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W. G. Runciman

Excerpt

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I

Introduction: the nature of social theory

SOCIAL THEORY AS SCIENCE

§ 1. The debate between those who affirm and those who deny that there is a fundamental difference in kind between the sciences of nature and the sciences of man has continued without resolution for more than two hundred years. If the arguments to be put forward in this volume are correct, the debate can for practical purposes be regarded as closed. But to show this is not so much to arbitrate among the contending parties as to rewrite the terms in which the debate ought initially to have been framed. My principal argument to this effect can be summarized in a sentence: there is no special problem of explanation in the human sciences, but only a special problem of description. Properly defined, explanation and description can be distinguished both from each other and from either the reportage of facts or the advocacy of values; and this in turn facilitates not only the analysis of the concept of understanding but the demarcation of methodological questions from either philosophical or technical ones. But how is this claim itself to be construed? The sciences of man have evolved in not only historical but methodological subordination to the sciences of nature, and this has meant among other things that the twentieth-century social scientist, whatever his* views on the scope and nature of his subject, cannot help being driven by the relative paucity of his results to the fear that he may be forever excluded from the enchanted garden in which the fruit of the tree of knowledge can be seen hanging so much closer within reach. He cannot but ask himself not simply how his methodological problems are to be solved but whether they are not problems of an altogether different order from those of the sciences of nature. He needs to know, in other words, in just what sense his difficulties *are* methodological in the first

* Or of course hers: in the absence of a singular pronoun covering both sexes, I propose for convenience to use 'he', 'him' and 'his' to denote both hypothetical researchers and their presumptive readers, whether male or female.

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place. How is he to tell a problem of technique from a problem of epistemology? And until he does, how can he settle the appropriate sense to be given to the terms 'explanation', 'description', 'fact', 'understanding' and the rest?

It does not immediately follow that theories of human behaviour must be philosophical in some way in which theories in the natural sciences are not, since if it turns out that the difficulties which are peculiar to the human sciences can, after all, be construed as no more than technical, then the contribution which methodological discussion can make to them will be limited to the elucidation of the logic of accepted scientific practice. But this conclusion cannot itself be construed as no more than technical. Even those who deny a fundamental difference between the sciences of nature and the sciences of man cannot deny that the question arises at all. This is itself already a difference; and if we ask how the difference comes about, the answer is not in dispute between rival methodological schools. It comes about because the study of self-conscious human behaviour is itself self-conscious human behaviour, and the differences between the two sides in the long debate can all be categorized in terms of the conclusions which they hold to follow, or not to follow, from that fact.

To speak of two sides is, no doubt, an oversimplification. Those who have affirmed that the difference between the sciences of nature and of man is fundamental have done so on many different and often incompatible grounds. Already in the nineteenth century some, like Dilthey, who held that the human sciences are marked off by their subject-matter, were attacked by others, like Windelband, who held that they are marked off by their method. Many who have agreed in rejecting the Positivist account of the social sciences have disagreed both over the interpretation of the doctrines attributed to it and over the diagnosis of the errors to which those doctrines are alleged to lead. On the other side, those who have held that the study of man is a natural science like any other have in practice interpreted that belief in altogether different ways: it is enough merely to contrast the anthropological method expounded by Radcliffe-Brown with that expounded by Lévi-Strauss. Those few authors who have succeeded in making a lasting contribution to the debate have for the most part done so, as Max Weber did, only by occupying and holding a limited area of the middle ground against attacks directed from either flank. Yet to the methodologist, as opposed to the historian, this diversity of opinions poses less difficulty than might appear, for much of it can itself be accounted for by the persistent compulsion of one side to reject all of the recommendations of the other,

however innocuous and even sensible some of them may be. The tacit assumption which has time and again bedevilled the progress of social theory is that both scientific Naturalism and philosophical Idealism either stand or fall as a whole; and this has been linked in turn with a recurrent tendency on the part of the practitioners of the human sciences to regard some one favoured method as the paradigm by reference to which their presuppositions can be vindicated. But the house of science has many mansions. If B. F. Skinner, author of *The Behaviour of Organisms* as well as *Walden Two*, is right about the way to predict the conditioned responses of bar-pressing rats, this does not make R. G. Collingwood, author of *Roman Britain* as well as *The Idea of History*, wrong about the way to reconstruct what went on in the minds of those who designed Hadrian's Wall.*

