

"THE IMPORTANCE OF (NOT) BEING THE HISTORY MAN"

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Discussion Paper

on

Ian Jarvie's

**"EXPLANATION, REDUCTION AND THE SOCIOLOGICAL TURN IN
THE PHILOSOPHY OF SCIENCE, OR KUHN AS IDEOLOGUE FOR
MERTON'S THEORY OF SCIENCE"**

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by Ian Jarvie

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Professor Jarvie raises some intriguing questions concerning why postwar science studies developed in precisely the way they did. Why should they have? After all, there were nodes from which other directions might have been taken just as readily. In particular, Karl Popper might have been associated with the sociology of science in the same way as he has been with the history and philosophy of science movement. Instead, Popper and some of his philosophical followers (followers both acknowledged and unacknowledged; academic marginal differentiation is not to be taken too seriously here) commonly give the impression of defining their own enterprise by what, in their view, sociological accounts fail to do.

From the perspective of "normal philosophy" (to coin a phrase; or is it rather, as Fuller, 1984, suggests, merely Oxford philosophy?), the effort to disavow sociological claims is somewhat understandable. But viewing contemporary philosophy from the outside, what is most remarkable about all the various post-positivist accounts of science is that their respective differences pale before the very large gulf fixed between them, on the one hand, and both earlier positivist views of science and the "protopositivism" (Gillespie, 1979) which informs ordinary folk epistemology and its attendant images of science. Consider Peter Medawar's portrait of the latter (1982:115):

A scientist is a man who has cultivated (if indeed he was not born with) the restless, analytical, problem-solving temperament that marks his possession of a Scientific Mind. Science is an immensely prosperous and successful enterprise...because it is the outcome of applying a certain sure and powerful method of discovery and proof to the investigation of natural phenomena: The Scientific Method...Scientific laws are inductive in origin. An episode of scientific discovery begins with the plain and unembroidered evidence of the senses -- with innocent, unprejudiced observation...and a great mansion of natural law is slowly built upon it...

The most striking feature of such positivist and protopositivist views is surely their lack of the sense in which "science" is the predicate of complex, differentiated collective structures of research and not the predicate of individual intelligences or what common sense can verify -- such as in the legendary Scientific Mind, for instance. From this standpoint, then, the effort of certain philosophers to backpedal on sociological claims produces some curious spectacles and predicaments. For instance, Popper himself began by directing attention in a forceful way to certain inadequacies in formalistic and individualistic views of scientific knowing. On his view (if not entirely in his terminology) science is fundamentally a process of inquiry (not just its later, formally justified results). Scientific inquiry nests in a certain sort of collective opportunity structure with shared theoretic frames and distinctive norms of truth-testing (and is not innocent individual observation). If Robert Merton didn't already hold the patent, Popper might well have coined the term "organized scepticism" to describe the fundamentally social operation of scientific inquiry. A rose by any other name...it all sounds very sociological. Popper might have taken the words from Durkheim's mouth; and yet he is thereafter found denouncing "the Myth of the Framework" (Popper, 1970) when other critics of the same tendencies, Thomas Kuhn being one, carry the criticism further into a positive account of the collective matrix of

metascientific conditions framing the eventual formalization and justification of scientific discovery.

Consider another instance. Larry Laudan began by following the root insight that whatever science can do, it cannot yield Truth (capital T) and that in default it gives us the most reasonable alternative: tentative results of conjectures refuted and critically modified within discernible communities of organized skeptics. These insights he then elaborated into an account of scientific progress whereby the conjectures are embodied in ongoing traditions of problem-solving (research traditions) in which the measure of success is the immanent and self-upgrading one of how many of its problems a research tradition can solve, rather than its approximation to ahistorical standards of Truth invoked independently of the critical scientific process itself. (In passing, the importance of an account such as Laudan's cannot be overestimated in the face of a variety of attempts -- they range from revived creationism to Taoist physics -- to re-enchant the world by invoking extra-scientific, and even religious constraints upon science. Cf. Gellner, 1973.) But Laudan then moves (1977) to dissociate his thesis from sociological accounts of science, on the grounds that such accounts will always minimize the rationality of scientific change.

