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HISTORY AND PHILOSOPHY OF SCIENCE: INTIMATE RELATIONSHIP OR MARRIAGE OF CONVENIENCE?

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I INTRODUCTION

Several years ago members of the United States National Committee for the International Union of History and Philosophy of Science became officially concerned with the rationale for the union of History of Science and Philosophy of Science. This concern was, not surprisingly, channelled into the organisation of a conference (held at the University of Minnesota in the fall of 1969) and into the publication of papers.¹ The original idea was to have pairs of historians and philosophers address limited and well-defined topics in science. These studies were to pave the way for higher level discussions of possible relations between historical and philosophical approaches to an understanding of the scientific enterprise. In actuality, only remnants of the original plan survived. What we have instead is a collection of thirteen papers, related to be sure, but hardly focused on a few well-defined topics. There are five papers (Feigl, Feyerabend, Hesse, McMullin, Thackray) dealing explicitly with the nature of history or philosophy of science and their mutual relations. Four of the papers (Hiebert, Rosen, Stein, Stuewer) are nearly pure history of science. Similarly, one paper (Salmon) is a purely philosophical study. Finally there are three papers (Achinstein, Buchdahl, Schaffner) which argue methodological theses using historical case studies for illustration and support. Six of the papers (Achinstein, Buchdahl, Hesse, Schaffner, Stein, Thackray) are followed by comments and replies, some of which are quite substantial.²

I will begin by commenting on the historical and philosophical contributions and then take up the papers focusing on history or philosophy of science and their mutual relationship. After summing up my reactions to the volume as a whole I

¹ Review of R. H. Stuewer (ed.) [1970]: *Historical and Philosophical Perspectives of Science, Minnesota Studies in the Philosophy of Science*, 5. Minneapolis: University of Minnesota Press, \$11.50. Pp. 384.

² One might question whether these papers exhibit sufficient unity of content or approach to justify inclusion in a single volume. But as this volume has greater unity than some other recently published collections in the philosophy of science, it seems unfair to pursue this question here. In the end, it seems such matters must be left to editors, referees and publishers.

will sketch an alternative analysis of the philosophy of science and its relations with history of science. My comments throughout will reflect an interest in this latter issue. Having spent my professional life thus far in a department of history and philosophy of science, I have often had occasion to wonder if the union is not primarily a marriage of convenience. It may be better than living with one's parents, history and philosophy respectively, or with one's rich relatives, the sciences. But does it have the passionate involvement and deep communication that one was led to expect? The overall situation is, I think, less reassuring than it might seem to many readers of this volume.

2 NOTES ON HISTORICAL AND PHILOSOPHICAL CONTRIBUTIONS

Turning first to primarily historical papers, Erwin Hiebert's 'Mach's Philosophical Use of the History of Science' attempts to trace the relationships among Mach's works as a physicist, historian of science, and philosopher of science. Hiebert argues that Mach was primarily a physicist whose interest in the history of science was at first pedagogical and only gradually came to be viewed as essential for an adequate understanding of scientific concepts. Mach's methodological reflections, Hiebert claims, 'were the out-growth of the scientific research in which he was engaged most of the time' (p. 192), and were thought by Mach to be justified by history (p. 197). Clearly Mach's thought provides a case study of the relation between history and philosophy of science. It is not clear from Hiebert's paper, however, that Mach's philosophical views, e.g. on the epistemological role of sensations or the economy of thought, have any 'logical' or 'conceptual' relations to either his physical or historical investigations, and if so, what precisely these relations might be.

Edward Rosen's 'Was Copernicus a Hermeticist?' follows the paper by Mary Hesse in which the influence of the Hermetic tradition figures as the primary example in a historiographic debate. Rosen directs a battery of quotations at Francis Yates and concludes among other things, that 'the hermetic association amounts to about 0.00002 of [Copernicus's] *Revolutions*' (p. 169). Whether he carries the day in this particular dispute I leave for others to judge.

Howard Stein's 'On the Notion of Field in Newton, Maxwell and Beyond', is part of a projected longer paper on the same topic. The section on Newton is substantial; the section on Maxwell and beyond is rather more sketchy. Stein's thesis regarding Newton is that the concept of a field, though of course not by this name, is essential to Newton's work at three levels: heuristic, theoretical, and metaphysical. Stein's commentators, Mary Hesse and Gerd Buchdahl, raise several substantial and detailed objections which elicit some sharp replies. I am not competent to judge the issues concerning Newton. It is interesting to note, however, that Stein's paper provides an example of that mode of approaching historical questions which by and large presupposes current philosophical categories. Thus Stein brings to his reading of Newton the distinctions among 'heuristics', 'fundamental theory' and 'metaphysics'. This is not to say that the philosophical categories are rigidly held or rigidly applied, but they clearly are tools in the historical investigation and not products of that investigation.¹

¹ Yet Stein's introduction contains the provocative parentheses: 'Data for the philosophy of science can come only from the history of science' (p. 265). Presumably the categories he uses here are based on other historical cases.

