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The principal thesis of the sociology of knowledge is that there are modes of thought which cannot be adequately understood as long as their social origins are obscured.

Why and how should one study the history of science? An extraordinarily diverse number of answers can be given to these seemingly innocuous questions. One may wish to gain an increased understanding of some contemporary scientific theory; display the merits of some political system or culture in which science is practised; exemplify some epistemological thesis; look at changes and developments in human culture across the globe; pursue an antiquarian hobby; provide information that may be thought useful in bringing about political change; support some thesis about human progress (or degeneration), ... Clearly, the kind of history written will depend on the objectives; and the means adopted will be likewise dependent. Moreover, the product will inevitably be shaped by the presuppositions — metaphysical, ethical, aesthetic, political, methodological, or whatever — that the historian brings to his task. In the present review, I wish to discuss one broad area of concern, namely the current interest, among Western historians of science, in the social history of science, and the implications of the theses of the sociology of knowledge for the study of the history of science.

Early work in the history of science, often carried out by scientists-turned-historians, was commonly influenced by positivism.¹⁾ The historian's task was seen as providing a full and accurate chronicle of the development of science, and thereby mankind's progressive intellectual development might be displayed. Rather little attention was given to what was regarded as the 'external' social context within which scientific activity proceeded. Indeed, science was seen, by its very nature, as an enterprise capable of generating knowledge that was 'context-free' and of ever-increasing truth content. Such an assumption is, I would say, itself ideological in character, and it was soon supplemented and then largely superseded. In the 1930s, for example, the notable French historian, H  l  ne Metzger,²⁾ produced remarkable new insights by attending to the hitherto unexplored philosophical presuppositions of early chemists. But an 'intellectualist' approach to the study of the history of science was maintained in her work, and was continued by others under the

1) The work of George Sarton is characteristic. See T. Frangsmyr, 'Science or history: George Sarton and the positivist tradition in the history of science', *Lychnos*, 1972/3, pp. 104-144.

2) On Metzger, see particularly three articles, by B. Vincent-Bensaudo, J. Golinsky and J.R.R. Christie in *History of Science*, 1987, 25(1).

influence of authors such as Alexandre Koyré, who emphasized the 'internal' development of scientific thought, within the context of its "intellectual and spiritual milieu".³⁾ It is true that, even pre-War, under Marxist influences certain Western authors such as J.D. Bernal gave much attention to the social structure of science and its social functions.⁴⁾ Particularly interesting was the work of the Russian, Boris Hessen, who, using the classical Marxist 'base/superstructure' model to assist his analysis, seriously raised the possibility that Newton's thinking was, at bottom, a product of the 'external' socio-economic system of which he was a part, rather than a purely intellectual construction. Thus, Hessen suggested, the agenda for the *Principia* was actually laid down by the external society, with its interests in gunnery, navigation and the like.⁵⁾ However, the 'Hessen thesis', though stimulating, was regarded as too crude to provide an adequate framework for the study of the social history of science and it was a considerable time before 'external' factors were given much consideration by Western historians of science.

In the post-war period, changing views of the nature of science have continued to dictate changing fashions in the study of its history, and the two have lived together in a kind of symbiotic relationship. Philosophers of science such as Popper or Lakatos have looked to science for support for their philosophical theses.⁶⁾ Others such as Kuhn have ostensibly generated their philosophies to a large extent on historical case studies.⁷⁾ Historians, such as Joseph Agassi, have explicitly set out to study the history of science under the aegis of a particular set of philosophical assumptions, there by displaying (or appraising) the merits of those assumptions.⁸⁾ All this symbiotic activity has, I think, been generally beneficial, both to philosophy (especially epistemology), and to historiography. It has caused new questions to be posed to both philosophers and historians, and it has led to an increased understanding of the way science functions. But some of the results have been rather surprising, even paradoxical, and the implications for the study of the history of science have been profound. I want to explore some of these matters briefly in the present review.

Needless to say, there can never be a 'total history' of any aspect of human endeavour. If there were such, it would be a useless monstrosity — like a map that is a complete replica of a terrain. Consequently, all history is necessarily written from some point of view. But some points of view are more useful, interesting and important than others. And

3) See: I.B. Cohen, 'Alexandre Koyré (1892-1964)', *Isis*, 1966, 57, pp.157-165.

4) See: J.D. Bernal, *The Social Function of Science*, Routledge & Sons, London, 1939.

5) B. Hessen, 'The social and economic roots of Newton's *Principia*', in: N. Bukharin (ed.), *Science at the Cross-Roads: Papers Presented to the International Congress of the History of Science and Technology held in London from June 29th to July 3rd, 1931 by the Delegates of the U.S.S.R.*, Kniga, London, pp.147-176.

6) For example: K.R. Popper, 'Science: Conjectures and refutations', in *Conjectures and Refutations: The Growth of Knowledge*, Routledge and Kegan Paul, London, 1963, pp.33-65; I. Lakatos, 'Falsification and the methodology of research programmes', in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970, pp.91-195.

7) For example: T.S. Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*, Harvard University Press, Cambridge (Mass.), 1957; and *The Structure of Scientific Revolutions*, Chicago University Press, Chicago, 1962.

8) See: J. Agassi, 'Toward an historiography of science', *History and Theory*, Beiheft 2, 1963.

we are offered a distorted and misleading picture of the past if some perspectives are deliberately or inadvertently excluded. I think this happened when historians' attention was focussed almost exclusively on the internal, intellectual history of science — with just the leavening, perhaps, of *scientists'* major concerns within the social system of science (such as priority disputes). This is not to say that we are now asking all the right questions. Doubtless, we have become obsessed with some insignificant issues, or are ignoring matters of importance. Nevertheless, I believe that we are now beginning to get a clearer understanding of what constitutes scientific knowledge, and how the system of science actually works. Moreover, history of science is, today, in no sense a kind of 'apology' for science, as it was to some degree in the heyday of positivism. Indeed, it may appear to some as a somewhat subversive enterprise.

As soon as one raises one's sights above and beyond the study of the 'internal', intellectual development of scientific thought, a host of new, interesting and useful questions present themselves. We may have studies of science education, science publication, the social interactions of scientists, the scientific bureaucracy, science and religion (which was, in fact, studied very early on), the relationship between science and the arts, science funding, scientific societies, the dissemination of scientific knowledge from one culture of nation to another, science in warfare, the relationship between science and government, social behaviour in laboratories, science policies, the emergence of new specialisms, the 'power politics' of scientific research, the relationship between science, technology and industry, and so on and so forth. Such matters are all, in their different ways, of highest and importance for understanding human culture as a whole. They are different aspects of the social system of science. But they are not, *per se*, philosophical matters. And what concerns me, in the present review, is the new insights for understanding science yielded by consideration of the social aspects of human cognitive activity. We shall find, then, that the development of theoretical studies in the 'sociology of knowledge' reaches round to generate new and interesting questions for historians of science; and study of these questions feeds back into our understanding of how human knowledge is acquired and developed.