There is at least one possible world in which the basic projects of Popper, Lakatos, and Laudan would be regarded along with that of Kuhn as contributing to a sociological understanding of science. Key elements of leading post-positivist philosophical accounts of science -- such as the metascientific conditions which frame scientific formalization; the collective organization of sceptical testing; the embodiment of rational norms in traditions -- fit quite comfortably within the tool-bag of working sociologists. By the same token, there is also at least one possible world in which disciplinary specialization was so far advanced that the speculations of Kuhn, an historian, would seem just as exotic to

serious Social Scientists as would those of his fellow humanists the philosophers Popper, Lakatos, and Laudan.

While there is reason to believe that both these possible worlds are more than mere possibilities at the moment, in fact the academic study of science exists somewhere between a reductive Comtean unification in which "we're all sociologists now," and total differentiation into hermetic specialties. Although (if it comes to a choice) the contemporary sociologist will have a certain professional preference for differentiation over unification, it would be unfair to describe the sociology of science as hermetic, even during its early stages of intensive specialization. Thus it might have been expected to do what Jarvie finds it does: to seek some broader justification. Given this predilection, why, of all possible worlds, does there develop the one in which Kuhn's star ascends over all others? This is the main question to which Jarvie directs our attention.

In an attempt to further this very interesting line of inquiry, I shall first summarize what I take to be the central thrust of the author's remarks. Thereafter I will consider, briefly, his claims about reduction. Finally, I will toss up an alternative conjecture about the respective fates of Kuhnian and Popperian views.

I.

The paper advances two distinct sets of claims. The first set begins by addressing the sociology of science as an instance of reductionism in scientific explanations. Is Merton's (1977) sociological account of the emergence of the sociology of science itself reductionist? The author finds it is not, at least not "illicitly" so (p.1), though there is fault to be found with it on several other counts. First, Merton is said not to have given a role to ideas (those of Kuhn and

Popper in particular). Second, "each man's ideas are needed to explain their 'presence' in the sociology of science;" and this is, interestingly enough, "a material sociological thesis" (p. 1).

Having framed such requirements for assessing, sociologically, the role of key philosophical ideas in the development of the sociology of science, Jarvie advances his second set of claims. They take the form of a sociological account, competing with Merton's own, which does claim to weigh the intellectual and ideological factors. On Jarvie's alternative account, Kuhn's star rises because the substance of his thesis provides a justification for a science establishment. As the author puts it (p. 18),

...Kuhn was one of the small group of scholars being recruited to the emerging subject of the sociology of science, under the auspices of elite institutions and scientists within them, in recognition of the need of the greying science establishment to find a legitimating ideology.

Kuhn was "recruited," as Jarvie says, "as part of a pool of talent whose job was to reinforce the shaky ideology of an establishment under threat." This was possible precisely because of the illiberal character of his ideas: scientists dismiss the work of outsiders with only cursory examination; they continually re-write and falsify history, for instance (p.19). In contrast, Popper -- "boat-rocking Popper," as distinct from "the Popper Legend" (p. 20) -- was never recruited to the establishment. This too is in consequence of the character of his ideas. Popper, for example, is in "total opposition to expertise" (p. 19). Therefore, Sir Karl is, unlike citizen Kuhn, 4-F -- unfit for draft by the establishment. This is exemplified in the fact, holds the author, that despite the success of The Open Society and its Enemies (p. 17),

(t)he acclaim did not turn into establishment recognition, but rather into establishment disbelief and disapproval by establishment radicals whose social, political and academic credentials it challenged.

II.

The second set of claims, concerning the relative fates of Kuhn and Popper, is pivotal. First, however, it is necessary to consider briefly the notion of reduction at work here. For it is founded on a certain reading of the sociology of science — mistaken, in the view of this commentator — without which the claims about Kuhn and Popper are substantially affected.