Roger Stuewer's 'Non-Einsteinian Interpretations of the Photoelectric Effect' is a straightforward attempt to explain why it took from 1905 until the publication of Compton's work in 1923 for Einstein's light quantum hypothesis to gain general acceptance by the physical community. The answer has several parts. First, there was naturally great reluctance, after Hertz, to believe that Maxwell's theory was inadequate. Second, the experimental evidence was inconclusive regarding Einstein's prediction of a linear relation between the frequency of incident radiation and the maximum photoelectron energy. Finally, there were a number of 'classical' alternatives that also accounted for what data there was. Stuewer discusses theories developed by Lorentz, Thompson, Sommerfeld, and Richardson. The first three were abandoned in 1914 with the acceptance of the Bohr model of the atom. Richardson's wholly macroscopic account survived until Compton. If there are any global historical or philosophical theses to be drawn from this episode in modern physics, Stuewer does not draw them. But it is a fascinating story nonetheless.

Turning to the primarily philosophical papers, in 'Inference to Scientific Laws', Peter Achinstein suggests two forms of inference to scientific laws, explanatory and inductive. He also gives a pragmatic analysis of the distinction between the contexts of discovery and justification. If a scientist first becomes acquainted with a hypothesis while reasoning to it, that reasoning, for him, is in a context of discovery. Similarly for justification. It turns out that both explanatory and inductive inference may occur in either the context of discovery, or justification, or neither! Achinstein goes on to apply these distinctions to the classic cases of Gay Lussac and Avogadro. The former is said to have reasoned inductively in both contexts; the latter explanatorily in the context of justification only. Now Achinstein claims that 'doing these things should provide a better understanding of the origin of the law' (p. 104). He also thinks that the formulation of philosophical distinctions and their application to historical cases are mutually interacting processes. In this essay, however, the 'interaction' is all one way. Moreover, Achinstein does not indicate the nature of the support which historical cases might lend to philosophical distinctions.

That Achinstein's analysis of the contexts of discovery and justification is not the usual one is clear from Wesley Salmon's 'Bayes's Theorem and the History of Science'. For Salmon, a fact is in the context of justification or discovery relative to a given hypothesis according as that fact is evidentially relevant to the hypothesis or only psychologically relevant. Now evidential relevance is determined by an inductive logic. Salmon argues that the inductive logic for theories is supplied by Bayes's Theorem—and laments the fact that most historians seem to hold some version of the inadequate *H-D* view. He clearly thinks it important for historians that they recognize the inadequacies of the *H-D* view. Finally, Salmon provides the history of science with an unexpected role. Application of Bayes's Theorem requires prior probabilities for theories. If these are to be based on experience, Salmon argues, they must come from our past experience with theories of various kinds, and this experience is recorded by historians. Thus the history of science provides a necessary ingredient in the logical evaluation (justification!) of current scientific theories. It should come as no surprise that most of this would be disputed by other inductive logicians. The fundamental difficulty, I think, is this: For Salmon, the most historians can provide for prior probabilities is a rough estimate of the relative frequency of *successful* theories of

a certain type. The posterior probability he wants from Bayes's Theorem, however, is the relative frequency of *true* theories of that type. But past success is not the same as truth. Thus unless Salmon is willing to settle for a posterior probability of *success*, and even this needs clarification, he must find some other account of the prior probabilities of theories.

Kenneth Schaffner's contribution is a *tour de force* entitled 'Outlines of a Logic of Comparative Theory Evaluation with Special Attention to Pre- and Post-Relativistic Electrodynamics'. The aim of this paper, which runs forty pages, is to reconcile 'historical' accounts, in which rational comparison of incommensurable theories is impossible, and 'logical' accounts, in which experimental data provide sufficient assessments either through falsification or differential confirmation. Schaffner first sketches modifications of standard analyses of the meaning of theoretical terms, correspondence rules and relations between theory and observation. He then introduces three criteria of evaluation adopted from the introduction to Hertz's *Principles of Mechanics*: (A) Theoretical context sufficiency, *i.e.* 'concordance between a theory to be assessed and the corpus of accepted scientific knowledge' (p. 321); (B) Experimental adequacy; (C) Simplicity. While admitting that the three criteria do not 'constitute a type of easily applicable schema', Schaffner insists that they 'do accurately characterize the process of comparative theory evaluation as it is practiced by scientists making the history of science' (p. 330). The second half of the paper is an attempt to support the latter claim by comparing Lorentz' electrodynamics with Einstein's theory of relativity in 1905. After sketching each of the theories, he considers them in the light of each criterion and comes up with a 'split decision': 'theoretical context sufficiency supports Lorentz's theory, relative simplicity supports Einstein's, and experimental adequacy . . . selects neither theory . . .' (p. 347).