Questions about the nature of knowledge have been asked since antiquity, of course. But it was not until the nineteenth century that Marx (supported by Engels) initiated a sociological discussion of epistemology when he suggested that people's notions of reality were systematically distorted by their ideologies. For example, he held that the social classes with power induce distorted views of reality in the minds of the less powerful in order to promote the interests of the ruling classes.⁹⁾ This suggestion was extended by Karl Mannheim, who claimed that all human beliefs about man and society were shaped by the society within which they were generated, and had social functions of one kind or

9) See, for example: K.Marx and F.Engels (ed. C.J.Arthur), *The German Ideology*. International Publishers, New York, pp.64-65 and *passim*.

another.¹⁰⁾ This might apply, for example, even to Marxism itself.

Unfortunately for Mannheim, in emphasizing the importance of the sociology of knowledge, he was immediately hoist with his own petard. Why should his own theory be free from ideological constraints, and not offer a distorted view of reality? With utter implausibility, he suggested that modern intellectuals, having no specific class allegiances and being accustomed by virtue of their education to view the world from a number of different perspectives, were a disinterested, self-aware group, whose beliefs were not significantly distorted by ideology and who might therefore be capable of producing an objective analysis of society and a 'science' of politics!¹¹⁾ More seriously, he held that whereas religious thought (say) was through-and-through ideological, science, mathematics and logic were forms of knowledge which could escape the ideological critique. Leaving aside here mathematics and logic, why should science be exempt from social constraints? It was so, Mannheim held, because science was "largely detachable from the historical-social perspective[s] of the investigator[s]",¹²⁾ and thus it might be presumed to be capable of generating 'objective' knowledge, free of the taints of ideology. The empirical study of nature, supported by the use of logical reasoning and the critical activity of scientists, supposedly yielded a form of knowledge that immune to the imposition of social constraints. It could be value-free and objective, even if not necessarily true — for scientists may obviously make mistakes sometimes. Indeed, the fact that scientific knowledge is constantly subject to revision would seem a virtue, not a vice, and a defence against any suggestion that it is dogmatic or ideologically constrained.

Mannheim was, of course, writing in the thirties at the high-tide of positivist philosophy of science. Developing out of the writings of authors such as Pierre Duhem¹³⁾ and Norman Campbell,¹⁴⁾ scientific theories had come to be understood as deductive systems with 'free-floating' axioms (or hypotheses), from which theorems (which gave rise to testable consequences) might be deduced. For the testing of the theorems suitable 'correspondence rules' needed to be invoked. For example, one might describe a gas in the language of everyday experience (in an 'observation language') as being of such-and-such a colour, volume, etc. And one might describe it also (in a 'theoretical language') in terms of molecules of certain masses, moving with certain velocities, etc. So suitable 'correspondence rules' would be required to express, for example, the temperature of the gas in terms of the kinetic energy of its constituent molecules. The positivist would also say that science proceeds by the successive proposal of improved hypotheses (or theoretical postulates, or 'axioms') and the empirical testing of the consequences of these hypotheses. Successful testing, and the vindication of predictions, would lead to gradually improved

10) K. Mannheim (trans. L. Wirth and E. Shils), *Ideology and Utopia: An Introduction to the Sociology of Knowledge*. Harcourt Brace Jovanovich, New York and London, n.d. This volume contains a number of Mannheim's writings. The first German edition of *Ideology and Utopia* was published in 1929.

11) *Ibid.*, pp. 154-155 and *passim*.

12) *Ibid.*, p. 290.

13) P. Duhem (trans. P. P. Wiener), *The Aim and Structure of Physical Theory*. Atheneum, New York, 1977 (first French edition, Paris, 1906).

14) N. R. Campbell, *Physics: The Elements*, Cambridge University Press, Cambridge, 1919.

theoretical understanding, and thereby an improved 'instrumental' control over nature. All this might not take place quite as nicely and tidily as just described, but in principle that was the way science worked; and if perchance it did not do so at the research front, but operated in a much more haphazard fashion, the positivist philosopher of science would be happy to provide a satisfying 'rational reconstruction' of the history of the development of scientific theories 'after the event', so to speak.

The positivists' construal of the structure of scientific theories, which had some *prima facie* plausibility, has come to be known as the 'received view'.¹⁵⁾ We should particularly note the clear-cut distinction between theory and observation. And observation was thought to be unproblematical, generally being intersubjectively testable and repeatable — certainly not subject to ideological constraints!

This 'received view' of scientific theories was a species of 'foundationism'.¹⁶⁾ It was supposed that there were at least two areas where the human mind could grasp the truth: in mathematics and logic (needed for the deduction of the scientific theorems from the theoretical axioms); and in observation, there supposedly being *correspondence* between thoughts about the world, based on observations, and states of affairs in the world (facts). To be sure, theories themselves could not be known to be true. The origin or source of hypotheses is uncertain. And the truth of theorems, as determined by observations aided by correspondence rules, does not logically entail the truth of the relevant axioms or hypotheses.¹⁷⁾ On the other hand, there was no reason to suppose that *social* factors were confusing matters. Logic/mathematics and observation were deemed adequate foundations for the construction of scientific knowledge. Hence, in the heyday of logical empiricism, Mannheim could feel some confidence in the objectivity of science and could suppose that it was free of ideological constraint.

There were, however, clouds on the horizon. As far back as the beginning of the century, the sociologist Emile Durkheim, working with the anthropologist Marcel Mauss, had suggested that men's systems of classification were dependent on their forms of social life.¹⁸⁾ Then in the 1930s the linguist Benjamin Lee Whorf, following ideas of his teacher Edward Sapir, suggested that people's whole cosmologies might be determined by the grammatical forms of the language systems that they employed.¹⁹⁾ Such arguments were, however, largely based on studies of 'primitive' cultures, and might conceivably be

15) For a history of the 'received view' of theories, see: F. Suppe, 'The search for philosophical understanding of theories', in F. Suppe (ed.), *The Structure of Scientific Theories*, University of Illinois Press, Urbana, 2nd edn, 1977, pp. 6-233 (esp. pp. 6-56).

16) That is, the view that human knowledge can have a certain foundation somewhere, whether it be in direct empirical observation, in innate ideas, in a suitable scientific method, or wherever. A critique of 'foundationism' has long been a major preoccupation of the work of Paul Feyerabend. See his *Knowledge Without Foundations: Two Lectures Delivered on the Nellie Heldt Lecture Fund*, Oberlin College, 1961.

17) This flows from the so-called 'fallacy of affirming the consequent'.

18) E. Durkheim and M. Mauss (trans. R. Needham), *Primitive Classification*, Cohen and West, London, 1963 (first French edition, 1903).

