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Edited by Christopher Hookway and Philip Pettit

Excerpt

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Theory and Value in the Social Sciences

MARY HESSE

I

Many reasons have been given for supposing that the social sciences require different kinds of method and justification from the natural sciences, and conversely for supposing that these methods and justifications are or ought to be the same. I don't want to rehearse all these arguments here, but rather to concentrate on two features of the *natural* sciences which already suggest that the conventional arguments about similarities and differences are inadequate. These features can be roughly summed up in the by now fairly uncontroversial proposition that all scientific theories are *underdetermined* by facts, and the much more problematic propositions that, this being the case, there are further criteria for scientific theories that have to be rationally discussed, and that these may include considerations of value.

Whether the natural and the social sciences are seen as similar or different depends of course on the view we take of the natural sciences. The view I am going to presuppose, but not argue here, is that made familiar in recent post-deductivist discussions, with the addition of a crucial pragmatic dimension.¹ Let me summarise as follows:

(1) Theories are logically constrained by facts, but are underdetermined by them: i.e., while, to be acceptable, theories should be more or less plausibly coherent with facts, they can be neither conclusively refuted nor uniquely derived from statements of fact alone, and hence no theory in a given domain is uniquely acceptable.

(2) Theories are subject to revolutionary change, and this involves even the language presupposed in 'statements of fact', which are

¹ I have discussed these matters in *The Structure of Scientific Inference* (London, 1974), chaps. 1, 2 and 12, and in 'Truth and the Growth of Scientific Knowledge', in F. Suppe and P. D. Asquith (eds.), *PSA, 1976* (Philosophy of Science Assn, East Lansing, Mich., 1976). Since the notion of 'underdetermination' has been exploited particularly by Quine, I should say that I do not accept his distinction between 'normal scientific induction' and 'ontological indeterminism', according to which it seems to be implied that purely scientific theories can eventually be determined uniquely by inductive methods. Some of my reasons for this rejection will emerge below.

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irreducibly theory-laden: i.e., they presuppose concepts whose meaning is at least partly given by the context of theory.

(3) There are further determining criteria for theories which attain the status of rational postulates or conventions or heuristic devices at different historical periods – these include general metaphysical and material assumptions, e.g. about substance and causality, atoms or mechanisms, and formal judgments of simplicity, probability, analogy, etc.

(4) In the history of natural science, these further criteria have sometimes included what are appropriately called value judgments, but these have tended to be filtered out as theories developed.

(5) The ‘filtering-out’ mechanism has been powered by universal adoption of one overriding value for natural science, namely the criterion of increasingly successful prediction and control of the environment. In what follows I shall call this the *pragmatic criterion*.

Points 4 and 5 need further explanation.

Value judgments related to science may be broadly of two kinds. They may be evaluations of the *uses* to which scientific results are put, such as the value of cancer research, or the disvalue of the nuclear bomb. But they may also be evaluations that enter more intimately into theory-construction as *assertions* that it is desirable that the universe be of such and such a kind *and* that it is or is not broadly as it is desired to be. Examples of positive evaluations of what is the case are: belief in the perfection of spherical symmetry, and consequent belief that the heavens are spherically symmetrical; belief that men ought to be and therefore are at the physical centre of the universe, and that they are biologically superior and unique among organisms; belief that mind is devalued by regarding it as a natural mechanism, and therefore that mind is in fact irreducible to matter. An example of negative evaluation of what is the case is the Marxist belief that in this pre-revolutionary stage of the class struggle various elements of social life that look like valuable supports of social stability are to be unmasked as in fact being obstacles to the desirable revolution. In the light of such a belief, for example, the immiseration of the proletariat becomes a positive value, and tends to become the essential category in terms of which complex social facts are described.

It is the second type of evaluation in science that I shall be concerned with. All examples of this type issue in assertions rather than imperatives, and hence involve a transition from *ought* judgments to *is* judgments. Are they not therefore immediately condemned as illicit

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in any form of scientific argument? In reply to this objection two points can be made. First, there is no doubt that there are historical examples in which the genetic fallacy was not seen as a fallacy, so that in describing the thought processes involved in such examples the historian at least has to recognise forms of quasi-inference such as those just sketched. But the second and more important consideration depends on point 3 above, namely that since there is never *demonstrative* reasoning from evidence to theory, further determining criteria may well include factual judgments about the way the world is, and these are sometimes based persuasively on judgments of how it ought to be. There is no fallacy of logical inference, for logical inference is not appropriate here; there is rather the choice of some hypotheses for consideration among many other possible ones, in the hope that the world will be found to be good as the accepted value system describes the good.