In his exceptionally clear comments, with which I largely concur, Arnold Koslow objects (i) that Schaffner's criteria pre-suppose and do not resolve the 'paradoxes' of the historical school, (ii) that Schaffner is wrong about the ontological status of the aether, and (iii) that the conclusion of a 'split decision' unjustifiably assumes weighing the criteria equally. Schaffner, of course, disagrees. The main difficulty, for me, is understanding the status of the three criteria. Are they primarily *descriptive* of criteria scientists in fact appeal to in comparing theories, or do they carry *normative* force. In the former case one would expect much effort to show that most scientists do use them. In the latter case one would expect some attempt to say why these criteria are appropriate, *e.g.* because they further the aims of theoretical inquiry. In fact we are offered only some of the former and none of the latter. The trouble is that without some claim of normative force, Schaffner's views go little beyond those of the historical school—and the claim to a 'logic' of comparative theory evaluation is spurious. No one denies that scientists do in fact use various criteria to compare theories. The question is whether these are *rational* evaluations or merely means employed to persuade others to change their allegiances. In short, it is unclear whether Schaffner is doing logic or sociology, and until this is clear it will seem that he has reconciled the logical and historical schools only by ignoring their real philosophical differences.

Gerd Buchdahl's 'History of Science and Criteria of Choice' is clearly aiming at results similar to Schaffner's paper. Like Schaffner, Buchdahl employs a triad of criteria for choosing one theory over another, but here the criteria are organised

in a hierarchical structure. At the top, so to speak, is the archetonic component consisting of regulative ideals and preferred explanation types. Next is the level of explication which requires that theoretical concepts be intelligible relative to other relevant concepts. Third is the constitutive component of choice which includes the explicit systematic formulation of a theory as well as its confirmation by facts. Buchdahl defends his methodological structure by arguing that it provides a way of understanding Newton's perplexities about the nature and cause of gravity. Action at a distance seems demanded at the constitutive level by the empirical success of the theory, but it is hard to reconcile with pre-existing concepts of space and matter. Newton resolves this tension, Buchdahl argues, by appealing to the archetonic idea of final causes. Laudan disagrees with some details of this analysis, but is in full agreement with the general enterprise.

Unlike Schaffner, Buchdahl explicitly defends his criteria as being principles of rational choice and thus explicitly rejects the traditional expiricist position that the fundamental basis for choice of theories is 'agreement with data'. I will take up this central issue below, after a brief survey of those papers which focus directly on the nature of history and philosophy of science and their mutual relations.

3 CONTRIBUTIONS ON HISTORY AND PHILOSOPHY OF SCIENCE AND THEIR MUTUAL RELATIONS

Herbert Feigl's introductory remarks at the Minnesota conference, 'Beyond Peaceful Coexistence', serve as the opening essay of the volume. Here Feigl confesses and repents of the worst sins of recent empiricist philosophers against the history of science; namely, citing examples, *e.g.* of Newton or Einstein, without any regard for the actual facts of the case and, moreover, without even making a serious attempt to find out the facts. He is, however, far from renouncing the chief doctrine of logical empiricism concerning history, that is, that there is a fundamental distinction between the 'historico-sociological' development of science ('discovery') and the 'logico-methodological' reconstructions of scientific claims ('justification'). Indeed, he thinks this distinction essential 'if we are to retain even a minimum of clear thinking in these badly confused matters' (p. 4). Thus Feigl explicitly rejects the 'anarchistic' position of Feyerabend. Moreover, he maintains that 'the good historian of science must devote a great deal of attention to the meaning and justification of scientific knowledge claims' (p. 4). On the other hand, Feigl insists on the Lakatosian paraphrase of the Kantian dictum for history and philosophy of science: History of science without philosophy of science would be 'blind', while philosophy of science without regard to history (*i.e.* analysis of specific cases in their cultural setting) would be 'empty' (p. 4). Now let us grant that philosophy of science without *science* would be empty. The question for one holding the 'Kantian' dictum is whether and how the historian of science, as *historian*, has anything essential to contribute to the content of contemporary philosophy of science. So far as I can see, Feigl's comments fail to answer this question.

One of the more provocative contributions is Arnold Thackray's 'Science: Has Its Present Past a Future?' Here Thackray laments the present lack of professional interest in historical issues relevant to contemporary social problems, most of which are, after all, connected with the post-war growth of science and

technology. He even attempts to trace this lack of interest to the social conditions of the immediate post-war period during which history of science emerged as a profession.¹ While not denying the value of the history of scientific ideas, Thackray strongly objects to its current dominance of the field and urges greater emphasis on such questions as the effects of secrecy and military sponsorship or of changes in the morale and status of scientists. In his equally spirited reply, Laurens Laudan argues that general historiographical debate is sterile, that history of science governed by considerations of relevance to current social problems would be bad history, and that research into the history of scientific ideas is justifiable even if it has no relevance to contemporary social issues. Thackray's response is that the interest and importance of an historical problem depends on a larger context. Within the framework of current internalist history, the suggested topics may seem unimportant or uninteresting; but these same questions may take on an entirely different light viewed in a different framework, and this is really what Thackray is urging.²