19) B. L. Whorf, *Language, Thought and Reality: Selected Writings of Benjamin Lee Whorf*, MIT Press, Cambridge (Mass.), 1956.

thought irrelevant to the case of advanced 'scientific' societies, which had the advantage of knowing how to depoly value-free scientific procedures for the acquisition of knowledge.²⁰⁾

But then positivism found itself attacked from within. Ludwig Wittgenstein, whose great work the *Tractatus Logico-Philosophicus*²¹⁾ had been an inspiration to the positivist philosophers of the logical-empiricist school, began to have second thoughts, expressed initially to his students at Cambridge in the 1930s, and eventually in his remarkable posthumous *Philosophical Investigations*.²²⁾ Wittgenstein came to the conclusion that the meaning of language could only come about and be understood in its *use*; and further that a correspondence theory of truth, such as he had endeavoured to sustain in the *Tractatus* was untenable. For if one apprehends the world through the medium of one's language and with the help of one's scientific theories, where can there be an independent vantage point from which one may compare the world with its descriptions (be they ever so scientific), and hence ascertain the truth of scientific statements? To be sure, science is useful to man; and the meaning of scientific language becomes manifest in its use, just like any other language. But regrettably the utility and pragmatic success of science offer no guarantee of the objective truth of scientific knowledge, or of correspondence between the world and theoretical descriptions of the world.

Two other major considerations led to the downfall of the positivist model of science. One of these was to be found in the work of Duhem himself, but its significance for positivism was not immediately recognized. Duhem pointed out²³⁾ that falsification can never be carried out unambiguously. No hypothesis can ever be freed from 'auxiliary' assumptions in the process of its testing. So if, in testing a hypothesis, observations are found to conflict with the expectations, then one cannot be *sure* where the problem lies. It may be the hypothesis supposedly under test that is at fault; *or* the problem may lie in the background auxiliary assumptions. One can, therefore, only make 'holistic' empirical tests, not unambiguous empirical falsifications of specific hypotheses. (And absolutely certain empirical confirmations are not possible either by reason of the 'problem of induction'.) Duhem's point was subsequently taken up by W.V.O. Quine, who maintained that *any* experimental result may be regarded as confirmatory of an hypothesis, for in principle it is always possible to modify the background auxiliary assumptions in such a way as to account for the observations made.²⁴⁾ Putting it another way, it may be said that theories are always, and in principle, 'underdetermined' by the empirical evidence: no amount of favourable evidence proves a theory (because of the problem of induction); and an indefinite number of theories of hypotheses can always be formulated to account for a given

20) Actually, Durkheim, like Mannheim, wished to sustain a 'privileged' region which would be immune from its own critique.

21) L. Wittgenstein (trans. D.F. Pears and B.F. McGuinness), *Tractatus Logico-Philosophicus*, Routledge and Kegan Paul, London, 1961 (first German edition, 1921).

22) L. Wittgenstein (trans. G.E.M. Anscombe), *Philosophical Investigations*, Blackwell, Oxford, 1953.

23) P. Duhem, *op. cit.* (note 13), p.187.

24) W.V.O. Quine, 'Two dogmas of empiricism', in: *From a Logical Point of View: 9 Logical-Philosophical Essays*, Harper, New York, 1953, pp.20-46 (at p.43).

set of observations (the problem of the underdetermination of theories).

The second large nail in the coffin of positivism derives from problems in the psychology of observation. There have been numerous historical case studies showing that scientists' observations are moulded by their theoretical presuppositions.²⁵⁾ Further, it has been argued on theoretical grounds — perhaps most persuasively by N.R.Hanson²⁶⁾ — that one can never 'see' things neutrally. One always 'sees them *as*' something or other. Thus the process of seeing or observing will always be circumscribed (or 'infected') by the observer's theoretical presuppositions, his language system, or indeed his very perceptual apparatus;²⁷⁾ and also, of course, his social formation[s]. If all this be so — and it appears to be a point on which historians, philosophers, linguisticians, psychologists, and even neuro-physiologists are in reasonably happy agreement — then the positivist distinction between the languages of theory and of observation becomes quite untenable. It follows, then, that we must look for a different philosophy of science altogether.

Given the foregoing impasse, it is scarcely surprising that a great deal of effort has been given to the reconstruction of philosophy of science in a way that takes account of arguments such as I have very briefly outlined above, but which also acknowledges the undoubted pragmatic success of science. I shall not, in this brief review, attempt to do more than outline one general approach that has attracted considerable attention, and not a little criticism, and I will then attempt to indicate and assess its implications for historians of science.

Let us disallow Mannheim's exclusion of the natural science from the clutches of sociological analysis, and see where this leads us. Let us argue that *all* knowledge is socially mediated, and thereby bears the mark of its historical, cultural, and linguistic determinants. Thus we may hold that the world, *as it is known to us*, is a socially constructed reality, for there is no way in which we can transcend our social circumstances and know the world as it is 'in itself.'²⁸⁾ There can be no 'Archimedean point' from which we may compare the real world with our socially constructed understanding of it. (This is Wittgenstein's point, really.) What *counts* as scientific knowledge, then, is what the scientific community itself sanctions — that is, what it publishes in its journals and teaches in its educational institutions. It is futile to point to the consensus among scientists as evidence for the truth of the claims of science. For it is social consensus which is, in fact, the source of the knowledge claims. To use Kuhn-like language, it is futile to say that people adhere to a paradigm because it is true. Rather, one might say that a paradigm is regarded as true because people adhere to it. (Similarly, the existence of God is not

25) An excellent example is described in: R.H.Austin, 'Uranus observed', *British Journal for the History of Science*, 1967, 3, pp.275-284.

26) N.R.Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science*, Cambridge University Press, Cambridge, 1958.

27) This has long been known to biologists, following the classical investigations of Jakob von Uexküll, See J. von Uexküll (1934), 'A stroll through the worlds of animals and men', in: C.H.Schiller (ed. and trans.), *Instinctive Behaviour: The Development of a Modern Concept*, International Universities Press, New York, 1957, pp.5-79.

28) Cf. the Kantian notions of *Noumenon* and *Ding an sich*.

demonstrated by widespread theism. Rather, people think there is a God because so many of their fellow-men think likewise, and they are educated in the belief.)

So whereas early theorists of ideology might suggest that erroneous thinking about the social world might be attributed to social factors (the doctrine of 'false consciousness'), or that 'bad science' might be the result of (say) external political pressures or the intrusion of religious beliefs, the modern sociologist of knowledge argues that we must regard all knowledge, scientific or otherwise, successful or unsuccessful, good, bad or indifferent, as inherently social in character. It follows, then, knowledge is not (say) 'true belief', but simply 'whatever we take to be knowledge'. There arises, from this definition, a huge and interesting programme of empirical enquiry for the historian of science. No longer will he be limited to the study of the intellectual production and transmission of theories, of experimental work in the laboratory or the field, of the production of scientific instruments, or the history of scientific controversies. Nor will he simply enquire into the structure of a scientific community, or scientific funding, or whatever, as objects of enquiry for their own sake. If the objective of the historian of science is to study how scientific knowledge is socially constructed, then all the types of questions listed on page 103 will come under consideration; and others too. But the point and purpose of the enquiries will be significantly shifted. The guide throughout will be the determination of how people come to represent nature the way they do by virtue of their social situations; how scientists '*construct*' a picture of reality and persuade others of the acceptability of their constructs; how the 'frames' by which, or according to which, they see the world are socially/linguistically/theoretically/empirically constructed. Such a programme of enquiry has been dubbed the 'strong programme in the sociology of knowledge' by one of its leading exponents, David Bloor.²⁹⁾ Unlike Mannheim, Bloor has no qualms about being hoist with his own petard. Indeed, he embraces the idea with apparent enthusiasm, being quite content that his program be 'reflexive', or applicable to itself.

