POSITIVISM, NATURALISM, AND ANTI- NATURALISM IN THE SOCIAL SCIENCES*

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1. Introduction

For well over a hundred years there has been a continuous debate as to whether the methodology of the natural sciences can appropriately be employed in the study of human behaviour. Clearly, this question depends partly on what view is taken of that methodology. I shall suggest that many arguments opposing the methodological unity of the natural and social sciences rest upon a view of the former which has been increasingly, and successfully challenged in the last ten years or so. In section 2, I shall describe what I believe to be the main features of that view: namely, *positivism*. In section 3, I shall outline the positions of two groups of anti-positivist writers, who will be called the *realists*, and the *neo-Wittgensteinians*. In section 4, I shall give some examples of arguments against methodological unity which seem to presuppose a positivist characterization of the natural sciences.

I shall use the terms 'naturalism' and 'anti-naturalism' to refer, respectively, to the claims that the methodology of the natural sciences can, or cannot, be applied to the social sciences. Though this is not the only well-established use of these terms, it is a fairly common one (see Kaufmann 1944, ch. 10; Popper 1957 *passim*). By 'methodology', I mean neither very general questions about 'science as a rational enterprise', nor highly specific ones about research design and experimental techniques, but a 'middle range' of questions concerning the structure of scientific theories, their relationship to 'evidence', the nature of explanation, and so on. I shall ignore, for lack of space, the important differences that exist within the division of sciences between 'natural' and 'social', and amongst the latter will occasionally be included historical studies.

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Finally, it should be remembered that disputes about 'the unity of science' are not concerned only with *methodological* unity, as I shall be; Hempel, for example (1969, pp. 186-94), distinguishes this claim of 'unity' from two others: the 'unity of language', i.e. the claim that all scientific terms can be reduced to physical terms, and the 'unity of laws', i.e. the claim that all scientific laws can be deduced from those of physics. There are, no doubt, important relationships between these three issues, but the latter two will not be discussed here.

2. Positivism

The characterization which I shall give in this section will be a deliberately limited one. First, it will be mainly confined to this century. Secondly, it will be concerned only with positivist philosophy of science, and not with the more general philosophical movement of this century, i.e. logical positivism. Of course, positivism as a philosophy of science has a long and fairly continuous history (see Duhem 1969 for an appreciative account of positivist views in early astronomy, and Popper 1963a on Berkeley's 'instrumentalism'), though it may well be that it has emerged most strongly at times of radical theoretical changes in the history of science: particularly, I suspect, when those changes have apparently involved the adoption of a new ontology, a different view about the ultimate constituents of the universe, which was for various reasons hard to accept (cf. Hesse 1969, pp. 85-6). In the nineteenth century positivism was not merely a philosophy of science but expressed a more general world view as a philosophy which lauded the achievements of science. And in the twentieth century, positivist philosophy of science was, in part, a manifestation of the epistemology and metaphysics of logical positivism, though one should remember that the latter was itself partly formed by a view of the natural sciences of a traditionally positivist kind.

The most important characteristics of positivist philosophy of science seem to me to be these: First, there is the belief in some theoretically neutral observation language, which is both epistemologically and ontologically 'privileged'. That is, statements made in that language are either indubitably true (or false), or at least verifiable (or falsifiable) in a simple and direct manner, and any statements containing descriptive terms which might appear to refer to entities not belonging to that privileged ontological class, can be translated into, or reduced to, statements in the privileged language. Descriptive terms not belonging to this language are described as 'theoretical' and their meaning is, from this point of view, problematic. It is in this way that the 'problem of theoretical entities' often arises. What the terms in the observation language are themselves held to refer to varies considerably. They might be sense-data, or instrument-readings, or 'physical objects', but they must be such as to justify their claim to epistemological priority. The precise manner in which the translation of 'theoretical' terms is carried out also varies. Indeed a major part of the history of positivism in this century has centred around the problem of giving a satisfactory account of these translation- or reduction-rules (see Hempel, 1965a and 1969; Shapere, 1965, Introduction). It is, of course, the claim that such translations are essential to any science that is manifested in the demands for 'operational definitions' made by naturalists in the social sciences. (For criticism of such demands in psychology, see Taylor 1964).