In any case, much mutual recrimination among social scientists over the 'scientific' standing of each other's work has rested on a limited, if not downright ignorant, conception of what in fact takes place within the sciences of nature. Methodologists of social theory may not need to know just how molecular biologists, oceanographers, mechanical engineers or inorganic chemists set about their research in the way that they may need to know just how art historians, political scientists, econometricians or demographers do. But they must divest themselves of any general pre-conception about the relative uniformity of the sciences of nature in contrast to the sciences of man. There is more in common between some human and some natural sciences than between many pairs of both. Not only biology but geology furnishes more illuminating parallels for the sociologist, anthropologist or historian than classical mechanics. Here too, the progress of social theory has often been diverted and sometimes retarded by the influence of mistaken analogies drawn from the most immediately fashionable of the sciences of nature. In the eighteenth century, it was bound to be tempting to think of society as a machine, and in the nineteenth as an organism. In the twentieth, it is bound to be tempting to think of it as a communication network. But the gap between analogy and reduction is still too wide to be spanned by the assertion, true as it may be, that social theorists have more to learn from cybernetics than from either engineering or physiology. Neither the substantive nor the methodological problems which are peculiar to the sciences of man will be solved just by drawing analogies to the sciences of nature.

* Although he was: Collingwood's hypothesis that the curtain was designed to screen the approach of troops assembling for a sortie through the sally-ports has been overturned by the discovery that the road running parallel to the inner side of the wall was built a century later. But that is the way science proceeds.

To whatever extent, on the other hand, the problems of the human sciences *are* analogous to those of the natural, the question whether those of the human sciences are technical or epistemological is effectively answered. If the analogy holds, it follows that the problem in question does not arise from the original difference that the one set of sciences is, but the other is not, dealing with self-conscious behaviour. Thus if, for example, the multifarious difficulties surrounding the notion of causality appear to the social scientist to be the impediment barring him from a satisfactory account of human behaviour, he needs to show in what way they are different from and more intractable than those which have not, after all, prevented the emergence of well-tested explanations in biology, chemistry and physics. Philosophers of science are no more nearly unanimous on such awkward questions as the logic of disposition concepts and counterfactual conditionals than they are on causality itself. But practising sociologists, anthropologists and historians have, fortunately, no need to be able to answer them before addressing themselves to the question what, if anything, marks off the practice of the sciences of man from the practice of the sciences of nature as different in kind.

§ 2. To put the question in this fashion at all may invite the objection that I am already assuming a part of what I claimed to be going to demonstrate. But it assumes no more than that there is *a* difference between science, whether of man or nature, and non-science. It carries no more implication about the kind of difference it is than about the position of the human sciences in relation to it. No doubt arguments can be advanced to diminish the epistemological status conventionally accorded to science but denied to metaphysics or religion. But if science has nothing to teach us, then there is nothing to learn. The lecture-room sceptic, here as elsewhere, cannot be controverted in his own terms. But the social theorist confronted with the claim that all aspiration to intersubjectively testable knowledge of the workings of the world is illusory can reply with perfect seriousness that he only wishes he had as many illusions as his colleagues in the Faculty of Engineering.

This rejoinder is at the same time quite compatible with a more modest degree of scepticism. To hold that there is a difference between science and metaphysics, or science and art, or science and the absence of any would-be systematic observation and reasoning about the world in which we find ourselves, involves no necessary commitment to the doctrines of Positivism. The arguments to be put forward in this volume are quite compatible with a reminder that science itself, when studied as a human activity, has certain affinities with religion; that no empirical observation

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is free of presuppositions; that progress in science is not progress towards any goal of final knowledge, but only away from a relatively less learned ignorance; and that not only the substantive discoveries of science but even its most deeply entrenched procedural rules are beyond a certain level not merely provisional but arbitrary. Still less controversial is the assertion that study of the actual course of scientific discovery discloses a wide and consistent discrepancy between the course of the research which led to it and the logic in terms of which its outcome will later be rationalized. But none of this deprives the idea of science of its meaning. The revolution in ideas which led to quantum mechanics or special relativity is still in another category from that which led to abstract painting or *musique concrète*. There need only be *some* additional constraint within which the natural or social scientist exercises his discretion for art to be marked off from science as an activity different in kind.