For some critics, perhaps most notably A.G.N.Flew,³⁰⁾ this reflexivity is an indication of the futility of the whole enterprise. If all knowledge is 'relative', if there is no foundation for knowledge other than the social system within which it is generated, then philosophy of science has, on Flew's view, spawned a monstrosity. We have an endless regress of accounts about accounts. The knowledge-claims of philosophy of science can scarcely be taken seriously if they themselves are contingent products of some social formation, not visibly wearing a badge of transcendental truth.

It would be tempting to enter this philosophical affray, but fortunately we are not required to do so on this occasion. Here, all we need do is accept the sociology of knowledge thesis *pro tem.*, and ask what interesting questions follow thereby for practitioners of history of science. We will find, I believe, that many fascinating lines of enquiry unfold, and we can see more clearly how the scientific enterprise functions in

29) D.Bloor, *Knowledge and Social Imagery*, Routledge and Kegan Paul, London, Henley and Boston, 1976, pp.2-5.

30) A.G.N.Flew, 'Is the scientific enterprise self-refuting?', paper presented to the Eighth International Conference on the Unity of the Sciences, Los Angeles, November 22-25, 1979.

practice. Thereby, we may gain an insight into the nature of scientific theories that was significantly obscured when they were regarded simply as intellectual products, generated with the assistance of empirical methods of enquiry, and immune from sociological analysis.

The theses of sociology of knowledge may be expressed in a number of different ways, not always mutually compatible with one another. But one straightforward way to look at the matter is to regard social concerns as 'intervening' between the entities of the real world and the scientific/theoretical (or common-sense) descriptions of these. By virtue of the thesis of underdetermination of theories, discussed above, it will always be possible to offer more than one theoretical description of a set of phenomena. This being so, we can look to social factors to decide which account is preferred at any given time and place, and possibly also to explain how a given account comes to be propounded in the first place. On examination of these social factors, the historian may seek to gain an improved understanding of what actually happens in the development of scientific knowledge. Given that there is a kind of competition between alternative theoretical descriptions of the world, we may think of scientists as seeking to 'construct' accounts of reality and get *their* version accepted as true by their peers and by the wider community. Thus this version of the sociology of knowledge is commonly called '*social constructivism*'.

So what kinds of 'intervening' or mediating factors might the historian of science look to in examining the social fabric that intervenes between the world and our descriptions of it? One popular view, deployed particularly at the University of Edinburgh (where Bloor teaches), supposes that the guiding notion to explain the process of social intervention or mediation is that of '*interests*'.³¹⁾ This appears to derive from the work of the German philosopher Jürgen Habermas,³²⁾ though the meaning given to the term seems to be somewhat different on the Continent and in Britain. Habermas locates the source of truth, for us, in our social community, and holds that it should be by free discussion within a community that decision is made as to which statements, theories, etc., are true and which are false. Truth, then, is *relative*, since different communities have different conceptual frameworks. In Habermas's ideal of a totally free-speech community, one may hope for a kind of asymptotic approach to an 'ideal' condition of truth. But in practice free communication is constrained, and personal or sectional interests intervene, distorting the understanding of reality.

One does not have to accept the totality of Habermas's exceptionally complex philosophical system to recognize that the notion of 'interests' will appeal to the historian concerned with the social analysis of science. On the contrary, the social-constructivist may perfectly well pursue his empirical enquires, on the look-out for the role of interests in the construction of knowledge, without being a thoroughgoing devotee of Habermas. And this seems to have happened amongst a number of British and American social historians of

31) The idea has been developed particularly by one of Bloor's Edinburgh colleagues, Barry Barnes: *Interests and the Growth of Knowledge*, Routledge and Kegan Paul, London, Henley and Boston, 1977.

32) J. Habermas (trans. J. J. Shapiro), *Knowledge and Human Interests*, Heinemann, London, 1972 (first German edition, 1968).

science. So what social interests might be looked for?

Needless to say, a great many persons seek power — either in the scientific community, or within the wider social frame. With power, the scientist can get *his* version of reality accepted. The ways of gaining power are legion, and many of them have been identified, for political life, since the time of Machiavelli. Features that are particularly important for the sociology of scientific knowledge are matters such as the following. How do people gain reputation, and hence preferment? How do they gain rewards, grants, prizes, honours? How do they gain credence for their views? How do they promote their interests — either cognitive, academic or social? What social formations do they seek to construct in order to further their interests? How do they promote their views by teaching, devising syllabuses, publishing, refereeing, writing testimonials or providing employment for their students or supporters, reviewing, editorial work, arguing at conferences, in professional journals or the media? How do they lobby governments or interact with industry or military? How do they seek to make *their* views seem both natural, appropriate and important? How do they present their views to their peers or the public, so as to show them to the best advantage?

Answers to such questions³³⁾ will differ for different times and places. In the nineteenth century, in the age of gentlemanly specialists in Europe, a goodly sum of private money was of the greatest help, and one such as the geologist, Sir Roderick Murchison, deployed his wealth and connections with consummate skill to further his views. Sometimes, one may find naked political opportunism, as (in an extreme case) in the career of the Russian agrobiologist, T.D. Lysenko, aided and abetted by Joseph Stalin. Or one may point to an 'entrepreneurial' scientist, such as Louis Pasteur, who sought to promote his theories and techniques through their commercial application. For Justus von Liebig, on the other hand, his way was to propagate his views with the help of a successful research school, whose students would disseminate and promulgate the teacher's theories and techniques.³⁴⁾ Michael Faraday promoted his picture of physical reality by the excellence of his public lectures. The geologist, Charles Lyell, wrote great treatises, as did Charles Darwin. Thomas Henry Huxley was active in the reform of science education in Britain, and wrote text-books for the new curriculums.

All these points may seem rather obvious, but they contribute to an interesting general picture about the nature of the scientific community when viewed through the framework of the interests model. One can also apply a further theoretical notion from the Habermas vocabulary: '*legitimation*'.³⁵⁾ By numerous techniques, scientists seek to 'legitimate' their

33) These questions are not, by any means, the exclusive concern of modern students of the sociology of knowledge or the sociology of science. They were, for example, considered by R.K. Merton and his followers. However, the 'old' questions can have new significance when posed within the framework of (say) the 'interests' model.

34) See: J.B. Morell, 'The chemist breeders: the research schools of Liebig and Thomas Thomson', *Ambix*, 1972, 19, pp. 1-46.