For the positivist, it is the aim of science to provide us with predictive/ explanatory knowledge concerning these privileged entities. Scientific theories are to be seen, primarily, as sets of highly general, law-like statements, preferably taking the form of mathematically expressed functional relationships between measurable variables. From these laws, together with statements of observable 'initial conditions', can be deduced statements in the observation-language describing events whose occurrence or non-occurrence are both tests of the truth or falsity of the theory, and also what it is that the theory enables us to predict and explain. Thus the relative merits of different theories can be decided by testing them against the same observations. A theory which predicts what does not happen can be rejected, and its rival, which does not fail in this way, adopted. Where one theory has more predictive/explanatory power than another which is not itself falsified by any observations, the former is to be preferred.

According to the positivist view, to explain an event is to deduce a description of it from statements of such laws and conditions. Similarly, to explain a law is to deduce it from 'higher level', i.e. more general laws; so the difference between laws and theories is essentially one of the degree of generality involved. Further, it is claimed that there is no important difference between explanation and prediction. Both must proceed by deduction. The difference is either one of the time at which this deduction is carried out, in relation to the event, or of the attitude or interest of the scientist concerned. The *logic* of explanation and prediction are the same.

What is the positivist attitude to the role of models in science? Two distinct positions are common. Either the term 'model' is taken in its logico-mathematical sense, i.e. as a 'sentential' model. Or models, which frequently *seem* to be attempts to describe entities which are not referred to by the observation language, are regarded as psychological aids to the understanding: helpful metaphors, particularly to those with less rigorous minds, but not genuine constituents of a theory. As such, they do not merit much discussion by the *philosopher* of science, but only by the psychologist, or perhaps historian of science. For the positivist is concerned only with the 'logic' of science, not the psychology of its practitioners, nor its history. What matters is not the 'context of discovery', but that of 'justification'. Thus little attention is paid to the manner in which theories are arrived at-only to their testing. For some positivists, 'logic' includes both deductive and inductive logic, and the latter might provide some justification for formulating a particular theory. Others, who reject induction, will describe this process only as 'forming hypotheses'.

Finally, one should mention something of the characteristic flavour of the positivist literature, since this is closely connected to this view of science. There are a large number of highly technical studies, utilizing the techniques of mathematical logic, attempting to set out the general form of the relationship between theories and their confirming, or disconfirming, 'evidence', or of the reduction sentences connecting 'theoretical' with 'observational' terms, and so on. These studies are, to some extent, self-perpetuating. Early formulations generated logical paradoxes, and attempts to deal with these formed an increasingly large proportion of the literature. (For a review of one such paradox, that of 'confirmation', see Mackie 1963. For a similar characterization to that set out here, see Shapere 1966 pp. 42-4. Positivist works on the methodology of science abound: perhaps the best single treatment from this standpoint is Nagel 1961).)

3. Alternatives to positivism

I mentioned earlier that the attacks on positivism fall into two main groups: the realists, and the neo-Wittgensteinians. The former term is a fairly standard one; the latter is my own. It would, of course, be wrong to suppose that all those who have opposed positivism belong to one or other of these groups, and some writers could be said to belong to both. None the less, the types of argument and views of science which have been advanced seem to me to have important differences, of roughly the following kind.

The realists (see, amongst many others, the listed works by Bohm, Bunge, Harré, Hesse, Maxwell, Smart) have been primarily concerned with ontological problems, and have directed their attacks at the positivists' attempts to 'explain away' the apparent references in science to entities which do not qualify as 'observable'. Frequently, too, they have been opposed to the Humean view of causation which underlies many of the positivists' arguments, and have seen the task of science as discovering the causal mechanisms by which (undoubtedly existent) 'theoretical entities' bring about the regularities of observable phenomena.

By contrast, the neo-Wittgensteinians have at least partly directed their attacks on positivist philosophy of science in ways which derive from the general opposition to logical positivism to be found in the later writings of Wittgenstein, and in the 'analytical' school of philosophy. Two elements of Wittgenstein's thought have been of particular importance to writers such as Toulmin (1958 and 1961) and Hanson (1958). (Some aspects of Kuhn's work (1970) seem to me also to have affinities with Wittgensteinian ideas). These are the notion of different language-games, and the comments on 'seeing' and 'seeing as' (see Wittgenstein 1953, Part II, section 11). Large-scale changes in scientific theories are often regarded as changes in language-games, with the consequence that communication between participants in the different theoretical frameworks is by no means easy. The view that there is some clear difference between what we directly observe, and what we infer from those observations, is frequently challenged, and the importance of different contexts to the meaning of various terms is emphasized.

