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Philosophy and History of Science: Beyond the Kuhnian Paradigm

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At issue in this paper is the question of the appropriate relationship between the philosophy and history of science. The discussion starts with a brief sketch of Kuhn's approach, followed by an analysis of the so-called 'testing-theories-of-scientific-change programme'. This programme is an attempt at a more rigorous approach to the historical philosophy of science. Since my conclusion is that, by and large, this attempt has failed, I proceed to examine some more promising approaches. First, I deal with Hacking's recent views on the issues in question, particularly his notion of a 'style of reasoning'. Next, Nickles's reconstructionist interpretation of the development of science and his views on Whig history are addressed. Finally, I propose an account of philosophy as a theoretical, an interpretative and explanatory, enterprise. Thus, three alternatives to the Kuhnian paradigm are discussed, alternatives that share a recognition of the relative autonomy of philosophy from history. Hence, they assume a less tight relationship between philosophy and history of science than is the case within the Kuhnian paradigm.

1. Introduction

In his *Structure of Scientific Revolutions*, first published in 1962, Thomas Kuhn claimed a decisive role for history in devising an adequate concept of science. Since the 1970s, his views have had a very wide appeal (see Fuller, 1992). In particular, they have led to a 'historical philosophy of science', an approach that has been institutionalized widely in so-called History and Philosophy of Science programmes. But, as is well known, Kuhn's influence has reached far beyond the area of philosophy of science. From about 1975 onwards, his work has also contributed to the rise of what is now known as the 'Sociology of Scientific Knowledge' (see e.g. Collins, 1975; Mulkay, 1977). Somewhat later, attempts were made to apply his views to the study of technology (see e.g. Dosi, 1982; Constant, 1984). In these cases, the historical approach has been broadened to include empirical studies of contemporary

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science and technology. Moreover, these studies have in turn produced results that are directly relevant to, and in a number of cases taken up by, the (historical) philosophy of science (see e.g. Rouse, 1987).

The developments sketched above have resulted in various, more or less far-reaching, interactions and mutual influences between the fields of philosophy and history of science (and, more recently, also sociology of science). Yet the recent state of the art has been evaluated differently by different commentators. While some claim that a fruitful and, by and large, unproblematic cooperation between philosophical and historical approaches is feasible, others come up with a much more pessimistic assessment of the present situation. Thus, Ronald Giere claims that his naturalistic, cognitive approach 'provides a basis for fruitful relationships between the history of science and the theory of science' (Giere, 1988, p. 19). In contrast, Rachel Laudan pictures the situation rather differently. Having reviewed the influential constructivist and cultural studies in the history of science and their implications for the philosophy of science, she concludes:

> I cannot but feel discouraged about the state and direction of relations between history and philosophy of science. The days of the late 60s and early 70s, when a productive relation between the two fields seemed well within reach, are long gone (Laudan, 1993, p. 480).

In this paper I will take these diverging evaluations as a starting point for a discussion of a number of answers to the methodological question of the appropriate role of historical work in doing philosophy of science. Thus, my treatment of the relationship between philosophy and history of science is asymmetrical in that it primarily deals with the question of the significance of history for philosophy.[1] Since my main interest is to make a contribution to the recent debates on the issue, I will for the most part leave aside the older disputes between Thomas Kuhn, Karl Popper, Imre Lakatos and Paul Feyerabend, which are well known anyway (see Lakatos and Musgrave, 1970).

In concluding this introduction, let me add three further observations. First, the paper primarily deals with (a particular type of) metaphilosophical issues. Yet, it would be hard to appreciate the import of such issues without any reference to more concrete philosophical results. Therefore, I will also discuss some substantive historical–philosophical claims and approaches (especially in Section 4 and 5). Second, I will finally arrive at a conception of philosophy that is clearly broader than the usual historical philosophies. My argumentative strategy, however, does not presuppose this broader conception right from the start. Instead, I will show that an adequate understanding of historical philosophy itself entails the relevance of certain broader issues, which may then be incorporated into a more comprehensive notion of philosophy of science.

[1]For a discussion of the complementary question of what philosophy can do for history of science, see Nickles (1995, especially pp. 148–151).

Consequently, the present paper does not offer a rendering of the relevant issues of philosophy of science 'in general' (for example, of rationality, objectivity or naturalism), but only in so far as they emerge from an analysis of the

produced by individual researchers. Furthermore, even if external influences may be present, it is assumed that internal historiography suffices for obtaining an adequate understanding of this practice. Moreover, in Kuhn's own studies, the focus has been on the cognitive activities of (groups of) scientists. The main task of a historical philosophy of science, then, is to provide a model of scientific development, which is based on historical studies of this kind.

Kuhn's emphasis on the essential historicity of science entails a sharp contrast to several other approaches (see De Regt, 1993, pp. 17–29). Most relevant in the context of the present discussion is the strong criticism he launches at the many philosophers and scientists who naively endorse presentist points of view. In presentist accounts of earlier science both what is to be counted as science and what is to be evaluated as good science, is determined on the basis of present knowledge and present standards (cf. Kragh, 1987, Chs 4 and 9). A particular form of presentism results from basing one's interpretations of science and scientific knowledge on textbooks. Views of scientific development that build on how it is rendered in textbooks will necessarily lead to highly distorted accounts of what really happened. According to Kuhn,

> the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text (Kuhn, 1970, p. 1).

For this reason, he advises historians and philosophers to focus on what scientists *do*, and not on what they (afterwards) *say* they have done.

The point can be illustrated by historical investigations of scientific discoveries. Such investigations clearly show the inadequacy of textbook views of scientific discovery as instantaneous events and as attributable to individual scientists. In fact, discovery—for example the discovery of oxygen—requires not just the realization that something is the case but also the articulation of what it is that is the case. Because of this, discovery is rather a (more or less prolonged) process, which takes place within or among scientific communities (Kuhn, 1970, Ch. 6).

In sum, according to Kuhn, history of science should provide the basis for philosophical accounts of scientific development. In Paul Hoyningen-Huene's words, 'it determines the questions that can, in a philosophical perspective, be sensibly asked with respect to science' (Hoyningen-Huene, 1992, p. 490). For instance, questions about the meaning of falsifiability in normal science are senseless, because falsification does not play any role in this stage of scientific development. Or, consider the distinction between the context of discovery and the context of justification. Many philosophers have defined philosophy as being essentially about questions of justification, as opposed to questions of discovery. In contrast, Kuhn says about such philosophical distinctions:

> If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate (Kuhn, 1970 p. 9).

In other words, philosophical theories of the justification of scientific knowledge should not be opposed to, but rather derived from, the historical study of scientific development.