35) The appropriateness of the term may be seen from the following quotation from Habermas: "Legitimacy means that there are good arguments for a political order's claim to be recognized as right and just; a legitimate order deserves recognition. *Legitimacy means a political order's worthiness to be recognized.*" (J. Habermas, *Communication and the Evolution of Society*, Beacon Press, Boston, 1979, p. 178.

theoretical claims: that is, they endeavour to make them seem natural and appropriate, such that any right-thinking person would naturally hold them. One may suggest, therefore, that a study of the ways in which scientists seek legitimacy for their views make an admirable topic of enquiry for the social historian of science. The study may, of course, be applied to groups of scientists as well as individuals. For example, the men who succeeded in forming the Royal Society sought the support of the King as a symbol of their social legitimacy, and the rightness of their actions and their political views. When these were criticized, Thomas Sprat published his history of the Society, seeking to give it legitimacy in the public mind.³⁶⁾ Aristocrats were, of course, more welcome in the early Society than commoners for they enhanced its social standing, thereby 'legitimizing' it.

A useful concept, related to the notions of 'legitimation' and power, is that of 'hegemony', propounded by the Italian Marxist philosopher, Antonio Gramsci. As Gwyn Williams has put it, a condition of 'hegemony' is: "an order in which a certain way of life and thought is dominant, and in which one concept of reality is diffused throughout society in all its institutional manifestations, informing with its spirit all taste, morality, customs, etc."³⁷⁾ Thus a particular social class has achieved a state of hegemony when it has succeeded, by whatever means, in persuading other classes to accept its own values. Thinking in these terms, Morris Berman has given a valuable account of how the tradition of the 'gentleman-amateur' was successfully inculcated in British science in the nineteenth century.³⁸⁾ The notion of hegemony as a historiographically useful concept has been extensively utilized by Roger Cooter in a recent study of the history of phrenology in the nineteenth century.³⁹⁾ It can also be a most useful theoretical tool for understanding the relationship between science in a colonizing country and in its colonies.

Looking to more modern times, it is also most instructive to examine scientists' career profiles and the way in which they typically gain preferment by the acquisition of credentials, by seeking powerful 'patrons', by publishing, citing and being cited. It soon becomes evident that nothing succeeds like success, and one encounters a kind of feed-back situation such that scientists of good credit attract more funds, which enables them to conduct and publish more research, and thereby gain more credit — and power.⁴⁰⁾ This 'credit' may be 'reinvested' in the establishment of new research projects; and hence we have what Latour and Woolgar, in an elaborate study, have aptly termed 'cycles of

36) T.Sprat (ed. J.I.Cope and W.H.Jones), *History of the Royal Society*, Routledge and Kegan Paul, London, 1966 (1st edn., 1667).

37) G.A.Williams, 'The concept of 'egemonia' in the thought of Antonio Gramsci: some notes on interpretation', *Journal of the History of Ideas*, 1960, 21, pp.586-599 (at p.587).

38) M.Berman, "Hegemony" and the amateur tradition in British science', *Journal of Social History*, 1975, 8, pp.30-50.

39) R.Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organization of Science in Nineteenth-Century Britain*, Cambridge University Press, Cambridge, 1984.

40) This common phenomenon is commonly called the 'Matthew effect', from the Biblical adage (in the Gospel according to St Matthew, Chapter 13, Verse 12): "For whosoever hath, to him shall be given, and he shall have in abundance: but whosoever hath not, from him shall be taken away even that he hath".

credit.⁴¹⁾ Or a scientist may find himself in a degenerating field — for if nothing succeeds like success, it is equally true that nothing fails like failure. Either way, the analogy drawn from the world of commerce is very apt for describing the activities of the scientific community.

There are, needless to say, always numerous competing cycles of credit to be found in the scientific market place. Which ones gain the ascendancy and which succumb is a highly complex social matter, which is, however, subject to historical analysis if sufficient facts are known. Some of the details may usefully be discerned with the help of citation studies — or the discipline of scientometrics.⁴²⁾ Others are more distant from the direct public gaze. And hence it may be desirable for the contemporary science historian to turn 'anthropologist' and enter the laboratory to ascertain exactly what is going on there. Several studies of this kind have been made in recent years, but the best-known one is still that of Latour and Woolgar at the Salk Laboratory in California, described in their text *Laboratory Life*.⁴³⁾ Here the reader will find displayed the fascinating processes of social negotiation whereby scientists seek to legitimate their theoretical views and gain acceptance for their preferred empirical techniques. Where the historical records are plentiful, in correspondence, minutes of society meetings, etc., the historian may go some way towards capturing the intimate details of such negotiations, and some recent publications have sought to do so in great detail with considerable success. Or one can engage in kind of 'oral history' in the laboratory, using the sociologist's technique of questionnaire and interview.⁴⁴⁾ One may look particularly for the way in which scientists aim to make their findings be regarded as uncontroversial — part of the generally-received wisdom. They are, as has been said, seeking 'legitimacy'.

Drawing on the work of the French scholar, Pierre Bourdieu,⁴⁵⁾ Latour and Woolgar have invoked the notion of an 'agonistic field' — literally a 'field of contest' — to characterize the realm of social negotiation in the scientific community.⁴⁶⁾ But this agonistic field is not like an athletic field, say. It is more like an electric field, in that all regions of the field are affected by changes anywhere in the field.⁴⁷⁾ For scientists are in constant dynamic social interaction with one another, trying to get *their* version of reality accepted by the community at large. But who shall be the judge of what is to be accepted or rejected? Here we encounter a curious feature of the social system of science: there is no

41) B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, Sage, Beverly Hills, 1979.

42) This is now an extensive field of research, with its own journal: *Scientometrics*.

43) See note 41.

44) For a recent example, see: M. Lynch, *Art and Artefact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*, Routledge and Kegan Paul, London etc., 1985. Such studies may, however, yield banal results, and this complaint has been made by Latour in reviewing Lynch's work — despite the fact that Latour led the way in studies of this kind. See: B. Latour, 'Will the last person to leave the social studies of science please turn on the tape-recorder?', *Social Studies of Science*, 1986, 16, pp. 541-548.

45) See particularly: P. Bourdieu, 'The specificity of the scientific field and the social conditions of the progress of reason', *Social Science Information*, 1975, 14, pp. 19-47.

46) B. Latour and S. Woolgar, op. cit. (note 41), p. 237.

47) I owe this analogy to my colleague W. R. Albury.

external arbiter. It is rather as if a person being tried in a court of law were his own judge and jury. What happens, of course, is that the persons of high credit any given specialist scientific community act as arbiters of what is to be accepted and what rejected. Referees, editors, the controllers of funds, the leading members of scientific academies, etc., all form judgements as to the truthfulness of knowledge claims. They are the 'gate-keepers' of knowledge. It follows, then, that observations made in the laboratory or field do not themselves bear the badge of truth. They are, we may take it, theory-laden. But that is no matter. The thing is that before being accepted as true, they have to be processed and then brought to the bar of the scientific community for acceptance, rejection or modification. The 'gate-keepers' constitute the bar of the community. All this has been admirably demonstrated in a recent masterly study by M.J.S. Rudwick on the history of a major nineteenth-century geological controversy.⁴⁸⁾ In this dispute, the geologists found it necessary to gain the approval of a majority of a small core of senior geologists in the Geological Society of London (where the issues were literally debated), in order that their views might prevail. Further, by an immensely detailed examination of the historical record, Rudwick has shown how a compromise theoretical structure (in this case a suitable classification for part of the stratigraphical column) was constructed drawing ideas and observations from the several contenders in the 'agonistic field'. Rudwick makes a further interesting theoretical claim, on the basis of his historical researches, namely that the geologists' observations were '*controversy laden*', as much as, if not more so, than '*theory laden*'. Thus geologists in the field were thinking how their observations would look when they came to be presented to the Geological Society, and how they might contribute to the geologists' enhanced standing in the 'agonistic field' — in the prosecution of the controversy and in improving their credibility. I suggest, therefore, that important historical studies, such as that of Rudwick, can be informed by, and inform, theoretical discussion in the sociology of knowledge.

