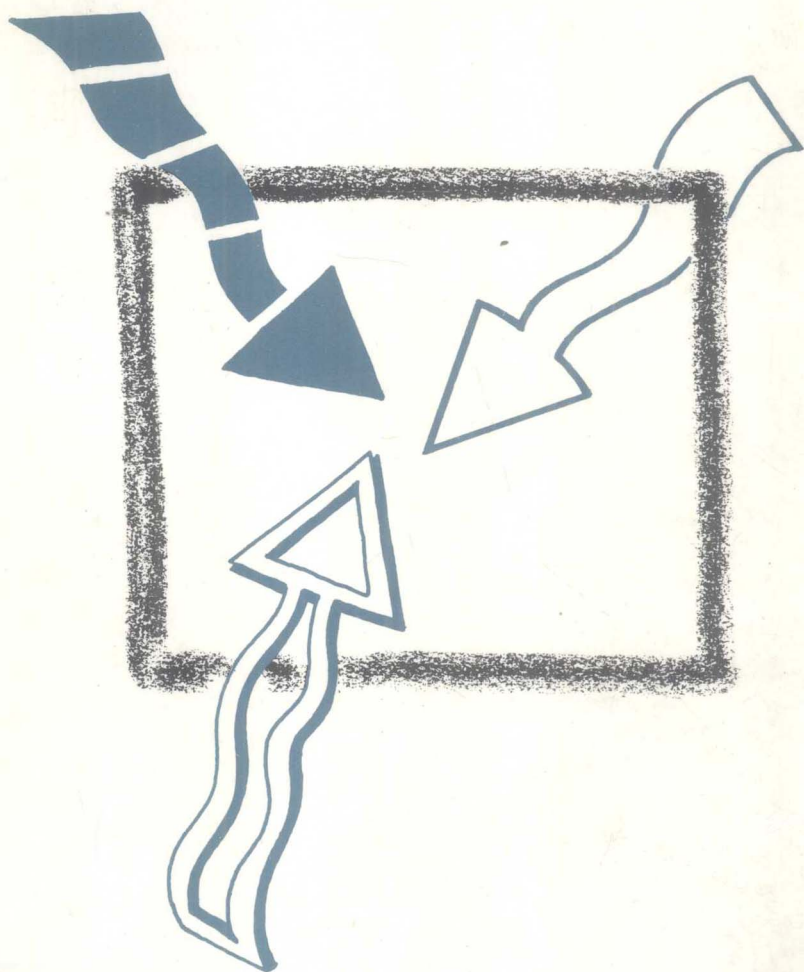


NEW PHILOSOPHIES OF SOCIAL SCIENCE

**Realism, Hermeneutics
and Critical Theory**



William Outhwaite

Theoretical Traditions in the Social Sciences

New Philosophies of Social Science

Realism, Hermeneutics and Critical Theory

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M
MACMILLAN
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To my parents

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W.O.

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Introduction

The social sciences in the English-speaking world have been through some dramatic changes in the last twenty years in their understanding of their own nature and methods. I shall call this area the philosophy of social science, though it includes the work, not just of philosophers but of very many sociologists and a smaller number of representatives of the other social science disciplines. In describing realism, hermeneutics and critical theory as 'new' philosophies of social science I do not mean to deny that these three movements have a much longer history. Realist philosophies of science are as old as science itself, though their conscious application to the social sciences dates from the early 1970s. Hermeneutic theory is at least 150 years old, and its application to history and the social sciences is not much more recent, while 'critical theory' was developed in the 1920s. All three however experienced a kind of take-off in the 1970s, moving into the space vacated by the previously dominant conception of social science.

Until this time, it is hardly too much to say that there was no philosophy of social science in the English-speaking world. Rather, there was an empiricist or positivist philosophy of science as a whole, primarily oriented to the physical sciences. This was widely held to constitute the methodological ideal to which the social sciences should aspire. *Ad hoc* modifications of the model were proposed to deal with the alleged greater complexity of social reality, the virtual impossibility of experimentation and the severe limitations on prediction, the problems of ideology and objectivity and so on. Only historians were mostly unable to identify their subject with the orthodox view that to explain an event is to be able to deduce it from a general law.¹

From this point of view, the three philosophical positions with which this book is concerned were distinctly marginal to the practice