3. Testing Theories of Scientific Change

After Kuhn had thus set the paradigm for a historical philosophy of science, many others have contributed to it by further articulating it. As we have seen, in Kuhn's view history should provide the basis for philosophical accounts of science. At face value, this appears to be a clear position. On closer inspection, however, it gives rise to certain questions. For instance, what kind of 'basis' is it that history is able to offer to a philosophical model of scientific development? Is such a model justifiable by means of historical data? And, more particularly, how should we evaluate the rather diverse models (say, of Lakatos, Feyerabend, Laudan and Kuhn), that all claim to be 'based' on history? Which model is the right one? According to Larry Laudan and his co-workers (see Laudan et al., 1986, 1988; Laudan, 1987), a more rigorous historical approach is needed to answer such critical questions. For this purpose, they propose their 'testing-theories-of-scientific-change programme'.

This programme starts from the premise that philosophy of science, if it is to be taken seriously, should itself be practiced in a scientific manner. Therefore, rival theories or models of scientific change should be systematically compared and tested against the historical data.[2] In order to historically evaluate rival theories, one should first formulate empirically testable hypotheses framed in a neutral vocabulary. For instance, the reformulation of both 'research programme' (Lakatos) and 'research tradition' (Laudan) as 'set of guiding assumptions', allows us to draw up the following hypothesis: 'The acceptability of a set of guiding assumptions is judged largely on the basis of *either* their problem solving capability (Laudan) *or* the successful novel predictions to which they give rise (Lakatos)'. After having made the theories of science comparable in this manner, 'crucial experiments' should be carried out by examining as many case studies as possible to see which of the two is right on the issue in question. Thus, the result of the test of the above hypothesis is that, generally speaking, guiding assumptions are accepted because of their problem-solving capacities, while their ability to make novel predictions is not crucial to their acceptance (Laudan *et al.*, 1988, p. 29). Ultimately, the aim of the programme is to establish which methodological theses cover the historical instances most adequately.

In this way, the testing-theories-of-scientific-change programme offers a clear-cut answer to the question of the relationship between philosophy and history of science. Unfortunately, on closer inspection the programme involves a number of questionable assumptions and proves to be far too ambitious. By now, this assessment appears to be widely agreed upon (Nickles, 1986; Richardson, 1992; De Regt, 1993, pp. 24–27). Yet it is instructive to discuss the main problems briefly, since they exemplify nicely the pitfalls of an unreflective 'application' of history to philosophy. Because I think that some of the criticisms by the above authors can be countered (see also Laudan *et al.*, 1988, pp. 12–13; Donovan *et al.*, 1992, pp. xiv–xx), I will restrict my discussion to the really questionable issues.

A first point—that remains largely unnoticed in the critiques of the programme—is the strong bias that is implicit in the identification of 'scientific change' with 'scientific *theory* change'. Experimental and observational research appears to be merely a source of *evidence* for or against theories. The complicated process of producing such 'evidence' is left out, and even the notion of empirical difficulty is defined as actually being a theoretical difficulty, since it results from an inaccurate or failed theoretical prediction of an—apparently evident—empirical result (see Laudan *et al.*, 1988, p. 9). By

[2]In an interview in 1990, Kuhn distanced himself from the idea of using history as a straightforward test of the truth or falsity of theories of scientific development: 'I never thought that *Structure* was more than a highly schematic sketch. I did not expect any direct lessons. I've always said, assimilate this point of view and this way of doing it, and then see what it does for you when you try to write a history, but don't go out looking at history to see whether this is true or false, to test the ideas' (Kuhn, 1990, p. 23). Moreover, in a recent but largely programmatic publication he even appears to retreat from the view that philosophy should be 'derived from' history (see Kuhn, 1992). In this paper, however, I will limit myself to the influential 'Kuhnian' paradigm, as outlined in the preceding and present sections.

conceiving of science as primarily a theoretical activity, a major part of scientific practice—namely observation and experimentation—is deemed irrelevant right from the start. It is highly implausible that an adequate model of scientific development might be obtained on the basis of such a selective starting point.[3]

Next, in several cases, aims and methods endorsed by the 'scientific' approach of the testing-theories-of-scientific-change programme contradict central claims that are taken to be typical of science by some of the theories tested. Let me just mention two examples of this. First, it is repeatedly stated that the ultimate goal of the programme is to bring about consensus. For instance: 'we do not wish to prolong the current state of dissensus in science studies' (Donovan *et al.*, 1992, p. xv). Thus, the results of the historical case studies are evaluated with a view to a confirmation or a refutation of particular theses concerning scientific change. However, this emphasis on consensus through elimination of refuted philosophical claims flatly contradicts the methodological pluralism advocated by Feyerabend and others. Second, consider the substitution of the notion of a set of guiding assumptions for the concept of a paradigm. Guiding assumptions are conceived as stable, wide-ranging and highly influential *theoretical* assumptions and guidelines. In contrast, Kuhn has emphasized the *practical* nature of a paradigm with its central feature of a shared exemplar resulting from a socialization process (Kuhn, 1970, pp. 187–198; cf. Rouse, 1987, Ch. 2). To identify this notion of paradigm with a set of guiding assumptions is to miss a number of crucial distinctions between explicit, theoretical knowledge and implicit, practical skills. For this reason, such an identification is certainly not 'neutral with respect to the theories being scrutinized' (Laudan *et al.*, 1988, p. 9).

A further question concerns the conception of historiography that underlies the testing-theories-of-scientific-change programme. The suggestion is that historiography, like any other science, is capable of producing a record of neutral, historical data that can be used in a straightforward manner to test philosophical accounts of science. However, whether or not there is one proper approach to the history of science, and if so which is the proper one, is an essentially contested question. Thus, Kragh (1987) reviews a large number of at least partly incompatible approaches. Since the arguments of the advocates of the programme depend crucially on what can and what cannot be achieved by historical accounts of science, the issue cannot be by-passed by simply proclaiming that historical testing can give us 'an empirically well-grounded picture of the workings of science' (Laudan *et al.*, 1988, p. 8). It may well be that the observed 'reluctance' of historians to join the testing programme is, to a large extent, due to its lack of historiographical sensitivity and reflexivity.

[3]An interesting model of change and continuity in experimental science is offered in Galison (1987). For an overview of various other accounts, see Hacking (1989).

Finally, there is the problem that some models of scientific change (for instance, those of Popper and Lakatos) are not just descriptive but also, or even primarily, normative. They intend to explain what constitutes 'good' or 'rational' science. In so far as these models are normative, they cannot, so it seems, be refuted on the basis of actual historical cases. According to the proponents of the testing-theories-of-scientific-change programme, however, this is not really a problem.

> The short answer to this criticism is that we do not believe the so-called naturalistic fallacy is a fallacy. We believe that standards of inquiry, like substantive claims about nature, ought to be exposed to and be obliged to survive empirical scrutiny (Donovan *et al.*, 1992, p. xv).

The argument for this proceeds in two steps. First, it is claimed that methodological norms should be reconstructed as hypothetical imperatives of the form: 'if one's (cognitive) end is x, then one ought to do y'. Next, such hypothetical imperatives are said to be based on the empirical statement that 'doing y is the most effective means for realizing the end x'. In this way, methodological norms are claimed to be empirically testable by historically investigating which strategies have been most effective for realizing our cognitive ends (Laudan, 1987, pp. 23–28).

