

# VALUES, THEORY AND REALITY\*

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## 1. Weber and value-freedom

Max Weber's conception of the place of values in social science involves several different claims, which are partly independent of one another. This is often concealed by discussions which ascribe to him a single, general position called 'value-freedom'. So we begin by outlining the six main elements which together constitute Weber's view of the relation between values and social science.

First, *value-judgments*, which he defines as 'practical evaluations of the unsatisfactory or satisfactory character of phenomena subject to our influence', cannot be logically derived from factual statements (Weber, 1949, p. 1). Thus the social sciences, which are concerned with factual descriptions and explanations, cannot establish the truth or falsity of any value-judgment. Second, social scientists should not make value-judgments, in either their teaching or their writing. Weber emphasizes that this is itself a value-judgment. Third, social scientists are necessarily committed to the scientific values of truth, objectivity, and so on. Fourth, it is important to study other people's values, since these may often be significant causal determinants of their actions; and it is possible to do this without the social scientist's own values distorting such studies. Fifth, we need some way of selecting objects of investigation from the infinitely complex concrete reality that confronts us. This we do by reference to the relationship that parts of this reality have to one or more different values. But to select objects in this way, by their *value-relevance*, does not mean that we make favourable value-judgments about either the objects selected, or the values to which they are related. Sixth, once this selection is made, neither value-relevance nor value-judgments have any further function in the investigation. The process of causal explanation must, and can, be guided solely by the objective canons of scientific

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argument and evidence.

Two central concepts in this multi-faceted position are those of value-judgments and value-relevance.<sup>1</sup> We will examine these in turn, together with those elements of Weber's position to which they relate. Weber's claim that value-judgments cannot be derived from the results of social science is based on a particular philosophical view of the logical status of such judgments. In adopting this view, he was strongly influenced by both Kant and Nietzsche. Weber holds that there is no way of rationally and objectively deciding between competing, substantive ethical doctrines. In particular, specific value-judgments about concrete phenomena cannot be justified by reference to factual enquiries. From such enquiries, all that can be discovered are the likely consequences of different courses of action, and thus the most effective means of achieving various ends. But the results of social scientific investigations do not enable one to make favourable or unfavourable judgments of the ends themselves. Nor does the fact that a particular course of action is the most efficient means to a favourably judged end mean that it should be performed, for one may also make a value-judgment about the means, that runs counter to the judgment of the end.

When Weber claims that the social scientist should not make value-judgments, he is well aware that this is itself a value-judgment, and that it cannot be supported solely by his philosophical view about such judgments. Thus Weber does not argue against making value-judgments on the grounds that this policy is, for social scientists, required by the logical status of their enquiry. Instead, Weber's grounds for this judgment are related both to his own political and ethical values, and to his views about the probable consequences of adopting or rejecting the policy. We will not discuss his values here. But we should note that, in assessing the likely consequences, Weber distinguishes between the effects of social scientists making value-judgments in their writing, and in their teaching in universities. It is with the second of these that he is mainly concerned. And one of his main arguments is that political pressures on universities at the time were such that many legitimate and significant political and moral positions would be prevented from being expressed.<sup>2</sup>

Let us now turn to the concept of value-relevance, and the relationship between selecting objects of study and causally explaining them. The need for such selection is justified by Weber (1949, p. 72) in the following way: reality presents itself to us as

... an infinite multiplicity of successively and coexistently emerging and disappearing events...  
. All the analysis of infinite reality which the finite human mind can conduct rests on the tacit assumption that only a finite portion of this reality constitutes the object of scientific investigation, and that only it is 'important' in the sense of being 'worthy of being known'.

But what are the criteria by which this segment is selected?

Weber argues that the answer to this question lies in the relations of various features of reality to cultural values. Through such relations, these features gain 'cultural significance'. Thus (1949, p. 76):

Empirical reality becomes 'culture' to us because and insofar as we relate it to value ideas. It includes those segments and only those segments of reality which have become significant to us because of this value-relevance. . . . We cannot discover . . . what is meaningful to us by a 'presuppositionless' investigation of empirical data. Rather, perception of what is meaningful to us is the presupposition of its becoming an *object* of investigation.

To illustrate his doctrine of value-relevance, let us consider what, for Weber, makes the emergence of European capitalism a significant object of investigation. It is, primarily, that capitalism embodies a particular form of 'rationalization', a systematic attempt to organize human activities in an efficient, calculable and impersonal manner. The growth of rationalization, manifested especially in the increasing dominance of bureaucracy, poses the central value-problem of contemporary society. For it leads to the progressive 'disenchantment' of humans with the world, to a state of 'mechanized petrification', from which only some kind of 'spiritual renewal' can rescue them. For Weber, this value-relevance of capitalism leads him not only to select it as an object of investigation, but also to select those of its features that are especially significant. Weber regards the 'internal rationality' of capitalism as its most essential feature, not in any absolute or metaphysical sense, but in relation to his particular value-conditioned interest.

Thus Weber's characterization of capitalism is 'ideal-typical'. For Weber, an ideal-type involves the deliberately one-sided accentuation of specific features that an object, or class of objects, may have. No concrete phenomenon precisely corresponds to an ideal-type, for three main reasons. First, any such phenomenon will have many features that are not included in the ideal-type. Second, those features that are included are represented in an idealized or 'purified' form. Third, not all the features of the ideal-type are present in each concrete exemplification of it.

For Weber, then, all knowledge of social reality is ideal-typical. And one cannot criticize one ideal-type, say of capitalism, as less objectively 'correct' than another. Relative to different values, one might wish to emphasize other features, such as its class structure, which are not so central to Weber's particular concerns in, for example, *The Protestant Ethic* (Weber, 1930). But Weber insists that, once an object of a study has been selected and defined in relation to values, these have no further part in the investigation: neither they, nor any value-judgments, should affect the objectivity of causal

explanations. Everything else that is studied gains its significance not from values, but either from the fact that it is a causal determinant of the object of investigation, or because it provides evidence of such causal relations. Weber's position on these issues is explicitly and strongly expressed in the following passage (1949, p. 159):<sup>3</sup>

This imputation of causes is made with the goal of being, in principle, 'objectively' valid as empirical truth absolutely in the same sense as any proposition at all of empirical knowledge. Only the adequacy of the data decides the question, which is wholly factual, and not a matter of principle, as to whether the causal analysis attains this goal to the degree which explanations do in the field of natural events. It is not the determination of the historical 'causes' for a given 'object' to be explained which is 'subjective' in a certain sense . . . — rather is it the delimitation of the historical 'object', of the 'individual' itself, for in this the relevant values are decisive.

Having identified the different elements in Weber's conception of value-free social science, and examined the two central notions involved in it, we will now consider a number of objections that have been made against his position. In doing so, we will show that attacks on some of these elements, even if successful, do not entail the rejection of others. In particular, Weber's views about value-relevance can be defended independently of his views about value-judgments. It is the former, which support his claim that values are relevant only to the selection of objects of study, and not to their explanation, which are the most significant for the philosophy of social science.

We begin by briefly examining some claims made by Charles Taylor (Taylor, 1967). His attack on 'neutrality in political science' is primarily directed against the first element in Weber's position, the claim that social scientific investigations do not support or justify specific value-judgments. Taylor's opposition to this derives from two main arguments. First, he rejects Weber's philosophical view of the logical status of value-judgments. For Taylor, such judgments are rationally defensible. To show that some state of affairs leads to the satisfaction of human wants, needs or interests is to show that it is morally desirable. It is unintelligible, though not strictly self-contradictory, to deny that such a state of affairs is desirable, unless it can also be shown that it contains elements leading to non-satisfactions. Second, Taylor argues that political science, once it goes beyond the mere collection of data, must develop theories embodying 'explanatory frameworks'. Such frameworks involve a set of assumptions about the relative importance of different causal variables, such as economic or political relations. The theories based upon them demonstrate that only a limited number of social and political arrangements are possible. We are thus posed with a choice between these options. And Taylor argues that, typically, this choice is straightforward, since it will be obvious which of the options is morally

preferable.