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of the social sciences. Scientific realism, the claim that the entities postulated by science may really exist, and are not merely convenient fictions, was a somewhat arcane tendency within the philosophy of science; discussion tended to centre around microphysics and the problems of quantum theory. Hermeneutics, the theory of textual interpretation, was known to have begotten Max Weber's concept of *Verstehen*. This had traditionally been wrapped up in a lot of Central European verbiage, but Theodor Abel had shown that all it really involved was the filling out of an explanation by invoking a more or less obvious 'behaviour maxim', e.g. that people tend to light their fires when the weather gets cold.² Finally, critical theory was beginning to have some impact, with the publication in 1964 of Marcuse's *One Dimensional Man*, but there was not much sense of its general shape, nor of its implications for the methodology of the social sciences. In methodological terms, as well as in its moral pathos, critical theory seemed irremediably exotic and obscure.

I shall not discuss in any detail the way in which these alternative philosophies and methodologies came into the prominent positions which they occupy in modern social theory. First, the positivist tradition and the critiques which it attracted have already been widely discussed. An earlier book in this series by Christopher Bryant provides an excellent survey,³ as do Peter Halfpenny's shorter *Positivism and Sociology*⁴ and Anthony Giddens's article on 'Positivism and its Critics'.⁵ Second, an adequate account of the eclipse of positivism would have to pay close attention to very general changes in the intellectual climate in Western Europe and North America and to the institutional expansion of the social sciences. This produced a generation of academics different in many ways from the previous one. Again, though the full history of these processes has not yet been written, their general outlines are quite well understood.

It should be noted, however, that the rise to prominence of the three traditions discussed in this book went hand in hand with the revival of interest in classical social theory, illustrated by the massive output of secondary works on Marx, Weber, Durkheim, Simmel and others. Furthermore, there was a considerable expansion of qualitative as opposed to quantitative methods of empirical research: participant observation, case studies,

unstructured interviews and so forth. On the whole, however, we are dealing with a process which was theory-led, rather than practice-led. In other words, it was not so much that sociologists got involved in qualitative research and subsequently developed a theoretical or philosophical rationale for this practice. Rather, one finds a great outpouring of theoretical and programmatic works, generally produced in isolation from empirical research. This was not just a phenomenon of the division of labour characteristic of a growing scientific discipline; it reflected a widespread feeling among sociologists and some other social scientists that the overall character of social theory required radical re-examination.

A crucial element in this development was of course the revival of Marxism both inside and outside educational institutions. The return to Marx was not just vastly larger in scale than the return to Weber or Durkheim. It was qualitatively different, in that the majority of the writers who engaged in it identified themselves as Marxists, often in opposition to sociology and the other social sciences. In terms of the contrast drawn by Tom Bottomore,⁶ it was a matter of 'Marxism against sociology' rather than 'Marxism within sociology'. Yet in institutional terms it was still Marxism *within* sociology, in the sense that this work was largely done by teachers of sociology. For this reason, as well as for reasons to do with the intellectual structure and content of sociology, the opposition between the two tended to become less and less clear-cut. Whereas Marxist economists can still be identified in many Western countries as a distinct sub-species of economist, there is no such clear dividing-line between Marxist and non-Marxist sociologists.

The revival of Marxism, then, like the eclipse of positivism, forms a backdrop to the rise of the three traditions discussed in this book. Its impact on them has of course been very different. Hermeneutics and *Verstehende* sociology have generally developed in opposition to Marxism, except where hermeneutics shades off into critical theory. The latter is of course unintelligible except in terms of its Marxist origins, however far it may have departed from orthodox Marxism. The case of realism is more complicated. A realist philosophy of science does not entail the adoption of any particular variant of social theory. On the other hand, Marx's own largely implicit philosophy of science can best be understood as a form of realism,⁷ and his conception of scientific practice has strong

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affinities with that of modern realists. Furthermore, as a matter of fact, realists concerned with the social sciences have mostly been very sympathetic to Marxist social theory.

The three traditions similarly diverge in their critical response to positivism, and this is the central theme of Chapter 1 of this book. I shall argue that the main initial impact of hermeneutics and critical theory was in stressing the distinctiveness of the social sciences from the sciences of nature. Conversely, the realist critique of positivism claimed that positivism had radically misunderstood the *natural* sciences, and suggested that natural and social science may not, after all, be so radically different in their methods.