Paul Feyerabend did not attend the Minnesota conference, but did submit a paper, 'Philosophy of Science: A Subject with a Great Past'. By Feyerabendian standards this is a modest effort (only eleven pages!), the bulk of which is devoted to a brief examination of Mach's philosophy of science.³ According to Feyerabend, philosophy of science has been in a decline since Mach, a decline that began in Vienna in the 1920s. The reason for the decline, in brief, was the abandonment of Mach's critical attitude toward both science and philosophy. The result is that people calling themselves philosophers of science devote much time and logical ingenuity to black ravens and grue emeralds, but little to real science. The cure for this dismal state of affairs is 'to replace the beautiful but useless formal castles in the air by a detailed study of primary sources in the history of science' (p. 183). Fortunately for all of us, the choice is not between Israel Scheffler and Imre Lakatos.⁴ There is plenty of room for maneuvering between these two extremes. More on this below. Here I will just note that like Feigl and others, Feyerabend fails to argue that it is history of science, *as history*, that is necessary to rejuvenate philosophy of science, and not simply closer attention to real live science.

Mary Hesse's 'Hermeticism and Historiography: An Apology for the Internal History of Science', is just that. Hesse argues that there is no way for the historian to distinguish internal from external factors, *e.g.* ideas about force from ideas about love or monarchy, *unless* he has a prior theory of rationality to determine which ideas are rationally related and which are not. But, the argument continues, there is no theory of scientific rationality which is both strong enough

¹ The thesis, oversimplified, is that social history of science was too tainted by its association with Marxism to be respectable in the 1950s. Koyré's intellectualist approach provided a much more comfortable paradigm, especially for those whose backgrounds lay in the sciences rather than in history.

² For a similar discussion of the relevance of philosophy of science to contemporary social issues see my [1971].

³ Incidentally, I found Feyerabend's brief remarks about Mach more enticing than Hiebert's sober and systematic treatment—that is, Feyerabend made me want to take another look at Mach. But perhaps this is only because my own interests are more philosophical than historical.

⁴ Feyerabend cites Israel Scheffler's *Anatomy of Inquiry* as a paradigm case of a useless enterprise. The reference to Lakatos, though not stated, is presumably Lakatos [1971].

to make the necessary distinctions and also acceptable to the majority of philosophers or historians. She therefore concludes that no *general* defense of an autonomous internal history of science is possible. Nevertheless, she goes on to insist that *particular* historical judgments of irrelevance can be made, *e.g.* regarding the influence of the hermetic tradition on seventeenth-century science.

Thackray, who represented the externalist position at the conference, would turn Hesse's own arguments against her. Putting internalist history on a solid foundation, he insists, does require sound *general* philosophical principles to demarcate internal from external factors. Thus Hesse's arguments against the existence of such principles are taken as arguments against the autonomy of internal history. This leaves the social historian of science much greater latitude. In particular, he is free to foresake philosophical analysis for modern empirical historical-sociological techniques. Now there are many philosophers of science, including several who attended the Minnesota conference (*e.g.* Adolf Grünbaum, Imre Lakatos, Wesley Salmon) who would agree with Thackray about the necessity for general philosophical principles of demarcation and disagree with Hesse's view that there can be no such principles. These philosophers would thus offer a stronger defence of an autonomous internalist tradition than Hesse thinks possible. Except for a few remarks by Feigl and Salmon, both of whom go out of their way to insist on the mutual relevance of history and philosophy of science, this viewpoint is missing from the volume. This is unfortunate because the latter is still the majority view among philosophers of science.¹

By far the most systematic investigation of current relations between history and philosophy of science, in this volume or anywhere, is Ernan McMullin's 'The History and Philosophy of Science: A Taxonomy'. McMullin begins by distinguishing two senses of 'science', one including the other: (S_1) 'a collection of propositions, ranging from reports of observations to the most abstract theories accounting for these observations' (p. 15); (S_2) 'everything the scientist does that affects the scientific outcome in any way' (p. 16). There are accordingly two types of history of science: HS_1 is basically a chronicle of theories and experiments; HS_2 is an attempt to explain how a particular piece of science came to be.² Philosophy of science, however, is not dichotomised along quite the same line. McMullin calls one type 'external' (PSE) 'because its warrant is not drawn from an inspection of procedures actually followed by scientists' (p. 24). 'Internal' philosophy of science (PSI), on the other hand, 'relies for its warrant upon a careful "internal" description of how scientists actually proceed, or have in the past proceeded' (p. 26). PSE is in turn characterised as either 'metaphysical' (PSM) or 'logical' (PSL) depending on the source of the external warrant. Plato and Descartes provide an example of PSM ; Carnap of PSL .³ McMullin

¹ It should be mentioned that Imre Lakatos did read a version of his [1971] which offers an account of scientific rationality and explicitly applies this account to the problem of demarcating internal from external history of science. Unfortunately Lakatos withdrew his paper from the volume so that it might appear together with a reply by Thomas Kuhn in the proceedings of the 1970 Philosophy of Science Association meeting. (Cohen and Buck (eds.) [1971].)

² McMullin cites E. T. Whittaker's *History of Theories of Aether and Electricity* and L. Pearce William's *Michael Faraday* as paradigms of HS_1 and HS_2 respectively.