It will be clear that this issue of normativity (including the related notions of rationality and naturalism) is rather intricate. However, for my present purposes it is not necessary to enter into the details of this debate.[4] As Laudan himself observes, his view of methodological norms differs significantly from the account of, say, Lakatos. Now the point is that the arguments for or against a certain account of methodological norms are themselves not 'empirically testable'. Instead, they require a more theoretical discussion of the philosophical presuppositions, assumptions and implications in question. And indeed, this is precisely what Laudan provides in his critique of Lakatos's notion of rationality and of his identification of rationality with sound methodology (Laudan, 1987, pp. 20–23). In fact, the metamethodological practice of assessing rival methodologies involves much more than just testing on the basis of historical data.[5] Consequently, the claim that historical case studies constitute 'the only source that can settle the issues before us' (Donovan *et al.*, 1992, p. xvi) proves to be implausible.

The same conclusion can be reached by a different route. It is to be expected (and it turns out to be the case in the test studies that have been carried out

[4]Yet I do not think that Laudan has really refuted the naturalistic fallacy. For one thing, on his own account (Laudan, 1987, pp. 30–31, note 19) the argument works only if we add the normative premise that one should not endorse 'transcendental aims', since these would be inappropriate for the progress of science. However, for someone who is opposed to 'scientific progress' it will be 'rational' to reject this normative premise.

[5]Note that it is not necessary to take sides in this dispute between Lakatos and Laudan in order to obtain this conclusion. For my own views on the normativity of philosophy of science, and on normative naturalism in particular, see Radder (1996, pp. 175–183).

within the programme) that general claims about scientific development will be confronted with ambivalences, anomalies, counterexamples, and so on. The question then arises of how significant these problems are taken to be. Since no answer to this question can be based on the historical 'data' alone, it follows that, unavoidably, other considerations do and should play a role in establishing the plausibility of philosophical claims concerning scientific change.

The conclusion of this section must be that the Kuhnian paradigm of a philosophy of science 'based on' the history of science is far less straightforward and unproblematic than it might seem at first sight. The most 'rigorous' articulation of this paradigm, the testing-theories-of-scientific-change programme, proves to be over-ambitious and fraught with problems. Thus, the pessimistic assessment by Rachel Laudan, quoted in my Introduction, appears to be justified in as far as it applies to this particular programme. Therefore, if a historical approach to the philosophy of science is possible at all, the relationship between philosophy and history will have to be conceptualized differently. In order to further investigate this question, I will discuss three other approaches and assess their accounts of the relationship between philosophy and history of science.

4. The Historical and Philosophical Study of Styles of Reasoning

In some of his recent work, Ian Hacking has proposed and started to develop a new conception of the relationship between history and philosophy of science. The two fields come together through the notion of 'styles of reasoning', but subsequently each field puts this notion to its own uses. Thus, there is both cooperation and differentiation.

According to this conception, the historian's main task is to devise a 'comparative historical anthropology'. The aim is a grand view of the historical development of the sciences, that also takes due account of their diverse cognitive and sociocultural contexts. Building on the work of the historian A. C. Crombie (see Crombie, 1994) and on a number of his own previous studies, Hacking identifies and starts to articulate six major styles of reasoning. These styles are long-term and large-scale historical entities that emerge and stabilize, but may also decline and disappear, in the course of time. Thus, the 'statistical style' started somewhere in the middle of the 17th Century with the 'emergence of probability', and is articulated in Hacking (1992b) into different stages up to the era of 'modelling and fitting' in the earlier decades of the present century. Other examples are the style of 'mathematical proof', starting with the Ancient Greeks, and the 'laboratory style', which arose during the Scientific Revolution. These examples show that styles of reasoning do not coincide with separate disciplines, but are larger entities that are employed in several disciplines at once.

Now the philosophers require the historians' work but they exploit it in the context of their own questions and purposes. In their hands 'style' is, or becomes, a metaphysical concept. It cannot be strictly defined but it can be exemplified and employed to explain historical possibilities and impossibilities. In this sense it is comparable to other historiographic meta-concepts such as 'paradigm', 'conceptual scheme' or 'discursive formation'.

A further analysis of the nature and function of styles of reasoning reveals a number of philosophically relevant characteristics. Every style introduces, in a sense, a new 'world' in the form of new types of possibilities, objects, sentences, laws, criteria, and so on. Hacking claims, for instance, that the sentence 'the population of New York City in 1820 was 123706' has no truth value independent of certain developments within the statistical style of reasoning, including its social institutionalizations. Although the specific truth value of a particular sentence is not yet fixed by the adoption of a style, styles are the 'standards of objectivity' in the Kantian sense of being the necessary conditions for theoretical sentences to have truth values at all. In this way, Hacking employs the notion of styles of reasoning for answering the major philosophical question of how objectivity is possible. More generally, he suggests a verificationist approach in which the meaning of a theoretical sentence, and in particular its being a candidate for truth or falsity, depends on the 'method of verification' through a style of reasoning.

Another significant feature of styles of reasoning is that, once present, they transcend the microsocial contexts in which they originated and become autonomous. According to Hacking, this autonomy is reinforced by what he calls the self-authenticating and self-stabilizing character of a style of reasoning. For instance, within the statistical style many criteria for the acceptance or rejection of statistical hypotheses are themselves couched in terms of probability (Hacking, 1992b, pp. 50–152).

One of the virtues of Hacking's approach is that it offers a comprehensive, heuristic framework for doing historical and philosophical research. One may, for instance, look for other styles in addition to the six proposed by Crombie and Hacking. Thus, Kellert (1993) has taken up their ideas in this vein and has come to the tentative conclusion that 'chaos theory' might well be a good candidate for a new, emerging style of reasoning. Furthermore, Hacking has so far offered only a (brief) account of the idea of self-authentication for the case of the statistical style. Hence a further possibility, which entails a great deal of interesting research, is to examine in more detail whether, and if so how, this idea works in the case of all styles of reasoning.

Another virtue of this approach is that it avoids a too narrow interpretation of philosophy of science by explicitly acknowledging that, generally speaking, history and philosophy focus on different questions and pursue different aims. In the Kuhnian conception and in the testing-theories-of-scientific-change programme, the historical studies are primarily used for the purpose of deriving or testing a generalized model of the historical development of the sciences. Hacking's approach deviates from these views in several respects. First, the philosophical notion of style is a historiographic meta-concept which should also be discussed and assessed in terms of its philosophical adequacy. Thus, the notion of the autonomy of styles may be questioned (as I will do below) in a philosophical debate that is relatively independent from particular historical considerations. Second, the philosophical theory of style is not being rigorously tested against the historical record. Instead, Hacking uses a weaker terminology: as a philosophical 'tool' it should be 'exemplified' and 'vividly illustrated', and it should provide 'a coherent and enlightening ordering of the record'. Third, Hacking claims that whether a particular ordering is enlightening is not just a matter of the past but also of the present.