In the case of the natural sciences, however, it may well be objected that the evaluative and teleological beliefs of past science either have been refuted, or have been eliminated by economy and simplicity criteria applied to theories. It would be a mistake to suppose that they could have been refuted by facts alone, because even if we do not accept the strong theory-ladenness thesis of point 2, it would generally be agreed that facts are susceptible of a multiplicity of theoretical interpretations, and that if such value judgments were regarded as of overriding importance (overriding, that is, all except logic), the facts could have been accommodated, though perhaps at a cost to economy. But the requirement of theoretical economy or simplicity is not an adequate general answer either, for at least two reasons. First, what have been held to be *prima facie* simple theories have often been abandoned for more complex ones. Examples are: field theories in place of action at a distance, atomic theories in place of phenomenal volume and weight relations in chemical reactions, and Copernicus's heliocentric universe, which in his theory required more parameters than the geocentric universe it replaced. In most such cases, what was of overriding importance was not facts plus *prima facie* simplicity, but facts plus interpretation in terms of some intelligible or desirable world-model. Secondly, no one has yet succeeded in presenting definitions of simplicity that are adequate for all the occasions on which appeal had been made to it. But it is at least clear that there is not one concept of simplicity but many, and the suspicion grows that simplicity is not in itself a final court of appeal but rather adapts itself to definition in terms of whatever other criteria of theory choice are taken to be overriding.

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The most important of these other criteria in natural science is what I have called the *pragmatic criterion* of predictive success. In considering historical examples the question to be asked in philosophy of science is not so much what were the special local (social, biographical, psychological, etc.) factors at work in the immediate and short-term decisions of individuals, but rather whether there is any *general* criterion for the long-term acceptability of one theory rather than another, and for the replacement of old theories by new. This is to ask a question which presupposes that because all formal criteria such as verification, confirmation, and falsifiability seem to have broken down as criteria for theory choice in particular short-term scientific situations, therefore there are no general criteria of theory choice over the long term. But revolutionary accounts have not disposed of the objection that natural science, as well as being revolutionary in respect of *theories*, is also in some sense cumulative and progressive, and retains contact with the empirical world by means of long-term testing of theory complexes taken as wholes. If we press the question ‘What is it that progresses?’, the only possible long-term answer is the ability to use science to learn the environment, and to make predictions whose results we can rely on not to surprise us. It is this modification of the traditional empirical criteria of confirmation and falsifiability that I intend by the ‘pragmatic criterion’.²

As successful prediction accumulates, the pragmatic criterion filters out both simplicity criteria and other value judgments. We can observe by hindsight that in the early stages of a science, value judgments (such as the centrality of man in the universe) provide some of the reasons for choice among competing underdetermined theories. As systematic theory and pragmatic success accumulate, however, such judgments may be overridden, and their proponents retire defeated from the scientific debate. Thus, the theological and metaphysical arguments against Copernicus, against Newton, and against Darwin became progressively more irrelevant to science. This is not to say, of course, that *our own* preferences in choices between underdetermined theories are not themselves influenced by our value judgments and by beliefs which we take for granted, or that these will not be visible to the hindsight of

² It was Duhem’s holist account of theory-testing in *The Aim and Structure of Physical Theory* (Princeton, 1954; first published in 1906 as *La théorie physique*) which foreshadowed the demise of later and narrower criteria of empirical test. The work of I. Lakatos has more recently familiarised philosophers of science with the problem of theoretical acceptability in long-term historical perspective, although his criteria for ‘progressive research programmes’ do not include the predictive aspects of the pragmatic criterion adopted here. See particularly his ‘Falsification and the Methodology of Scientific Research Programmes’, in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge* (Cambridge, 1970).

