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Author(s): John Urry

Source: *The British Journal of Sociology*, Vol. 24, No. 4 (Dec., 1973), pp. 462-473
Published by: Wiley on behalf of The London School of Economics and Political Science
Stable URL: https://www.jstor.org/stable/589735
Accessed: 22-02-2019 06:38 UTC

John Urry*

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INTRODUCTION

In view of the profound controversy that has developed in many disciplines regarding the significance of Thomas S. Kuhn's *The Structure of Scientific Revolution*, it is surprising that there have been so few systematic discussions of his work in sociology.[1]

In this paper I shall make two points that are raised for sociology by Kuhn's argument. The first point will detail, what I term, the non-radical and the radical responses to his account, both responses being characterized by a belief that Kuhn's argument about natural scientific development somehow legitimates the sort of sociology they are currently practising. Both these naturalistic responses will be shown to be incorrect, since what Kuhn takes to be the nature of natural scientific development is not necessarily relevant to sociological development. The second point is that it does not follow from this that Kuhn is of no interest to sociologists; on the contrary, he has an extremely interesting although limited sociology of natural scientific knowledge. The limitations of this indicate limitations in the sociology of knowledge in general.

THE NON-RADICALS AND THE RADICALS

What are the non-radical and radical sociological reactions that have developed or could develop in response to this thesis? Central to both and to Kuhn's argument is the notion of a paradigm. He now (1970, p. 175) identifies two aspects: (*a*) the entire matrix of beliefs, values, law, theory, application and instrumentation which are shared by a given scientific community within a particular specialty; (*b*) the concrete puzzle-solutions used as exemplars that replace explicit rules as the means of solving the remaining normal scientific problems. Kuhn argues that it is a sign of maturity within a discipline when scientists

* John Urry M.A. PH.D. Lecturer in Sociology, University of Lancaster.
† I am grateful to the following persons who commented upon an earlier draft of this paper: Nick Abercrombie, Max Atkinson, John Hillard, John Hughes and Russell Keat of the University of Lancaster, and Graham Cox of the University of Cambridge.

operate with a paradigm. Prior to that point being reached there is a variability of fact-gathering and interpretation. This is only overcome when one or other of the pre-paradigms assumes dominance within that field, when, in a sense, a profession or discipline is established for the first time. Such a paradigm does not enable all questions to be answered. Indeed it is in the very nature of a paradigm that what Kuhn terms normal science is highly restricted and selective. There is no attempt to derive the unexpected novelty, problems are chosen not for their intrinsic interest but because they have solutions.

The non-radical sociological reaction is the claim that if sociology is to develop it must transcend the present situation where there are multitudinous pre-pre-paradigms competing with each other, where work is done at all sorts of levels and significances, and where there is excessive methodological neurosis. Rather we must get to the stage of normal science by establishing our own paradigm, by building a unified and hegemonic constellation of beliefs, values, techniques, etc., and by ensuring that well-known examples of sociological work are shared and pervasive. The obvious candidate for such development is the system-function-empiricism paradigm.[2] Thus many sociologists who read Kuhn have responded or could respond by arguing that although they do not accept all of his account it is nevertheless essential to establish a sociological paradigm. As Herminio Martins points out, formalization, quantification, extrusion of 'soft' data, social behaviourism, 'ethical neutrality', discouragement of 'philosophical' controversy, etc., can all be justified on the grounds of maximizing communitarian values, monopolizing control for paradigmatic take-off, and magnifying the exemplificatory role of Durkheim's *Suicide*. Two Kuhnian articles written around the time that *The Structure of Scientific Revolutions* first appeared strengthen the argument for paradigm-entrenchment within sociology.