This difference is linked in turn to the familiar contrast between discovery and creation. It is true that to the degree that progress in art proceeds by way of advances in technique it too proceeds by discovery: the painters of the Italian Renaissance were no less significantly dependent on the discovery – or, strictly, rediscovery – of perspective, or the architects of the Gothic cathedrals on the discovery of the vault, than twentieth-century molecular biologists on advances in X-ray crystallography. But we still cannot speak of the history of art as cumulative in the way that we can speak of the history of science. The contrast may not be as clear-cut as has sometimes been assumed by Positivist philosophers of science. But it is not wholly spurious. The successes of science may be both historically fortuitous and methodologically provisional, but some, at any rate, of its failures are irredeemable. It is not simply a matter of academic fashion that phlogiston theory, or humoral physiology, or phrenological explanations of criminality have yet to return to favour.

Yet this, as methodologists of all persuasions will acknowledge, is not the end of the matter. For even when the validity of the – or at least a – contrast between science and art or metaphysics has been accepted, it can be used to make the status of the social sciences seem more problematic rather than less. If the history of science is narrated as, so to speak, an ontologist's story, then the social sciences should fit well enough into it: it has merely been a matter of finding out whether there are or aren't such things in the world as action at a distance, photons, biological species, the collective unconscious, perfect competition, schizophrenia, totemism, stateless societies and so on. But methodological questions cannot simply be reduced to ontological. We cannot

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bypass the difficulty of analysing the kinds of knowledge at which social scientists claim to have arrived. Are we really to say that Marx discovered (in Engels's phrase) the 'law of the development of human history' in the same sense that Newton discovered the laws of motion? Or that Malthus discovered that population increases geometrically if unchecked in the same sense that Theodorus of Cyrene discovered the irrationality of $\sqrt{3}$? Or that Marshall discovered the elasticity of demand in the same sense that Harvey discovered the circulation of the blood?

These are, at least for the moment, only rhetorical questions. But they are already symptomatic of the ambivalence which seems inherently to attach to the claims of the social sciences to be sciences. Is it not, after all, puzzling that Marx's writings should still be read by sociologists at all? No physicist, unless also a historian of physics, reads the writings of Newton, or needs to. No biologist needs to read Darwin's *Origin of Species*. Yet Marx, and his commentators, and his commentators' commentators are read and reread a century after his death quite as though *Capital* made no more pretence to be a work of science than, say, Carlyle's *Sartor Resartus*. A possible answer is that Marx's discoveries still stand intact and no further advance has yet been made on them. But this will hardly do. Not only have there indeed been modifications and advances, but it is in any case impossible that a social theory framed both in and for mid nineteenth-century Europe should be directly applicable to circumstances and events of a kind which its author could not conceivably have foreseen. It is this which makes it plausible to regard Marxism, as so many of both its adherents and its detractors do, not as a scientific theory about the workings of human societies so much as a metaphysics in terms of which to view them, coupled with a programme by means of which to change them; and is not this enough by itself to show that the sciences of man and of nature are different in kind?

Yet even if it is agreed that Marxism is both a metaphysics and a programme, and that this is why it continues to be debated as widely and as vehemently as it is, it does not follow that social science is inherently 'ideological' in some sense that natural science is not. Celestial mechanics was ideological in the time of Copernicus and Galileo; geology was ideological up to and including the time of Lyell; biology was ideological in the time of Darwin and, later, of Lysenko. They ceased to be so not just because the opponents of certain of their findings came to change their politics but because the grounds of those findings were such that they had in the end to change their beliefs. No doubt those findings and therefore beliefs must, like all scientific findings and beliefs, be regarded as susceptible in principle to some sort of possible revision. But in this

there is no distinction between the sciences of man and of nature. Some further argument needs to be adduced before it is claimed that twentieth-century debates over wage-push versus cost-pull inflation are inherently more 'ideological' than sixteenth-century debates over the number of Jupiter's moons.