Jarvie, as previously noted, finds Merton's account not illicitly reductionist. Presumably, then, there is some protocol for distinguishing reductio licita. What might that be? This unfortunately is not spelled out, though he does give as an instance of reductio illicita the attempt "to explain socially a crucial component in the success of science, namely the truth or otherwise of scientific ideas" (p. 1). Well, who does this? Not Merton himself — "innovators are not responsible for their followers" (p.3) — but rather "the radical programme in the sociology of knowledge, centered at Edinburgh" (p.2). And in what does their offense consist? Here is the bill of particulars (pp. 3-4):

- (1) scientific ideas are social epiphenomena, appearing as needed;
- (2) science citations and citation indicators are taken for granted;
- (3) intellectual success can be reduced to counting, weighing, and measuring various factors.

This, apparently, is illicit reductionism; and its use taints Edinburgh, but (somehow) not Merton, his students and colleagues. It is correct to dissociate the institutional sociology of science from (1). At the same time there are several things the matter with the remainder of this account. First, it is certainly possible to overestimate the differences between the Mertonian program and the

newer sociology of scientific knowledge, including the radical program of Edinburgh (Gieryn, 1982; cf. comments following). But to say that the Edinburgh program is essentially the carrying out of (2), the Merton and Price style of indicators research? This is a radical underestimation. (Cf. Collins, 1983.) If these practices are somehow objectionable, it is precisely against Merton and Zuckerman and the Coles and Price and Crane, et al., (and not, say, Barnes and Bloor) that they rebound.

Second, from what conceivable standpoint is the use of science indicators objectionably reductionist, as in (3)? I do not know of anyone who claims that intellectual success means nothing more than, for instance, frequency of citation or publication. Is the empirical finding that there are definite extra-deliberative conditions of science taken to indicate an attempt "to explain socially a crucial component in the success of science, namely, the truth or otherwise of scientific ideas"? If so, this rests on a misconception. Empirical findings about the extra-deliberative conditions of scientific growth — for instance, that there is systematic bias in the reward and reporting structure (Merton, 1973:439-459; S. Cole, 1970); that quantity of publication correlates in certain instances with peer judgements of quality (Blume and Sinclair, 1973); or that the scientific literature expands exponentially and has long since passed the point at which any but a small fraction can be managed by any single scientist (Price, 1975) — stand on their own merits.

Just what, if anything, such research findings can explain is not a foregone conclusion. Perhaps (however far-fetched this may appear) extra-deliberative conditions of science will comprehensively explain such major features as the success of science, its cultural authority, or key shifts in scientific ideas. Bloor and colleagues seem to think they will; Merton and colleagues tend to avoid saying, just as they generally shy away from bolder

epistemological implications in favor of modestly empirical claims; many others pour scorn on the very idea. But in any event, it is safe to say that the case is far from being made. That being so, where is the wisdom in ruling out, in sweeping and a priori terms, such research findings about extra-deliberative conditions of science? Could one imagine, for instance, simply discarding research findings about the workings of criminal justice systems simply because they fail to match the preferred self-images of lawyers? Philosophy might at least accord the same consideration to the empirical findings of social science research that it is willing to accord to those of natural science. It might be better, for instance, to approach the Price findings in the same spirit as Durkheim's claims about suicide, as if to say: here is an interesting dimension of science, a latent property of a collectivity that escapes the manifest consciousness of its members; so how then do we square the two? But it will not do simply to dismiss the findings.

Third -- here is an important issue in the paper -- there seems to be a confusion of several empirical and normative claims about science. Popper's normative claim about science (that it should operate as a liberal, deliberative community of organized skeptics, with a distinctive norm of falsification) is being contrasted with Merton's empirical claim (that the scientific community exhibits considerable deviance from its professed norms, including penumbral and multiplicative advantage effects in reward), as if Merton's empirical claim were intended to legislate and not simply describe. The suggestion that Merton thinks that it is safe to ignore what is intellectually distinctive about science -- which would be objectionable -- performs a rhetorical task for the thesis that Kuhn's allegedly illiberal model is useful for Merton but not for Popper. But is this a fair representation of the views of Merton, or any of his colleagues? I submit that it is not. Price (1975:161-195) treats the exponential growth of scientific literature under the heading "Diseases of Science;" likewise Merton, in his concern for

deviance in science (as elsewhere in society) knows that deviant activity presupposes the sharing of norms to the contrary, and that (where norms and counternorms conflict) even empirical deviance is a product and a property of the normative structure of science. As far as normative views are concerned, Merton's conception seems quite close to Popper's. Several (Barnes and Dolby, 1970; King, 1980; Mulkay, 1977:245) have even complained that Merton extends far too much empirical credit to the professed norms of science, such that Kuhn's account is the preferred alternative. Whether their complaint is fair or not, the proponents do make a good case for fundamental divergence between Merton and Kuhn.