In one, Kuhn (1970a) emphasizes the importance for scientific development of convergent rather than divergent thinking. In the other (Kuhn, 1963), he maintains that scientific research is best developed through a dogmatic reliance upon tradition; a closed rather than an open mind is the path to scientific utopia. The natural scientific emphasis within sociology upon quantification and limited hypothesis-testing, upon disdain for history and philosophy, and upon careful and precise research rather than say the 'sociological imagination' of C. Wright Mills all reflect sociology's dogmatism, traditionalism and convergence (see Brown and Gilmartin (1969) and Gouldner and Sprehe (1965)). Thus what I have argued is this: one reaction to Kuhn's account of the natural sciences is naturalistic in two senses. It makes natural scientific claims about sociology—that is, positivist claims (see Keat, 1971, on the confusion of naturalism and positivism)—by reference to a model of development operative within the natural sciences.

This is what I called above the non-radical response. It is now necessary to consider the radical naturalistic reaction.

The argument here may take one of two forms. The first derives from the way in which Kuhn attempts to refute the thesis that scientific progress results from accumulation. Kuhn argues instead that there are periods of normal science in between which are scientific revolutions. He says that instead of one development simply being added onto another, the scientific revolution replaces one time-honoured scientific theory with another essentially incompatible with the older theory. There is paradigmatic replacement, one indicator of which is the rewriting of the discipline's textbooks. These he sees as centrally crucial vehicles of scientific socialization, but their general tendency to refer to past scientists only to show their contribution to the present, has the effect of emasculating science's revolutionary history. It is the emphasis on revolution in the Kuhnian account, on how development proceeds *revolution*–normal science–*revolution*, that is the basis for the first radical reaction. The claim is that we must develop a revolution or revolutions in sociology as fast as possible. It is thus presumed that we are or have recently been in a period of normal science and that the scientific revolution will consist of its replacement by a 'critical', 'dialectical' paradigm consisting of the obverse of those features listed above.[3] The second radical response is based on Kuhn's argument that in the revolutionary period the existent normal science does not provide the criteria by which inter-paradigm selection can be made. Since each paradigm exists as a separate world-view the practitioners within each do not share a common universe of discourse. A scientific revolution results in a more or less total transformation of the scientists' conception of the world such that their very data, the facts of their science, are transformed by the paradigm shift. The radical reaction here is to claim that there are many paradigms in sociology, each involving a particular world-view and theoretical and observational language, so we need not bother with established paradigms and can go ahead and establish one or more of our own which will be different from but no worse than anyone else's (Bandyopadhav, 1971, p. 7; Gellner, 1970, p. 207; Lakatos, 1970, p. 93; and Feyerabend, 1970).

I now want to show, firstly, how these three responses are based on certain misunderstandings of Kuhn's argument or upon inconsistencies in his account. Secondly, I shall try to show that they are all derived from a fallacious belief that what is true of natural scientific development is necessarily true of sociological development.

The first argument, that which pleads for scientific paradigm consolidation, is based upon the supposition that sociology is at the point where it is appropriate to establish such a paradigm. There are three points to note. The first is that if we are not at this point but are still grovelling in the pre-paradigm stage then it is false to argue that what we have to establish is a single paradigm. Kuhn implies that at this stage in scientific development there will be various and competing paradigm candidates. A second point follows from the two meanings of the term paradigm, the active constellation of beliefs, practices, etc., on one hand, and the 'scientific example' on the other. Kuhn now agrees that all science at all times operates under paradigms in the first sense (1970c, p. 179). The difference he now argues between the pre- and the post-paradigm stage is that it is only in the latter that normal exemplar-based puzzle-solving research is possible. But are there any grounds for believing that we are at the transition stage in sociology? Part of the difficulty in answering this question arises from the third point here. This point is that it is always ambiguous, perhaps deliberately, as to whether Kuhn is intending to describe or prescribe the nature of scientific change. Is the process more or less inevitable such that when the time comes each group of practising sicentists automatically slip into a paradigmatic mode of puzzle-solving research? Or does Kuhn's account function as a prescription? How this leaves pre-paradigmatic sociology is clearly ambiguous. Does it mean that we should just sit tight and if a paradigm, in the puzzle-solving sense, is to develop it will do so without our help; or does it mean that we should set about at a top speed eliminating paradigm-candidates and establishing a single puzzle-solving paradigm?