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future historians of science.³ This is very likely to be so, but it does not conflict with the notion of accumulation of pragmatic success in science past, present, and future. There is also a sense in which value judgment enters into the very adoption of the pragmatic criterion itself – the judgment that the requirement of predictive success should override all other possible criteria of theory choice. This is the one value judgment that, of course, is *not* filtered out, but rather is presupposed in the pragmatic criterion. It is a judgment that has perhaps rarely been consciously adopted by any scientific society of the past, but it is one which, it is becoming increasingly apparent, may be consciously rejected in the future.

It is not my purpose here to discuss in detail the relation between what I have called the pragmatic criterion and more orthodox theories of objectivity and truth. But something more must be said to avoid misunderstanding. First, there is a difficulty about the notion of ‘successful prediction’. If we were able to ignore the much-discussed difficulties referred to in point 2, and assume that there is a theory-neutral observation language for which there are clearly applicable truth criteria, we might be tempted to define ‘increasingly successful prediction’ in terms of an accumulating set of true observation statements deducible from the corpus of scientific theories. We cannot ignore these difficulties, however, or the consequent tendency to understand ‘truth’ not in a correspondence sense but as coherence within a given theory, and hence as theory-relative. Since theories of truth are themselves in considerable disarray, it is better to find some way of understanding ‘successful prediction’ independently of them. Here I suggest a pragmatic or ostensive appeal to the actual state of natural science since the seventeenth century, in which we can recognise an accumulation of successful prediction which overrides changing theories and is *independent* of particular conceptual schemes in which scientific successes are described in conflicting theories. The space-ship still goes, whether described in a basically Newtonian or relativistic framework. Pragmatic knowledge can be obtained without an absolutely theory-neutral descriptive language.

³ Recent studies in the history and sociology of natural science indicate that there has been far more influence upon theories from evaluations and non-scientific standpoints than has generally been realised. See for example P. Forman, ‘Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment’, in R. McCormach (ed.), *Historical Studies in the Physical Sciences*, vol. 3 (Philadelphia, 1971); papers in M. Teich and R. M. Young (eds.), *Changing Perspectives in The History of Science* (London, 1973); and many references in Barry Barnes, *Scientific Knowledge and Sociological Theory* (London, 1974) and David Bloor, *Knowledge and Social Imagery* (London, 1976).

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It may be illuminating to draw an analogy (only an analogy) between the method of natural science and the program of a computer designed to process environmental data and to learn to make successful predictions (for example, a character-recognition device). The criteria of success of such a device can be made independent of the actual 'language' system used in the computer to store and process data and to give the orders for testing theories on more data. Equal success in two or more computers is consistent with their having widely different internal language systems, although of course some language systems may be more convenient than others for given kinds of data, and indeed there may be feedback mechanisms in the program which permit change of language to a more convenient one when this is indicated by the success and failure rate.

A second possible misunderstanding of the pragmatic criterion arises from the fact that technological applications are the most striking examples of accumulating successful prediction. Philosophers of science should perhaps disengage themselves from the Popper-induced prejudice that pragmatic application has nothing to do with the logic of science. On the other hand, successful prediction does not necessarily issue in technical control. Many theories enlarge our pragmatic knowledge (for example, about fossils, or quasars), without necessarily forming the basis of technology.

A third difficulty is that the relation between the pragmatic criterion and any theory of truth is obscure and needs much more examination than can be given to it here. But in the particular case of some kind of correspondence theory, to which philosophers of truth seem now to be increasingly drawn, there does not seem to be any *prima facie* conflict between such a theory and the pragmatic criterion. Current correspondence theories of truth tend to be expressed in terms of some relation of 'satisfaction' which holds between the world and true statements, and are in themselves independent of the question how such satisfaction is identified in particular instances. There are notorious difficulties about such identifications – the same difficulties that underlie the notions of underdetermined theories and criticisms of the basic observation language. The pragmatic criterion trades these difficulties for others by bypassing the question of the reference of theoretical language, and resting on the non-linguistic concept of successful prediction.

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II

In considering whether natural science as defined by points 1 to 5 is an adequate model for the social sciences, we can add two further points:

(6) There are not at present, and perhaps can never reasonably be expected to be, general theories in the social sciences that satisfy the pragmatic criterion of point 5 – namely, theories that provide increasingly successful prediction and control in the social domain.