³ Having proceeded thus far in his taxonomy, McMullin pauses to consider several recent philosophical works that seem to rely essentially on history of science, particularly Lakatos [1970] and Feyerabend [1969]. McMullin concludes that in Lakatos the role

also classifies philosophy of science by subject matter: (i) epistemology, (ii) ontology, and (iii) philosophy of nature. Since in principle one might pursue any problem internally or externally (and this in two ways), there are nine possible pure modes of philosophy of science, plus all possible mixtures. The main points of entry for history of science, according to McMullin, are via internal epistemology and ontology. Both points are worth considering.

History of science is crucial for deciding the ontology of a theory, according to McMullin, because only the temporal development of a theory reveals its ontological commitments. Thus, for example, the Bohr model of the atom suggested both new phenomena, *e.g.* the Stark effect, and novel explanations for old data, *e.g.* the Pickering series of spectral lines. The ability of the model to anticipate effects not considered during its inception can only be explained, McMullin claims, by assuming that there is some 'resonance' between the model and real existing objects. Now while I am quite sympathetic with this line of argument for the reality of certain theoretical entities, I question whether it provides support for the relevance of history to the philosophy of science. First, it may be questioned whether the temporal dimension, as such, is playing a crucial role in the argument. It is at least possible that the reason successful predictions (in the temporal and not logical sense) support the ontological claim is simply that valid *testing* of a theory requires that the theory not be designed to fit the data. Explicitly predicted results insure that this requirement is met. This, anyway, was Peirce's view of theory testing and it may be supported by some modern views of hypothesis testing as well.¹ So the issue may not be one of temporal development as such, but of confirmation (though not in the sense of logical probabilities).

Secondly, even granting that the temporal development of a theory reveals its ontological commitments, it does not follow that history of science, as history, is crucial, except in cases where the theory in question is one held in the past. Suppose, for example, that properly to assess the evidence in 1953 for the existence and character of *DNA* one had to look at the development of that theory from 1945 to 1953. This would not require the special talents of a historian of science. To argue that any consideration of temporal development brings in history would commit one to arguing that dynamics is a historical science. Moreover, to argue, as McMullin appears to, that temporal development is not subject to logical and mathematical analysis would remove dynamics from physics. Surely this is giving the historian of science more than he seeks.

The above point raises the general question concerning the essential role of history of science, as history, even for internal philosophy of science. McMullin

assigned to history of science is equivocal, being 'at once emphasized and called upon as evidence, yet systematically "reconstructed" in the service of a prior theory of rationality' (p. 34). An answer to some of McMullin's objections may be found in Lakatos [1971], the paper Lakatos read at the conference but did not publish in the volume. Feyerabend is found to be using history only to illustrate a prior notion of rationality which is not really based on history at all, despite appearances to the contrary.

¹ I have touched briefly on the role of a temporal gap between hypothesis formation and data gathering for statistical hypotheses in Giere [1969]. The conclusion is that a temporal gap is sufficient but not necessary to insure satisfaction of a necessary condition for valid statistical inferences—at least on one common account of statistical inference. Thus I would agree with McMullin in opposing standard philosophical accounts of confirmation according to which temporal relations are necessarily *irrelevant*.

himself considers the possibility that *PSI* might appeal only to *contemporary* scientific practice and not necessarily to the history of science. He rejects this possibility because: (1) history 'provides complete case studies of a kind one could not recover from contemporary science'; and (2) history 'allows one to study science in its all-important temporal dimension' (p. 29). But surely we know (or can learn) more about the discovery of *DNA* than of bacteria, and surely the study of recent developments in science requires no peculiarly historical techniques—or at least not the techniques now taught by most historians of science.

Turning finally to epistemology and the history of science, the crucial question, in McMullin's own words, is 'Can a *PSI* be normative?' (p. 42). His answer seems to be that it cannot. If one grants that epistemology is normative, it follows that one cannot get an epistemology out of the history of science—unless one provides a philosophical account which explains how norms are based on facts. This ought to be a central problem for historically oriented philosophers of science, but few seem willing even to acknowledge the question, let alone attack it head on.

S U M M I N G U P

Except for a few contrary indications voiced by Feigl, Salmon, Stein and Koslow, the overall picture of recent philosophy of science that emerges from this volume is one of a discipline dominated by a concern with formal systems having little relevance to actual scientific theories or practices. History of science is seen as the means, or at least *a* means, of remedying the situation.

Now it is certainly true that the growing dissatisfaction with the logical empiricist approach during the late 1950s and early 1960s coincided with criticism which appealed to the history of science. Works like N. R. Hanson's *Patterns of Discovery*, Stephen Toulmin's *Foresight and Understanding* and Thomas Kuhn's *The Structure of Scientific Revolutions* come readily to mind. It is not obvious, however, that this criticism was effective (I do not say valid) *because* of its appeal to *historical* cases. Its effectiveness might be explained by the fact that it appealed to real science. That is was the science of Kepler and Darwin rather than that of R. P. Feynman and J. D. Watson may have been incidental.