§ 3. Here already, however, three related objections are likely to be put forward by those who deny the methodological unity of natural and social science. First: have I not chosen examples to suit my case which presuppose, but do not by any means demonstrate, that there is no divide between nature and culture? Second: am I not simply avoiding the whole question of cultural relativism by which the force of my examples is effectively undermined? And third: do I not assume a sense of 'ideological' which ignores the fact that the social sciences, unlike the natural, are not and cannot be value-free?

To the first objection, the answer is that no more need be assumed at this stage of the argument than that there are some empirical questions about human behaviour which are empirical in the same sense and to the same degree as in any science of nature. To say this is not covertly to define 'behaviour' to exclude what those who deny the methodological unity of natural and social science rightly claim to be distinctive of it: it is not to talk merely of automatic reflexes and neurophysiological events. Whatever may be the difficulties in specifying what it is that is distinctive about human behaviour, even the most distinctively human forms of it do still lend themselves to some straightforward questions of observation and measurement. Historians may not yet be able to agree about the causes of the First World War, but none of them (as Clemenceau is reported to have said) maintains that it was caused by the Belgian army's invasion of Germany. Demographers may not yet be able to account satisfactorily for the remarkable rise in population which took place in England and Wales between the end of the eighteenth and the end of the nineteenth centuries; but none of them, even in the absence of public registration of births and deaths for the earlier part of that period, claims that it actually went down. Psychologists may not yet be able to define, let alone to explain, schizophrenia; but none of them disputes that there is a distinctive condition – porphyria – which is brought about by the presence of a dominant autosomal gene and commonly manifests itself in the third decade of life through behavioural symptoms which sometimes lead to its being mistakenly diagnosed as a 'schizophrenic' illness. Sceptics about the impossibility in principle of a science of man are perfectly entitled to point to the limitations of our

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knowledge of human behaviour by comparison with our knowledge of the workings of purely physical systems. But they are not entitled to infer from these limitations that nothing whatever is as well known, in a 'scientific' sense of 'known', about human behaviour as about the movements of the galaxies or the properties of the nucleic acids.

To the second objection, the answer is not, as it has sometimes been argued to be, that the doctrine of cultural relativism is self-refuting, but only that it is self-defeating. To deny that universal validity attaches to any claim to knowledge advanced by members of a 'scientific' culture is not to be committed to a claim that this proposition itself is universally true. But it is to deprive all academic enquiry of its (admittedly self-set) purpose. There is in practice no escape for either the natural or the social scientist from a correspondence conception of truth. This again is not a simple matter of logic. Any discussion of truth-conditions raises issues which are, and are likely to remain, controversial among philosophers. But to assert that 'copper expands on heating' or 'the Battle of Hastings was fought in 1066' is true – or for that matter false – is to presuppose a relation of some kind between observation-statements and the state of the world. The nature of this relation is a further question which the practising sociologist can well afford to leave to the philosophers of science. But he cannot, whatever his view of the nature of it, afford to do without a distinction between statements which assert something to be true by virtue of there being such a relation and statements which do not. Among the observation-statements which he makes will no doubt be the statements both that what is held to be true on one side of the Pyrenees is held to be false on the other, and that what it means to say of a statement that it is true is held to be one thing in one Department of Philosophy and another in another. But neither he nor his fellow-sociologists from rival theoretical schools will dispute that 'what it means to say of a statement that it is true is held to be one thing in one Department of Philosophy and another in another' is true if and only if what it means to say of a statement that it is true is held to be one thing in one Department of Philosophy and another in another.