(7) Moreover, since adoption of the pragmatic criterion itself implies a value judgment, it is possible to decide *against* it as an overriding goal for social science, and to adopt other value goals.

Point 7 does not presuppose the truth of point 6. I doubt if point 6 can be proved in any general way. On the actual present situation one can only observe what underlies complaints about the backwardness, theoretical triviality, and empirical rule-of-thumb character of most social science, in spite of limited success in establishing low-level laws in isolated areas. On the logical possibility, there have been attempts at general proof of the non-natural character of social science, attempts which derive from features of the social subject matter such as complexity, instability, indeterminacy, irreducible experimental interference with data, self-reference of social theorising as part of its own subject matter, etc. I do not believe such proofs can ever be conclusive, if only for the reason that most of these features are also found somewhere in the natural sciences. If we use as an analogy for the method of natural science the computer which learns to predict its environment, an immediate consequence is that there will be some environments and some types of data which do not permit learning by any computer of limited capacity, for any or all of the reasons just listed. The social environment *may*, wholly or partly, be such an environment. I doubt if anything stronger can be said, and I doubt whether any attempt to formalise the situation further at this general abstract level is worthwhile. Satisfaction of the pragmatic criterion by particular social-science theories needs to be argued case by case.

Point 7, however, remains. It is explicitly recognised in Marxist writings on the social sciences, and also in the older *Verstehen* tradition and in its more recent offshoot, hermeneutics (although the latter two traditions neglect the dimension of ‘interest’ that inevitably infects social theory according to Marxism). The rest of this paper will be devoted to exploring the consequences of point 7.

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It is important to notice that point 7, together with points 1 to 5, imply a distinction between two sorts of 'value-ladenness' in social science. The first is analogous to theory-ladenness in natural science, and is the sense primarily in mind when empiricist philosophers have attempted to disentangle and exclude value judgments from scientific social theory. It is the sort of value judgment that I have mentioned in point 4, which becomes associated with theoretical interpretations by virtue either of the selective interest of the investigator (e.g. in preferring to investigate stable systems as norms), or of adoption of those hypotheses which assert the world actually to be in some respects as it is desired to be (e.g. acquired characteristics either are or are not inherited according to preferred ideology). I have suggested that the crucial point about the natural sciences is that though such judgments may function heuristically in hypotheses, operation of the pragmatic criterion frequently filters them out, and how the world 'ought to be' frequently fails in face of how the world is, or rather in face of the only plausible and coherent ways that can be found of interpreting facts and successful predictions. Where the pragmatic criterion works in social science we shall expect some value judgments to be filtered out in a similar way – for example, it seems not impossible that currently controversial questions about the relationship, if any, between intelligence quotient and racial origin might be sufficiently defined to be made rigorously testable, and laws might be derived which satisfy the criterion of successful prediction. (Whether it would be *desirable* to adopt and try to rigorously apply the pragmatic criterion here is entirely another question.)

But where the pragmatic criterion cannot be made to work in a convergent manner it is not possible to filter out value judgments in this way. A second type of value judgment may then be involved, which in varying degrees *takes the place of the pragmatic criterion* in selecting theories for attention. These judgments will be *value goals* for science that are alternatives to the pragmatic goal of predictive success. Such alternative goals have often been recognised in the literature, for example by Weber in his category of value-relevance, and by Myrdal in arguing for explicit adoption of a value standpoint, preferably one that corresponds to an actual power group in society.⁴ But alternative value goals have usually been recognized in the negative sense of the

⁴ M. Weber, *The Methodology of the Social Sciences*, ed. E. A. Shils and H. A. Finch (New York, 1949), and *The Theory of Economic and Social Organization* (Oxford, 1947), chap. 1. The second part of *Methodology* and chap. 1 of *Theory* are reprinted in M. Brodbeck (ed.), *Readings in the Philosophy of the Social Sciences* (New York, 1968). G. Myrdal, *The Political Element in the Development of Economic Theory* (London, 1953), *Value in Social Theory* (London, 1958), and *Objectivity in Social Research* (London, 1970).