If this is so, however, then it must seriously damage Jarvie's thesis about the differential reception of Kuhn and Popper within the sociology of science, insofar as this thesis depends on there being a drastic divergence between Merton's and Popper's ideas about science, and a convergence between Merton's and Kuhn's.

To say that Popper's normative view is being contrasted with Merton's empirical account as if the latter were also normative is not to suggest that Popper's view is only normative. Indeed, Popper conjectures that science does operate in the way it ought to operate; it is in consequence of this, for instance, that we ought to seek to explain the history of science as the outcome of its theoretical struggles, conducted according to the distinctive norms of theory choice and debate. This also is an empirical claim, and we should demand research evidence to back it up just as we demand the same from Merton's claim. As empirical, is Popper's claim incompatible with the empirical findings of the sociology of science?

If Merton et al., and Popper et al., are describing the same thing, then there is a conflict; the same scientists cannot both be simply upholding and evading the same norms at the same time. But should we assume that these two parties are comparing apples with apples? The internalist historians and philosophers who study major episodes of scientific change are, arguably, not engaging exactly the same things as the sociologists and econometricians who study the long-range and unintended consequences of the work of a variety of scientists, from the ordinary bench-bound laboratory worker eager to play with the newest technological toy or to boost his publication record (and in all cases to advance a career), to that rare researcher at the cutting edge of a major theoretical puzzle.

And who has the better part, anyway? Perhaps, if the primary goal is to understand those instances of scientific change selected as especially successful or pivotal, the internalist historians and philosophers do. If on the other hand the goal is to understand science as it normally is practiced, with its many ins and outs, then the answer is less obvious. For as it is practiced, today, science is predominantly Big Science (Price, 1963); variety (the variety which accompanies any internally stratified complex organization) and not undiluted intellectual acumen is its watchword. For every Watson or Crick, there is a larger population of scientists who are benchbound and tunnelvisioned; and, as Medawar (1982:275) observes, the ability to coordinate the activity of the diverse population of scientific animals is one of the hallmarks of contemporary science.

To be a first-rate scientist it is not necessary (and certainly not sufficient) to be extremely clever, anyhow in a pyrotechnic sense. One of the great social revolutions brought about by scientific research has been the democratisation of learning. Anyone who combines strong common sense with an ordinary degree of imaginativeness can become a creative scientist, and a happy one besides, in so far as happiness depends upon being able to develop to the limits of one's abilities.

With this "democratisation" comes differentiation; with differentiation, identity is increasingly the property of the collective structure. Without saying so, Medawar restates a classic argument ordinarily associated with Durkheim. On this view, there will no more be the explanation of science than there will be the explanation of criminality or religiosity, simply because (however these sets of activities may differ!) the activities, and the practitioners themselves, are differentiated in virtue of the differentiated social structures in which they operate. Thus for some aspects of scientific activities, it may be terribly illuminating to consider extradeliberative conditions; for other aspects, though such conditions may be presupposed, it may be much less interesting or important to make them bear much explanatory weight.

Thus (if Medawar is correct) given what we know of the social character of Big Science, there is at least one way of worrying about sociology and reductionism which is utterly chimerical.

IV.

Jarvie holds that Kuhn and Popper were differentially received within the sociology of science, in consequence of the different ideas they hold. But since these ideas are not markedly distinct at the relevant points, we might seek elsewhere for an explanation of their differential reception. I suggest that this is due not to different ideas but to different opportunity structures, specifically, those provided by the elite universities in the United States and Great Britain, respectively.