We will not criticize Taylor's position here, though we believe that both of the main arguments for it are partly defective. Instead, we will note that, even if correct, it would not affect Weber's claims about value-relevance. What it would show is that value-judgments are 'objective', in the sense that they can be supported by social theories. But it would not show that those theories are themselves influenced or distorted by values. The acceptance or rejection of value-judgments would be dependent upon social theories, but the truth or falsity of the latter would not be affected by the former.<sup>4</sup>

If Taylor's view of the logical status of value-judgments were correct, it would remove one of the grounds for the second element in Weber's position, the prohibition on value-judgments by social scientists. But this policy was, as we saw, advocated by Weber on other grounds as well. We will now examine how Alvin Gouldner challenges this prohibition on value-judgments. In 'Anti-Minotaur: The Myth of a Value-Free Sociology' (Gouldner, 1964), he tries to explain why this policy has proved so popular amongst orthodox sociologists, especially since few of them show any understanding of the complex issues involved in Weber's defence of it. Gouldner disclaims any interest in the logical arguments relevant to value-freedom. Instead he analyses the function of the dogma 'Thou shalt not commit a value judgment' for individual sociologists, for the development of sociology as an institutionalized profession, and for the apparent autonomy of the modern university from the political arena. He claims, for example, that this dogma enables sociologists to resolve the personal problem generated by fearing the consequences of taking a critical attitude to their own society, yet not wishing to be seen as 'unmanly or lacking in integrity' (Gouldner, 1964, pp. 204-7). They resolve the dilemma by arguing that it is not legitimate for them, in their role as social scientists, to make value-judgments.

Gouldner's explanation is thus partly functionalist. But he does not regard these functional consequences of the dogma as justifying its adoption in contemporary sociology. On the contrary, he claims that it diminishes and impoverishes the content of political debate, and prevents the taking of an openly critical stance towards society. It therefore supports the status quo. And it is harmful to the process of education, since students are subject to the unwitting influence of the values that are normally implicit in what they are taught. Thus Gouldner rejects Weber's value-judgment about social scientists making such judgments in their teaching. Why do they disagree about this? Partly, because of their different political values, and partly because of their different assessments of the actual consequences of this policy. Further, we should note that the latter disagreement is itself affected by differences in the conditions of political life, and in the relation of universities to society, that existed

at the times Weber and Gouldner were writing.

We can give additional support to Gouldner's position, with which we basically agree, by briefly elaborating on one of his points. Many academics who defend value-neutrality in their work as teachers and writers do so on a doubly erroneous basis. First, they present their stand as following from the fact that value-judgments cannot be logically derived from the results of social science. They thus disguise a value-judgment as a purely methodological claim. Second, they equate their responsibilities as people working in universities with what they mistakenly regard as the methodological requirements of their role as professional academics. By thus identifying themselves with their professional roles, they further justify their commitment to neutrality. But both manoeuvres are wrong, the first logically, and the second morally.

We turn now to a later attack by Gouldner on the doctrine of value-freedom. In 'The Sociologist as Partisan' (Gouldner, 1968), he is not concerned with the prohibition on value-judgments. Instead, he criticizes the deficiencies of a contemporary approach in sociology, advocated especially by Howard Becker (Becker, 1963, 1964). This approach involves sociologists adopting the standpoint of those whom they study: in particular, in studies of the 'underdog', the sociologist in some way 'takes sides' with people such as drug addicts, jazz musicians, prostitutes, or street gangs. Gouldner (1968, p. 106) claims that Becker's interest in the underdog derives in part from

a titillated attraction to the underdog's exotic difference, and easily takes the form of 'essays on quaintness' . . . . It expresses the satisfaction of the Great White Hunter who has bravely risked the perils of the urban jungle to bring back an exotic specimen.

Gouldner maintains that the nature of this orientation to the study of the underdog results in two failings in Becker's work. First, there is no analysis of the way in which social deviance is generated by the 'master institutions' of society. Second, the deviant is seen as a passive 'victim', and there is no conception of the deviant's life as an attempt to oppose those institutions. The overall effect is a failure to identify the possibilities for radical social and political change, which would, indeed, destroy this connoisseur's collection of specimens. Thus, for Gouldner, Becker's approach lends further support to the status quo. For this way of identifying with the underdog ultimately involves adopting the values and standpoint of the highly placed bureaucratic officialdom, of the American administrative class. Its critical edge is directed only at middle-level bureaucrats, an attack that is perfectly acceptable to the powerful liberal establishment in universities and political life.

Gouldner himself presents the following reasons for studying the life of the underdog. First, it is a part of social reality generally ignored or misrepresented by other members of society. Second, it is a life of human suffering, and this alone should make a compelling demand upon the sociologist. Third, much of this suffering is avoidable, though this fact is concealed by the way in which the underdog's life is often regarded. Gouldner believes that these reasons imply the adoption of specific value-commitments, but he argues that these, rather than being inimical to sociological objectivity, are instead required by it. What is objectionable about Becker's value orientation is not merely the values themselves, but that they lead to an erroneous and inadequate explanatory analysis. For Gouldner, the sociologist's commitment to the elimination of human suffering will lead to a less distorted analysis. In particular, it will point our attention to the master institutions of society, and to the potentialities for changing society in a morally preferable direction.

How do Gouldner's claims affect Weber's conception of a value-free social science? We suggest that they do not involve the direct rejection of any of its various elements. Rather they should be seen as mainly concerned with a different, though significant set of issues, about the kinds of motivation and values that are most likely to generate an adequate, and objectively correct analysis of society. Thus the fifth and sixth elements of Weber's position, which deny the logical relevance of values to the truth or falsity of causal explanations, is not undermined by Gouldner's arguments.<sup>5</sup> So let us now consider a writer whose wide-ranging attack on Weber would, if successful, seriously challenge his view of value-relevance.

Leo Strauss (1953) not only rejects Weber's theory of the logical status of value-judgments, but also argues that it is neither possible nor desirable to expel such judgments from our descriptions and explanations of social phenomena. Strauss notes that, although Weber characterized the alternatives for contemporary society as 'mechanical petrification' or 'spiritual renewal', he claimed that this did not belong to a 'purely historical presentation', since it involved 'judgments of value and faith'. Strauss (1953, pp. 49-50) responds:

But is this not absurd? Is it not the plain duty of the social scientist truthfully and faithfully to present social phenomena? How can we give a causal explanation of a social phenomenon if we do not first see it as it is? Do we not know petrification or spiritual emptiness when we see it? And if someone is incapable of seeing phenomena of this kind, is he not disqualified by this very fact from being a social scientist, just as much as a blind man is disqualified from being an analyst of paintings?

Thus for Strauss, to describe a social phenomenon is necessarily also to evaluate it. Value-judgments cannot be avoided, since they are an essential part of knowing what something is. He considers, as examples, the problems facing a sociologist of art or religion. In both cases, the sociologist must be able to make value-judgments, to distinguish between art and trash, or between genuine and spurious religion. Without such evaluations, we would have the absurd situation of a sociologist who 'claimed to have written a sociology of art but who had actually written a sociology of trash' (Strauss, 1953, p. 50). So, whereas on Weber's view, values function only in the selection of objects for study, Strauss maintains that values are partly constitutive of these objects, and also of those to which we refer in causal explanations. It is not that there is a pre-existing, value-free social reality from which, because of its infinite complexity, we must select and define objects for investigation by reference to values. Rather, the value of those objects is part of their real nature, so that any adequate description of them must involve their correct evaluation.