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'unmasking' of so-called non-objective biases, rather than in the positive sense of being consciously adopted goals other than the pragmatic criterion. It is difficult to make such standpoints conscious and explicit while they are operative, but the literature is now full of studies in the critical sociology of sociology, where the standpoints of the past, and of other contemporary groups of sociologists, are 'unmasked'. It is a well-known Marxist ploy to uncover the non-intellectual interests even of self-styled positivists: those who argue most strongly for a value-free and objective social science are shown to be those whose social and economic interest is in the status quo, and in not having the boat rocked by encouragement to explicit criticism and value controversy. And such studies are not found only in Marxist writers. Robin Horton, for example, has given an interesting analysis of the styles in social anthropology during this century in terms of the changing attitudes of the West, in its imperialist and liberal phases, towards its former colonies as they become politically independent and aspire to cultural autonomy.⁵

Weber carefully distinguished value-relevance from the value-freedom of the social scientist with respect to political action. That is to say, he accepted that judgments of interest select the subject matter of the human sciences, but denied that the social scientist as such should use his theories to argue any particular political practice. Even with respect to value-relevance, he argued that theories must ultimately be shown to be causally adequate. Thus Weber's own value-interest in studying, for example, the interrelations of capitalism and the Protestant ethic was doubtless to refute Marx's contention that the ideological superstructure is unilaterally determined by the economic substructure. But Weber insists that his theory of such relationships must be shown to be a factual theory of cause and effect, confirmable by positive instances and refutable by negative. Without going into the detail of Weber's discussions of methodology, it can I think be fairly concluded that he sees the goal of knowledge and truth-assertion as essentially the same in the natural and social sciences, but that he has an over-simple view of the nature of causal laws in the natural sciences, which misleads him into extrapolating an almost naive Millian method into the social sciences. He does not doubt that judgments of value-relevance are separable from positive science, and can in this sense be 'filtered out' of cognitive conclusions. Thus he has

⁵ R. Horton, 'Lévy-Bruhl, Durkheim and the Scientific Revolution', in R. Horton and R. Finnegan (eds.), *Modes of Thought* (London, 1973). For another unmasking of positivism, see A. Gouldner, *The Coming Crisis of Western Sociology* (London, 1970), especially chap 4.

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not yet made the ‘epistemological break’ involved in recognising, questioning, and perhaps replacing the pragmatic criterion for social sciences, nor has he distinguished two sorts of judgments of value-relevance – those which can ultimately be eliminated by the pragmatic criterion and those which cannot because they depend on a view of causality that presupposes it.

There are others who have not understood the nature of this epistemological break. In a commentary on Myrdal’s requirement of total explicitness of value standpoint and identification with some actual power-group, John Rex⁶ finds an implied suggestion that objectivity inheres in the balance of power between such groups, and that this balance of power ‘can be relatively objectively determined’, as if what is is determined by the standpoint of the most powerful group. However, while it may be true that the most powerful group can to a greater or lesser extent impose its will upon the development of the social system, it does not at all follow that the theory informed by its value standpoint gives the true dynamical laws of that system on a pragmatic criterion, or the best theory on any other criterion except that truth resides in the barrel of a gun. Whether the unions or the sheikhs eventually gain control in Britain is irrelevant to the theoretical acceptability of either of their implied economic doctrines. And Christ and Socrates may have the best theories after all.

Myrdal himself is more careful, but he too leaves largely unexamined the exact relation between objectivity as sought in the natural sciences and the value criteria which are inevitably adopted in social science. Of science in general he writes:

Our steadily increasing stock of observations and inferences is not merely subjected to continuous cross-checking and critical discussion but is deliberately scrutinized to discover and correct hidden preconceptions and biases. Full objectivity, however, is an ideal toward which we are constantly striving, but which we can never reach. The social scientist, too, is part of the culture in which he lives, and he never succeeds in freeing himself entirely from dependence on the dominant preconceptions and biases of his environment.⁷

If ‘objectivity’ in this sense is the ideal which is unattainable, then valuations are a necessary evil. Seen in such negative light, it is unlikely that the choice between valuations will be subjected to logical or philosophical scrutiny, and the vacuum is likely to be filled by power criteria or worse, in the manner of Rex. But if it is true that the ideal objectivity is unattainable and that valuations are necessary, the philosopher will surely be better advised to present this necessity in

⁶ J. Rex, *Key Problems of Sociological Theory* (London, 1970), pp. 164–6.

⁷ *Value in Social Theory*, p. 119.