that for Lakatos the reasons that historical actors themselves would give in support of their decisions, were immaterial for the purposes of internal historiography. In the same way it is an *a priori* thesis of the strong program that these reasons are of little use to historians who aim at reconstructing past scientific practice. These reasons, according to the strong program, are not the 'real' causes of the actors' behavior, which instead must be explained by appealing to underlying social factors. In the same way that Lakatosian scientists were infallible apostles of the *MSRP* the actions of 'strongly programmed' scientists are mere epiphenomena of underlying social realities. *A priori* sociology has replaced *a priori* methodology as a guide of historiographical practice. There is an important difference, however, between the two historiographical approaches. Whereas Lakatos, at least in some of his moods, recognized that his rational reconstructions were philosophical fairy tales, 'strong programmers' insist that their sociological reconstructions are fully realistic.[33]

IV. CONCLUDING REMARKS

I have argued that both Lakatos's theory of scientific rationality and the strong program's sociological theory of scientific practice lead to a similar ahistorical predicament, since their 'application' to history of science entails a dismissal of the explanatory value of the reasons that historical actors would give in support of their beliefs. This was the negative message of my paper; both reconstructions, to the extent that they are ahistorical, have not much to offer to historiographical practice.

Despite my criticism, there are some valuable elements in both the rational and the sociological approaches. One of the most interesting aspects of Lakatos's *MSRP* was that it tried to capture the internal dynamic of the Popperian world of objective knowledge. Popper introduced a distinction between three different worlds. In Lakatos's words, "The 'first world' is that of matter, the 'second' the world of feelings, beliefs, consciousness, the 'third' the world of objective knowledge, articulated in propositions."[34] It is a hard and unresolved problem whether the third world is entirely reducible to the second. If not, then its internal dynamic might transcend the beliefs, abilities, and wishes of human actors and act as a constraint on the development of scientific knowledge. Scientists might be confronted with problem situations that admit of a limited range of solutions, regardless of the abilities and goals of specific human actors. For instance, one could argue that after the development of Maxwell's electromagnetic theory a tension arose between electrodynamics and mechanics which constrained, albeit not determined, the further development of physics. Although Lakatos's attempt to capture this internal dynamic has not succeeded he has brought to our attention a subject which is ripe for exploration.

The value of the sociological analysis of scientific practice should also not be underestimated. For example, studying the wider cultural and social context of scientific activity can provide an understanding of the conceptual resources upon which the scientists draw for furthering their analyses. Furthermore, a case can be made for a more moderate version of social constructionism. The word 'social' encompasses not only macro-social factors, such as the wider social and cultural milieu, but also the micro-social realm, the social interactions between the members of the scientific community. Even if epistemic considerations of empirical adequacy, consistency, scope, simplicity, and fruitfulness (all these values which, as Kuhn himself has argued, are essentially involved in theory-choice) sufficiently constrain the generation and acceptance of scientific knowledge, their bearing on theory construction and theory choice is eventually decided by the relevant scientific community. Although such considerations play a crucial role in the establishment of consensus within the scientific community, their relative weight is subject to 'social negotiations'. The decision as to their appropriate weight requires such negotiations, since the relative significance of each epistemic criterion is not unambiguously specified.

Micro-social processes are also essential in understanding experimental practice, an aspect of science which has been until relatively recently ignored. Logical positivism presupposed the neutrality and unproblematic status of observational data. Hanson, Kuhn, and Feyerabend among others stressed the 'theory-ladenness' of observation and undermined its privileged status in empiricist epistemology. For those authors, however, the problematic status of data was a consequence of the theory-ladenness of perception and of the fact that observational reports are couched in theoretically contaminated language. The various judgements involved in the experimenter's decision to refine and conclude a particular experiment were not perceived as a potential threat to the validity of experimental results. Not surprisingly, the judgmental aspects of experimentation reinforced the social constructionists' scepticism about the validity of scientific findings. In its most radical version social constructionism maintains that the constraints of nature on the products of scientific activity are minimal and that the historical and sociological study of science should dispense with 'nature' as an explanatory ingredient in the generation and acceptance of scientific knowledge.[35] Data are selected or even constructed in a process which, if we believe the social constructionists, reflects the social interactions within the relevant scientific community. Scientific discoveries are not only or primarily a matter of finding laws or theories which account for the data, but also a matter of selecting or constructing data themselves.[36]