We can best examine Strauss's claims by considering what precisely would be wrong with a sociology of art that was actually about trash. In particular, would it follow from the fact that systematic errors had been made in the identification of 'works of art', that the explanations of the items wrongly identified as art were thereby mistaken? In other words, does the fact that a sociology of art is 'really' an explanation of trash mean that it is an erroneous explanation of trash? We think not. What matters is whether the objects picked out by means of the concept of art that is being used have been correctly explained. Their status as art or trash is not relevant to this issue. Thus, although the dispute about their artistic status may involve value-judgments, the truth or falsity of their explanation is not dependent upon the acceptance or rejection of these value-judgments.

Our response to Strauss's position can be further developed by considering the place of specifically political value concepts, such as justice, in the description and explanation of social phenomena. On this issue, MacIntyre (1971, p. 278) claims that:

to insist that political science be value-free is to insist that we never use in our explanations such clauses as 'because it was unjust' or 'because it was illegitimate' when we explain the collapse of a policy or a regime... . I take it to be no objection to the methodology which I propose that it is clearly not able to purge its explanations of evaluative elements.

MacIntyre notes how political scientists tend to redefine concepts such as justice in terms of people's beliefs. For example, that something is just may be defined as meaning that it is believed or perceived to be just by the agents concerned. He objects that such definitions rule out the possibility of explaining, say, the collapse of a regime by reference to its actual injustice, as distinct from people's



perceptions of this.

What would the kind of explanation that MacIntyre wishes to allow be like? Consider the fairly common definition of justice in terms of equal distribution of goods and resources between all the members of a society. Using this definition, we might explain the collapse of a regime by referring to its injustice, that is, to its unequal pattern of distribution. There are, however, many competing conceptions of justice, disagreements about the relative value of justice and other political ideals, and differences about what, if anything, makes justice a desirable ideal. To resolve such disagreements is, at least partly, to make value-judgments. Nonetheless, we believe that, just as explanations of art can be assessed independently of the judgment ‘art or trash’, the truth or falsity of explanations in terms of ‘injustice’ can be assessed independently of questions such as whether the concept of justice involved is the correct one. What matters is that, whatever definition of justice is used, the justice or injustice of a society can be identified independently of value-judgments.<sup>6</sup>

It is unclear which specific elements in the concept of value-freedom MacIntyre sees himself as attacking. But our response to his, and other arguments we have examined, has so far involved a defence of Weber’s views about value-relevance, and a separation of these from his claims about value-judgments. A successful challenge to the former would require a rejection of the ontological assumptions that enable Weber to regard values as performing a purely selective function upon an independently existent reality. We will consider the problematic nature of these assumptions later in the chapter. But first, we turn to a different set of issues, raised by one element in Marx’s concept of ideology: the class-relatedness of systems of beliefs, and thus their social determination.

## **2. Rationality and the explanation of beliefs**

It is often held that giving a causal explanation for the beliefs of some person or group undermines the truth, rationality or objectivity of those beliefs. Conversely, it is held that causal explanations are only required, or legitimate, for beliefs that are in some way defective. Thus, at the most general level, it has been argued that the thesis of determinism — that every phenomenon is causally determined — is incompatible with the ascription of truth or rationality to beliefs. More specifically, it has been claimed that any sociology of knowledge committed to the view that all systems of belief are socially determined, must inevitably lead to some form of relativism, to the denial of universal, objective standards of truth or reason.<sup>7</sup>

We disagree with these claims. We can develop an alternative position by returning to one of the main themes in our discussion of reasons and causes in section 2 of chapter 7. We argued there that reason-explanations are open to two, mutually compatible, modes of assessment: causal and evaluative. One

can assess whether the reasons proffered by agents are the causes of their actions; and also whether these constitute ‘good’ or ‘bad’ reasons for the kinds of actions concerned. A negative evaluative assessment does not entail that these bad reasons were not causally explanatory; nor does the claim that these reasons were ‘rationalizations’ (that is, not causally operative) entail a negative evaluation of them. Let us apply these points to the explanation of beliefs. The reasons that people give for their beliefs can be both evaluatively and causally assessed. If such reasons provide inadequate justification, this does not show that these are not why people accept those beliefs. Equally, our judgment that these reasons are not why the beliefs are accepted is consistent with our assessment of them as either good or bad reasons for those beliefs.

Thus, to give a causal explanation of beliefs does not entail that the beliefs are defective. First, because the causal explanation may often make reference to reasons that constitute justifications for them. Second, because even if this is not so, there may well be justifications, despite the justifying reasons not being part of the causal explanation for the beliefs’ acceptance. We can elaborate these claims by examining briefly the so-called ‘genetic fallacy’, which involves a distinction between the causal ‘origins’ of a belief, and its ‘validity’. What is usually seen as fallacious by those who attack the genetic fallacy is to regard the origins of a belief as in any way relevant to its validity. But what precisely is the nature of the ‘relevance’ that is denied? We accept that it is mistaken to argue from the fact that a belief has causal origins to its justification or dismissal as valid or invalid. To make this mistake is basically to deny the compatibility of causal and evaluative assessment. But it does not follow that the origins of a belief are, in every sense, ‘irrelevant’ to its validity. For it may be possible to establish empirical relationships between the specific ways in which beliefs have been generated, and the defective or non-defective nature of their content.

We can illustrate this last point with a very simple example. Suppose someone makes predictions about which horse will win a race, by sticking a pin in a list of runners. One can demonstrate that this is a poor method of prediction, and perhaps also explain why this is so. Thus, if we know that a particular prediction has been made by this method, our causal knowledge about the origins of the belief gives good grounds for doubting the truth of that belief. For pin-sticking tends to generate defective beliefs. To argue in this way would not be to commit the genetic fallacy, since all that is involved is an analysis of the way in which the specific origins of the belief affect the likelihood of its validity.

What is the relationship between the position we have so far defended, and the conception of ideology in Marx’s writings, and its later development by Lukács? As we noted in section 1 of the previous chapter, Marx regarded the ideological nature of systems of belief as explicable in terms of features of

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the class structure of a given society. But he did not see these causal explanations as, by themselves, constituting a rebuttal of those beliefs. Rather, he paid considerable attention to the analysis of their defects, and to the specification of the kinds of methodological errors involved in them. Thus there is no reason to think that Marx was guilty of the genetic fallacy. Further, we can reasonably ascribe to Marx, both a general commitment to the social determination of beliefs, and a defence of the possibility of arriving at correct, undistorted theoretical accounts of different social formations. We have argued that this is a consistent position to maintain. So, also, is Lukács's view of historical materialism as the theoretical articulation of the class-consciousness of the proletariat, combined with his view that historical materialism is a methodologically correct and undistorted theoretical standpoint. For there is no essential error in positively evaluating a system of beliefs whose content is held to be related to the structural location and interests of a specific social class. Likewise, Lukács's attempts to identify the methodological defects of bourgeois consciousness, the inadequacy of its fundamental categories and dichotomies, and to explain these by reference to certain features of the position and interests of the bourgeoisie in capitalism, are in principle defensible, whatever doubts we may have about the actual substance of his analyses. And if Lukács's claims were correct, there would be good reason to doubt the adequacy of particular bourgeois theories whose generation and acceptance could be explained in this way.<sup>8</sup>

Thus we are opposed to any general rejection of the legitimacy or necessity of causally explaining non-defective beliefs. Let us now consider some arguments which are partly designed to challenge this position. MacIntyre argues that there are important differences between the types of explanation that are logically appropriate for non-rational, and rational, beliefs (MacIntyre, 1971, pp. 244-59). He says that, because of this difference, the social scientist must assess the rationality of the beliefs that are being explained, since otherwise an inappropriate type of explanation may be given.