> I as philosopher am decidedly Whiggish. The history that I want is the history of the present. That's Michel Foucault's phrase, implying that we recognize and distinguish historical objects in order to illumine our own predicaments (Hacking, 1992a, p. 5).

One important characteristic of such a history of the present is that it does not strive for historical completeness. As such, it clearly contrasts with the testing-theories-of-scientific-change programme, which does aim at a theory of science that covers the historical record most fully.

This then, is the core of Hacking's ambitious but attractive approach to the history and philosophy of science. Obviously, a comprehensive analysis and assessment of its historical and philosophical merits is well beyond the scope of the present paper. Instead, I will discuss some closely related issues that are most directly relevant to my theme, since they bear on the relation—or perhaps the tension—between philosophical and historical aspects of the notion of styles of reasoning.

Consider first the self-authenticating and self-stabilizing character of styles of reasoning. Styles are conceptualized by Hacking as self-sufficient entities that live a life of their own. In this respect they are reminiscent of paradigms and thus are confronted with comparable problems.[6] One of these is mentioned by Hacking himself (Hacking, 1992a, pp. 16–17). It is framed in the question of why a style, if it is really self-authenticating and self-stabilizing, should ever die out. More generally, Hacking admits that his approach is unable to explain philosophically substantial changes in styles of reasoning. Not only the disappearance, but also the emergence, the interaction, the transformation, the fusion and the splitting up of styles have to be accepted as contingent facts of history that elude further philosophical explanation. In this way, however, the sensible emphasis on historical continuity and stability has been made at the

[6]Cf. my critique of the notion of a monolithic, self-sufficient paradigm (Radder, 1988, especially Ch. 4); see also Galison's objections to accounts that divide science into separate 'island empires' (Galison, 1995).

expense of the intelligibility of change and fluidity. To me the old issue of how to mediate between change and continuity, between being and becoming, constitutes a deep and significant philosophical problem and challenge rather than an 'historical chestnut' as Hacking (1992a, p. 16) calls it.

The claimed autonomy of styles of reasoning also requires further debate. Hacking writes:

> Styles are completely impersonal, anonymous, just like Foucault's discursive formations. They became, like a language, there to be used, canons of objectivity. They were indeed formed and fixed in social traffic. We can find spokesmen for a style, a Hobbes or a Boyle, say, but we shall not find an author. We shall find authorities, but oddly enough, once the style is fixed, the experts get their authority from the style (Hacking, 1992b, p. 139)

I have no problem with this if it means that styles by far transcend the sociocultural contexts in which they once originated. In this sense, styles of reasoning are just one example of 'non-local patterns' in the development of science (see Radder, 1992, pp. 150–155). However, to reify such patterns into autonomous forces is to go one questionable step further. Its main drawback is that it will tend to underexpose the work that is permanently being done to produce and maintain the 'reproduction conditions' of the style. It is true that Hacking sees detailed accounts of how styles stabilize as an integral part of his programme. He also stresses the importance of material and institutional requirements for stability. My point is, however, that to grant the significance of material and social stabilization techniques is at the same time to question the autonomy of the styles themselves.

This point is the more important because no style will be fully uncontroversial. For instance, as a consequence of their views on the nature of human beings and, thus, on what constitutes appropriate medical treatment, some proponents of alternative medicine disagree with the usual statistical approaches. Instead, they favour long-term prospective methods of testing on individual patients. Hermeneutic critiques of the laboratory style in psychology and corresponding defences of interpretative approaches provide another illustration. Therefore, attributing autonomy to styles of reasoning might make us lose sight of the power relationships involved in the dominance of certain approaches and the corresponding marginality of alternatives.

The latter point may be elucidated by looking at the somewhat unbalanced use Hacking has made of Michel Foucault's work. As is apparent from the above quotation, the notion of styles of reasoning is congenial to Foucault's idea of autonomous discursive formations, which he advocated in his 'archaeological approach' to the historical development of knowledge. However, in the course of the 1970s Foucault came to criticize this 'illusion of autonomous discourse' for two reasons. First, he realized that without explicit reference to social practices and institutions, the causal power attributed to autonomous discursive formations was bound to remain mysterious. In the second place, he felt that the archaeological approach, by its very nature, hampered his aspiration to connect his views on the history of the (human) sciences to his concerns about present-day society. Therefore, Foucault (e.g. Foucault, 1977) turned to a view in which knowledge is intrinsically connected to social practices, social power and social institutions (cf. Rouse, 1987). This view involved a transformed conception of historiography. Foucault now aimed to write not an archaeology of knowledge but a history of the present. Such a historiography starts from an explicit and self-reflexive, critical diagnosis of the current situation and then focuses on the question of how this situation has been brought about.[7]

As we have seen, Hacking claims that, as a philosopher, his goal is also that of writing a history of the present. Consequently, he owes us a more elaborate answer to two important questions concerning this claim. First, what is his critical diagnosis of the current situation and what social concerns is it related to?[8] And, second, how is he, in contrast to Foucault, able to reconcile the notion of autonomous styles of reasoning with a historiography that starts from a social critical analysis of our present world? I take it that a plausible answer to these questions will entail the weakening of the strong notion of autonomy and its associated features of self-authentication and self-stabilization.

5. Historical and Whig Reconstructions of Science

As enduring standards of objectivity, styles of reasoning are one source of scientific stability. In his analyses of the development of scientific knowledge as a continuing process of *reconstruction*, Thomas Nickles (1986, 1988, 1989) points to another stabilizing mechanism.

> Science transforms itself by more or less continuously reworking its previous results and techniques. To miss the dynamical, self-reconstructive nature of scientific work is to miss the extent to which scientific inquiry is a bootstrap affair. I shall call non-reconstructive views of science *single-pass* or *one-pass* models of scientific inquiry (Nickles, 1988, p. 33).

Let me mention two simple examples to illustrate the basic idea. First, published articles are not primarily intended as more or less faithful accounts of already completed discoveries, but rather as a next step in an ongoing process of discovery (cf. also Gutting, 1980). For instance, when scientists explain in their public accounts how certain facts might have been discovered or certain

[7]Hence, writing a contingent history of the present can and should be distinguished from both 'presentism' (conceiving the past in terms of the present) and 'finalism' (seeing the past as no more than the pre-history of, and as leading necessarily to, the present). See Dreyfus and Rabinow (1983, pp. 118–125).

[8]Some clues to an answer might perhaps be found in his account of the notion of the normal as 'one of the most powerful ideological tools of the twentieth century' (Hacking, 1990, p. 169).

claims derived, they are already involved in a reconstruction of the knowledge in question that goes beyond the earlier stages of the discovery process. Second, reconstruction is also abundant in experimental science. For instance, replicating an experimental result through a procedure that is basically different from the original one, constitutes an important stage of experimental reconstruction. In general, successful reconstructions contribute to a greater stability or robustness of scientific results and techniques. A crucial point is that these kinds of reconstruction are a normal part of scientific practice itself. Hence, they are neither retrospective, rhetorical rationalizations of a 'real-time' discovery process, nor should they be confused with the rational reconstructions advocated by (some) philosophers of science.