The first radical response, that a scientific revolution has to be established today, is also tricky; firstly, because it presumes that we are already in a period of normal science; and secondly, because according to Kuhn, a scientific revolution will only follow when all the moves within an established paradigm have been played out. Before that point is reached, a scientific revolution is not possible. Do we sit tight waiting for the playing out of the moves, or should we set about eliminating them ourselves? The second radical reaction, that any group should go ahead and establish a paradigm, is not congruent with Kuhnian argument unless sociology is simply still at the pre-paradigmatic stage; but even here it does presume that there will be eventual paradigmatic consolidation and the elimination of paradigm-competitors.

Thus all three responses are to some extent inadequately derived from Kuhn; but all three are based on a fundamental misunderstanding of the purpose of his analysis. I shall now try to establish this latter point. Kuhn's work, whether descriptive or prescriptive, is an investigation of certain dynamics of change and development within the natural sciences. His arguments do not necessarily apply in any other discipline, subject or inquiry. Thus to argue that *social* scientific development should follow natural scientific development is, I think, wrong.

Kuhn himself (1970c, p. 209) *suggests* that this is so when he says that there is something 'strikingly different' about scientific development as opposed to development elsewhere (1970c, p. 209). This is because of three combined features of science: a scarcity of competing schools, other members of the scientific community being the only audience reference group and scientific education as puzzle-solving. Sociology is probably somewhat different from normal natural science in terms of these criteria, but how 'strikingly different' it is is a matter of opinion. I suggest however that merely to identify the continuities and discontinuities between sociology and natural science is to miss quite crucial considerations. These occur because we have not yet confronted older anti-positivist arguments. If we confront them we can begin to comprehend how development within sociology will not be what Kuhn describes as scientifically paradigmatic. I want to advance this strong claim because if sustained we can begin to appreciate that the importance of Kuhn's argument for sociology is not what it tells us about sociological development but for his analysis of *natural* science, and generally for the sociology of scientific knowledge.

The first reason for supposing that his model of development is inappropriate to sociology can be seen if we consider two points more carefully, namely, the nature of a Kuhnian paradigm and the features of an anti-positivism in sociology. First of all, Kuhn argues that such paradigms are centred within the scientific specialty; that paradigms within different specialties are equivalent and independent of each other; that paradigms only change as a consequence of factors endogenous to the community of scientific specialists; that there is a unity of law, theory, etc., which comprise each paradigm; and that scientific maturity is measured by scientific unanimity (see Martins, 1971). Secondly, the features of an anti-positivism within sociology are, minimally, that universal laws are impossible, that man is purposive, creative, and gives meaning to his actions, that it is necessary to *understand* why an individual has acted in the way he has, and that value-neutrality is a largely erroneous posture. But this sort of anti-positivist argument, which is the basis of much contemporary critical sociology, is radical because it eliminates not just the distinctions between the specialties of say, the sociology of the family, religion, education and so on, but rather the distinctions between sociology, psychology, philosophy and history. A scientific revolution here is always concerned with changing relationships between disciplines, it is always premised upon the transformation of basic ontological assumptions. It is not and cannot be simply intra-specialty change. Revolutions in sociology are revolutions in the whole disciplinary structure of the study of men within history and cannot be confined to a single academic specialty.

The second reason why it is false to argue that Kuhn's model of scientific development is applicable within sociology is a derivative of the first. It has been pointed out that for Kuhn there is within a paradigm a unity of law, theory, application, techniques, instrumentation, etc. Thus one cannot overthrow one aspect without overthrowing them all. The first stage of a scientific revolution is that an anomaly develops; the taken-for-granted assumptions of scientific life no longer solve the puzzles with the same efficiency. But Kuhn only sees such anomalies developing when a paradigm has been fully worked through, when most of the main sorts of moves have already been played. But why do anomalies arise? And why are they seen as anomalies? In the natural sciences Kuhn believes that we can answer these questions at least *ex post facto*. I do not know about that. But I am sure that such questions cannot be answered in sociology, where there are no grounds for not supposing a total lack of relationship between playing out all the moves in a paradigm and the existence of anomaly.