MacIntyre illustrates the contrast between non-rational and rational beliefs, and their respective explanations, by two examples. First, he outlines the explanation given by H. R. Trevor-Roper for the European witch-craze in the sixteenth and seventeenth centuries (Trevor-Roper, 1967, chap. 3). According to this, the social tensions resulting from religious conflicts generated fears and anxieties for which the widespread belief in witches, and their active persecution, provided a convenient rationale and outlet. Such an explanation, for MacIntyre, is perfectly appropriate for these non-rational beliefs. But it would be thoroughly inappropriate in his second example, the acceptance by most astronomers, by 1630, of the existence of Jupiter's satellites. To explain this rational belief, one must refer to the use by scientists of various canons of argument and observation, and to the way in which the results of Galileo's telescopic investigations were regarded, in terms of these canons, as

constituting good reasons for the belief.

Contrary to some earlier claims (MacIntyre, 1962), MacIntyre holds that an agent's viewing something as a reason for doing or believing something can be a cause of the action or belief (see MacIntyre, 1971, pp. 214-17). Nonetheless, he maintains (1971, p. 247 — our italics) that:

... the explanation of rational belief *terminates with an account of the appropriate intellectual norms and procedures*; the explanation of irrational belief must be in terms of causal generalizations which connect antecedent conditions specified in terms of social structures or psychological states — or both — with the genesis of beliefs.

Furthermore, MacIntyre denies the possibility of finding causally sufficient conditions for the emergence and institutionalization of the norms and procedures involved in the explanation of rational beliefs. Such conditions have never been discovered, and to insist that they can be discovered is only to express the conviction that every event or state of affairs must be fully determined causally.

MacIntyre's arguments would, if correct, have important consequences for the sociology of knowledge, and especially the sociology of science. We will consider some features of the latter further on. But first we will introduce a distinction between two senses in which a belief can be said to be 'rational', which will enable us to criticize MacIntyre's position. In the first sense, 'rational (1)', a rational belief is one for which there are good reasons, whether or not those who accept the belief do so because of these reasons. In the second sense, 'rational (2)', a person's belief can only be called rational if, not only are there good reasons for it, but also it is because of these that the person accepts it. In other words, someone's belief is rational (2) if and only if it is rational (1), and the reasons that make it rational (1) also causally explain its acceptance.

Which of these two senses is involved in MacIntyre's argument? If MacIntyre means rational (2) beliefs, then his claim that the social scientist must assess the rationality of a belief, in order to explain it appropriately, is peculiar. For, to identify a belief as rational (2), we must already know what at least some of its causal determinants were, that is, the reasons which justify it as rational (1). If, on the other hand, he means rational (1) beliefs, he is wrong in thinking that there is any necessary relation between the rationality of a belief and the nature of its causal determinants. Thus, in his astronomy example, it is perfectly consistent to maintain that the belief is rational (1), since there are good reasons for it, whilst also claiming that these were not the reasons why it was accepted, and that these latter reasons do not constitute an adequate justification for it.

Such a situation is quite common in the history of science. Rational (1) beliefs are often discovered to have been accepted for reasons which we regard as insufficient, misguided, irrelevant, and so on. One obvious type of example concerns the part of religious beliefs in governing scientific activity. Thus, it has been argued that one of the main reasons for Newton's belief in absolute space and time, was his theological conviction about the omnipresence and everlasting existence of God. Whatever the merits of this explanation, it would be wrong to regard it as 'appropriate' only if the belief in absolute space and time can be shown to be non-rational (1). In other words, even though, on at least some criteria of rationality, this belief is rational (1), there is nothing essentially misguided in explaining Newton's acceptance of it by reference to reasons which do not in fact justify it.

The distinction we have drawn between rational (1) and (2) beliefs is, however, highly simplified. An adequate formulation of it would require a specification of what are to count as justifying reasons; a differentiation between, for example, reasons, good reasons, and sufficient reasons; and an examination of the relations between justifying reasons in different types of discourse, such as science and religion.<sup>9</sup> The complexity of these issues is partly indicated by the kinds of reasons which are normally operative for the members of a scientific community when they accept or reject the results and theories of other scientists. A plausible account of this aspect of scientific activity is given by Barry Barnes (1972b, p. 279):

Of the work he utilizes, no scientist personally checks more than a small fraction, even of that he is fully competent to evaluate, nor, in general, is any responsibility laid upon him to do so. Instead more limited norms of scepticism exist: critical attention is directed to work anomalous with respect to a paradigm, results produced by a recently developed technique, and experiments either charged with technical inadequacy or unavoidably unreliable due to external constraints or dependence on measurements near a technical threshold. Results produced by practitioners reputed to be technically unreliable are given a similarly guarded reception whereas other work is likely to be accepted routinely, especially if it emanates from a highly reputable source. On logistic grounds alone such channelling probably makes good sense; without it work would proceed very much more slowly, and the consequent gain in reliability would be minimal.

This passage raises two interesting points. First, should we characterize these scientific practices as rational or non-rational? This will depend partly on what criteria of rational (1) beliefs are adopted. But there will clearly be some such criteria according to which the acceptance by scientists of many of their beliefs must be seen as not rational (2); since, although there may be good reasons for these beliefs, these are not why they are generally held. Instead, something more akin to an 'appeal to

authority' is involved, which, on stringent criteria of rationality (1), would be non-rational. Second, the last sentence of Barnes's account suggests that the practices he describes are beneficial to the development and progress of science. However, to make this kind of claim requires a prior, philosophical specification of the nature and objectives of scientific enquiry. Only by doing this can we assess the merits of specific types of practice in promoting or retarding the development of scientific knowledge.

A similar point is relevant to the following statements made by Kuhn: 'Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. That position is intrinsically sociological' (Kuhn, 1970c, p. 238). For, although it is partly correct to describe this kind of account as 'intrinsically sociological', it is important to emphasize that it must also be 'intrinsically philosophical'. If the object of Kuhn's enquiry is 'scientific progress', we must first know what sorts of belief and enquiry are to count as *science*; and this is a philosophical issue, that cannot be avoided or eliminated by any amount of sociological, psychological, or historical investigation. Furthermore, we have reservations about the substantive content of Kuhn's sociological enterprise: in particular, his failure to analyse the relations between the institutional features of the scientific community, and other elements of the societies in which these institutions exist. Connected with this, there is a tendency in Kuhn's work to assume that, once established, institutional arrangements do not historically change in response to, amongst other things, changes in these other elements in society. Perhaps because of this, Kuhn does not consider the important question of whether the institutional organization of science that, at one time, is responsible for its 'progress', may become subject to external influences that weaken or destroy the tendency of that organization to generate true or rational beliefs.

The remarks we have made about the essential place of philosophical issues in the sociology of science are related to those made in our discussion of Strauss's objections to a value-free social science, in the previous section. We argued there that a sociological explanation of 'art' might be a correct account of the objects which this term is taken to denote, whether or not their evaluation as 'art' is itself correct. Nonetheless, for such investigations to be properly described as a sociology of *art*, it must also be true that the objects explained are genuine instances of this concept. And this will depend upon the philosophical issues involved in the question 'what is art?'. It is precisely this point that we have just made about the sociology of science. This parallel between the two cases suggests a further point about attempts to account, sociologically, for 'scientific progress'. For if the conception of science which these assume is philosophically unsound, what may result is a correct explanation of social phenomena that are not 'science', but something else — perhaps a more general type of

phenomenon, of which science is only one species.<sup>10</sup>

Let us return now to MacIntyre's argument that the explanations appropriate for rational beliefs differ from those for non-rational beliefs. He concludes from this that, since it is therefore necessary for the sociologist to assess the rationality of the beliefs that are to be explained, 'the positivist account of sociology in terms of a logical dichotomy between facts and values must break down. For to characterize actions and institutionalized practices as rational or irrational is to evaluate them' (MacIntyre, 1971, p. 258). One significant feature of this conclusion is that MacIntyre broadens the ways in which the concepts of value, and evaluation, have normally been interpreted in disputes about the value-free nature of social science. Typically, these concepts have been limited to moral and political values. But MacIntyre also includes rationality-assessments as evaluative. Our own suggestions, in the previous paragraph, about the parallels between Strauss's claims and the problems involved in the sociology of science, give support to this broader concept of evaluation.