To the third objection, the answer is that it has relevance to what has been so far said only if it is the case that every observation-statement about human behaviour logically entails, or is entailed by, some value-judgement; and the onus is on anyone who holds this to demonstrate it. It is not in dispute that there is in practice a connection between observation-statements and value-judgements in the sciences of human behaviour for the simple reason that we do all have standards of value which apply to the behaviour of ourselves and other people but not to

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the behaviour of inanimate objects (their applicability to animals being itself a matter of one's values). Nor is it in dispute that statements of the form of observation-statements may on closer inspection turn out to be value-judgements; or that many value-judgements are dependent on empirical assumptions which, if discarded, would require the value-judgement to be revised; or that the practice of social science raises issues of policy about which different people's value-judgements are likely to conflict; or that many social scientists will, in the absence of definitive evidence, incline to believe what best accords with the value-judgements to which they are committed; or that a social scientist's initial choice of topic for study, even if not psychologically determined by his moral or political values, is a value-judgement of a kind in itself. But from none of these assertions does it follow that no statement of purported fact about human behaviour can be value-neutral – that is, true (if true) or false (if false) irrespective of what the sociologist who states it holds to be morally, politically or aesthetically good or bad.

In stating and dismissing these three objections, I do not pretend that the philosophical questions underlying them are not serious. Nor do I pretend that anything said in this volume will bring those questions closer to resolution. But to the methodologist, as opposed to the philosopher, of social science, discussion of them is necessary only to the extent that the practice of social science would be modified if one rather than another answer to them is thought to hold. There are many contentious issues in the philosophy of the social sciences, and on many of them philosophical as well as merely technical discussion can be of demonstrable benefit even to the least philosophically-minded of their practitioners. But practising social scientists need no more be inhibited from doing research at all by their inability to provide a cast-iron refutation of cultural relativism than their colleagues in the Faculty of Engineering by their inability to provide a cast-iron proof that the world has been in existence for longer than half an hour. The considerations which make relativism plausible may make the construction of explanatory, descriptive and/or evaluative theories of human behaviour problematic in a way that theory-construction in natural science is not. But this does not require practising sociologists, anthropologists and historians to abandon their conviction that they can report at least some of it as a matter of fact independently of rival conceptions of the nature of human agency, rival accounts of what it means to claim of a proposition that it is trans-culturally valid, or rival doctrines of the role in social science of moral and political value-judgements. They would have to do so only if it were to be convincingly demonstrated to them that nothing

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whatever which they have succeeded in reporting as having happened or been the case can be treated as factual in the same sense and to the same degree as the findings which natural scientists report and which are accepted as such by their fellow-practitioners even from rival theoretical schools.

§ 4. To say that the study of human behaviour is at least to some degree an empirical and not a purely philosophical matter is not, perhaps, to say very much. Indeed, it may be that not even the most radical Idealists* have ever seriously believed that a rise in the population of England and Wales or the German army's invasion of Belgium are not matters of fact but of ideological interest or metaphysical speculation. The more formidable charge of the Idealists is that the would-be sciences of human behaviour consistently fail to live up to their own conception of what constitutes a science: they fail, that is, to achieve the cumulative theoretical progress, or even the first decisive rupture with common sense, which marks off the activity of science from the casual inductive empiricism by which ordinary people order and classify their world and the other people in it. Where, the Idealists will ask, in all the centuries in which attempts have been made to study social behaviour scientifically, have theories deserving of the name been found? What is 'sociological theory' except history without the dates? What is 'anthropological theory' except a compendium of travellers' tales? What is 'economic theory' except glorified book-keeping?

The last of these questions is particularly provocative, since of all the sciences of human behaviour economics is by general consent the most fully developed and the most firmly established, and its history from the days of Quesnay and Adam Smith can be (and often has been) told in terms of a cumulative extension of observation, development of theory, diagnosis and correction of previous error and refinement of technique. But it can also be told as a sequence not of scientific discoveries but of ideological shifts whereby the concepts of money, labour, value and trade are redefined to fit the institutions and interests of each succeeding period. Even with hindsight, it is difficult to retell the history of previous disputes between rival schools in such a way as will convincingly fit the model of cumulative test and revision. On the cumulative view, the

* By 'Idealists' is meant simply those sometimes designated 'anti-Naturalists' – those, that is, who deny the methodological unity of the sciences of man and of nature. The label thus includes Collingwood, although he himself repudiated it. But it has no more to stand only for the doctrines of Hegel and his followers than 'Positivist' has to stand only for either Comte and his followers on the one hand or the members of the Vienna Circle on the other.