Part of the explanation of this is also the third reason here. Alan Dawe (Dawe 1971) develops this well in his review of Robert Friedrichs' *A Sociology of Sociology*. He points out that the difficulty in trying to apply the Kuhnian account to sociology stems from its scientistic character, from the fact that it is an account of change *within* a scientific community. It is an account of such change which abstracts it from the social, economic and value concerns of the encompassing society. I briefly query below whether this is appropriate for natural science. But it is quite clear that this is inappropriate to the human sciences, since it is impossible to consider them as a closed system separate from the world outside. To believe in the opposite is to believe that only sociologists are exempt from the domain assumption that men's actions are shaped by their social environment (see Gouldner, 1970, *passim*).

So far I have shown that various reactions to Kuhn that have been or might be made in sociology are not adequate and do not provide legitimation for claims that may be advanced for other reasons. I now want to show that there is something important within Kuhn and that is an interesting albeit limited sociological explanation of how we come to acquire increasingly correct knowledge of our physical environment.

KUHN AS A SOCIOLOGIST OF KNOWLEDGE

Herminio Martins points out how Kuhn's account of the development of science is able to transcend the blundering artificiality of the distinction between the sociology of science (based on role, collectivity, institution, norm, function, etc.), and the sociology of knowledge (based on ideology, false consciousness, myth, utopia, etc.). In other words, it realizes dialectical re-integration of what was previously polarized in Mannheim's *Ideology and Utopia*. This is in itself very important although Polanyi had made some of the opening moves in *Personal Knowledge* (1958). But we must ask, what contribution does Kuhn's account make to the sociology of knowledge? Does it highlight certain problems involved in such a sociology?

Two critical problems are customarily involved in the sociology of knowledge. The first problem is the crudity of sociological explanation which is often based on a Durkheimian ontology. The second problem is an epistemological relativism. Thus, firstly, Kuhn systematically neglects to consider the sociological nature of the scientific group or community, he fails to distinguish between different sorts of social objects to which the individual scientist may refer, and he ignores the different meanings which a scientist may place upon his reference to the scientific community. This community of the academic specialty is not, as he supposes, a simple undifferentiated entity within which individuals are submerged. On the contrary it is stratified, differentiated, and related in varying ways to other such communities. Individuals within a scientific community have varied orientations to it, and Kuhn is incorrect in presuming that it will act *in toto* both as the source of an individual's norms and values and as the scientist's exclusive audience reference group. Different social objects will at different times be the objects of different orientations and there is no good reason, and Kuhn provides none, why the speciality's community will be the only relevant group for the individual scientist.

In a sense the foregoing criticisms, that I think correct, are a little unfair. After all, we owe to Kuhn (1970b, p. 238) this extremely important statement:

> Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. That position is intrinsically sociological.[4]

The reason I would argue for Kuhn's sociological crudity is that he is operating within presuppositions of Durkheimian ontology. The scientific community is seen by Kuhn as constituting an objective facticity for the scientist. He acts, he solves puzzles within the framework of established norms and values. The scientist is given the community's paradigm, and to a significant extent he is given the period of revolutionary science. Each community in each period has a paradigm.[5] But to see the scientist as simply the player of an essentially given and communally defined scientific role is to ignore certain features of normal science and paradigmatic-change.

1. Difficulties are raised by Kuhn's account of the process by which the scientific norms of the new paradigm come to be established. He argues that as a consequence of normal humdrum puzzle-solving scientific work there is the genesis of anomaly, a violation of the paradigm-induced expectations of normal science. A more general realization of novelty turns anomaly into crisis but the old paradigm will not be replaced unless there is the security of a new one which seems to solve more of the immediate problems than did the old. Further, because paradigms are incompatible each scientist within his

specialty is confronted by a straight choice of commitment between the old and the new. But Kuhn's account neglects these points:

(*a*) sometimes there is compromise between the old and the new because the degree of paradigm-incompatibility is a variable;

(*b*) among sectors of a specialty community there is subcultural redefinition rather than rejection of the old paradigm; and

(*c*) since social psychological research emphasizes that fundamental changes in an individual's objects of identification necessitate the existence of pervasive childhood-type relationships, it is likely that they will not involve a whole specialty but more likely a primary group not necessarily all of whom will be members of a single scientific community. (See Brim and Wheeler, 1966, p. 9.)