In Chapters 2 and 3 I move on to a detailed examination of realism as a philosophy of natural science and in its implications for the practice of social science. The remaining chapters discuss the further development of hermeneutics and critical theory, and the extent to which their essential insights may be incorporated into a realist conception of social science. My basic message is an ecumenical one. Unlike many representatives of hermeneutics and critical theory within the social sciences, I see these movements as compatible, in the last analysis, with a broadly realist understanding of both natural and social science.

1

Philosophies of Social Science: The Old and the New

As I said in the introduction, I do not intend in this book to add to the existing literature on positivist philosophies of science. We need, however, to look at them briefly in order to set the scene for the growth of the hermeneutic, critical and realist alternatives. One of the best recent books on positivism distinguishes no fewer than twelve senses of the term.¹ For present purposes, however, we can get by with three variants.

The first is Comte's original formulation in the early nineteenth century. Positive knowledge, so called to distinguish it from the theological and metaphysical conceptions of the world from which it emerged, yields a methodologically unified and hierarchical conception of science, based on causal laws of phenomena, derived from observation. The progress of knowledge is a process by which the individual sciences, each with its own distinct level of analysis, successfully attain the state of positive, scientific knowledge. 'Sociology' (the term is Comte's invention) is the last to achieve this status and provides the coping-stone for the entire edifice of science and the basis of a positivist morality and politics.

This conception was immensely influential in nineteenth-century thought. In the philosophy of history, for example, H. T. Buckle insisted on the need for a science of history based on the operation of universal laws,² such as those determining crime and suicide or relating the frequency of marriage to the price of corn. In a rather different way, Marx and Engels, despite their contempt for Comte himself, shared the aspiration to discover 'natural' laws of human social development. Darwin's account of biological evolution gave

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a further impetus to this way of thinking (and Engels, of course, drew an explicit parallel with Marx's discovery of the laws of social evolution).

The mid-nineteenth century also saw the beginnings of the hermeneutic critique of positivism – a critique whose basic thrust has remained the same right up to the present. The German philosopher of history J. G. Droysen deplored the spread of 'crass positivism' from France and Britain to Germany and published a hostile review of Buckle's *History of Civilisation in England*. Droysen's *Historik* is one of the first documents of the hermeneutic alternative, stressing the distinction between nature and mind/spirit (*Geist*), and the way in which our 'understanding' of the latter is something radically different from our comprehension of natural phenomena.³

The second variant of positivism is to be found in the 1920s, in the logical empiricism of the Vienna Circle. These philosophers preferred to avoid the term positivism, since they considered Comte's philosophy of history to be itself metaphysical. Their own critique of metaphysics was sharpened up in a theory of language, according to which propositions which could not be tested and verified were literally meaningless. The other major way in which they diverged from Comte was in their analysis of the relations between the sciences. For them, the unity of science meant that the laws or, more generally, the language of the 'higher' sciences in Comte's hierarchy could be 'reduced' to that of the lower ones. The propositions of sociology could ultimately be analysed down into those of physics or of material-object language. In this conception, the hermeneutic stress on understanding the meaning of social action was a residue of metaphysical thinking. As Otto Neurath put it, intuitive understanding *might* be of some use to the social scientist, but no more so than a reviving cup of coffee.

This conception of science led, in a modified form, to the third variant of positivism which I shall identify here, and which is the most important for our purposes. It has sometimes been called the 'standard view' in the philosophy of science – a term which indicates its dominant position in the English-speaking world in the 20 or 30 years around the middle of this century. Its main representatives were Rudolf Carnap, Carl Hempel and Ernest Nagel, who emigrated from central Europe to the United States, and Karl Popper, who settled in Britain.

Without going into the details of this position here, I need to bring out one of its central elements, and the one which is of most relevance to the philosophy of social science. This is what I have elsewhere called the law-explanation orthodoxy.⁴ The basic theme will, I think, be familiar. It is that all science, including history and the other social sciences, is devoted to the pursuit of explanations, which take the form of general laws, sometimes called covering laws. To explain an event is to relate it to a general law, analysed as a universal generalisation. In a rather hackneyed example, the freezing of my car radiator is explained by the general laws governing the behaviour of water plus the low temperature last night (initial conditions). The roots of this conception of explanation lie in Hume's theory of causation, according to which all we can ever observe is the 'constant conjunction' of events, such as freezing temperatures and burst radiators. This is all we can know, and all we need to know for empirical science to be possible.