The social interactions and the various 'negotiations' which take place in the scientific community over the validity of experimental findings are undoubtedly an important and relatively neglected area of study. However, I suspect that far from implying scepticism, the social nature of experimental activity should be viewed as one of its main strengths. After all, as an outcome of this activity, "experimental conclusions have a stubbornness not easily cancelled by theory change"[37] and "experimental phenomena persist even while theories about them undergo revolutions."[38] The study of the micro-social aspects of various scientific communities can, thus, enhance our understanding of the judgmental aspects of experimental practice, as well as of the processes of theoretical decision-making.

In conclusion, any attempt to reduce the historiographical enterprise to rational or sociological reconstruction is doomed to fail. Only a metatheoretical account of science which would incorporate the intellectual, social, and material constraints on scientific practice would be a promising historiographical tool.

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NOTES

[1] In the afterword to the 2nd ed. of his *Black-Body Theory and the Quantum Discontinuity, 1894–1912* Kuhn declared that "I do my best, for urgent reasons, not to think in these terms [paradigms, revolutions, gestalt switches, and incommensurability] when I do history, and I avoid the corresponding vocabulary when presenting my results. It is too easy to constrain historical evidence within a predetermined mold." (T. S. Kuhn, 1987, p. 363) The same feeling, as Gary Gutting correctly pointed out, is shared by the majority of historians of science who "are not currently very interested in general interpretative schemata, which may have a Procrustean effect on their efforts to understand specific episodes." (G. Gutting, 1980, p. 3)

[2] Lack of space prohibits me from extending my analysis beyond the theoretical pronouncements of the two schools, even though their followers have produced a considerable amount of historical case studies. For a collection of rationally reconstructed historical episodes see C. Howson, 1976; and for a comprehensive bibliography of case studies which either support or were directly influenced from a sociological approach to scientific knowledge see S. Shapin, 1982. For a more up to date treatment of the latter see J. Golinski, 1990.

[3] I. Lakatos, 1970.

[4] "The act of discovery escapes logical analysis ... But it is not the logician's task to account for scientific discoveries ... logic is concerned only with the context of justification." H. Reichenbach, 1951, p. 231. Incidentally, Reichenbach's view was shared by Karl Popper: "the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. ... the logical analysis of scientific knowledge ... is concerned ... only with questions of *justification or validity*". K. Popper, 1965, p. 31.

[5] For an excellent and detailed treatment of Lakatos's philosophy of science see I. Hacking, 1981.

[6] I. Lakatos, 1971, p. 99.

[7] J. Agassi, 1963.

[8] I. Lakatos, 1971, p. 106. For a discussion and critique of Lakatos's idiosyncratic conception of internal history see T. S. Kuhn, 1971, pp. 140–41 and I. Hacking, 1981, pp 138–39.

[9] I. Lakatos, 1971, p. 102.

[10] I. Lakatos, 1971, p. 92.

[11] One of the aims of his "History of Science and its Rational Reconstructions" was "to explain *how* the historiography of science should learn from the philosophy of science" and in particular from the *MSRP*. I. Lakatos, 1971, p. 91.

[12] Cf. E. McMullin, 1984, p. 137.

[13] T. S. Kuhn, 1980, p. 188. For the papers in question see C. Howson, 1976.