Actually, Rudwick's discussion seems to be influenced as much by suggestions of Harry Collins, of the University of Bath,⁴⁹⁾ as by French scholars such as Bourdieu or Latour. Collins has given considerable attention, in a number of studies, to what might be called the 'sociology of experimentation', and he has shown particular interest in the way in which difficult experiments (such as are involved in the search for 'gravity waves') are replicated.⁵⁰⁾ On his view, it is not possible to make successful replications from one laboratory to another without some direct form of social interaction — for example by persons travelling from one site to another in order to demonstrate the manner of using the apparatus, or by students gaining expertise in a given laboratory and then transplanting it after serving their apprenticeship on a particular kind of experimental programme. The

48) M.J.S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists*, Chicago University Press, Chicago and London, 1985.

49) See particularly: H.H. Collins, 'The role of the core-set in modern science: social contingency with methodological propriety in science', *History of Science*, 1981, 19, pp. 6-19.

50) H.M. Collins, *Changing Order: Replication and Induction in Scientific Practice*, Sage, London, Beverly Hills and New Delhi, 1985. (See Chapter 4, for Collins's discussion of the replication of experiments on 'gravity waves'.)

point here is that much of the knowledge required to conduct experimental work can only be learned by example, for much of it can never be codified in complete detail. It must remain, as Michael Polanyi used to put it,⁵¹⁾ as 'tacit knowledge'. This emphasizes the 'craft-work' character of much empirical research.

Collins then introduces the notion of a 'core-set' of workers within a given experimental discipline. We may suppose that for much of the time in research we have just 'normal science', as Thomas Kuhn put it in his well-known work.⁵²⁾ But from time to time, serious disagreements or controversies may break out, which will be required to be settled by empirical means. When this happens, it is commonly the case that arbitration is required as to *which* particular empirical technique, or *which* laboratory's work, is to be accepted. But this is not simply an empirical matter, or a decision reached by considering the relative merits of competing sets of observations. Again, it is a question of social negotiation, between the members of a scientific specialty, with a particular influential/powerful 'core-set' of scientists acting as arbiters in the matter. Interestingly, some of the participants in the dispute will usually be members of the 'core-set'. So once again, following our legal analogy, we find that a scientist may be a litigant and judge at one and the same time in the agonistic field of the scientific community.

Such a model can provide ample work for the historian of science, who may also throw light on the question of whether the course of scientific research is indeed punctured by occasional major scientific revolutions, as Kuhn used to suppose, or whether typically a more on-going, smaller-scale set of changes is taking place, with the appearance of occasional 'hot-spots' which will require the adjudication of the appropriate core-set of investigators, as Collins supposes.⁵³⁾

Contemporary studies of 'laboratory life' and of scientific controversies can and are now providing models for historical investigations of considerable subtlety and interest. A recent study that has attracted much attention is that of Steven Shapin and Simon Schaffer⁵⁴⁾ on the controversy in the seventeenth century between Robert Boyle and Thomas Hobbes over Boyle's studies with the air pump. This involved dispute as to the practical efficacy of Boyle's apparatus, and a complex social negotiation between Boyle and Hobbes ensued, conducted by means of pamphlets, etc. At that early period of the history of science, issues as fundamental as the utility of experimental techniques for the establishment of natural knowledge were still a matter for disagreement, but then, as now, the problem of the replicability of experiments was an important issue. Shapin and Schaffer use Collins's

51) M. Polanyi, *Personal Knowledge*, Routledge and Kegan Paul, London, 1958; *The Tacit Dimension*, Anchor, New York, 1967.

52) T. S. Kuhn, op. cit. (note 7, 1962).

53) In one investigation that I have undertaken recently, I have found Collins's model to be extremely useful. See: D. R. Oldroyd, 'Punctuated equilibrium theory and time: a case study in problems of coherence in the measurement of geological time (the KBS Tuff controversy and the dating of rocks in the Turkana Basin, Kenya)', in: J. Forge (ed.), *Measurement, Realism and Objectivity*, Reidel, Dordrecht, 1987, pp. 89-152.

54) S. Shapin and S. Schaffer, *Leviathan and Air-Pump: Hobbes, Boyle and the Experimental Life*, Princeton University Press, Princeton, 1985.

work as a model to understand what was going on as the seventeenth-century investigators sought to copy Boyle's original apparatus and conduct experiments with air pumps beyond the confines of Boyle's laboratory. Shapin has also shown in an engaging manner⁵⁵⁾ how Boyle sought to gain acceptance for his views by impressing his readers by the sumptuous and voluminous character of his literary productions. Such a consideration provides further grist to the mill of the historian of science with 'constructivist' proclivities.

In very recent work, Shapin has been trying to find out more about what actually went on in seventeenth-century laboratories, about which we know surprisingly little — not even where they were in most cases. It is an interesting question as to who worked in them and under what conditions. Here the question of *authority* arises once again. The problem that was raised in the controversy between Hobbes and Boyle was how persons who had not actually witnessed a given experiment could be persuaded of the truth-claims made on the basis of the experiment. (Herein, of course, lay the problem of the replication of experiments.) The solution worked out by Boyle, and others, was that trials made in the laboratory should then be repeated in front of the members of the Royal Society. (The curator of experiments, Robert Hooke, who was also for a time Boyle's technical assistant, did most of this work in the early days of the Society.) When duly satisfied with the demonstrations, the Society would then sanction an account of them to be published under the authority of the Society. In this manner, knowledge claims were to be substantiated — after due social negotiation. Our modern system of refereeing and publication is a direct descendant of the scheme for authenticating empirical/experimental knowledge that was devised in the early scientific academies. Seen in this light, the so-called 'Scientific Revolution' of the seventeenth century was as much a social affair as one of theory, or the unification of empiricism and rationalism.

The studies of Shapin and Schaffer, and other sociologists of knowledge interested in seventeenth-century science, can be used to illustrate a further interesting point. In the dispute between Boyle and Hobbes, there more at stake than just the efficacy of empirical techniques for the establishment of knowledge of nature (though that *was* certainly most important). It appears that there was, during the course of the controversy, a kind of 'hidden agenda', concerned with political and theological issues. It has been argued by J.R. Jacob⁵⁶⁾ that Boyle's matter theory, which envisaged the existence of *inert* corpuscles of matter at the 'micro'-level moveable at the direct behest of God or by mental activity, provided an indirect or analogical way of talking about the relationship between peasants and aristocracy — the former in Boyle's preferred political system only 'moving' at the direction of the latter!