2. Kuhn's argument that paradigm competitors cannot precede normal scientific crises is surely incorrect. Rather normal science should be regarded as consisting of a major paradigm with various paradigm competitors, and with a continual struggle for hegemony between the adherents. The production of knowledge is a struggle for power. To explain science as such is to allow for scientific subcultures, for normal scientific activity being the process by which paradigms are maintained, for the insecurity of established paradigms, and for the dialectical quality of paradigmatic change.[6]

The last point is implicitly Kuhnian. It can be developed further by considering Marx's argument as elaborated in both Althusser (Althusser and Balibar, 1970) and Frankenberg (1970). The point is that scientific knowledge is a process of production by which certain men (scientists) with certain means of production (technical equipment and scientific concepts) transform a set of raw materials (scientific ideas) into knowledge. But knowledge is not realized one-dimensionally since the process of production transforms the conditions of acquiring knowledge. This is what was meant in the abbreviated exegesis of Kuhn when it was shown that the condition for both paradigm-consolidation and paradigm-change is continued normal science puzzle-solving. There is analogy here with the characterization of capitalism by Marx (1958, p. 363):

> At a certain stage of their development, the material productive forces come into conflict with the existing relations of production . . . From forms of development of the productive forces these relations turn into their fetters. Then begins an epoch of social revolution . . . No social order ever perishes before all the productive forces for which there is room in it have developed; and new, higher relations of production never appear before the material conditions have matured in the womb of the old society itself.

In the development of both capitalism and of natural science, the crucial mechanism is production. Capitalism is essentially a form of society consisting of certain combined elements where raw materials are transformed into commodities by the labour of one class using the means of production owned and controlled by another class. Because surplus-value is created by one class and appropriated by another, and because of the law of the falling rate of profit, there is a consistent strain towards expansion of productive forces, there is overproduction and there are periodic crises.

There are two problems in this account which limit the analogy between capitalism and natural science. First of all, even where in Marx the analysis of the structures of economic practice is satisfactory to explain the process of economic crisis, the relationship between this and revolutionary political, ideological and theoretical practice is unclear and needs elucidation and specification. Second, the analysis of economic crisis is highly complicated and it is difficult to know what would constitute analogous mechanisms in the analysis of scientific change. Could Mulkay and Williams's (1971) argument regarding the crucial nature of the process in science by which information is exchanged for professional recognition be the *first* steps in developing a theory of scientific development? (See Hagstrom, 1965.) Thus we could say that there is a long run reduction in the professional status that can be acquired *within* a paradigm. Also, stretching the analogy, we could argue that there is an appropriation of surplus status by the already powerful group within a scientific community. The combination of these propositions means that, as the realizable status within the existing paradigm diminishes, scientists with low power and low status who are committed to the process of producing their paradigm's variety of scientific knowledge, have no alternative but to try to answer more radical questions than hitherto envisaged. But how and why should this occur given the normal effectiveness of paradigmatic socialization? On the one hand, the attempt to answer radical questions is one way to ensure that professional status is partly realizable for people at an early stage of their professional career. On the other hand, it is because incipient paradigm competitors will be fostered perhaps in scientific centres remote from the traditional focus of the paradigm, or in disciplines close to the one in question, or because the working through of the paradigm has revealed a significant and pertinent area of ignorance.

Kuhn can be criticized in a rather more general way. I would not wish to explain sub-cultural and contra-cultural definitions of scientific activity as merely the product of a failure of socialization on the part of the normal scientific community (see Kuhn, (1970c, pp. 89–90) on the young scientist as revolutionary). This is because I do not see individuals in acting as merely responding to the scientific community's norms and values. On the contrary, action should be seen as a process by which actors seek to produce a set of determinate outcomes. They are active agents engaging in a process of tenuous, delicate and tensionful production. It is not, as Kuhn argues, a matter of an individual responding to particular norms and values; it is that actors intend to produce certain outcomes and in producing them various contradictions, conflicts and tensions arise within and without that scientific community.