We will consider this issue again in section 4. But we believe that MacIntyre is wrong to locate the importance of such evaluations in the way that he does, as prerequisites to appropriate forms of explanation. Instead, we should see philosophical evaluation as relevant to the correct specification of objects of sociological investigation as 'science', 'art', and so on. Yet the kinds of explanation given for such phenomena are not logically dependent, as MacIntyre claims, upon the correctness of these evaluations. For there is no *a priori* relation between, for example, the rationality (1) of a belief, and the nature of its causal determinants.

Nonetheless, as we noted in our earlier discussion of the genetic fallacy, there may well be important *empirical* relations between the rationality (1) of beliefs, and their causal determinants. In particular, it would be strange if we discovered that rational (1) beliefs could typically be explained with no reference to the reasons which justify those beliefs as rational. In other words, it is unlikely that the reasons why people accept beliefs that are rational (1) bear no close relation to the justifying reasons. Thus we should expect the cases where people's beliefs are rational (1) but not rational (2), to be relatively rare. But this is an empirical question that cannot be settled purely by philosophical argument.

However, if this suggestion about the relation between rational (1) beliefs and their causal determinants is correct, it has an important implication for the sociology of knowledge, and especially for the study of science: it is essential to begin such investigations by examining the reasons which people give for their beliefs. Indeed, much of the opposition to the sociology of knowledge can be seen as deriving from the way in which many sociological theories systematically dismiss, as causal

determinants of beliefs, the agent's reasons for these. In the case of science, such an approach may lead to an exclusive interest in social determinants that cannot possibly explain the specific content of scientific beliefs and theories. We argued against such rejections of agents' reasons in section 3 of chapter 7. For us, sociological accounts of science must neither exclude this level of explanation, nor be limited to it. The question, for example, of what it was that led to the emergence of a particular conception and practice of science in seventeenth-century Europe is a legitimate one, for which, in principle, sociological answers may be discovered. But such answers will not be found without an examination of how those who were involved in this historical episode both perceived, and explained, what they were doing. With beliefs, as with actions, the agent's viewpoint can be neither ignored nor left unexplained.

We conclude this section by briefly considering the consequences of our position for some arguments often used against determinism and relativism. One such argument is this. If the causal determination of beliefs entails that they are defective, then the statement of the general thesis of determinism must be self-refuting. For the determinist is claiming that this thesis is correct: but if it were correct, then all beliefs, including the belief in determinism, would be defective. Thus the determinist cannot, without self-contradiction, defend the truth of determinism. Further, it is sometimes argued that this counter to determinism also provides a strong reason for rejecting relativism, the view that there are no universal and objective standards of truth and rationality, but only standards that are accepted by particular social groups or societies. For, if relativism is defended by reference to determinism, as it often is, then the self-refuting nature of determinism entirely undermines its support for relativism.

For us, however, these arguments against determinism and relativism are mistaken. They rest upon the assumption that the causal explanation of beliefs is incompatible with a positive evaluation of their truth or rationality. We have argued against this assumption. And we should note that these kinds of objection to determinism often rely upon the view that people's reasons for beliefs and actions cannot be interpreted as causes. On this view, the causal explanation of beliefs cannot be given in terms of the reasons which justify those beliefs. But we reject this view, and with it, these arguments against determinism and relativism. Nonetheless, we must distinguish the argument against relativism which rests upon the self-refuting nature of determinism, and an apparently similar objection to relativism which is in fact independent of the issue of determinism. According to this, the statement of the general thesis of relativism is self-refuting, since it claims that relativism is true, whilst at the same time denying the existence of universal standards of truth. We regard this objection as a significant one. We should note, however, that our position does not entail any form of relativism to which such an objection could reasonably be made.<sup>11</sup>



### 3. Concepts and reality: the problem of Zande witchcraft

In 'Understanding a Primitive Society' (Winch, 1964), Winch argues against certain elements in Evans-Pritchard's account of Zande witchcraft. According to Evans-Pritchard, the Azande explain particular types of misfortune that befall them in everyday life by reference to the activities of witches. Witches are people with a specific physical condition, the presence of *mangu* in their intestines, which gives them the power of witchcraft. The witch's soul goes out at night and harms those towards whom the witch has a hostile attitude. When a Zande believes that his misfortunes are due to witchcraft, he may resort to a complex procedure of oracular divination, to discover the identity of the witch. One part of this procedure involves feeding a fowl with *benge*, a reddish powder (probably containing strychnine), while asking if a named individual is the witch concerned. It has been decided in advance whether the fowl's dying, or living, will count as an affirmative or negative answer to this question. The procedure is then reversed, by feeding *benge* to a second fowl, and asking if the first answer was correct. If the two answers are inconsistent, a variety of explanations may be given: that bad *benge* was used, or that the procedures had been carried out incorrectly, or that the oracle was itself influenced by witchcraft.<sup>12</sup>

The main disagreement between Evans-Pritchard and Winch is whether to interpret these beliefs and practices as 'poor science', or as not science at all, but some other form of activity which should be understood in terms of the specific context in which it occurs, and its relation to certain central features of human existence, namely birth, death, and sexual relationships. According to Winch, Evans-Pritchard adopts the former view, whilst the latter is in fact the correct one. Why does Evans-Pritchard evaluate Zande witchcraft, as science, unfavourably? He has three main reasons. First, since witches do not exist, its claims are 'not in accordance with objective reality'. Second, it makes use of 'mystical notions', which he defines as 'patterns of thought that attribute to phenomena supra-sensible qualities which, or part of which, are not derived from observation or cannot be logically inferred from it, and which they do not possess'. Third, the Azande do not abandon their beliefs despite internal inconsistencies, and contradiction by experience. Nonetheless, Evans Pritchard does not regard the acceptance of such beliefs as indicating the inferior intelligence, or irrationality, of those who adopt them. For the members of modern Western societies accept their scientific beliefs for much the same reasons that groups like the Azande accept witchcraft beliefs: in both cases, people adopt the patterns of thought provided for them by the societies they live in.

We can begin our analysis of this dispute by noting how Evans Pritchard's evaluation of witchcraft rests upon a conception of science that we have challenged, in several respects, in part 1. Thus, his third criticism assumes an over-simplified view of the procedures involved in the testing of scientific theories, and of the point at which it becomes reasonable to abandon them (see chapter 3, section 1).

His second criticism involves a definition of ‘mystical notions’ which would rule out, as unscientific, many theoretical concepts which have been central to the development of science, especially when this is viewed from a realist standpoint (see chapter 2, *passim*). Finally, although, as we will argue later, it is legitimate to criticize belief in the existence of witches as false, it is wrong to identify good science with true beliefs, and poor science with false ones. To do so would be to write off a large proportion of the major achievements in the history of science, since many of these have later been abandoned as incorrect. It is the general adequacy and rationality of methods and theoretical frameworks that are important in evaluating scientific activity, not the truth or falsity of particular results.<sup>13</sup>

We do not wish to exaggerate the similarities between witchcraft and science. But it is important to notice how philosophical issues about scientific knowledge affect the way in which a social phenomenon such as Zande witchcraft is characterized and evaluated. Let us turn now to Winch’s reasons for objecting to Evans-Pritchard’s account. For us, this account implies an unduly negative evaluation of the merits of witchcraft as science. For Winch, however, Evans-Pritchard’s account is more radically mistaken: it interprets as science a social practice that does not belong to this category. Indeed, it is partly because Winch accepts that to interpret witchcraft as science would entail a strong rejection of its merits, that he objects to that interpretation. Winch, it seems, is committed to some form of the following principle: if, as a consequence of interpreting a system of beliefs and practices in a particular way, we are forced to regard it as fundamentally erroneous and irrational, we can be sure that we have misinterpreted it. Thus, to characterize Zande witchcraft as even an attempt at scientific explanation and prediction must be mistaken, since if we do so, we must evaluate it as systematically erroneous and irrational.