Such a suggestion is obviously open to question. But it is certainly an interesting hypothesis, worthy of investigation by the social historian of science. And it illustrates a

55) S.Shapin, 'Pump and circumstance: Robert Boyle's literary technology', *Social Studies of Science*, 1984, 14, pp.481-520.

56) J.R.Jacob, 'The ideological origins of Robert Boyle's natural philosophy', *Journal of European Studies*, 1972, 2, pp.1-21; *Robert Boyle and the English Revolution: A Study in Social and Intellectual Change*, Franklin, New York, 1977.

general kind of claim that has been made by a number of sociologists of knowledge, namely that *science has social uses*. Science can be and frequently is deployed for the purpose of advancing wider social claims. The case of Boyle's matter theory may seem somewhat recondite, and perhaps it is. Nevertheless, the sociologist of knowledge would argue, science is typically deployed for social purposes; and this can explain why there is commonly a discernible relationship between scientific theory and social theory, practices, structures or concerns, or between ideas about knowledge and ideas about society.

Take the case of the Darwinian notion of 'survival of the fittest'. One can propose two hypotheses relating to the suggestions of the previous paragraph. It may be that Darwin recognized a struggle for existence in his own society and imputed an analogous struggle to animals and plants; or it may be that Darwin proposed the theory and it was found specially congenial to the society in which he lived, and hence it was widely accepted and came to exert such a powerful influence on the history of human affairs. It was congenial to society because it could be used to provide a warrant for certain things that were going on in Europe in the nineteenth century: according to Darwinian theory struggle yields improved adaptation, so social struggle might be held to be beneficial. It may be, of course that neither of the two hypotheses is correct; or perhaps both of them; or just one. Whichever way, it is a fine topic for enquiry for the social historian of science or the sociologist of knowledge.⁵⁷⁾

These reflections about the social uses of science may lead us to contemplate the general question of the reception of theories. According to the sociologist of knowledge, a theory is not something that is possessed of some kind of transcendental objectivity. It is customarily devised with some audience in mind: its originator has a social as well as a cognitive purpose. But once it has been written down it will be (or perhaps will fail to be) disseminated to various 'interested' readers. Each reader of a text — say Newton's *Principia* — will read it in a slightly different way, according to his different cultural situation, cognitive and social interests, etc. And it may be found, then, that the intellectual responses to a theory (or a system of thought) may differ profoundly according to the prevailing social circumstances. An obvious illustration would, I suppose, be the different attitudes towards Western technology in Japan in the Meiji and the Tokugawa periods. Thus the question of the comparative reception of theories can be the subject of appropriate historical research and falls within the ambit of the programme of the sociologist of knowledge.

I referred above to the 'anthropological' or 'ethnomethodological' investigations in the laboratory of people such as Latour and Woolgar. These authors gave special prominence to the study of discourse within the laboratory, and attempted to show how it might be construed as illustrative of the kind of activity to be found in the scientists' agonistic field. If one is alert to what is going on, seemingly innocuous statements can be recognized for

57) Actually, the weight of current opinion amongst sociologists of knowledge would, I think, be that the Darwinian theory has 'done well', outside biology, because it is socially useful, rather than because it was somehow, at its inception, a reflection of the capitalist society within which it was generated.

what they are : attempts to promote one's own position in the field, gain acceptance for one's theories or one's preferred empirical techniques, or to denigrate the position of one's opponents. 'Shop talk' may be carefully analyzed with these considerations in view, and interesting results can be obtained, which add quite considerably to one's understanding of how the social system of science functions. In recent years, there has been a small 'industry' developed, which gives attention to 'discourse analysis' of this kind, the two leading exponents of the art in the English-speaking world being, perhaps, Nigel Gilbert and Michael Mulkay.⁵⁸⁾ By careful analysis of laboratory or seminar talk, one can indeed learn a great deal about how scientific knowledge is socially constructed — how some notions come to be accepted and others rejected in the social 'negotiation' that goes on ceaselessly as knowledge grows. One can also learn much from a study of the processes of drafting and editing of scientific papers — a topic that has been studied particularly by Karin Knorr-Cetina.⁵⁹⁾ These kinds of enquiry are certainly of highest utility to the social historian of science. However, as Shapin has warned,⁶⁰⁾ it should not be supposed that they provide anything like a complete program of investigation for the social historian of science. They should merely be one line of possible investigation within a program of broader compass. And one may add that a study that directs attention exclusively to talk rather than texts — which are supposedly less 'immediate' in character — is in danger of falling outside the range of interest of the historian, particularly as the 'discourse analysts' seem to be becoming more interested in the 'structure' of talk than its meaning.

I have referred above to David Bloor's so-called 'strong program' in the sociology of knowledge. The term itself seems to exercise some special kind of attraction, and possibly seeking to capture comparable scholarly attention some writers have invoked what they are pleased to call the 'weak program'. In the hands of Daryl Chubin and Sal Restivo,⁶¹⁾ the weak program emphasizes the *ethical* concerns of mankind and the desirability of science (and metascience) serving man's ethical aims. The analyst should, on this view, be a social critic concerned to change society for the better, just as much as describe it or understand it. He assumes that there can never be any final or absolute truth about anything, that there is never any universally valid method for acquiring insight and understanding of the world, and that it is always possible to examine a broader context for an enquiry than that which is being considered in any particular investigation. The exponent of the 'weak program' should, for example, seek to demonstrate how scientific data have been obtained, either intentionally or unintentionally, in such a way as to serve the interests of some particular culture, business group, political party, or whatever. He may try to demonstrate that science seeks to present itself as neutral, objective, and value-free, whereas in fact it

58) See, particularly : G.N.Gilbert and M.Mulkay, *Opening Pandora's Box : A Sociological Analysis of Scientists' Discourse*, Cambridge University Press, Cambridge, 1984.

59) K.D.Knorr-Cetina, *The Manufacture of Knowledge : An Essay on the Constructivist and Contextual Nature of Science*, Pergamon, Oxford, 1981.

60) S.Shapin, 'Talking history', *Isis*, 1984, 75, pp.125-130.

61) 'The 'mooting' of science studies : reseach programmes and science policy', in : K.D.Knorr-Cetina and M.Mulkay (eds), *Science Observed : Perspectives on the Social Study of Science*, Sage, Beverly Hills, 1983, pp.53-83.

is commonly subservient to particular vested interests. The exponent of the 'weak program' will try to reveal scientists in the work-place seeking to further their interests by all the many devices that are available to them. But he will not be content merely to reveal what has been or is going on. He will endeavour to bring about changes and improvements in science policy, and hence in society. A good example of such work is, I suggest, provided by S.J.Gould's study of the iniquitous history of anthropometrics.⁶²⁾ But why all this should be called a 'weak' form of analysis is not at all clear to me! The name was, we must presume, chosen with a sense of irony.