It might also be supposed that to criticize Kuhn for his Durkheimian ontology would be to imply that his epistemology could be similarly attacked, but this is not so. Kuhn believes neither in science as an increasing approximation to true knowledge of the external world, nor in the invariance of Aristotelian categories to differing social contexts (see Martins, 1971). But the sort of epistemology Kuhn does believe in is somewhat uncertain. He maintains both that scientific development is 'evolutionary . . . unidirectional and irreversible' (Kuhn, 1970b, p. 264); and that, after paradigm-change, there is neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm (Kuhn, 1970c). That Kuhn espouses a relativistic viewpoint can be shown by considering the four activities in which the (serious) sociologist of knowledge may engage: (1) the explanation of the social selection of existent ideas, (2) the explanation of the social genesis of ideas, (3) the explanation of the social selection of true ideas, and (4) the explanation of the social genesis of true ideas. Since Kuhn explicitly maintains that he is not arguing that paradigm-change implies increasing isomorphism with the reality of nature (1970b, pp. 264–5), and since he clearly gave no account of the genesis of different paradigms, his contribution to the sociology of knowledge is type (1) (see Kuhn: 1970c, pp. 198–207).

If Kuhn's contribution was really a 'sociology of knowledge', that is, an explanation of the *genesis* of *knowledge*, it would require an analysis of the relations between science and society, between science and different parts and groups within society, and between different parts and groups within science; and it would require a theory of cognition and of truth. A theory of cognition and truth implies both an explanation of why actors come to believe in something for good reasons, and an analysis of whether such beliefs are correct. Kuhn approaches the former at least in showing that the process of selection between scientific theories is unequivocably social and the criteria used are 'context-dependent' (Lukes, 1970). But this does not mean that there are no context free, or universal criteria of truth. Kuhn seems to argue that because it is *difficult* to *maintain* that scientific development results in increasing representation of 'reality' so it is *impossible* to argue that there are universal criteria, that there is a truth about the world (1970c, pp. 206–7). Kuhn's 'sociology of knowledge', like most of the other writing falling under that umbrella term, pays too much formalist attention to the dictum that if a belief is real in its consequences, then it is real. It may be real and crucial to explanation; but it may not be right. What the sociology of knowledge should be able to show is why certain criteria operative within particular worlds (for example, as Lukacs (1971, p. 68) argues in the proletariat) are universal and true, and why the criteria within other worlds are 'context-dependent'. Unfortunately though the sociology of knowledge, from Scheler and Mannheim to Berger, Luckmann and Kuhn, has not done this. The fact that these and other writers may begin to explain why people *believe* that they know the truth does not mean that there is any account of whether in fact they do. It is in showing this in the sociology of knowledge that Kuhn is also important.

Notes

1. One major exception to this generalization is Martins (1971); other exceptions are Bandyopadhyav (1971), Dawe (1971), Frankenberg (1970), Friedrichs (1970) and Weigert (1970).

2. See Friedrichs (1970, ch. 4), Simpson (1961) and Gouldner (1970), for discussion of the nature and pervasiveness of this paradigm.

3. See above, p. 2; and see Friedrichs (1970, ch. 3) for an account of this paradigm, and the whole book for an advocation of such a scientific revolution.

4. See Mulkay and Williams (1971) especially pp. 71–3 on 'originality within conformity', for an account of what it is like to be a scientist working within a paradigm.

5. Kuhn's Durkheimianism is even clearer in the parallels he draws between a scientific and a religious community; thus he talks of 'professional initiation' and says that 'except perhaps in orthodox theology' education is 'narrow and rigid', that science's rewriting of history backwards 'distinguishes it from every other creative pursuit except perhaps theology', that normal science suppresses novelty 'because they are . . . subversive of its basic commitments', and that revolutionary science is a 'conversion experience' and a matter of 'faith'. (See Watkins, 1970, p. 33.)

6. See Barnes (1972) for a very useful collection of articles some of which amplify the points made here.

References

Althusser, L., and Balibar E. (1970) *Reading Capital.* London: New Left Books.