For Winch, this principle of interpretation (which, we should emphasize, is not explicitly advocated by him), is supported by some radical claims about the concepts of reality and rationality. Thus he objects to the view that Zande witchcraft does not accord with ‘objective reality’, on the grounds that there is no intelligible concept of reality that can be used to evaluate the truth or falsity of the belief-systems involved in every form of life or social practice. In a much-quoted passage, Winch (1964, p. 82) claims that:

Reality is not what gives language sense. What is real and what is unreal shows itself *in* the sense that language has. Further, both the distinction between the real and the unreal and the concept of agreement with reality themselves belong to our language.

In particular, there is not only a *scientific* concept of reality, that is internal to the language of science, but also a *religious* concept of reality, internal to its language, and so on. The same is true of logic and rationality. In an earlier work, Winch (1958, pp. 100-1) claimed that criteria of logic

... arise out of, and are only intelligible in the context of, ways of living or modes of social life. It follows that one cannot apply criteria of logic to modes of social life as such. For instance, science is one such mode and religion another; and each has criteria of intelligibility peculiar to itself. So within science or religion actions can be logical or illogical... . But we cannot sensibly say that either the practice of science itself or that of religion is either illogical or logical; both are non-logical.

How do these philosophical claims support Winch's principle of interpretation? Since each form of life has its own criteria of reality and rationality, to interpret any such form in a way that leads us to reject it as systematically failing to accord with reality, or as irrational and inconsistent, must result from a basic misidentification of it. A correct identification will show each form of life to be, in its own terms, coherent and accordant with reality. Winch accepts that it is legitimate to criticize particular beliefs within one form of life as false, or inconsistent with others. But we should do so only by reference to the concepts of reality and rationality specific to that form. We cannot employ universal standards to evaluate the form as a whole, for there are no such standards available. It follows that we must not use the criteria of Western science to evaluate the form of life involved in Zande witchcraft.

But Winch's position generates serious difficulties. Two of these have been identified and criticized by Ernest Gellner (Gellner, 1962 and 1973). First, the operation of the principle of interpretation implicitly adopted by Winch inevitably conceals a significant aspect of most societies: the existence of systematic divergences between beliefs and reality, and inconsistencies between beliefs, which are functional to the continuation of those societies in their present form. Gellner's claim can be illustrated by noting that Winch's principle excludes the applicability to any belief-system of the concept of ideology, a central feature of which is the specification of various kinds of distortion. Amongst these is the misrepresentation of the nature of, and the possibilities of change within, a particular society. Second, Gellner argues that Winch's claims about the concepts of reality and reason make it impossible to give a logically coherent account of a certain type of historical change that often takes place. Such changes result partly from the rejection, by members of some social group, of the truth or rationality of that group's basic beliefs and practices.

Consider, for example, the European Reformation. Gellner claims that this involved the rejection of the pre-Reformation form of religious life, 'X', as incoherent and irrational. This view of X was an essential part of the Reformers' religious life, 'Y'. How is Winch to deal with the view, held in Y, that X is irrational? He is committed to challenging neither X nor Y. But not to challenge Y is to accept Y's rejection of X. So, as Gellner puts it, 'either the pre-Reformation Church was wrong, or the Reformers were, in supposing it to be wrong. . . . One way or another, *someone* must be wrong' (Gellner, 1973, p. 61). For Gellner, it is clearly Winch who is wrong.<sup>14</sup>

One interesting feature of the example Gellner uses here is that it assumes the possibility of different forms of life within the general category of 'the religious form of life'. If Gellner's argument about this example is correct it indicates another difficulty for Winch's position: how is one to distinguish between different forms of life; or, what is to count as a single form? Without criteria for these discriminations, there is a considerable danger that Winch's arguments about the relations between forms of life, and concepts of reality and rationality, will become circular. For, whenever it seems that the members of two different forms of life are engaged in mutually intelligible, systematic criticisms of each other's beliefs and practices, we would be faced with the claim that, by definition, the two forms must in fact be one.

We can elaborate upon these difficulties in Winch's position by comparing it with Kuhn's views about the incommensurability of scientific paradigms, and the nature of scientific revolutions, which were outlined in sections 3 and 4 of chapter 3. In the first edition of *The Structure of Scientific Revolutions* (Kuhn, 1970a), Kuhn suggested that those who operate within different paradigms live in 'different worlds'. If this suggestion is taken seriously, it becomes unintelligible to evaluate the merits of competing paradigms with respect to the truth or falsity of their claims about what exists, or what the characteristics of these items are. Yet an important fact about scientific revolutions is that many scientists live through them. And at least some of them are able to give reasons for preferring the later paradigm to the earlier one. Thus we have another example of the type of historical change that Gellner points to, in his criticism of Winch. If different paradigms have their own standards of reality and rationality, then it is difficult to make sense of the objections made by supporters of the later paradigm to the views they accepted as supporters of the earlier one. Once again, someone must be wrong. It is perhaps to avoid this problem that Kuhn often likens the scientist's change of allegiance to a 'conversion experience'. But even if this correctly describes the psychological experience that at some point normally occurs, it does not remove the necessity of providing a consistent account of the criticisms of one paradigm made by advocates of another.

Kuhn, though, recognizes the philosophically problematic consequences of his claims about incommensurability, especially as they make it difficult to construct a concept of scientific ‘progress’ which, as we noted in the previous section, is important for the specification of his sociological approach to science. But for Winch, a strong sense of incommensurability is required, to rule out the possible evaluation of Western science as superior in its account of reality to that given by Zande witchcraft. As we can now see, however, the view that he adopts of the relationship between language and reality is such that it tends to generate the paradoxical consequences of Kuhn’s views about scientific paradigms. For Winch does not provide a means of distinguishing different forms of life that would prevent one from talking of different forms *within* both science and religion, and not merely of the differences between them.<sup>15</sup>

We conclude this section by making some remarks about the relations between language and reality. Whatever the defects of Winch’s and Kuhn’s positions, they are correct in emphasizing the centrality of these relations to any philosophical account of our knowledge of physical and social reality. First, let us make two general claims that it is reasonable to accept, and which function as ‘outer limits’ for more precise articulations of these relations. On the one hand, many of the beliefs that might possibly be proposed, especially in scientific theories, can properly be judged defective because they ignore, deny, or mis-describe features of a world that exists independently of those beliefs. On the other hand, there can be no beliefs about such features which are not couched within a particular set of concepts; and there is a considerable diversity in the conceptual frameworks that have seemed to their users both convenient and successful in their dealings with physical and social reality.

Second, the phrase ‘concept of reality’ is open to misinterpretations that lead to unnecessarily relativistic consequences. For example, it might be said that, whilst witches are ‘real for the Azande’, they are not ‘real for the Western scientist’; that this shows that the two groups have different ‘concepts of reality’; and that, since one cannot talk about what is real independently of any concepts, one cannot intelligibly claim that one of these concepts of reality is correct and the other incorrect. But this kind of argument is misleading. Instead, we should say that the Azande believe that witches exist, whilst Western scientists do not: the two groups do not have different ‘concepts of reality’, except in the sense that they disagree about what exists. But it does not follow from the fact that people disagree whether a certain type of item exists, that it is in principle mistaken to regard one group as right and the other wrong. Thus there is no inconsistency in maintaining both that ‘witches are real for the Azande’, and that ‘witches do not exist’. In particular, there is no need to add, to the second statement, ‘*for us*’. We may nonetheless be wrong about the existence of witches, and the Azande right: what matters is that this situation, too, can be intelligibly described.