It should be remembered that, though both these groups are opposed to positivism, there is no obvious compatibility between their own views of science. But at least one thing they have in common is a strong interest in the history and practice of science, in how science actually develops, and a belief that any philosophy of science, whilst not being merely a description of scientific practice, must bear some close relation to it. And there is little doubt that one of the more important influences on both groups has been the results achieved by the work of recent historians of science, with many realists and neo-Wittgensteinians having themselves made contributions in this field. (This point is emphasized by Shapere 1966; amongst these contributions are Hanson 1963, Harré 1964, Hesse 1961, Toulmin & Goodfield 1961).

Thus both groups have attacked most of the positivist views described in section 2, though often from different standpoints. The realists have insisted that no apparent *epistemological* priority of observation-statements should lead us to deny the *ontological* status of entities referred to by terms in the 'theoretical' language. Rather than attempt to *define* theoretical terms in observational ones, observations should be seen as giving us the *means of identifying* the presence and nature of unobservable entities. The 'correspondence rules' of the positivist can often be viewed as describing causal relationships, not meaning-rules (see e.g. Schaffner 1969), and 'theoretical entities' are to be regarded, not as a problem to be explained away by philosophers of science, but as an essential feature of scientific theories, with scientific developments often taking the form of postulating some new member of an already accepted class, or a new ontological class altogether (see Harré 1961).

For the neo-Wittgensteinian, by contrast, what is primarily wrong with the positivist position is the belief that there is any theory-neutral observation language. (See Spector 1967 for a criticism of the distinctions between theory and observation offered by positivists). All our observations are 'theory-laden', and in some sense, when our theories change, so does what we observe. This view, of course, raises crucial problems for any rational account of major theoretical changes, since it may seem that two theories cannot be directly compared as to their adequacy in explaining the 'same phenomena'. (See Shapere 1966 for a discussion of some of these problems, as they arise in Kuhn 1970 and Feyerabend 1962 and 1965, amongst others).

Objections to the positivist view of explanation have also differed between the two groups. One point that both agree upon is that explanation and prediction are not to be regarded as logically equivalent. For the neo-Wittgensteinian, the most objectionable feature of the deductivist view is that a *single* model of explanation should be proposed. Explanations are what people give to others in perplexity; and since both the types of perplexity, and the degree of knowledge of the questioner, are highly various, no single answer to the problem of 'What is an explanation?' can be given, other than 'The removal of puzzlement'. What counts as an explanation depends on the context of the inquiry (see Scriven 1962). For the realist, however, the deductivist model is mistaken because it fails to show the distinctive features of *causal* explanation. We may well be able to deduce an event from suitable laws and conditions, but this may merely show us that the event was to be expected, not *why* it happened.

(Though he himself does not reject the deductivist view, White (1965, ch. 2) states very forcefully the difficulty of distinguishing 'explanatory' from 'non-explanatory' laws. The best survey of the problems of the deductive model is provided by its most important proponent, Carl Hempel (1965b). Hempel now accepts, it seems, that the model does not provide a *sufficient* condition for explanation, only a *necessary* one, though it is worth noting that his arguments even for this modest proposal are rather thin. My own suspicion is that any successful attempt to distinguish explanatory from non-explanatory laws would make the whole model redundant, since we would then have a criterion for causal explanation, as distinct from mere predictability. It could, I think, be argued that the logical necessity built into the deductive model is but an unsatisfactory replacement for the causal necessity which most of its proponents believe Hume to have shown to be unintelligible).

Closely related to their disagreements with positivists over the theory-observation distinction, and explanation, is the realists' view of the role of models in science. For them, theories do not merely consist in sets of universal laws, but also of models, which may often be analogical descriptions of the unobserved entities and mechanisms which are causally responsible for the observable regularities. The purpose of theories is to explain such regularities as may be expressed by laws, and models often perform just this function. They are not merely psychological aids to the understanding, but the key both to the explanatory power, and fruitful development, of theories (see Hesse 1961 and 1963; Harré 1961 and 1970). Finally, the positivist distinction between the psychology and logic of science has been questioned in various ways. The importance of considerations other than those represented by either inductive or deductive logic has been emphasized, e.g. the use of analogical arguments (see Hesse 1963). The nature of scientific theories cannot, it is claimed, be described adequately as sets of laws which are given a purely logical characterization, and the process by which theories are arrived at is a proper subject for study by philosophers of science, despite its not proceeding according to the canons of inductive logic. There are reasons and justifications both for adopting, and for rejecting, theories, which cannot be properly represented in these terms. The whole subject of the growth and development of scientific theories has now become central to both realist and neo-Wittgensteinian approaches to the philosophy of science.