Contrary to the spirit of some remarks in this volume, *e.g.* by McMullin and Buchdahl, no logical empiricist ever thought that the form, content, or methods of science may be derived from formal logic alone. For traditional logical empiricists, the task of the philosopher was always the rational reconstruction and explication of theories, methods and meta-concepts found in actual scientific practice.¹ Where logical empiricism failed was in the application and justification of this approach. The classic examples of reconstruction and explication reveal much about the preferred logical tools and empiricist epistemological programmes of logical empiricism, but little about actual science. For example, traditional discussions of explanation and the interpretation of theories, usually carried on in the context of first order languages, have little relevance even to current philosophical discussions of the existence and nature of conventions in

¹ Indeed, one of Feyerabend's complaints in this volume is that logical empiricists have been too slavish in following recent developments in science and thus have not played a sufficiently critical role.

physical theory, *e.g.* in special relativity. Similarly, the vast literature on the justification of induction and the paradoxes of confirmation is hardly relevant to fundamental problems concerning hypothesis testing in psychology, let alone to the confirmation of hidden variable theories of micro-processes. Moreover, the theoretical account of the link between the facts of scientific practice and the normative conclusions of philosophical analysis was never very well developed. The requirement that the explication 'resemble' the explicandum was seldom examined and too often ignored. The distinction between scientific fact and philosophical convention, like that between synthetic and analytic statements, was applied uncritically. Even Carnap's distinction between internal and external questions—the latter resolved by pragmatic decisions—left open as many problems as it answered.

On the above account of the failing of logical empiricism, it does not follow that history of science, as such, promises any remedies. The most direct response would be to develop more flexible logical and mathematical tools, to pay closer attention to actual scientific theories, and to worry more about the nature of philosophical conclusions about science. At the moment there are many philosophers of science, particularly among the younger generation, who are following just this course. Moreover, this approach seems to be yielding dividends, especially in the philosophy of physics and in inductive logic.¹ It is regrettable that many of the contributors to this volume, apparently unaware of recent trends among their more formalistically inclined colleagues, write as if the only alternative to historical studies is the mould set by Carnap in the nineteen-thirties and forty's.

Although there are substantial differences among them, those philosophers of science who make serious use of the history of science form a loosely connected school within the philosophy of science. It is natural that members of such a school should see their discipline in a different light from others. One would hope, however, the members of the school will not be content merely to practice their art but will make repeated efforts to explain and argue the rationale of their approach. I have already indicated a number of questions for the historical approach which seem to me to need further discussion. Let me emphasise one key set of questions by taking a further brief look at an issue that accidentally emerged as a central topic of the volume, the choice of theories.

McMullin describes Kuhn's conclusion 'that changes of paradigm cannot be justified on empirical or rational grounds' (p. 62) as a prime example of the impact of historical studies on logical and epistemological doctrines in the philosophy of science. Yet at key points in his argument for the non-rationality of paradigm choice, Kuhn appeals to the fact that there can be 'no proof' that one view is the true one. This is not a conclusion based on history but a logical point, one for which Hume is justly famous. The claim is, at bottom, that there can be no non-deductive *reasoning* in favour of a general theoretical framework. Would anyone argue that this claim follows from historical case studies? Moreover, when Kuhn claims that 'mass' means something different in classical and relativistic mechanics, thus rendering these theories incommensurable, he is not appealing to history but to an analytical criterion of difference in meaning. What *historical*

¹ See, for example, the following recent papers: Earman [1971], Fine [1972], Glymour [1971], Hooker [1972], van Fraassen [1970] and [1972], Winnie [1970].

facts, by themselves, would show a difference in meaning here?¹ A similar move occurs in Buchdahl's paper. He considers and rejects the view that the main support for a theory must come from 'empirical confirmation' (p. 227). His rejection, however, is not based on an appeal to historical cases but on a *philosophical* rejection of the 'old problem of induction' (p. 228). So even in the rejection of philosophical theses it is not so much historical cases as contrary philosophical theses that are operative.²

The situation is even more problematic if one considers the possible positive bearing of historical studies on philosophical conclusions concerning the rational choice of theories. As I have indicated several times above, none of the contributors attempts, or even cites other attempts, to show in a systematic way just how philosophical theses about theory choice may be supported by historical case studies. To raise this issue is not necessarily to hold dogmatically to a distinction between the descriptive and the normative. On the contrary, I would argue that all norms have their roots in facts. The general problem is to show that philosophical conclusions may be supported by historical facts and just how this comes about. Until this is done, the historical approach to philosophy of science is without a conceptually coherent programme.

There is a further problem which is especially acute for any historical approach to criteria for the rational choice of theories. Suppose, as McMullin and others suggest, that history provides *empirical* data for one's account of theory choice. In this case the account of theory choice is itself an empirical conclusion, or, broadly speaking, a theory. But to choose a theory of theory choice on the basis of historical data one must already have some criteria for theory choice. Where are we to find these latter criteria? So not all our criteria for theory choice can be empirical conclusions supported by historical data. What then are they? And what is their relation to historical studies?