[14] Of course this point has been made before. Bertrand Russell, for example, maintained that "In studying a philosopher, the right attitude is neither reverence nor contempt, but first a kind of hypothetical sympathy, until it is possible to know what it feels like to believe in his theories." (B. Russell, 1945, p. 39; cited with approval in T. S. Kuhn, 1977, p. 149)

[15] For an insightful discussion of epistemic factors and their significance for historical explanation see E. McMullin, 1984, esp. pp. 129–136. For this and many other points I am indebted to McMullin's analysis.

[16] See, for instance, R. K. Merton, 1973; and J. Ben-David, 1984.
[17] T. S. Kuhn, 1970a, p. 237.
[18] The tenets of the 'strong program' were initially articulated in David Bloor's *Knowledge and Social Imagery* (D. Bloor, 1976). The focus of my critical remarks will be on this particular formulation of a wider movement in science studies, known as social constructionism. Occasionally, however, I will refer to authors who do not, strictly speaking, belong to the Edinburgh School but who share a similar, and sometimes more radical, perspective.
[19] Both Mannheim and Durkheim, however, did not believe that sociological analysis could provide any insight into the formation and consolidation of scientific beliefs. See J. R. Brown, 1984b, pp 3–6; and S. Woolgar, 1988, pp. 23–24.
[20] D. Bloor, 1976, p. 7.
[21] For a discussion of whether the reflexivity principle poses a threat to the viability of the strong program see D. Bloor, 1976, pp. 17–18; J. R. Brown, 1989, pp. 41–45; and M. Hesse, 1980, pp. 42–45.
[22] D. Bloor, 1976, p. 7.
[23] See, for instance, L. Laudan, 1984; and D. Bloor, 1984.
[24] Note that I have deliberately excluded the dichotomy between true and false beliefs because epistemic factors could very well lead to the adoption of beliefs which eventually might turn out to be false. Thus, both critics and proponents of the strong program agree that this dichotomy is irrelevant for the purposes of historical explanation. Cf. L. Laudan, 1984, pp. 56–57.
[25] B. Barnes and D. Bloor, 1982, p. 23.
[26] D. Bloor, 1984a, p. 80.
[27] T. McCarthy, 1988, p. 90.
[28] Some hints for such an attempt can be found in E. McMullin, 1984, pp. 154–155.
[29] See B. Barnes and D. Bloor, 1982, pp. 27–28.
[30] This is indeed Barnes and Bloor's position. See B. Barnes and D. Bloor, 1982, pp. 28–29; and D. Bloor, 1984b. This position is, however, at odds with the initial formulation of the strong program and, in particular, with the symmetry principle. Explanations in terms of contextual reasons are not of the same kind as explanations in terms of professional interests, class membership and other such social parameters. Moreover, this retreat to 'reasonableness' is not endorsed by all social constructionists. Steve Woolgar, for instance, claims that "SSS [Social Study of Science] favours the conception of rules as a *posthoc* rationalization of scientific practice rather than as a set of procedures which determine scientific action." S. Woolgar, 1988, pp. 17–18.
[31] I have paraphrased here one of van Fraassen's arguments against scientific realism. See van Fraassen, 1980, p. 24.
[32] As Kuhn remarked "such values as accuracy, scope, and fruitfulness are permanent attributes of science." (T. S. Kuhn, 1977, p. 335).
[33] I should point out, however, that the historical reconstructions that have been carried out by historians who were influenced by the strong program have not been characterized by the deliberate falsification of the historical record that was a standard feature of Lakatos's case studies.
[34] I. Lakatos, 1971, p. 127. For a very helpful discussion on the autonomy of the third world see I. Hacking, 1981, pp. 136–38.

[35] Harry Collins, for instance, claims that "the natural world has a *small or nonexistent* role in the construction of scientific knowledge." Cited in M. Hesse, 1988, p. 105.
[36] The classic in this genre is B. Latour and S. Woolgar, 1986.
[37] P. Galison, 1987, p. 259.
[38] I. Hacking, 1984, p. 172.

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