CONTEMPORARY SOCIAL THEORY

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T. S. Kuhn and Social Science

Barry Barnes

University of Edinburgh

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Say what you choose, so long as it does not prevent you from seeing the facts. (And when you see them there is a good deal that you will not say.)

Wittgenstein

It is a truism that anything is similar to, and also different from, anything else.

Kuhn

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Preface

When I was invited to write this book I was immediately attracted by the importance of its subject, and by the pleasure I knew I should derive from working upon it. None the less I did have one initial reservation. Thomas Kuhn's work is widely known, readily intelligible and easily obtained. Its account of scientific research and the growth of knowledge is an accepted point of reference throughout the academic world. Hence I could find no excuse for something which simply expounded Kuhn's views, at a time when many areas of sociology are already so involved in the examination of seminal texts that they risk becoming backwaters of intellectual history.

Fortunately, however, the brief for this present series demanded analysis more than description, and allowed me to write not so much about Kuhn as about the sociological questions upon which he offers important insights. This is a book about the sociology of scientific knowledge which takes Kuhn's work as its point of departure. It provides no general intellectual biography of Kuhn, nor a comprehensive discussion of his significance as a philosopher, historian, or even sociologist. (Kuhn's sociological functionalism, for example, which actually finds expression in the titles of some of his papers, is not considered here.) It is what Kuhn has contributed to the social sciences that I discuss, how those contributions have been developed in recent years, and how they might usefully be further developed.

Since I look at Kuhn's work through the eyes of a sociologist, certain themes are highlighted and extended to their furthest limits; others are passed over. The result is something markedly divergent from the general drift of the numerous commentaries by philosophers and historians. This is not to say that it is necessarily better.

Conditioned as it is by narrow interests and objectives it can claim no special standing for itself. But where is the account that could claim such a standing? Even if Kuhn himself were to look back upon his earlier work and offer (as undoubtedly he would) a different framework within which to understand it, that would merely be yet another commentary, interesting and important certainly, but no touchstone of the significance of other accounts. All exegesis, even self-exegesis, is informed by specific aims and interests.

My aim here is to help sociologists to make profitable use of Kuhn's work. I do not attempt the task of regenerating his thinking. Nor do I take any interest in the precise extent of his own individual originality. None the less it is worth noting that Kuhn has made one of the few fundamental contributions to the sociology of knowledge. It fell to him to provide, at the time it was most needed in the 1960s, a clear indication of how our own forms of natural knowledge could be understood sociologically. This encouraged the empirical studies of scientific culture which were then beginning in the social sciences. And it inspired a revived awareness of the constitutively social character of knowledge generally — an awareness which had faded away in the post-war period. This is why a discussion of current views in the sociology of scientific knowledge can be developed from an examination of Kuhn's work, even though Kuhn himself is not a sociologist.

Having outlined the aim of the book, it only remains for me to offer some words of warning to the reader. First, I wish to ensure that the style of the text does not disguise how very little we know about learning, about credibility, and about our modes of inference. No writer dwells upon his own ignorance; there is no point. But as a result there are texts, whether in physiology, psychology, information theory, sociology or philosophy, which leave the false impression that they have penetrated to a deep understanding of how we come to know things. They even occasionally imply that the key to the matter lies in a single theory or a single field. I have no reason to assume that my own style could not create similar false impressions, so I seek to anticipate them here. In particular, I want to emphasise that a sociological approach can only be part of the larger project of gaining a proper *empirical* understanding of the characteristics of knowledge and cognition.

Second, although the various chapters of the book centre upon different topics, they cannot be taken as independent essays. The

sociology of knowledge is among the least developed of all areas within the subject, and there are few routinely accepted ideas and assumptions which can be taken for granted as one writes. Accordingly, I introduce a number of generally useful ideas as I go along, attempting to explain them clearly on their first appearance and subsequently using them freely.