Bandyopadhyav, Pradeep (1971) 'One Sociology or Many: Some Issues in Radical Sociology', *Sociol. Rev.*, vol. 19 (February), 5–30.

Barnes B. (ed.) (1972) *Sociology of Science.* London: Penguin.

Brim, O. G., Jnr., and Wheeler, Stanton (1966) *Socialization After Childhood: Two Essays.* New York: John Wiley.

Brown, Julia S., and Gilmartin, B. G. (1969) 'Sociology Today: Lacunae, Emphases, and Surfeits', *Amer. Sociologist*, vol. 4 (November), 283–91.

Dawe R. A. (1971) 'Extended Review of *A Sociology of Sociology*', *Sociol. Rev.*, vol. 19 (February), 140–7.

Feyerabend, P. K. (1970) 'Consolations for the Specialist.' Pp. 197–230 in Lakatos and Musgrave (1970).

Frankenberg, R. (1970) 'Social Prequisites of the Development of Science, Kuhnian Paradigms, Chinese Parallels and African Prospects.' Paper presented at the 7th World Congress, Varna, 1970.

Friedrichs, R. W. (1970) *A Sociology of Sociology*. New York: Free Press.
Gellner, E. (1970) 'Myth, Ideology and Revolution.' Pp. 204–20 in B. Crick and W. A. Robson (eds.), *Protest and Discontent*. London: Penguin.
Gouldner, A. W. (1970) *The Coming Crisis of Western Sociology*. London: Heinemann.
Gouldner, A. W., and Sprehe, J. T. (1965) 'The Study of Man: Sociologists Look at Themselves', *Transaction* (May/June), 42–4.
Hagstrom, W. O. (1965) *The Scientific Community*. New York: Basic Books.
Keat, R. (1971) 'Positivism, Naturalism, and Anti-Naturalism in the Social Sciences', *J. Theory of Social Behaviour*, vol. 1 (January), 3–17.
Kuhn, T. S. (1963) 'The Function of Dogma in Scientific Research.' Pp. 347–69 in A. C. Crombie (ed.), *Scientific Change*. London: Heinemann.
Kuhn, T. S. (1970a) 'The Essential Tension: Tradition and Innovation in Scientific Research.' Pp. 342–59 in L. Hudson (ed.), *The Ecology of Human Intelligence*. London: Penguin.
Kuhn, T. S. (1970b) 'Reflections on my Critics.' Pp. 231–78 in Lakatos and Musgrave (1970).
Kuhn, T. S. (1970c) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
Lakatos, I. (1970) 'Falsification and the Methodology of Scientific Research Programmes.' Pp. 91–195 in Lakatos and Musgrave (1970).
Lakatos I., and Musgrave A. (eds.) (1970) *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
Lukacs, G. (1971) *History and Class Consciousness*. London: Merlin.
Lukes, S. (1970) 'Some Problems About Rationality.' Pp. 194–213 in B. Wilson (ed.), *Rationality*. Oxford: Basil Blackwell.
Martins, H. (1971) 'The Kuhnian "Revolution" and its Implications for Sociology.' In A. H. Hanson, T. Nossiter and Stein Rokkan (eds.), *Imagination and Precision in Political Analysis. Essays in Memory of Peter Nettl*. London: Faber.
Marx, K. (1958) *Selected Works*. Moscow: Foreign Languages.
Mulkay, M., and Williams, A. (1971) 'A Sociological Study of a Physics Department', *Brit. J. Sociol.*, vol. 22 (March), 68–82.
Polanyi, M. (1958) *Personal Knowledge*. London: Routledge.
Simpson, R. L. (1961) 'Expanding and Declining Fields in America Sociology', *Amer. Sociol. Rev.*, vol. 26 (June), 458–66.
Watkins, J. W. N. (1970) 'Against Normal Science.' Pp. 25–37 in Lakatos and Musgrave (1970).
Weigert, A. J. (1970) 'The Immoral Rhetoric of Scientific Sociology', *Amer. Sociol.*, vol. 5 (May), 111–19.