The views summarized so far imply a critique of all non-reconstructive, or *single-pass*, conceptions of science, in favour of reconstructive, or *multi-pass*, accounts. Two major conclusions, that apply both to historical and to philosophical studies of science, follow from this critique. A *first* consequence is the repudiation of the sharp opposition between 'real-time' scientific practice on the one hand and the 'merely' pedagogic or rhetorical activities of scientists on the other. As we have seen in Section 2, Kuhn makes essential use of this opposition, for instance when he likens textbook reconstructions to mere tourist brochures.[9] In his wake many recent students of science (David Bloor, Steve Woolgar and Andrew Pickering, among others) can be seen to advocate such one-pass accounts, in which a single process of discovery is contrasted with its '*post hoc* rationalizations'. On a multi-pass account, however, published papers and also textbooks are rehabilitated as perfectly legitimate sources for historians and philosophers of science, *provided* that they take due account of the fact that these sources can only illuminate specific stages in the reconstruction of scientific knowledge. Up to now, Nickles's own studies of reconstructions in the practice of science are still somewhat sketchy. Yet his approach entails that a lot of interesting, historical and philosophical research may be done through following, analysing and interpreting the ongoing reconstructions of particular scientific practices, processes and products. For example, the notion of reconstruction immediately raises the question of what is and what is not preserved in scientific developments. Thus, the problem of change and continuity—discussed in relation to Hacking's views in the previous section—comes up here as a natural and significant subject of philosophical study.[10]

A *second* important conclusion from the multi-pass approach is the rejection of the genetic fallacy and its associated essentialism.

> In ignoring the self-transforming character of scientific work, one-pass models can hardly avoid committing the *genetic fallacy*—the mistake of thinking that its

[9] See also the comments on the notion of normal science in Nickles (1989, p. 312, note 33).

[10] In this vein, I have dealt with the issue of continuity and change in the uses of the (generalized) correspondence principle. See Radder (1996, Ch. 3).

conditions of origin determine forever the character or 'essence' of a thing (Nickles, 1988, pp. 35–36).

The reconstructionist view of scientific knowledge implies that earlier claims will be transformed—sometimes almost beyond recognition—in later passes.[11] Consequently, reconstruction may also transform the meaning of the earlier claims. Therefore, to think that this meaning is fixed once and for all during the genesis of the claim is to commit the genetic fallacy. Thus, constructivist studies of science (e.g. Latour and Woolgar, 1979) commit this fallacy, if they take the original negotiations within a particular laboratory as definitely constitutive of the meaning of the resulting facts. The argument also implies that philosophical interpretations of science may have to be different, depending on the stages of reconstruction to which they apply.

The force of the above line of reasoning may be illustrated by a brief comment on the way it is used by David Gooding in his historical philosophy of science. Gooding (1990, pp. 4–9) provides a classification and discussion of six different types or stages of reconstruction: cognitive, demonstrative, methodological, rhetorical, didactic and philosophical. His focus is on the first two types and his book offers a number of important insights into what goes on at these early stages of scientific inquiry. Yet his discussion does not seem to capture the full significance of Nickles's approach. First, it still sticks to the Kuhnian dichotomy by characterizing cognitive and demonstrative reconstructions, in contrast to the other types, as 'real-time'. Does this imply that, for instance, textbooks are written, used, and interpreted outside of 'real-time'? Second, on the basis of his analyses of the first two stages, Gooding puts forward a number of strong criticisms of other, mainly philosophical, interpretations of science. According to his own approach, however, these criticisms are either premature or an instance of the genetic fallacy. After all, the later stages of reconstruction might well endow the relevant scientific practices, processes and products with a transformed meaning, which might (or might not) be adequately captured by the criticized interpretations.

So far, these views can be seen as a plausible development of the simple premise that 'scientists are not historians'. But what about the relationship between philosophers and historians of science? At first sight Nickles's approach appears to fit smoothly within a strictly historical philosophy of science. After all, isn't he just arguing that the historical record shows the significance of processes of reconstruction and therefore that these processes have been unjustly neglected in current models of scientific change? Indeed, in 1986 Nickles advises philosophers to stay as close as possible to science as it is

[11]Thus, I remember quite vividly the difficulties I had, on my first reading, to see any connection at all between Heisenberg's 1925 'Quantum–Theoretical Reinterpretation of Kinematic and Mechanical Relations' and quantum mechanics as I knew it from my textbooks. Beller (1983) details some of the reconstructions Heisenberg's paper went through between 1925 and 1927.

historically practised in order to avoid the danger of Whiggism. Thus, in his discussion of the testing-theories-of-scientific-change programme he writes:

> The presupposition that there are general models of change to be found can itself be empirically investigated, but if it is not open for serious investigation, then it amounts to an attempt to escape from history and invites whiggish accounts of historical developments. The (testing-theories-of-scientific-change) group would achieve compatibility and a convenient division of labor between history and philosophy by drawing history closer to philosophy. My recommendation is just the opposite: draw philosophy closer to history! (Nickles, 1986, p. 257)

In his more recent work, however, he appears to have made a certain turn with respect to the question of the relationship between philosophy and history of science. Now, to a certain extent, he acknowledges the fact that 'philosophers are not historians'. Phrased in his own terminology, the reconstructions of science made by philosophers and by historians are not of the same type. Just like the scientist, the philosopher may legitimately employ methods and pursue goals that differ from those of the historian. In particular, good philosophers will have to say something relevant about how to deal with the central problems of our present-day situation. For instance, philosophers involved in methodological or science policy discussions will necessarily endorse certain future oriented, normative claims. In anthropological terms, they will be engaged in making interested members' accounts rather than disinterested strangers' accounts. For these purposes history may be legitimately used whiggishly, as a *resource* for wedding the past to the future.[12] In this way, 'whiggism helps to solve the major problem we face as we *make* history, as we alter our former ways of life, as we live forward' (in the words of Dewey).[13] This line of argument implies that historicism must be tempered by an appropriate dose of pragmatism. Thus, in his recent work Nickles has qualified and clearly weakened his earlier recommendation of 'drawing philosophy closer to history'.

Generally speaking, I consider Nickles's approach to philosophy and history of science as thoughtful and promising. In particular, his central notion of reconstruction offers rich opportunities for a host of detailed and varied philosophical and historical studies. My only comments regard some of his more specific claims. First, I agree that the phenomenon of reconstruction implies the essential non-locality of scientific facts and technological artefacts, and hence the untenability of strictly localist interpretations. But this does not

[12]Nickles (1992, p. 85) equates 'whiggism' with 'presentism', but he realizes that actual whiggish accounts may take on different forms that require different evaluations; see also Nickles (1995, pp. 151–155).