Third, we must distinguish between concepts, and statements or propositions. It is an important feature of any language that it is always possible to express the negation of any statement made within it. Thus, from the fact that a group of people, through sharing a language, share the concepts embodied in that language, it does not follow that they must also share the same set of beliefs. To have common concepts is not thereby to have common beliefs, since, for any statement made within a particular set of concepts, it is always possible to deny the truth of that statement. It is therefore misleading to talk of a set of beliefs as constituting a 'conceptual framework', since it may seem to follow, either that it is impossible to reject those beliefs without also rejecting the 'conceptual framework', or that people whose beliefs differ must therefore be operating within different 'conceptual frameworks'. There is thus an important sense in which concepts do not *determine* beliefs.

But finally, although no one set of concepts determines a single set of beliefs, it is equally true that not all beliefs can be expressed in a given set of concepts. As a simple example: we can believe either that a table is red, or that it is not red, while using the same concepts; but we cannot believe that it is square without an additional concept. Concepts therefore limit beliefs, so there is a genuine problem about what makes one set of concepts preferable to another. More specifically, we have to consider whether one such set can be preferred because it enables us to make more, or all, true statements about a reality that exists independently of those concepts, and thus independently of the beliefs expressed by means of them. Such problems themselves depend, for their solution, upon an adequate analysis of the relations between the knower and the known, between the subject and object of knowledge. We turn now to an examination of a group of writers for whom this analysis, together with closely connected issues, is of central importance. In doing so, we will be returning to the problems of value-free social science left partly unresolved at the end of section 1.

#### **4. Critical theory**

The expression 'critical theory' has been applied to a wide range of different theoretical standpoints. In its narrowest sense, it refers to the views advocated by members of the Frankfurt School, especially in the early writings of Max Horkheimer and Herbert Marcuse. We will begin by outlining some of their basic claims. Later we will examine, in greater detail, a more recent writer connected with the School, Jürgen Habermas.<sup>16</sup>

A central concept in the early attempts to articulate a critical social theory is that of *reason*. Its meaning derives from the primarily Hegelian tradition within which these attempts were made. Marcuse (1968, pp. 135-6) characterizes the concept of reason in the following way:

Reason is the fundamental category of philosophical thought, the only one by means of which it has bound itself to human destiny. Philosophy wanted to discover the ultimate and most general grounds of Being. Under the name of reason it conceived the idea of an authentic Being in which all significant antitheses (of subject and object, essence and appearance, thought and being) were reconciled. Connected with this idea was the conviction that what exists is not immediately and already rational but must rather be brought to reason. Reason represents the highest potentiality of man and existence; the two belong together... Reason was established as a critical tribunal ... [and] the concept of reason contains the concept of freedom as well. For such examination and judgment would be meaningless if man were not free to act in accordance with his insight and to bring what confronts him into accordance with reason.

Thus, for Marcuse, reason can be used to criticize and challenge the nature of existing societies. Reason is a 'critical tribunal'; and its judgments are based upon the values of both freedom and pleasure. Marcuse, like Horkheimer, opposed the way in which personal gratification and hedonic pleasure are rejected in bourgeois culture. They also defended the ideal of humans as free, autonomous agents, able to create and control their own lives in a society where there is an absence of the forms of alienation and reification characteristic of capitalism. Further, unlike many political and moral philosophers, they did not regard the ideals of pleasure and freedom as ultimately conflicting with each other. But existing types of pleasure are to be evaluated in the light of freedom. For Marcuse especially, the satisfactions available in capitalist society reflected the presence of 'false' wants and needs: only in a free society could true wants and needs be generated. Thus capitalism leads not only to the non-satisfaction of existing wants, but also to the satisfaction of false ones. The elimination of this irrational state of affairs requires the transformation of society into one where the means of production are controlled by the community, who actively participate in directing the productive processes towards the needs and wants of the whole society (Marcuse, 1968, p. 193).

For Marcuse and others, a transformation of this kind must take place through political practice guided by a critical theory of society. We can note four important features of such a theory. First, it starts from an evaluation of existing reality as fundamentally irrational. Second, it attempts to identify the possibilities for change in that reality, the relations and developments that are already operating to undermine the continuation of its present form. Third, critical theory challenges the ideological, reified consciousness which is generated by existing social reality, and which systematically conceals these potentialities for radical transformation. Finally, it is opposed to positivism, and to the positivistic assumptions of most types of social science.

Let us consider this last point in more detail. For the early critical theorists, 'positivism' refers not only to the specific view of science which we outlined in chapter 1, but also to the divorce of facts from values, to the denial of any distinction between essence and appearance, and to the claim that empirical science is the only legitimate form of human knowledge. The restriction of human knowledge to science is seen by these writers as leading to the elimination of philosophy itself. For philosophical knowledge is not empirical science, and the positivist's conception of knowledge means that most of the traditional concerns of philosophy are condemned as meaningless metaphysics. In particular, positivism is regarded by critical theorists as denying the intelligibility of the concept of reason. As a consequence, positivism performs the function of supporting the status quo, since it rules out the possibility of a theory of society based upon the critical, philosophical concept of reason.<sup>17</sup>

We will now consider how far this conception of critical theory departs from Weber's claims about the place of values in social science. First, critical theorists reject Weber's view of the status of value-judgments. We can see the differences between their positions by contrasting their concepts of rationality. For Weber, judgments of rationality are limited to an assessment of the relations between means and ends, and of the internal consistency of a given set of ends. But there is no possibility of evaluating the ends themselves as rational or irrational. It follows that the only sense that can be given to the idea of a rational society is that of a society organized in such a way that the most effective means of achieving the values accepted in it are systematically employed. For critical theorists, however, a rational society is one that embodies the values supported by reason. A society that is rational, in Weber's sense, can thus be criticized for the irrationality of the ends pursued in it.

Second, though, we must examine how this disagreement about the status of value-judgments affects Weber's view of value-relevance, his claim that values can be limited to a selective function and are not relevant to the truth or falsity of social theories which investigate the objects selected in this way. A Weberian might argue that one should regard the 'values dictated by reason' as directing the social scientist's interest towards specific features of an infinitely complex reality. These value-relevant features would be the elements in a given society that are potential causes of its transformation into a form which realized the values of human freedom and pleasure. But there could be no guarantee that a social theory oriented in this way would in fact discover such potential sources of change, or be able to show the present likelihood of the hoped-for transformation. To think otherwise would be to permit interest-directing values to distort the objectivity of social science.

Could critical theorists accept this Weberian characterization of the place of values in their theory of society? Certainly, they did not regard the values of reason as guaranteeing the 'right' results for sociological enquiries. Indeed, the early members of the Frankfurt School were generally pessimistic



about the chances of a radical transformation of capitalism. This pessimism was partly due to their belief that the proletariat was no longer the potential agent of revolution, and partly also to the growth of Fascism at the time they were writing. Nonetheless, critical theorists entirely rejected a basic assumption of Weber's position, the logical heterogeneity of facts and values. They regarded this assumption as itself deriving from a more fundamental philosophical error, a denial of the essentially *active* part of the human subject in the process of cognition. They therefore rejected the view of perception and knowledge as passive processes of sensory reception by which humans can achieve a correct representation of a world that exists independently of them. Thus Weber's position is seen as resting upon mistaken epistemological and ontological doctrines.<sup>18</sup>

Before going on to examine Habermas's work, which has several themes in common with the early critical theorists, we will explore some of the epistemological and ontological issues raised in the previous paragraph. We will do so by outlining the position developed by Leszek Kolakowski (1969), who, it should be emphasized, is regarded neither by himself nor by the Frankfurt School as a critical theorist. But his attempt to rescue, reconstruct and defend the epistemological views in Marx's early writings is of considerable relevance to the discussion of critical theory.