4. Anti-naturalism as anti-positivism

In this section, I shall try to show that several anti-naturalist arguments assume a positivist characterization of the natural sciences, and must therefore be reassessed if we take seriously the attacks on positivism outlined in section 3. It will probably become obvious in what follows that my own position is a realist one. I am not, of course, thereby arguing directly for naturalism. But so far, there have been very few attempts to examine the methodology of the social sciences, and the issue of naturalism, from philosophical positions deriving from these recent challenges to positivism in the natural sciences, and this is clearly an important task. (Two recent attempts to adopt realist views of the social sciences can be found in Fodor 1968, and Ryan 1970, especially ch. 4.)

Many of the examples of anti-naturalist arguments I shall discuss are taken from Winch (1958). I have done this, partly for convenience, and partly because it is deservedly one of the best-known anti-naturalist works. But the examples discussed are not chosen because they are central to Winch's position; I do not attempt to engage with what are his central arguments, concerning rules and reasons, and my discussion of these specific examples should not be taken as a general attack on Winch‘s position. (For one such attack, see MacIntyre 1967).

One of the clearest indications that Winch assumes a positivist view of the natural sciences is that his main naturalist opponent is J. S. Mill. In objecting to the extension of Mill's methodology of the natural sciences to the social sciences, Winch is at least partly objecting to positivism, since there can be little doubt that Mill's view of science was positivist. (See Lloyd 1959 for a similar suggestion, which has not, so far as I know, been developed elsewhere.) As Winch himself (1958) points out,

> "Mill's conception of scientific investigation . . . is based on Hume's ideas about the nature of causation. . . .To say that A is the cause of B is not to assert the existence of any intelligible (or mysterious) nexus between A and B, but to say that the temporal succession of A and B is an instance of a generalization to the effect that events like A are always found in our experience to be followed by events like B". (p. 67).

Winch's main objection to Mill's naturalism is, of course, that when we explain human actions, we do so by discovering the *rules* with which they are in accordance, and not the *regularities* of which they are instances. But at several points, the objections he makes to 'explanation by instantiation' could equally well be made by those who reject the deductivist model of explanation for the natural sciences, and its equation of explanation and prediction. Consider the following passages (Winch 1958):

> "... we might well be able to make predictions of great accuracy ...and still not be able to claim any real understanding of what these people were doing." (p.115).

> "It is not through Simmel's generalization [that the degeneration of a difference in convictions into hatred and fight occurs only when there were essential, original similarities between the parties] that one understands the relationship he is pointing to between Roman and Old Catholicism: one understands that only to the extent that one understands the two religions themselves and their historical relations." (p. 135).

> "Historical explanation is not the application of generalizations and

theories to particular instances: it is the tracing of internal relations." (p. 133).

It would be misleading to suggest, by quoting these passages, that what Winch wishes to substitute for deductive explanation is similar to what a realist might wish to substitute in the natural sciences. After all, Winch's 'internal relations' are not *causal*, but *conceptual*. None the less, the view of explanation which he is opposing is the view that many anti-positivists also oppose. And if one were to accept the possibility that the fact that someone is 'following a rule' may be the (causal) reason why he behaves in the way he does (see MacIntyre 1967), the social scientist would have to become acquainted with such rules in order to give a causal explanation of social behaviour. (But one should also remember a possible objection to Winch's view, namely that the 'rules' that the agent sees himself as following may not be those that he is in fact following. Burns (1967) in effect makes this point in emphasizing the 'critical' function of sociology, as exemplified in Vilhelm Aubert's study of the judiciary in Norway).

The view that the mode of explanation employed in the natural sciences is not appropriate to human, or social behaviour is, of course, quite a common one. Many philosophers of history, for example, have held this view, and implicit or explicit in their anti-naturalist arguments has been the assumption that the deductive model is a correct characterization of natural science explanation. Some of the force of Collingwood's (admittedly metaphorical) contrast between the 'inside' and 'outside' of human actions seems to me to derive from his characterization of science as concerned only with 'external regularities' of observable phenomena (see Collingwood 1948, especially pp. 214 *ff.*). And at least part of the grounds for Dray's (1957) arguments for a distinctive mode of explanation in history is his rejection of Hempel's 'covering-law' model of explanation.