[13]Nickles (1992, p. 113). Apart from this, he argues that pure antiwhiggism is also problematic for historians of science: 'Strong historicism in the sense of strong antiwhiggism, understood as strong antipresentism—the requirement that historians must efface every trace of their own, present historical position—is incompatible with strong historicism understood as the thesis that everything is historically situated, including the historian—that there can be no neutral, ideal observer' (Nickles, 1995, p. 153).

justify the stronger claim that reconstructed facts can be 'decontextualized' and that standardized artifacts can be made to work 'under nearly any conditions', or 'almost everywhere' (Nickles, 1992, p. 102). Regarding this issue it is important to keep in mind that the production of scientific facts and the working of technological artefacts always require the realization of certain material and social conditions (Latour, 1983, 1987; Radder, 1996, Chs 2 and 6). Hacking's views discussed above are put in different terms but they lead to a similar conclusion: the objectivity of scientific knowledge is never unconditional, since it depends on the contingent existence of styles of reasoning and their stabilizing techniques.

Finally, although I agree with Nickles's criticisms of antiwhiggism, the use of Whiggism as a central or defining notion for the philosophy of science has a number of disadvantages. First, as is clear from Nickles's own analyses, 'Whiggism' refers to a cluster of ontological, epistemological and methodological doctrines. At the least, this cluster should be disentangled if we wish to obtain a reasonably clear conception of philosophy of science. Second, Nickles certainly does not advocate a return to a naive, ahistorical approach to the philosophy of science. Hence, his criticism merely affects 'pure' or 'excessive' antiwhiggism. Again, this may be prudent enough, but it will also tend to hamper a conceptual clarification of the relevant issues. Third, Nickles occasionally (e.g. 1995, p. 153) suggests that Whiggism—at least in a certain form and to a certain extent—is unavoidable in principle. If this is true, however, it appears to be incompatible with his main conclusion 'that we science studies scholars must stop being automatic antiwhigs and ask ourselves when and under what conditions we must or must not be whiggish' (Nickles, 1995, p. 155). For these reasons, the notion of Whiggism as such seems to be less appropriate for the purpose of developing a more comprehensive account of philosophy of science, including its relation to the history of science. Hence, I propose to retain the insights gained from Nickles's discussion of Whiggism, while dropping the notion itself.

6. Philosophy as Interpretative and Explanatory

Elsewhere (Radder, 1994, 1996, especially Ch. 8) I have presented a detailed account of philosophy as a *theoretical*, *normative* and *reflexive* enterprise. When I portray philosophy in this way, I do not mean to offer a rigorous definition in terms of necessary and/or sufficient conditions. The characterization rather bears upon a non-local pattern that can be recognized in all kinds of philosophical approaches, conceptions and debates. Furthermore, I do not primarily refer to philosophy as a distinctly institutionalized discipline but rather as a certain type of scholarly research. Whether or not this research is being practised within philosophical departments or by people formally educated in philosophy is less important.

First, I want to make a few remarks about the normative and reflexive dimensions of philosophy. As we have seen, the recent views of Hacking and Nickles leave some room for normativity. In Hacking's work this is more implicit. Yet his Foucauldian notion of a history of the present—if developed in more detail—may well imply certain normative stands. Nickles more explicitly acknowledges the legitimacy of normative philosophy, especially in methodological and science policy debates. The significance of reflexivity is hinted at—but no more than that—by both Hacking (1992a, p. 20) and Nickles (1992, p. 118). When we look at philosophical practice, broadly defined, we come across quite different forms of normative and reflexive philosophy. Its normativity may be epistemological, social critical, methodological or policy oriented, while its reflexivity may be foundational, skeptical, or differentially situated. Thus, it will be clear that much more could and should be said about these different interpretations of normative and reflexive philosophy (see for this, Radder, 1996, especially Ch. 8). Here, however, I will focus on theoretical philosophy, which is the most relevant to the theme of this paper. Hence I just want to add at this point the observation that adding the dimensions of normativity and reflexivity significantly broadens the perspective of a strictly historical philosophy of science. For example, following up on the discussion in the previous section, the view of philosophy as theoretical, normative and reflexive can be seen to account for a number of different aspects of the notion of Whiggism. First, reflexivity requires taking into account the situatedness of philosophers, philosophical communities or philosophical positions. Second, normative philosophy includes discussing issues and options that have a direct bearing on 'making the future'. And third, as I will explain now in more detail, philosophy as theory cannot avoid being committed to particular interpretative and explanatory preconceptions.

Theoretical philosophy, as I see it, aims at exposing and examining structural features that *explain or make sense of non-local patterns* in the development of science. By non-local patterns I mean patterns that are not (or not necessarily) universal but still possess a broader historical significance. Kuhn's pattern of normal science, crisis, revolution and normal science, or Crombie's and Hacking's styles of reasoning as historical entities, may serve as one kind of illustration. But one may also think of the staggering militarization of science, especially in this century (MacKenzie and Wajcman, 1985; Latour, 1987; Smit, 1994).

On the one hand, it will be clear that tracing plausible non-local patterns requires a considerable familiarity with scientific practices, processes and products. For this purpose philosophers need the work of historians. Although these claims may seem obvious, they do contrast with at least two other approaches. They are incompatible with those views that strongly emphasize the distinction between philosophy and history by situating philosophy entirely within a context of justification. And they also contrast with the testing-theories-of-scientific-change programme. As we have seen in Section 3, the (ultimate) aim of this programme is to cover the historical cases as fully as possible. Accordingly, its empirical claims are not conceived as referring to non-local patterns, but as universal generalizations which can be refuted by counterexamples.

On the other hand, it is not difficult *not* to see certain patterns by looking more closely at the details of the historical cases. However, the argumentative force of such 'deconstructions'—which can be found quite frequently in constructivist interpretations of science (see Radder, 1992, pp. 150–155)—amounts to no more than the truism that any 'seeing as' presupposes a particular viewpoint. Consequently, patterns that are clearly visible from a certain distance may resolve when attention is focused on the separate, local details. But this fact alone does not diminish the significance of non-local patterns, which is that they shape the activities and results in question in a specific way. Thus, what is required is a view that does justice to both the non-local patterns and the various ways in which they have been materially and socially realized in particular local contexts.

A first important characteristic of theoretical philosophy, then, is that it at once aims at and presupposes certain general *interpretations* of science. Any account of science—including the one that claims that science 'as such' does not exist—employs and tries to vindicate a number of rather general assumptions. These assumptions constitute, in a hermeneutical sense, the meaning that is being attached to science. Thus, Willard Quine (1985) can be seen to both presuppose and articulate a thoroughly physicalistic ontology, rooted in a comprehensive, scientistic world view. At a rather different position within the spectrum of philosophical interpretations, Steve Woolgar (1988) endorses a social voluntaristic perspective and argues for a constitutive reflexivity in science and technology studies. And above, we have seen that Ian Hacking offers a metaphysics of styles of reasoning, in which their stability is interpreted as a criterion of the objectivity of knowledge claims.

