

LARRY LAUDAN

THOUGHTS ON HPS: 20 YEARS LATER

I HAVE BEEN asked by the editors of *Studies* to write a short piece on my impressions of developments in the field over the last two decades, particularly in light of this journal's initial mission. That task cannot be discharged without a brief personal note about how *Studies* came to be what it is, indeed about how it came to be at all. So I hope I will be forgiven if I begin with a first-person narrative.

Immediately prior to the academic year 1964–1965, I had scraped together a grant or two to fund my way to the University of Cambridge. I was attracted there and away from Princeton by the then “sub-department” of history and philosophy of science — an anomalous appendage to the (as then was) Department of Moral Sciences. As fate would have it, I left Princeton just as Thomas Kuhn was arriving, driven by a curiosity about a scholar who was teaching many of us how to read Descartes and Kant. My previous interests had centered on mainstream philosophy of science; but, having used Pierre Duhem's *Le Système du Monde* as a vehicle for teaching myself French, I was intrigued by the presence in Cambridge of a group of scholars who — very like Duhem — were asking philosophically interesting questions about the evolution of science. Hesse, Young, Whiteside, Hoskin and Buchdahl more or less constituted the HPS faculty then; but, at least for me, Buchdahl's persuasive rhetoric on behalf of an integrated approach to history and philosophy of science was the most telling.

After a year or so at Britain's most active center for history and philosophy of science, I took up a Lectureship at Britain's oldest (and likely its most moribund) unit for that subject: the Department of History and Philosophy of Science at University College London. For most of the preceding decade, that department had been chaired by the historian of chemistry, Douglas McKie — a scholar with decided views about the irrelevance of philosophy to the history of science (and to everything else). A succession of philosophically-minded scholars had previously occupied my post (including, I believe, Alasdair Crombie and Mary Hesse), but they had wisely moved on to greener pastures.

Fortunately, it quickly became clear that London's intellectual resources went well beyond the confines of Jeremy Bentham's college. Indeed, those were lively days in London for a young scholar interested in linkages between the history of science and its philosophy. The Halls (Rupert and Marie Boas)

and G. J. Whitrow were at Imperial College; Gombrich and Yates were holding court at the Warburg; Popper and Lakatos were fighting it out at the LSE, ostensibly to determine who was the *real* Popperian; and even University College was being dragged kicking and screaming into the act by the occasional but always lively presence of its (sometime) Professor, Paul Feyerabend, then commuting between apparently full-time appointments at Berkeley, London and Berlin.

The vigour and variety of work at Cambridge and London in history and philosophy of science were in marked contrast to the unrelieved sameness of the professional journals of the field. *The British Journal for Philosophy of Science* like its U.S. counterpart, *Philosophy of Science*, was then publishing a pretty steady pabulum of logical empiricist work, generally untainted by any hint of real science — whether historical or contemporary. Or so it seemed to this young enthusiast. The corresponding historical journals, *British Journal for History of Science* and *Isis*, were (and twenty years later *still* remain) unabashedly a(nti)philosophical.

For those of us who believed that there were philosophical lessons to be learned from the history of science and philosophical insights needed for understanding the history of science, there were few if any natural outlets for anything short of book-length manuscripts. Moreover, it is in the nature of the case that philosophically-sensitive historical case studies turn into shaggy-dog stories, typically much too long to remain within the usual journal restrictions on length. Necessity being the proverbial mother of invention, Gerd Buchdahl and I schemed together during 1968 (while he was visiting at Western Ontario) about how to congeal some of the excitement about history and philosophy of science into a forum which might serve as an outlet for sustained scholarship in history and philosophy of science. *Studies*, conceived as a journal blind to the disciplinary boundaries between history and philosophy of science, and willing to publish lengthy monograph-sized pieces, was the natural response to that perceived need. Having hatched the idea of an interdisciplinary quarterly devoted to the study of the conceptual history and foundations of science, we spent the next few months persuading the powers that were at Macmillan's that ours was a venture worth gambling on. Fortunately, John Maddox, then editor of *Nature* for Macmillan's, took us under his wing and in a matter of months, we were up to our ears in the chores required to piece together a quarterly journal.

Gerd and I shared editorial chores on *Studies* for its first five years, at which time Macmillan's sold the journal to Pergamon Press. At that point, the journal returned wholly to a Cambridge editorship, an entirely fitting arrangement since that is (spiritually at least) where the journal began.

It has been interesting to watch the fortunes of the field of history and philosophy of science wax and wane in these two decades. *Institutionally*,

there have been several ups and downs, not unlike a game of musical chairs. HPS has grown and flourished at places like Pittsburgh, Indiana, Western Ontario, Leeds, Cambridge, LSE, Boston, Chicago, Notre Dame, San Diego and Konstanz; by contrast, it slowly atrophied, and was finally officially pronounced dead, at Princeton and Pennsylvania. It flickered briefly at numerous other places (e.g. Minnesota, MIT, Brandeis, Oxford, Oklahoma, Sussex, Melbourne, Johns Hopkins) but only occasionally became more than a rocky marriage of convenience at any of the latter.