The 'standard view' formed the basis of a philosophy of social science which, as I suggested in the Introduction, was not really a philosophy of social science at all. Although it had given up the strong Viennese thesis of the reducibility of all other sciences to physics or to material-object language, physics remained the ideal, and this is as important a fact for present purposes as anything else in the modified logical empiricism which predominated in the English-speaking countries around the middle of this century. Despite the early optimism of Otto Neurath,⁵ it proved impossible to beat the social sciences into a shape acceptable to this philosophical view of science.

The consequences of this conception can be found in a wide variety of textbooks in the philosophy of social science. The epistemic privilege awarded to physics shines forth from the merest glance at the chapter headings and examples. Generally the social sciences are kept out of the book until the later chapters, when the feast of models is complete; their own offerings are so mediocre that they would lower the tone of the volume.⁶

What exactly was wrong with the social sciences, on this view? More important than difficulties of experiment or measurement or the problem of ideological influences – though these were also a source of concern – was the problem of laws and explanations in the social sciences. Here one may distinguish between the general appeal of the idea that the social sciences might one day obtain a

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body of laws comparable to the fundamental principles of physics and chemistry, and the much more precise form which this ideal took in empiricist philosophy of science, in which as we have seen, explanation was identified with deducibility from covering laws. No law, no explanation. One of the earliest statements of this view is in Karl Popper's *Logic of Scientific Discovery*, first published in German in 1934.

To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*.⁷

It is this conception of laws and explanation which is the central target of many realist critiques. For the moment, we need only consider its massive implausibility as an account of explanation in the social sciences. Hempel's classic paper, 'The Function of General Laws in History',⁸ attempted to resolve these problems. Hempel's aim was to demonstrate that

in history no less than in any other branch of empirical inquiry scientific explanation can be achieved *only* by means of suitable general hypotheses, or by theories, which are bodies of systematically related hypotheses.⁹

Hempel suggests as an example

the statement that the Dust Bowl farmers migrate to California 'because' continual drought and sandstorms render their existence increasingly precarious, and because California seems to them to offer so much better living conditions. This explanation rests on some such universal hypothesis as that populations will tend to migrate to regions which offer better living conditions. But it would obviously be difficult accurately to state this hypothesis in the form of a general law which is reasonably well confirmed by all the relevant evidence available.¹⁰

Difficult, but also irrelevant. It is one thing to assert the importance of comparative evidence about farmers in a similar situation

elsewhere who did or did not choose to migrate; such evidence might, for example, suggest further elements in the explanation such as the fact that mobility is something of a tradition in the US and may therefore be adopted more readily than in other parts of the world. But it is difficult not to feel that Hempel's ideal of a historical explanation, compared to which actual explanations are usually incomplete explanation-sketches, is a metatheorist's fantasy.

Michael Scriven reduced this neatly to absurdity by explicating the following (paraphrased) explanation of Cortes's third expedition to Baja California after the failure of the first two: 'The prospect of gigantic booty, and considerable confidence that by leading the expedition himself the previous causes of failure could be overcome.'¹¹ In a scientifically 'complete' form, this becomes, for example:

- (i) All confident wealth-seeking people undertake any venture which offers wealth.
- (ii) The third voyage envisioned by Cortes offered wealth.
- (iii) Cortes was confident and wealth-seeking.¹²

Not only is the first premise clearly untrue, but the point of the whole analysis is utterly obscure.

I have dealt with the problem of explanation at some length because it illustrates most starkly the paradoxical character of this philosophy of science and the problems it created. As Mokrzycki has pointed out, not only was logical positivism, at least since the time of Neurath, 'a product of a milieu almost completely lacking in contacts with research practice in the social sciences';¹³ it also failed almost entirely to fit that practice. The seriousness with which it was taken by social scientists is probably best explained, as Mokrzycki suggests, by the chronic inferiority complex of the social sciences compared to the sciences of nature:

sociology, together with related disciplines, such as psychology and political science, is in an exceptional position: it is a discipline in which the very status of being scientific is at stake.¹⁴

The solace afforded to the social sciences by the 'standard view' in the philosophy of social science may seem to resemble that of the therapist who explains an inferiority complex by telling the patient that he or she *is*, in fact, inferior. But it also offered an image of