General philosophical interpretations cannot be inferred from historical case studies. Again, this may seem evident. Yet, given certain influential (meta)empiricist tendencies in recent science studies, it is worth stating it explicitly. Thus, Michel Callon and Bruno Latour claim that 'in Paris and Bath we all agree that the touchstone of any position is its empirical fruitfulness,' and that in debates within science studies, the empirical (dis)agreement is 'the only one that really matters' (Callon and Latour, 1992, pp. 345 and 352). In fact, however, the plausibility of philosophical interpretations certainly does not depend exclusively on empirical evidence about the historical development of scientific theories or the historical practice of working scientists. Consider, for example, the question of ontology. First, different scientific theories may and often do entail different ontologies. For instance, if we start from classical mechanics, we may opt for a physicalistic ontology. But if we are committed to quantum mechanics as the fundamental theory, we will need to develop a rather different ontology. Second, sticking to our favourite scientist does not help either, since different favourites may and often do endorse different ontologies (De Regt, 1993). Einstein's and Bohr's different interpretations of classical and quantum mechanics strikingly illustrate the point. I do not mean to say that we cannot learn a lot, including about ontology, from studying scientific theories and scientific practice. The point is, however, that adopting and vindicating a particular ontological interpretation will require additional philosophical argumentations. This applies just as well to the monistic actor-network ontology advocated by Callon and Latour. In philosophical practice, such argumentations are usually discussed and evaluated on the basis of a variety of (explicit or implicit) notions, such as logical coherence, conceptual clarity, heuristic power, empirical fruitfulness, normative stake, or sociocultural significance.

A second main task of theoretical philosophy is to search for *explanations*. As is usual in theoretical approaches generally, theoretical philosophical explanation does not primarily aim at presenting knowledge of separate events or historical episodes but rather at developing more general insights into the practices, processes and products of science. Because no explanation will ever capture each and every aspect of science, explanatory philosophy has to face two important questions. A first question that arises is what *is* the *explanandum* and, in particular, what role does it play and what relevance does it have compared to other aspects of science? Thus, in this view it is perfectly legitimate to aim at an explanation of the products (in contrast to, for example, the practice) of science, provided one takes explicit account of the role and relevance of these products. It is in this sense that I have supported Nickles's plea for the significance of reconstructed knowledge.

Furthermore, there is the question of precisely how the theoretical *explanans* is related to the *explanandum*. After all, only then will it be possible to judge what understanding has been accomplished by the more general insights and what by the more specific articulations that are required to make these insights apply to the various *explanandum* contexts (cf. Cartwright, 1983). One way to answer this question is to point out the conditions under which the theoretically postulated structural features are manifest in practice. Consider, for example, the notion of the description of the material realization of experiments on the basis of processes of communication and division of labour between theoretically informed experimenters and theoretically non-informed laypersons (Radder, 1988, Ch. 3; Radder, 1996, Ch. 2). This specific operationalization of the process of material realization is not meant to be a straightforward description of empirically manifest patterns in experimental practice but rather it is intended to characterize certain structural features of experimental action and production, including their relationship to theoretical interpretation. Yet, although the account is not meant as being descriptive of all scientific practice, this does not imply that it cannot be empirically supported. In fact, the account itself includes the conditions under which it can be practically substantiated, namely in cases of division of manipulative and theoretical labour.

Thus, this second aspect of theoretical philosophy of science emphasizes the significance of explanatory power. Although I certainly do not agree with all of Max Weber's views, there seems to be a similarity to some of his methodological ideas (see Weber, 1949, pp. 85–112). In his case also, the starting point is the question of 'the significance of *theory* and theoretical conceptualization for our knowledge of cultural reality' (Weber, 1949, p. 85). In answering this question Weber introduces the notion of 'ideal types', which enable the explanation of culturally significant, historical patterns. A well known example is his analysis of the protestant ethic as a necessary condition of the emergence of early capitalism in the 16th and 17th Centuries. Here, the 'protestant ethic' and 'early capitalism' are to be understood as ideal types. Such ideal types are not meant to cover all actual historical episodes, since in practice they may be realized to a greater or lesser extent. According to Weber, the initial choice of a particular ideal type is value laden, since what is taken to be 'culturally significant' depends on the value orientation of the researcher or research community. Yet ideal types are not normative ideals. Instead, they are ideal in the sense of being concepts that enable a clear and illuminating structuring of historical practice. Therefore, the plausibility of ideal typical analyses does not primarily derive from their descriptive adequacy but from their heuristic and explanatory power. My suggestion, then, is that such an explanatory approach may also be fruitfully applied in a theoretical philosophy of science.

Let me conclude. The metaphilosophical discussions in this paper explicitly acknowledge the relative autonomy of philosophy from history. Thus, they clearly go beyond the Kuhnian paradigm of a historical philosophy of science. Instead, three alternative approaches to the relationship between philosophy and history of science have been sketched: Hacking's account of styles of reasoning as a history of the present, Nickles's reconstructionism with a view to making the future, and the idea of philosophy as an explanatory and interpretative theory of the practices, processes and products of science. I have also pointed out, however, that these alternatives remain committed to the history of science as an important resource. Hence, a return to the ahistorical approach of the logical positivists and their followers is as undesirable as the narrow historicism of the Kuhnian type.

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References

Beller, M. (1983) 'Matrix Theory before Schrödinger', *Isis* **74,** 492–508.

Callon, M. and Latour, B. (1992) 'Don't Throw the Baby Out with the Bath School! A Reply to Collins and Yearley', in A. Pickering (ed), *Science as Practice and Culture* (Chicago: University of Chicago Press), pp. 343–368.

Cartwright, N. (1983) *How the Laws of Physics Lie* (Oxford: Oxford University Press).

Collins, H. M. (1975) 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology* **9,** 205–224.

Constant, E. W. (1984) 'Communities and Hierarchies: Structure in the Practice of Science and Technology', in R. Laudan (ed.), *The Nature of Technological Knowledge* (Dordrecht: Reidel), pp. 27–46.

Crombie, A. C. (1994) *Styles of Scientific Thinking in the European Tradition*, 3 volumes (London: Duckworth).

De Regt, H. W. (1993) *Philosophy and the Art of Scientific Discovery* (Amsterdam: doctoral dissertation, Vrije Universiteit).

Donovan, A., Laudan, L. and Laudan, R. (1992) 'Introduction to the Johns Hopkins Edition', in A. Donovan, L. Laudan and R. Laudan (eds), *Scrutinizing Science* (Baltimore: Johns Hopkins University Press), pp. xi–xxiv.

Dosi, G. (1982) 'Technological Paradigms and Technological Trajectories', *Research Policy* **11,** 147–162.

Dreyfus, H. L. and Rabinow, P. (1983) *Michel Foucault: Beyond Structuralism and Hermeneutics*, 2nd edition (Chicago: University of Chicago Press).