In the first place, there are several things which are odd about the claim that Kuhn becomes the ideologue for the scientific establishment, whereas "boat-rocking" Popper foreclosed that possibility by alienating "establishment radicals" (p.17). Absent specific references, it is hard to know what to make of these "establishment radicals;" but whoever, or whatever, they might be, it is clear that their influence did not preclude that very substantial elite recognition which accompanies a knighthood -- and especially one which singles out a philosopher. Popper and Kuhn both enjoy substantial establishment connexions, and in both cases these connexions have helped their work become well-known. Howbeit, is there not superficially a better case for Popper than for Kuhn as the ideologue of a scientific establishment? How likely is it that any modern establishment would publicly justify its right to exclude outsiders, or to falsify history -- whether or not they could claim the backing of an historian? The words of a prominent survivor, "don't bet the ranch on it," leap to mind.

On the face of it, there is considerably more appeal in the heroic image of pure charisma whereby seekers after truth (or at least verisimilitude) soberly take upon themselves the burden of falsification norms, where weaker mortals impose lesser burdens. As a justificatory myth, by the way, this is not too bad; and if we must have mythology, is Prometheus not preferable to Animal Farm (in which all intellectual animals are equal, only some more so than others)? If potential utility alone settled the issue, Popper's account would be the clear winner. His story offers a much more flattering self-image than does Kuhn's. And, although it is certainly plausible to believe that, sub rosa, a widespread gentleman's agreement that normal science lacks the stringent critical standards (normative on Popper's view) can only comfort powers-that-be, such a gentleman's agreement it is just the sort of thing not likely to enjoy public airing. So the matter of who does what for whom, and how, is as usual rather complicated.

In respect of their empirical characterizations of how social norms of authority normally operate within science, Kuhn is no doubt closer to Merton than he is to Popper. But this is not simply in virtue of Kuhn's claims about authority being empirical and sociological in character, but rather in virtue of the kinds of empirical and sociological claims he makes, in contrast with the kinds of empirical and sociological claims Popper makes on the same subject. One side of Popper's story is his heroic, Promethean image of the pure and unfettered charisma of Reason. But charisma is commonly routinized, in institutions and paradigms and traditions and the like. The other side of Popper's story is that he does find a place for a rational theory of routine cognitive closures (1963:120-135), and insists on the importance of distinguishing the rational merits of various traditions of closure; though (as he complains in his autobiography — 1974:1170-1172) some of his readers are slow to recognize this facet. (Cf. MacIntyre, 1980:66-74). The upshot is that Kuhn and Popper offer different sociological accounts of rational closure within science.

Why then should Kuhn, and not Popper, have been associated with the sociology of science? There is, so to say, a "pull" side and a "push" side to this question — pull referring to the factors drawing the historian into the sociology of science, push referring to those forcing him into that mold. As for the pull side, at least part of the answer lies in the fact that the embryonic sociology of science (and it was, like an embryo, unobtrusive if steadily growing until roughly the 1970's) was, like Kuhn and unlike Popper, an American offspring. The key is that we are not dealing with "the" establishment, but with two establishments; and they are not always coordinate. Here we should look to the different opportunity structures provided by the respective educational establishments in America and Britain, and the respective accidents of biography governing the uses made of them.

When we attend to these respective opportunity structures and their uses, we find that, as Merton documents (1977:71-109), Kuhn was recruited as a young man and was accorded, right from the start, the very considerable advantages that Harvard and related components of the American academic establishment can offer. Popper, as Jarvie notes (p. 17), waited decades before he "began to accumulate the academic and political honours that a Mertonian recognition of talent should have been bestowing." Well, perhaps — if, like Kuhn, he had been recruited as a very young man and passed along through the comparable pathways of a single establishment. Popper's career was abnormal, as were those of many of his generation. Though he began well in Vienna, he languished in New Zealand throughout the war. His teaching load was demanding, and the authorities at his university made it clear that they regarded his time spent on research as theft from working time for which he was paid! (Popper 1974:95) Yet (given sufficient time) the British academic elite is quite capable of assimilating talented social thinkers from abroad, whether that of an emigre like Popper or a visitor like Ralf Dahrendorf (Scott, 1984). A full two decades did elapse between the time that Popper received his Ph.D. and began to earn a living as a secondary school teacher and his reception, in 1949, of a professorship at London where he was at last encouraged to do research. However once the war was over he was able, by his own account, to utilize a network of connexions within the British academic establishment. It was simply not the academic establishment most in touch with the development of the sociology of science, of which Kuhn was the very scion.