By contrast, hyphenated “history-and-philosophy-of-science” has blossomed intellectually. Particularly among philosophers of science, one sees it now fully taken for granted that there is an “historical” school of philosophy of science (including Kuhn, Lakatos and his followers, McMullin, Toulmin, Feyerabend, Laudan, Shapere, Sneed and a host of others). Even among those who do not themselves practice philosophy in that way, there is a growing — and now less grudging — acknowledgement that some of the central epistemic challenges to traditional views of science have come from those quarters.¹ As I have pointed out elsewhere, however, historically-based philosophy of science often remains more a slogan than a reality. While “models” of scientific change and progress proliferate, serious efforts to check those models against the historical record are still perilously few and far between. There are many reasons for this reluctance to join theory and practice. The historical evaluation of philosophical claims requires, among other things, a judicious selection of relevant cases, easy familiarity with the historical cases under examination, and a breaking out of general models into their constituent parts. Headway is being made on all three tasks² but the articulation of a well-tested theory about theory change has scarcely begun.

If that challenge remains as acute as it was twenty years ago, other issues have faded from prominence. Much of the historically-based philosophy of science in the 1960s consisted, one way or another, in positivist-bashing. One thinks, for instance, of Kuhn and Feyerabend’s attack on the cumulativeness postulate, or the Hanson–Feyerabend critique of the theory/observation language distinction. Or Toulmin arguing against the existence of mechanical algorithms of scientific inference. Those battles are now simply ended; the issues at stake were, as far as most of us are concerned, brought to closure. Where the logical positivists were the primary targets of work in this area in the 1960s, it now tends to be strong relativists who are drawing much of the fire. Indeed, one of the weird by-products of the “historical revolution” has

¹For documentation of that indebtedness see my “Historical Methodologies”, in: H. Kyburg and P. Asquith (eds), *Current Research in Philosophy of Science* (Philosophy of Science Association: East Lansing, 1979), 40–54.

²For a lengthy treatment of these matters see L. Laudan *et al.*, “Testing Theories of Scientific Change”, *Synthese* 69 (1986), 141–223; and A. Donovan *et al.*, *Scrutinizing Science* (Reidel: Dordrecht, 1988).

been the emergence of a new historicist and relativist orthodoxy — associated first with Kuhn and Feyerabend and lately embraced by, e.g., Rorty and many social constructionists. The message that we need a richer theory of rationality than positivism to capture the rhyme and reason of scientific change (and that was the message of almost all the HPS community through the 1960s and 1970s)³ has of late been taken as a license for the claim that there are no general principles or patterns of rationality in science (save the self-interest of scientists themselves). Relativists and non-relativists alike are claiming that the historical record supports their position, and the pages of *Studies* have seen many of these struggles over the last decade. All of which is still further evidence for the claim that we lack a generally adequate normative theory of scientific change.

To return once more to the autobiographical, I must report that I have been struck — in retrospect — by what a very improbable team Buchdahl and I were to be editing a journal. Buchdahl came at these issues from a background in continental philosophy — especially Kant and Hegel. I brought a decidedly pragmatic set of culture heroes — Peirce and F. C. S. Schiller to name the more important. Our intellectual styles too were (and remain) quite distinct. Perhaps Buchdahl's greatest virtue is his ability to take an apparently simple issue and to exhibit just how complex it is; my chief strength sometimes appears to be that of making the complicated seem straightforward. One can appreciate how this arrangement worked only if one understands that our collective concern was not to promote a certain ideology of science (for we shared little by way of philosophical background) but instead to advocate a certain heuristic for tackling philosophical problems about science.

The intellectual battle that he and I, along with a handful of better-known folks (Kuhn, Toulmin, Hanson, Lakatos and Feyerabend come quickly to mind), were waging in the late 1960s and early 1970s has been exactly 50 per cent won. Contemporary philosophers of science, whatever their persuasion, are now prepared to grant that historically-based philosophy of science is not only a viable but a valuable venture. By contrast, many (perhaps most) professional historians of science have refused to see the point. Indeed, the distance between mainstream history of science and the philosophy of science is probably greater now than it has ever been, notwithstanding that many historians of science still take philosophical issues seriously.

Studies is now, as it was in 1969, virtually the only general journal which encourages historians of science to grapple with issues concerning the methodological and conceptual foundations of the sciences. But the melancholy fact of the matter is that, more by default than by design, philosophers have taken on an increasing share of the responsibility for doing conceptually-

³With the notable exception of Paul Feyerabend.

based history of science, a fact that erstwhile historians like Kuhn are quick to acknowledge⁴ and that many historians of science (e.g. L. P. Williams or Erwin Hiebert) are quick to deplore. Nor is there anything new in that. Duhem, Mach, Whewell, Meyerson, Burt, Strong and a host of others long ago realized that philosophers of science cannot wait for, nor should they expect, historians of science to do their work for them.

⁴See Kuhn's article from *Daedalus*, reprinted in his *The Essential Tension* (Chicago, 1977).