A more complex example from Winch relates to the role of models in scientific theories. He argues against Popper's advocacy of methodological individualism in the following way. For Popper, "the theories of social sciences apply to theoretical constructions, or models, which are formulated by the investigator in order to explain certain experiences, a method which he explicitly compares to the construction of theoretical models in the natural sciences" (p. 127). Winch quotes a passage from Popper's *Poverty of Historicism* (Popper 1957, section 29), in which the latter argues that if we realize that scientific models are not intended to depict some 'observable ghost or essence' lying 'within or behind the changing observable events', we will not be misled into a 'methodological essentialism', which regards the abstract constructs of theoretical sociology as descriptions of real entities, such as institutions, classes, wars, etc. Winch, however, insists that this view of social institutions as merely "explanatory models introduced by the social scientist for his own purposes is palpably untrue". The member of a country at war with another, for example, has his behaviour governed by his conception of himself as a member of a belligerent country. The concept of war belongs essentially to his behaviour; but "the concept of gravity does not belong essentially to the behaviour of a falling apple in the same way; it belongs rather to the physicist's explanation of the apple's behaviour" (p. 128).

The point which emerges from this argument that I wish to emphasize is this: that Winch seems to accept Popper's view about the role of models in the natural sciences and proceeds to contrast this with their role in the social sciences: the 'theoretical models' of the latter are not merely explanatory devices, introduced for the convenience of the scientist, but attempts to describe features of a society that actually influence its members' behaviour. Yet this, of course, is not dissimilar from the view of the function of models in the natural sciences which is adopted by several anti-positivists.

(I do not want to suggest here that Popper is a positivist. Although the view of models he espouses in the passage referred to is a characteristically positivist one, as is his espousal of the deductivist view of explanation and prediction (Popper 1959, pp. 59-60), there are many anti-positivist elements in his work. To mention only a few: his rejection of instrumentalism (Popper (1963b)); his rejection of any simple distinction between theory and observation statements (Popper (1g59), ch. 5); his general hostility towards the logical positivist movement; and his interest in questions concerning the growth of scientific knowledge).

The problem of models is, of course, closely related to that of the theory-observation distinction, and here there are many examples that could be cited of the way in which anti-naturalist arguments assume a positivist view of this distinction. One way of putting such arguments is this: whereas the natural scientist is concerned with 'mere physical behaviour', the social scientist is concerned to explain *actions*, and the same action can be manifested in highly various bits of behaviour (and *vice versa*). Thus, in defending a (moderate) anti-naturalist position, Alfred Schutz insists that

> "The identification of experience with sensory observation in general, and of the experience of overt action in particular, excludes several dimensions of social reality from all possible enquiry. [For even] an ideally refined behaviourism can... merely explain the behaviour of the observed, not of the observing behaviourist. The same overt behaviour (say a tribal pageant as it can be captured by the movie camera) may have an entirely different meaning to the performers. What interests the social scientist is merely whether it is a war dance, a barter trade, the reception of a friendly ambassador, or something else of this sort." (Schutz 1963, pp. 236-237).

The assumption made here seems to be this: if we adopt a naturalist standpoint, our descriptions of the social world will have to be given in a purely 'sensory' way: we must limit ourselves to the 'strictly observable', adopt a 'behaviourist' vocabulary, and classify things in terms only of their physical similarity. But obviously, in view of the attacks made on the notion of an observation-language for the natural sciences, mentioned in section 3, this assumption is not justifiable. What is being opposed by Schutz is not naturalism, so much as positivism.

Consider an example similar to one discussed by Winch (1958, pp. 124-5). Two noises might be exactly the same, auditorily, yet one is gunfire and another is thunder. What makes two noises, which might well be quite dissimilar, auditorily, *thunder*, is that they have the same cause; and two similar noises might be described differently, because they have different causes. Indeed, in such examples, there need be no incompatibility between the realist and neo-Wittgensteinian positions; proponents of the former would not find it surprising that the causes, and effects, of certain objects (or events) should be part of the meaning of the terms we apply to them. And it could be plausibly argued, in the case of Schutz's 'tribal pageant', that what makes one description rather than another appropriate has some connection with *why it is* that these movements are taking place.