Some will claim that these latter questions merely betray an old-fashioned justificationism. Perhaps. But those with more modern views should explain clearly why such worries are misguided and how it is that everything comes out all right if we ignore them.

5 AN ALTERNATIVE VIEW

In so far as my own views are fairly representative of the majority of philosophers of science outside the historical school, and in so far as such views are not well represented in this Minnesota volume, I will conclude with a brief sketch of an alternative analysis of the philosophy of science and of its implications for relations between history and philosophy of science.

The most important divisions within philosophy of science should be by problems, not methods. I would focus on three main problem areas: (1) The *structure*

¹ Kuhn's appeal to the fact that no 'proof' can decide between comprehensive theories can be found, for example, in Kuhn [1962], pp. 147, 150-51. For the key remarks on the meaning of 'mass' in classical and relativistic physics see Kuhn [1962], pp. 100-1.

² The rejection of the 'symmetry thesis' regarding explanation and prediction and the 'deducibility requirement' for the reduction of theories are commonly taken to be examples in which a philosophical thesis was refuted by historical cases, e.g. evolutionary theory and statistical mechanics respectively. Yet even here the role of history of science, as such, seems minimal. These cases are important because they are part of the *currently accepted* body of scientific knowledge.

of knowledge, especially theoretical knowledge, (2) the *validation* of knowledge claims, and (3) the *strategy and tactics* of research (methodology, in a narrow sense). In each case one may be concerned with the problem as it applies to a *particular* theory, field, science, etc., or as it applies to *all* theories, fields, sciences, etc. It is his interest in generality that primarily distinguishes the philosopher of science from the theoretical scientist.¹ Let us consider the possible bearing of history of science on each of these problem areas.

Among questions concerning the structure of knowledge are questions about theoretical explanation, the meaning of theoretical terms, etc. Concerning questions about the structure of particular theories, e.g. quantum mechanics, philosophers will be guided by the same basic criteria as scientists, except in so far as they seek to follow patterns concerning the structure of theories in general. Do general theses about the structure of theories have to fit historical cases? I think not. If philosophy is to maintain its critical role, one may even refuse to accord the title 'theory' to something many scientists now call a theory. Indeed, all currently held theories may be judged somewhat defective, though not too much so else the claim to be talking about scientific theories becomes problematical. Philosophical theses cannot be completely *a priori*.

Now some members of the historical school seem to hold that a theory is an historical entity, or at least that one could not really understand a theory without knowing some temporal developments. Thus the structure of a theory cannot be captured by any single logical or mathematical structure. The difficulty with this claim, besides its vagueness, is that scientists learn theories from texts containing little history and what history there is would usually be judged bad history. So unless one is willing to claim that most scientists do not really understand the theories in their fields and could not learn them from standard texts, the claim that history necessarily enters consideration of the structure of theories must be rejected. There remains, of course, a question as to whether and to what extent theories may be set out explicitly in mathematical systems, but this problem may be treated separately from questions about the role of history of science.

Questions concerning the strategy and tactics of research may be viewed in two quite different ways. They may be treated quite generally and used to help *define* rationality in the scientific context. This is how we should understand Peirce's maxim, 'Do not block the way of inquiry', as well as more recent pronouncements by, say, Popper or Lakatos. That history of science as such plays an essential role in this enterprise, and does not serve merely as a convenient source of examples, is something that needs to be argued.² Concern with the process of inquiry does not automatically make one an historian. The needed connection between process accounts of rationality and real science may be made solely within the context of contemporary science.

The second way of viewing the strategy and tactics of research is as a straight forward, though second level, empirical inquiry. Now in science, as in any field, there is no substitute for genius, and of course no research strategy could guarantee success, e.g. in finding a satisfactory unified field theory. But if one

¹ I have sketched a similar taxonomy in Giere [1971].

² This is the key point in McMullin's examination of Lakatos [1970] mentioned in footnote 3, p. 288.

examines the events leading up to particular discoveries, it at least seems that some strategies are better than others. And there are good reasons for thinking that this is true in general. Given a particular phenomenon of which some theories are true, and given the capabilities of human investigators, some actions are empirically more likely than others to lead to discovery of true theories. The only question is whether we can discover any useful patterns. This, however, is an empirical matter which can only be answered by trying. Moreover, the usefulness of history of science in this enterprise is likewise only to be discovered by trying. So here is a place where history of science may have a direct input into the philosophy of science. The trouble is that not many philosophers of science regard the empirical study of research strategies as part of philosophy. Thus the point where the relevance of history may be greatest is just the point where the philosophical component seems weakest. Perhaps the best conclusion here is that both history and philosophy of science are contributing to a third area, say, the science of methodology.