Finally, there is the statutory warning which must always accompany any thoroughgoing sociological treatment of science. When social scientists study scientific knowledge, the focus of attention is the knowledge associated with a specific social context. The question is why such knowledge is accepted as knowledge in the context. The answer must expose every sociologically interesting factor which has a bearing on this question. The assumption is that such factors are always involved in the network of causes which maintains the credibility of a body of knowledge. The plausibility of the assumption derives both from the merit of the work it inspires, and the glaringly obvious inadequacy of its converse, which denies that knowledge is a social phenomenon. The appropriate method of investigation is that accepted for the study of thought and action in general; in this case the thought is scientific thought and the action is research. This sociological orientation is very different from that of scientists themselves, who only explicitly consider how their knowledge relates to experience or to the performance of technical tasks. And it can lead to findings which contradict the self-images and forms of justification professed and accepted by scientists. There is no escaping the fact that sociological work could conceivably be employed in attempts to weaken some legitimations of science. But the sociology of knowledge is emphatically not, as its critics often mistakenly believe, itself a denigration of science; on the contrary, it is in many ways modelled upon scientific investigation, and any claims to credibility it comes to have must be closely related to those of science itself. The sociology of knowledge is a matter-of-fact, empirical field of study which happens to include, among its subject-matter, the knowledge and culture of science. It must examine that knowledge without any concern for the implications of its findings if it is to maintain its own integrity.

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I should like to express my gratitude to the many people who helped in the preparation of this book, and in particular to the staff and students at the Edinburgh Science Studies Unit for providing so suitable an environment in which to work and write. Michael Barfoot, Celia Bloor, David Bloor, David Edge, Anthony Giddens, John Law, Donald MacKenzie and Andrew Pickering all read earlier drafts of the manuscript, and helped me to identify and eliminate many weaknesses in substance and in presentation. Carole Tansley made light of the daunting task of producing a typescript from my initial handwritten material. And Moyra Forrest assembled an index incomparably superior to anything I myself should have been able to accomplish.

Figure 5.1 first appeared in *Centaurus*, vol. 3, 1953, p. 135; and I am grateful to the editors of that journal for allowing it to be reproduced here.

Author's Note

It is convenient to divide Kuhn's publications into three classes. First, there are concrete historical narratives, produced in the 1950s and the early 1960s, and addressed mainly to professional historians of science: particularly noteworthy here are *The Copernican Revolution* (1957), and a series of papers on the history of thermodynamics. Second, there are publications, beginning around 1960, which represent an attempt to understand science in general terms and to identify its distinctive features. This is the work through which Kuhn is most widely known, and in which most of his sociologically interesting ideas are to be found. It includes his book on *The Structure of Scientific Revolutions*, first published in 1962, but referred to here in the slightly longer second edition of 1970. And it embraces also a wide-ranging series of papers on historical, philosophical and sociological issues, many of which have recently been reprinted in *The Essential Tension* (1977). Wherever possible, when I expound or quote from Kuhn's papers, I give page numbers which refer to this readily available volume. Finally, there is work which reflects the detailed attention Kuhn has devoted in recent years to the history of quantum mechanics. *Black Body Theory* (1978) is the principal contribution to date of this kind. It is a fascinating and important work; but its central concern is to establish, through meticulous care and uncompromising attention to detail, a particular description and interpretation of a specific historical episode. Since *Black Body Theory* breaks new ground only as historical narrative, and deals with developments of daunting technical complexity, I do not discuss it here. Inevitably most of my citations relate to the wide-ranging and comparatively speculative material of what perhaps should be called Kuhn's 'second period'.

One matter of terminology also requires preliminary clarification. An important concept in Kuhn's work is that of the scientific *paradigm*. But in *The Structure of Scientific Revolutions*, where it is extensively employed, its meaning is not as clear and consistent as might be wished (cf. Masterman, 1970), and this has given rise to some significant misunderstandings. Kuhn (1970, Postscript) himself has recognised the problem and sought to eliminate it, but difficulties remain for anyone who would discuss the development of his work over a period of years. I have decided to use 'paradigm' to denote an accepted problem-solution in science, a particular concrete scientific achievement. This is the sense of 'paradigm' which, in his most recent work, Kuhn conveys by the term 'exemplar'. Purely as a matter of terminological decision, then, to which I try to adhere throughout, paradigms are exemplars.

B.B.

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