It is worth mention here that the new relativist/'strong' program in the sociology of knowledge may properly be characterized as 'conventionalist' in character. In his recent book, *Changing Order*, Harry Collins has written: "It is not the regularity of the world that imposes itself on our senses but the regularity of our institutionalized beliefs that imposes itself on the world."⁶³⁾ We may compare this the classic statements of the French mathematician, Henri Poincaré, at the beginning of this century, who held that major physical principles such as Newton's laws of motion were analogous to mathematical principles: they were not empirical in character but *true by convention*,⁶⁴⁾ or were concealed definitions. That being so, one would never expect them to be falsified — or *allowed* to be falsified — by empirical means.

Collins's thesis is analogous, but not identical. He is, of course, an epistemological relativist: there can be many ways of describing the world theoretically, none of which can lay claim to objective truth, and all of which are the product of different social situations and negotiations. Our particular social enterprise, which we call science, agrees to see the natural world in the way it does by courtesy of its preferred set of social conventions and training. For Collins, as we have seen, the process of *replication* of experiments is the essential feature of the way in which scientists come to form a picture of the world. What they accept as replications, after due social negotiation, provides the basis of what they take to be objective knowledge. So scientific knowledge can be said to be 'conventional' in character: and there is no limit to the number of possible conventions. Indeed, on one possible reading of Collins, it would seem that there is no empirical control from the real world at all, in the prosecution of scientific enquiry. Scientific knowledge is 'nothing but' the product of social interactions, powerplays, persuasions, decisions in accordance with the interactions of specific interests.

All this seems scarcely conceivable, no matter how seriously we may regard the problem of induction or the question of the underdetermination of theories. But in fact, as has been pointed out by Mary Hesse in a trenchant review,⁶⁵⁾ Collins fails to sustain his case satisfactorily, and allows realism in by the back door, so to speak. This is partly so

62) S.J.Gould, *The Mismeasure of Man*, Norton, New York, 1981.

63) H.M.Collins, *Changing Order: Replication and Induction in Scientific Practice*, Sage, London, 1985, p.148.

64) J.H.Poincaré (trans. W.J.G.Walter), *Science and Hypothesis*, Scott Publishing Co., London and Newcastle, 1905, p.110.

65) M.B.Hesse, 'Changing concepts and stable order', *Social Studies of Science*, 1986, 16, pp.714-726.

because his case is itself made on the results of his own (most valuable) empirical investigations of how decisions are arrived at by scientists as to what is, and what is not, to count as replication. And partly it is so because Collins seems to accept that "science is the best institution for generating knowledge about the natural world that we have".⁶⁶⁾ Indeed, Collins seems to have a high regard for science and holds that its progress is to be applauded. Why this should be so, however, if we take his relativist arguments really seriously, is surely mysterious.

The fact that we may constantly hoist the relativists with their own petards has provided a field of discussion for great numbers of philosophers of science, and the literature is weighed down with discussions of the problem of 'reflexivity'. So far as I am aware, the problem remains intractable and has not been solved satisfactorily. For some (perhaps including Collins), there is a secret corner of their (otherwise relativist) souls where they seem to maintain allegiance to the old god of objective knowledge. For others, such as the Australian David Turnbull, a total acceptance of or submission to relativism and reflexivity is acceptable — indeed to be embraced with enthusiasm.⁶⁷⁾ Perhaps this is the way to go. One accepts that full understanding is never possible. We are always in a dialectical process of *seeking* understanding. And this is all we may ever hope for. Even if the program of sociology of knowledge is logically flawed, it still is a worthwhile enterprise since it generates improved understanding, offers new insights, and is an antidote to dogmatism and scientism.

Fortunately we are not required, in this brief review, to solve the relativists' philosophical puzzles. It is enough for us to accept that fascinating new insights into the workings of science are emerging from recent historical investigations. The historical program laid out before us, with the invitation to examine the social processes whereby knowledge is constructed, is surely one that we cannot refuse. So many old assumptions have already been revealed as unfounded. So many new and interesting questions have been raised. So many new perspectives have been revealed. All this is surely of highest consequence to the historian of science, even if the philosophical foundations of the new enterprise may seem to be insecure. The historian of science can, in any case, take comfort from science itself. Years of philosophical endeavour have yet to display an agreed firm philosophical foundation for science. Yet science itself does not seem in any way incommoded. The same may be said, perhaps, for studies in the social history of science.

I would conclude with an observation on the present 'state of play' in the study of the history of science. It has for many years now been recognized that one does not achieve a complete or adequate understanding of the history of science by restricting attention to its 'internal' development, and as we have seen there has been an increasing emphasis on the significance of 'external' factors, with work much more sophisticated than that of Hessen in the thirties. But in time, this externalism came to be regarded as providing only a

66) H.M.Collins, op. cit. (note 63), p.165.

67) D.Turnbull, 'Relativism, reflexivity and the sociology of knowledge', *Metascience*, 1984, 1/2, pp.47

partial and incomplete way of studying the history of science, and efforts were made to try to find ways of integrating the externalism and internalism through a 'contextual' study of science.⁶⁸⁾

It is interesting to observe, then, at the present time the study of the history of science seems to be moving towards a new kind of internalism, having for the most part moved resolutely away from old Koyré-type work in the last twenty years or so. Consider, for example, the discourse analysis of writers such as Knorr-Cetina, Gilbert and Mulkay. In her *Manufacture of Knowledge*,⁶⁹⁾ Knorr-Cetina takes the reader in detail through successive drafts of several scientific papers during the course of their writing, revision and editing, the aim being to show how the final product emerges as a result of intense social negotiation between co-authors, referees, editors, and others. All this is admirable in its way, and surely an advance on simply studying published texts and trying to display their meaning and significance for the progress of science, as was a typical method of procedure amongst early historians of science. But it does seem to tend to take us back towards attempts to reconstruct the actual-thought processes of scientists in the performance of their work, the difference now being that we appreciate more fully the *kinds* of thoughts that go on in scientists' minds. It may have as much to do with funding or inter-laboratory rivalries as the structure of matter or the age of the Earth!⁷⁰⁾

My own attitude to the new developments in the sociology of knowledge and their implications for historians of science is one of enthusiasm. I believe that work in this field, though not grounded with entire theoretical security, opens up a vast number of interesting and important questions, the study of which must surely increase our *understanding* of science, which is, after all, perhaps the most important role for the historian of science.

I regret that I am unable to read the Korean-language writings in the history of science, and my ignorance in these matters may have led me to discuss some points that are in fact commonplace in Korea today. I hope, however, that this is not so, and I take the liberty of appending a short list of recent English-language books on the social history of science that are influential and currently under active critical discussion. It would, I think, be of service if all of these were to be translated into Korean.

Acknowledgement

I am most grateful to my colleague, Dr D.P. Miller, for his valued comments on a draft of this paper.

68) See: R. Johnston, 'Contextual knowledge: a model for the overthrow of the internal external dichotomy in science', *The Australian and New Zealand Journal of Sociology*, 1976, 12, pp.193-203.

69) K.D. Knorr-Cetina, *op. cit.* (note 59).

70) Discourse analysts, however, seem to like to claim that they are investigating the structure of discourse rather than its meaning. So they may disclaim any suggestion that they are engaged in trying to discover what scientists are actually thinking!

SUGGESTIONS FOR FURTHER READING

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