Foucault, M. (1977) *Discipline and Punish* (Harmondsworth: Penguin Books).

Fuller, S. (1992) 'Being There with Thomas Kuhn: A Parable for Postmodern Times', *History and Theory* **31,** 241–275.

Galison, P. L. (1987) *How Experiments End* (Chicago: University of Chicago Press).

Galison, P. L. (1995) 'Context and Constraints', in J. Z. Buchwald (ed.), *Scientific Practice: Theories and Stories of Doing Physics* (Chicago: University of Chicago Press), pp. 13–41.

Giere, R. N. (1988) *Explaining Science* (Chicago: University of Chicago Press).

Gooding, D. (1990) *Experiment and the Making of Meaning* (Dordrecht: Kluwer).

Gutting, G. (1980) 'Science as Discovery', *Revue Internationale de Philosophie* **131–132,** 26–48.

Hacking, I. (1989) 'Philosophers of Experiment', in A. Fine and J. Leplin (eds), *PSA 1988*, Vol. 2 (East Lansing, MI: Philosophy of Science Association), pp. 147–156.

Hacking, I. (1990) *The Taming of Chance* (Cambridge: Cambridge University Press).

Hacking, I. (1992) ' "Style" for Historians and Philosophers', *Studies in History and Philosophy of Science* **23,** 1–20.

Hacking, I. (1992b) 'Statistical Language, Statistical Truth and Statistical Reason: The Self-Authentication of a Style of Scientific Reasoning', in E. McMullin (ed.), *The Social Dimensions of Science* (Notre Dame, New Brunswick: University of Notre Dame Press), pp. 130–157.

Hoyningen-Huene, P. (1992) 'The Interrelations between the Philosophy, History and Sociology of Science in Thomas Kuhn's Theory of Scientific Development', *British Journal for the Philosophy of Science* **43,** 487–501.

Kellert, S. H. (1993) 'A Philosophical Evaluation of the Chaos Theory "Revolution" ', in D. Hull, M. Forbes and K. Okruhlik (eds), *PSA 1992*, Vol. 2 (East Lansing, MI: Philosophy of Science Association), pp. 33–49.

Kragh, H. (1987) *An Introduction to the Historiography of Science* (Cambridge: Cambridge University Press).

Kuhn, T. S. (1970) *The Structure of Scientific Revolutions*, 2nd, enlarged, edition (Chicago: University of Chicago Press).

Kuhn, T. S. (1990) 'The Nature of Scientific Knowledge: An Interview with Thomas Kuhn by Skuli Sigurdsson', *Harvard Science Review* Winter, 18–25.

Kuhn, T. S. (1992) *The Trouble with the Historical Philosophy of Science* (Cambridge, MA: Rothschild Lecture, Department of History of Science, Harvard University).

Lakatos, I. and Musgrave, A. (eds) (1970) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press).

Latour, B. (1983) 'Give Me a Laboratory and I will Raise the World', in K. D. Knorr-Cetina and M. Mulkay (eds), *Science Observed* (London: Sage), pp. 141–170.

Latour, B. (1987) *Science in Action* (Milton Keynes: Open University Press).

Latour, B. and Woolgar, S. (1979) *Laboratory Life* (London: Sage).

Laudan, L. (1987) 'Progress or Rationality? The Prospects for a Normative Naturalism', *American Philosophical Quarterly* **24,** 19–31.

Laudan, L. *et al.* (1986) 'Scientific Change: Philosophical Models and Historical Research', *Synthese* **69,** 141–223.

Laudan, R. (1993) 'The "New" History of Science: Implications for Philosophy of Science', in D. Hull, M. Forbes and K. Okruhlik (eds), *PSA 1992*, Vol. 2 (East Lansing, MI: Philosophy of Science Association), pp. 476–481.

Laudan, R., Laudan, L. and Donovan, A. (1988) 'Testing Theories of Scientific Change', in A. Donovan, L. Laudan and R. Laudan (eds), *Scrutinizing Science* (Dordrecht: Kluwer), pp. 3–44.

MacKenzie, D. and Wajcman, J. (eds) (1985) *The Social Shaping of Technology* (Milton Keynes: Open University Press).

Mulkay, M. (1977) 'The Scientific Research Community', in I. Spiegel-Rösing and D. de Solla Price (eds), *Science, Technology and Society* (London: Sage), pp. 93–148.

Nickles, T. (1986) 'Remarks on the Use of History as Evidence', *Synthese* **69,** 253–266.

Nickles, T. (1988) 'Reconstructing Science: Discovery and Experiment', in D. Batens and J. P. van Bendegem (eds), *Theory and Experiment* (Dordrecht: Reidel), pp. 33–53.

Nickles, T. (1989) 'Justification and Experiment', in D. Gooding, T. J. Pinch and S. Schaffer (eds), *The Uses of Experiment* (Cambridge: Cambridge University Press), pp. 299–333.

Nickles, T. (1992) 'Good Science as Bad History: From Order of Knowing to Order of Being', in E. McMullin (ed.), *The Social Dimensions of Science* (Notre Dame, New Brunswick: University of Notre Dame Press), pp. 85–129.

Nickles, T. (1995) 'Philosophy of Science and History of Science', *Osiris* **10,** 139–163.

Quine, W. V. O. (1985) 'Epistemology Naturalized', in H. Kornblith (ed.), *Naturalizing Epistemology* (Cambridge, MA: MIT Press), pp. 15–29.

Radder, H. (1988) *The Material Realization of Science* (Assen: Van Gorcum).

Radder, H. (1992) 'Normative Reflexions on Constructivist Approaches to Science and Technology', *Social Studies of Science* **22,** 141–173.

Radder, H. (1994) 'Wetenschapsfilosofie en wetenschapsonderzoek: op weg naar een vruchtbare lat-relatie?', *Kennis en Methode* **18,** 157–168.

Radder, H. (1996) *In and about the World: Philosophical Studies of Science and Technology* (New York: State University of New York Press).

Richardson, A. W. (1992) 'Philosophy of Science and Its Rational Reconstructions: Remarks on the VPI Program for Testing Philosophies of Science', in D. Hull, M. Forbes and K. Okruhlik (eds), *PSA 1992*, Vol. 1 (East Lansing, MI: Philosophy of Science Association), pp. 36–46.

Rouse, J. (1987) *Knowledge and Power* (Ithaca: Cornell University Press).

Smit, W. A. (1994) 'Science and Technology, and the Military: Relations in Transition', in S. Jasanoff, G. E. Markle, J. C. Petersen and T. Pinch (eds), *Handbook of Science and Technology Studies* (London: Sage), pp. 598–626.

Weber, M. (1949) ' "Objectivity" in Social Science and Social Policy', in E. A. Shils and H. A. Finch (eds), *The Methodology of the Social Sciences* (New York: Free Press), pp. 49–112.

Woolgar, S. (1988) *Science: The Very Idea* (Chichester and London: Ellis Horwood/ Tavistock).