Section 06

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History and Philosophy of Science: A Marriage for the sake of Reason

The close cooperation of history of science (HS) and philosophy of science (PS) comes to be appreciated ever more widely and has reached institutional recognition as history and philosophy of science (HPS) in some places. Not rarely, however, it is still looked upon as being no more than a "marriage of convenience". With this term Giere [1] pointed at once to the undeniable usefulness of the union and to the lack of a rationale of it. A number of attempts have been made to provide a rationale ([2]-[5]), but none has met satisfactorily Giere's central challenge, viz. "to show that philosophical conclusions may be supported by historical facts and just how this comes about" ([1],292). The problem is that the philosophical claims to be supported are taken to be normative, whereas history only supplies the factual course of events, possibly including explanations, but no justification of either scientific or philosophical propositions. Indeed, since science (S) does not appeal to history, why should PS? An answer to this question is acceptable only if it explains (a) why the scientist need not (and perhaps should not) resort to history, although the philosopher does (and perhaps should), and (b) how normative contentions are related to historical facts.

An easy answer to question (a) is the following: S is concerned with nature, whereas PS is concerned with our knowledge of nature; nature is independent from human history, knowledge is a piece of it. Indeed, the philosopher (but not the scientist qua scientist) will ask questions about e.g. the progress of science, its conditions etc.. The recent interest of PS in HS is partly due to the growing concern with science as a factor in history. This is only a partial answer to (a), which, moreover, underlines still more the need for an answer to question (b): How can the philosopher validly infer general statements about the growth of knowledge from the contingent vagaries of past scientific development? In the absence of historical laws it remains tempting to base analyses, explanations, rules etc. of S on the nature of knowledge and rationality and in this sense on norms regardless of historical fact.

But the experience with aprioristic philosophy is discouraging and the lesson of historicism hardly deniable: the concepts of knowledge and rationality themselves have developed through history. A further historicist conclusion, however, does not follow: An analysis and understanding of S, will not, in the last instance, be historical or sociological. For not all of history was rational or pertinent to knowledge. There is no way of drawing a demarcation on a purely descriptive basis. Hume construed belief as a kind of involuntary natural event. This view will, if at all, be adequate only for immediate sense experience. Theories and methods do not simply force themselves upon us. They become ours after critical examination through a conscious

and voluntary (though by no means arbitrary) act of acceptance. And, as scientific development is a history not of Humean belief but of acceptance, also HS (as its record and explanation) cannot be purely descriptive. Rationality is not only to be observed but first of all to be shared. Given now that there is no eternal notion of rationality, the sharing is always as of today. It is, and should be, participation in present-day S. The need for acceptance is the deeper reason why scientists do not bother with history. Tradition is never an argument, only its contents can be. The extent to which they are determines the extension of the present for the scientist, which present is naturally no specious moment of zero duration. (It is therefore a mistake to argue that PS whenever it turns to actual S is eo ipso historically based -- [6], 350; [5], 159.)

Now, if the reasons for acceptance are taken from current S, then PS will not be normative in the sense of legislating over S. It can only be a critical companion of S, critical, that is, not from "above" or "below" but from within. The apparent "norms" of method and rationality are no more than the result of acts of acceptance (whose spectrum is broad, reaching from something like Humean belief through cognitive acts of recognition that are more or less determined all things considered to mere conventions). Along this line the answer to question (b) is to be found.

What follows from this for HS? If it is merely descriptive, it will not provide a true understanding of S. If it derives its

concepts of rationality, method and knowledge from basic value judgements of the scientific elite ([7], sect. 2 A) or from our pre-analytic intuitions ([5], 160), it misidentifies the only genuine source of cognitive commitments we have: our present science, which includes of course the competition of alternative and controversial options. (These commitments may partly coincide with the judgements of great scientists or our intuitions, to be sure.) To avoid misunderstanding: this is not to say that all beliefs and methods rejected today will be excluded from past S. It only entails that HS combines factual with counterfactual considerations, i.e. that the account of the actual development is always given in the light of what would have been rational. HS in this sense will be impossible or inadequate only if either of the following conditions would not obtain: (1) There are some common items of acceptance throughout history in whose light to judge the changing lesser rules and statements; and (2) what we can accept as justified reasons (for a given time in the past) were among the effective causes of the development.

Without these two conditions there would be neither continuity nor rationality in the course of events. How could we then recognize it as the history of science? Conversely, if there are continuity and rationality the present state of knowledge will embody important traits of its coming about (traits as appear to be articulated e.g. in the web of interrelated theories of current physics -- [8]), which is another reason for PS to learn from HS -- an HS which, in turn, is informed by PS and,

last not least, S. History of science and philosophy of science can only thrive as companions of living science itself. A marriage à trois, though, will hardly be convenient.

[1] R.Giere: History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?, Brit.J.Philos.Sci. 24 (1973), 282-297.

[2] E.McMullin: History and Philosophy of Science: A Marriage of Convenience?, in: R.S.Cohen et al. (eds.), PSA 1974, Boston Studies Philos. Sci. 32, Dordrecht 1976, 585-601.

[3] E.McMullin: Philosophy of Science and Its Rational Reconstructions, in: G.Radnitzky and G.Andersson (eds.), Progress and Rationality in Science, Dordrecht 1978, 201-232.

[4] R.M.Burian: More than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science, Philosophy of Science 44 (1977), 1-42.

[5] L.Laudan: Progress and Its Problems - Toward a Theory of Scientific Growth, Berkeley etc. 1977, Chap.5.

[6] P.Achinstein: History and Philosophy of Science: A Reply to Cohen, in: F.Suppe (ed.), The Structure of Scientific Theories, Urbana/Ill., 2d ed. 1977, 350-360.

[7] I.Lakatos: History of Science and Its Rational Reconstructions, in: R.C.Buck and R.S.Cohen (eds.), Boston Studies Philos.Sci. 8 (1971), 91-136.

[8] L.Krüger: Wissenschaft zwischen Natur und Geschichte, Geschichte und Gesellschaft 4 (1978), 452-471.

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Section 06

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Title of the talk

A SOLUTION TO RULING OUT ALL PARTIAL AND TOTAL SELF-EXPLANATIONS IN D-N ARGUMENTS†.

1. Having an adequate definition of explanation is central for the post-popperian philosophers of science, because giving explanations (of singular events and phenomena) is the very most important aim of scientific research in the opinion of these philosophers. But an unobjectionable logical model of explanation could not be found until now, not yet for the deductive-nomological case. Since the fundamental study of Hempel/Oppenheim a lot of work was done, especially upon ruling out partially self-explaning arguments. The attempts of Kaplan, Kim, Ackermann/Stenner, Omer, Tuomela, Thorpe and Gärdenfors, however, have been disapproved (see Kim |4|; Morgan |6|,|7|,|8|,|9|; Höll |2|).

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