On the push side, what does it mean that Popper could have been Merton (or Medawar, Durkheim) and yet was not? Following the second world war, social thought became sociology — more precisely, it became almost exclusively Professional Social Science — in the United States. This was especially true of its

elite universities (Harvard, Yale, Chicago; Columbia, Berkeley, etc.). Social thought did not so develop in the elite universities of Britain. The causes of this divergence are complex. They would have to include differential association of sociology with social work; the relative presence of social democratic politics, in the context of more or less pronounced class divisions, and differing attitudes toward the expansion of mass education. British sociology expanded during the postwar era, but it did so primarily in the redbricks and outlying universities and, of course, down in London. But even the visits, in the mid-1950's, of representatives of the American social scientific elite (Parsons and Homans) did not encourage — the general opinion is that they actively hindered — the reception of sociology at Oxford and Cambridge. Even today there are only a handful of sociologists there.

Thus it is not at all surprising for Merton (1977:115n9) to report:

It meant much to me to have (Joseph) Needham remark, in a kind review of my dissertation, that it "exhibits a quantitative sense unusual in an historian." That Needham should have described the author of the book as an historian...is not altogether strange. Sociologists were then largely an alien academic breed in England — suspect if not downright illegitimate within the precincts of Cambridge and Oxford, their strange doings largely confined to the London School of Economics. Although he delivered the Herbert Spencer Lecture in the mid-1930's, even so unorthodox a scientist as Needham, then Dunn Reader in Biochemistry at Cambridge, would seldom have encountered an academic sociologist in the flesh.

Even prominent social thinkers might remain aloof from professional sociology. Thus it is not entirely strange to find Stephen Toulmin, erstwhile student of physics and philosophy at King's, Cambridge (where Popper, incidentally, was a familiar presence during the 1940's) and as conversant as anyone with modern social thought, confusing Robert Merton and Thomas Merton (Toulmin, 1972:99; Merton, 1977:124n54). If a student of mine did that, I would want to be sure that he was sufficiently familiar with the works of both Mertons

not to confuse them again! Surely Toulmin was not unaware of the difference; but this slip of the pen may well betoken how prominently the leaders we American sociologists take for granted figured into the intellectual horizon of British social thought. In this context it is worth noting that it was not even Merton, but Kuhn who in the seminal 1965 symposium at Bedford College was chosen to receive comeuppance for having violated the 13th commandment (as vouchsafed to Auden): "thou shalt not sit, With statisticians nor commit, a Social Science." Nor, given this climate of opinion, should there have been any surprise that one of the literary events of 1970's Britain was the appearance of Malcolm Bradbury's The History Man, a novel which traces the meteoric rise of an appallingly opportunistic and unprincipled sociologist as he rends the lives of various friends and acquaintances. The book is (characteristically, for Bradbury) a biting funny read; unfortunately many seem to have taken it for journalism rather than fiction.

In strategic places, sociology was just not on. Do these respective features of intellectual and university life in America and Britain mean that ideas about science and society did not expand in the latter? No, merely that they took different paths, through different opportunity structures, employing different rhetorics. In Britain no less than in America public discussion of science and society was institutionalized in established circles. It was perfectly all right for Popper, or Medawar, or C. P. Snow or John Ziman or others to carry on about such matters. It was not politic to call it sociology, nor, apparently, to associate with those who did. In the rhetoric of the British academic establishment — the case was very different in the American establishment — the term "sociology" was a Schimpfwort reserved as a marker, not for social thought made systematic, but for irresponsible relativism or whatever else deserved stigma. In this there is no

necessity. It is simply a custom, grown up of happenstance usage within the particular possible world we inhabit.

From the point of view of social studies of science, this is not the best of all possible worlds. Yet I am forced to conclude that it is this accident of usage, and not the perception that Kuhn's ideas were congenial to an establishment while Popper's were not, to which we owe the differential reception of historian Kuhn as a sociologist and philosopher Popper as not.

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