A similar argument might be applied to some of Winch's examples. On pp. 73-4, he discusses our description of a cat's movements as 'writhing': "Suppose I describe his very complex movements in purely mechanical terms, using a set of space-time co-ordinates. This is, in a sense, a description of what is going on as much as is the statement that the cat is writhing in pain. But the one statement could not be substituted for the other." One possible way of answering this point would be this: the concept of writhing involves the notion that these movements are taking place *because the cat is in pain*, and this is why the description of its movements does not have the same meaning as the description 'writhing'.

I do not want to suggest that all differences between, e.g., 'behaviour' and 'action' descriptions can be explained by means of the additional causal connotations of the latter; neither, of course, would this be true of all the differences between 'observational' and 'theoretical' descriptions in the natural sciences. Nor would it be fair to give the impression that Winch himself is unaware of the existence of 'theory-impregnated' terms in the natural sciences. On pp. 124-5, he argues that, despite this fact, the connection between 'electrical storms' and 'thunder' is radically different from that between, e.g., 'command' and 'obedience'. According to Winch, "an event's character as an act of obedience is *intrinsic* to it in a way which is not true of an event's character as a clap of thunder: and this is in general true of human acts as opposed to natural events". And he adds, in developing this point, that an

> "... act of obedience itself contains, as an essential element, a recognition of what went before as an order. But it would be senseless to suppose that a clap of thunder contained any recognition of what went before as an electrical storm: it is our recognition of the sound, rather than the sound itself, which contains that recognition of what went before."

But despite *this* difference, there is also an important similarity between these two cases which Winch, partly because of his acceptance of a Humean view of causation in the natural world, and partly because of his unwillingness to regard the reasons that people give for their actions as possible *causes* of them, does not mention: namely, that just as it is not merely the fact that certain noises *follow* certain electrical disturbances that is involved in our calling the former 'thunder', so it is not merely due to the fact that an action follows after a command that we call the former 'obedience'. One might instead say: an action is an act of obedience only if it is causally connected with the preceding command, the connection involving recognition and acceptance of the command by the agent. This is a different kind of connection to that between electrical disturbances and thunder-noises, but in neither case is it merely a matter of this event or action being an instance of some regular sequence.

5. Concluding remarks

If the main argument of the preceding sections is accepted, then we should also reject a commonly accepted division, which has been closely connected to that between naturalism and anti-naturalism: that is, between 'positivism' and 'idealism' (or 'intuitionism'), as mutually exclusive and exhaustive accounts of the methodology of the social sciences. Historically, positivism has involved both specific views about the methods of the natural sciences, and the claim of methodological unity, or naturalism. So much so, that in several contemporary works on the philosophy of the social sciences, positivism is often initially defined as maintaining this naturalist view. Thus, e.g., Runciman (1969), p. 8: 'The positivists are those who in general regard the social sciences (under which they may well include history) as methodologically equivalent to the natural sciences'; and Dray (1964), p. 2:

> "Without entirely denying that it may have certain local peculiarities, one group of philosophers has argued that there are no fundamental peculiarities that would justify a separate critique of history. Those holding this position are nowadays often referred to as 'positivists'; and despite certain misleading connotations of this term, it will be used for convenience of reference in what follows. Their opponents have frequently been called 'idealists'."

(Similarly, 'idealism' has been used to refer *both* to a specific view about the social sciences, and *also* to a rejection of naturalism).

One result of these terminological traditions is to make it rather more difficult to disentangle the issues involved, and in particular to see that one need not be committed to a positivist view of science, in being a naturalist, and that one need not adopt an idealist view of the social sciences, in being an anti-naturalist. There are, of course, writers who have tried to avoid placing themselves firmly on one side or other of the positivist-idealist division, such as Max Weber. As Runciman points out: "Weber, like Marx, is one of those authors who tends to have as many interpretations as readers, and he has on occasion been claimed as an ally, or denounced as a heretic, by both sides"(1969, p. 8). I suggest that one of the reasons for this is that the traditional vocabulary provides too few alternatives for an adequate description of a position like Weber's. No doubt Weber's own understanding of the methods of the natural sciences was not itself very sophisticated; but now that that greater sophistication exists, and now that positivism cannot be assumed to be the correct account, the whole question of naturalism versus anti-naturalism must be re-examined.

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