Turning to the problem of validation, I think a majority of philosophers of science would, contrary to Kuhn, Feyerabend, and others, agree that there is such a thing as empirical validation. That is, there is such a thing as non-deductive reasoning, though few agree about its specific form. The question here is whether history of science has any role in, so to speak, validating an account of empirical validation, whatever its specific character. I have already argued that history of science cannot be regarded as providing empirical evidence for a philosophical account of empirical validation. This view presupposes an account of validation. But our theory of validation cannot be purely *a priori*. What is left? The only way out, I think, is to admit that the choice of a theory of validation is ultimately not wholly a reasoned choice, but partly just the result of a causal process. Which causal process? In principle any process involving anyone who adopts a method of validation. In practice, however, the most relevant process is that involving philosophers, mathematicians and scientists who collectively formulate and apply various validation methods. For a method of validation to be accepted is for it to be thought correct and adopted in practice by an effective majority of philosophers and scientists. A philosopher can only influence this process by criticizing, clarifying and developing methods of validation and then persuading scientists to try his recommendations. There is no guarantee, of course, that any single method will ever be effectively accepted. One can only try.¹

The history of science can only indirectly enter into the process by which an account of validation becomes accepted. Philosophers and scientists may be influenced by their understanding of historical cases. But history of science need not enter the process, and it would be difficult to argue that it should. What we seek is a unified method of validation to be applied in current scientific inquiry. To argue that our understanding of past science, which is itself based on empirical evidence, should be fed in the process of choosing a theory of validation is to assume that we are right about the past and that this past experience is relevant to present scientific inquiry. Yet even historians would agree that relevance to present science decreases the further back we go. Moreover the period in which relevance to current scientific inquiry becomes problematic is just the period

which has received the most attention from historians, namely the seventeenth century. Thus in so far as there is a role for history of science in resolving the problem of validation (including theory choice), it is only the history of recent science (say the nineteenth and twentieth centuries) that is at all important.

It may be thought that the above account of validation is little different from Kuhn's view that theory choice is a non-rational process. There is, however, an important difference. Here theory choice is rational—it is a matter of empirical validation. All the blood, sweat, toil and tears that go into the process of conceiving a new theory are, it is true, largely irrelevant to validation, but this process is hardly irrational and certainly subject to systematic empirical study. Moreover, it is assumed that the problems concerning incommensurability and the 'theory ladenness' of the observation language are resolvable. With these provisos, the intervention of non-rational processes is confined to the next level up—the choice of a method of validation. This is in accord with the idea that the real revolution in the seventeenth century was one of method rather than content.

If history of science is somewhat relevant but not essential for the philosophy of science, what of the reverse? Here I agree with McMullin that the primary goal of the historian is to explain the occurrence of particular occurrences in science, *e.g.* why Newton did what he did, when and where he did it. To do this well requires no understanding of current philosophy of science. What it does require is an understanding of seventeenth century philosophical ideas about science. It also requires a great deal of other knowledge about the seventeenth century, from social conditions to theology. Everything is potentially relevant. The historian's task is to weigh their relative importance in producing Newton and his physics. The dispute between internal and external history arises only if one approaches the question with the view that the character of Newton's physics is determined solely by his ideas about space, time, matter, force, *etc.* Conversely, one might start out convinced that social conditions alone determine the outcome. I doubt that many historians of science fit either caricature, though many do exhibit some bias when it comes to weighing the relative importance of various kinds of factors.

There is another way of approaching the history of science, though this is not the way it is usually done. This is to begin with current philosophical conceptions of the nature of theories, validation, *etc.*, as well as current views of the scientific material. Then, in the case of Newton, for example, one could ask why he strongly believed something even though his evidence for it was very poor? Here an account of validation creates a clear distinction between 'internal' and 'external' factors influencing belief. Similarly, one could fairly clearly separate retroductive arguments from inductive (to use Peirce's distinction). Or again, given that such-and-such a physical condition is necessary for a certain conclusion, on what grounds could Newton assume it even though he never acknowledges the assumption? The answers to such questions should be historically interesting—they would tell us something about Newton we could not learn simply by immersing ourselves in Newton's own world. If anyone is to do this sort of history, however, it will probably have to be a philosopher. To an historian of science this approach will seem too much like that of the proverbial retired scientist who goes through history ticking off the things that someone got right. The analogy is unfair, of course. The point of such studies would not be to judge Newton, but to understand him.

¹ This approach to the choice of a method of validation, as well as an account of validation itself, is discussed in Giere [1973].

6 CONCLUSION

This latest volume of the *Minnesota Studies* grew out of an attempt to investigate the rationale for the union of history of science with philosophy of science. At best the results of that investigation must be judged inconclusive. Advocates of the union have not produced good reasons for thinking that the union is particularly intimate. Indeed, viewed from a more orthodox position within the philosophy of science, there is every reason to think that the primary relationships for philosophy of science are with philosophy and science. Likewise, the primary relationships for history of science are with history and science. What they have in common is science. But this common interest is not a sufficient basis for other than a marriage of convenience. However, even if one agrees that the union between history and philosophy of science lacks a strong conceptual rationale, it does not follow that the marriage of convenience should be dissolved. The proliferation of centres, departments and programmes for history and/or philosophy of science during the past decade shows that neither historians nor philosophers of science are happy with their parent disciplines. In these circumstances a marriage of convenience may currently be the most practical institutional arrangement. Whether this arrangement will prove to be relatively permanent or only transitional remains to be seen.

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