Marx, especially in the 'Theses on Feuerbach' (Bottomore and Rubel, 1963, pp. 82-4), accepts the view of philosophical idealists that human perception and cognition are active, rather than passive, processes; yet he insists, against idealism, that the activity involved is not that of a detached consciousness, but the conscious activity of human agents engaged in a dialectical interaction with nature. In that interaction, humans attempt to satisfy their existing needs, and as a result, both the natural world, and human agents, are repeatedly transformed. Kolakowski (1969, p. 66) states the implications of Marx's views for the relations between concepts and reality thus:

Human consciousness, the practical mind, although it does not produce existence, produces existence as composed of individuals divided into species and genera. From the moment man in his onto- and phylogenesis begins to dominate the world of things intellectually — from the moment he invents instruments that can organize it and then express this organization in words — he finds that world already constructed and differentiated, not according to some alleged natural classification but according to a classification imposed by the practical need for orientation in one's environment. The categories into which this world has been divided are not the result of a convention or a conscious social agreement; instead they are created by a spontaneous endeavour to conquer the opposition of things. It is this effort to subdue the chaos of reality that defines not only the history of mankind, but also the history of nature as

an object of human needs — and we are capable of comprehending it only in this form.

The position outlined here contains elements of what is normally termed ‘pragmatism’. But it differs from several forms of pragmatism. For it accepts the idea of a world of things that exists independently both of our practice upon it, and of our theories about it. And it locates the philosophical significance of human practice primarily in the constitution of categories and concepts, rather than in the assessment of the truth or falsity of the beliefs and theories that are expressed within those concepts. It thus rejects two views often associated with pragmatism. First, the view that the main importance of practice is in the testing of beliefs and theories. Second, the view that truth or falsity is to be identified with successful or unsuccessful practice. Further, we should note that, if human needs change historically in the way that Marx suggests, one should expect consequent changes in the concepts and categories through which our beliefs and theories about nature can most appropriately be expressed.<sup>19</sup>

A central feature of Marx’s epistemology is the attempt to relate the foundations of knowledge to fundamental characteristics of the human species. In *Knowledge and Human Interests* (Habermas, 1971), Habermas accepts this general approach but argues that Marx was mistaken in his exclusive concern with one of these characteristics, namely work or labour. For Habermas, there are three distinctive forms of knowledge, of which only one, involved in the *empirical-analytic sciences*, can properly be understood by reference to this characteristic of the human species. The other two forms are those involved in the *historical-hermeneutic sciences*, and in *self-reflection*. Habermas says that each form of knowledge is constituted as such by a separate ‘knowledge-constitutive interest’. The three interests are, respectively, the *technical*, the *practical* and the *emancipatory*. It is important to notice that Habermas does not use the term ‘practical’ in the way we have used it in our discussion of Marx and Kolakowski. Instead, the latter sense of ‘practical’ is closest, in Habermas’s definitions, to what he calls ‘technical’. We will see this in examining Habermas’s account of the relation between empirical-analytic science and its technical, knowledge-constitutive interest.

Habermas claims that the empirical-analytic sciences, which include both the natural sciences, and economics, sociology, and political science, are aimed at the discovery of nomological knowledge about natural and social relations. Their particular status as knowledge is constituted by their interest in technical control, in increasing the possible extent of human domination over natural and social reality. These sciences provide us with the information that is required for rational, feedback-controlled instrumental activity. Let us consider two questions raised by these claims. First, what does Habermas mean by ‘technical interest’? Second, in what sense is this interest ‘constitutive’ of knowledge?

Concerning the first question, Habermas does not mean that scientists typically engage in their enquiries intending to discover laws that can be used for instrumental control. The technical interest is not a particular type of motivation on the part of scientists. Rather, it is related to an essential characteristic of the manner in which the human species historically transforms itself: to labour, productive activity upon nature, by which the human species reproduces itself biologically and culturally. This productive activity both requires and generates a specific 'interest' in technical control. Thus Habermas maintains that his concept of interest is neither purely empirical nor purely transcendental: it does not refer to actual intentions, yet it remains rooted in the general characteristics of human existence, and not in an abstract realm of philosophical categories.

As for the second question, about 'constitution', Habermas argues that the technical interest constitutes knowledge at two related levels. First, it provides a criterion for what is to count as 'real', that is, for what counts as an 'object', about which the propositions of empirical-analytic science give us knowledge. What is real, in relation to the technical interest, is what can be detected, measured and manipulated in the situation of controlled experiments, in which information is received through our perceptual mechanisms. Second, the technical interest determines the general character of the standards employed in assessing the truth or falsity of statements made about these objects. An example of such a standard is that of falsification, which requires us to reject those statements whose predictive consequences are unsuccessful. For Habermas, it is only by reference to the technical interest that we can understand, and justify, the limitations that are imposed in the empirical-analytic sciences upon what is real, and upon what is an acceptable statement or system of statements about such real objects.<sup>20</sup>

But Habermas maintains that the empirical-analytic sciences cannot be seen as definitive of human knowledge: they involve only one form of knowledge, whose criteria of reality and standards of validity are relative to the interest in technical control. This interest derives from one essential characteristic of the human species. But, just because there are other such characteristics, that give rise to non-technical interests, there are also other, distinctive forms of knowledge, with their own criteria of reality and validity. One of these forms is involved in the historical-hermeneutic sciences, and is constituted by the practical interest. The objects of these sciences (that is, what is defined as real), are inter-subjectively established meanings. Their criteria of validity, or standards of assessment, are those of *interpretation*, the paradigmatic example of which is the interpretive understanding of linguistic communication in everyday discourse. For Habermas, communicative interaction is an irreducible category of the mode of existence and self-transformation of the human species. In particular, it cannot be reduced to the category of work. Thus the hermeneutic-historical

sciences involve a distinctive form of knowledge that is constituted by the practical interest. It follows that these sciences cannot intelligibly be criticized for failure to define their objects, and criteria of validity, in the manner of the empirical-analytic sciences.

The third form of knowledge that Habermas distinguishes is that constituted by the emancipatory interest, an interest in human autonomy and responsibility (*Mündigkeit*). It is a form of knowledge that involves *self-reflection*, and which is itself involved in critical theory. To understand what Habermas means by these claims, we must consider his general opposition to ‘positivism’, a term which has, for him, a similarly broad sense to that given it by the earlier critical theorists. His central criticism of positivist philosophy is that it is unable to account for the epistemological status of its own claims.

We can best see the point of this criticism by an example that is not used by Habermas himself. Consider the principle proposed by many logical positivists in the twentieth century, that all statements are either empirical (synthetic), *a priori* (analytic), or meaningless. A frequent objection to this has been that this principle conforms to neither of the two ‘meaningful’ types of statement allowed by it. It thus has the paradoxical character of being meaningless if true. Habermas, we believe, would regard it as a philosophical claim, for which various grounds could be advanced; but such grounds could not justify it in either of the ways that logical positivists allow as legitimate types of knowledge.

For Habermas, philosophical knowledge involves self-reflection, by which we reflect upon particular features of human existence, and especially upon the nature and status of human knowledge itself. Thus Habermas regards the examination of the relations between technical and practical interests, and their respective forms of knowledge, as itself a case of self-reflection, which positivism, through its implicit abolition of philosophical knowledge, cannot comprehend. Further, philosophical knowledge, in its concern with issues such as the criteria of validity appropriate to different types of science, is thereby concerned with questions about values and standards. These questions are to be answered by means of critical argument, which is concerned with the defence or challenging of any kind of evaluative standard. Critical argument is one of the three general uses of language. Thus Habermas (1974, p. 216 — our italics and insertion) states:

A critical discussion, regardless of whether it concerns the acceptance of proposals or propositions, includes a threefold use of language: the descriptive, in order to describe a state of affairs; the postulatory, in order to establish rules of procedure; and *the critical, in order to justify such decisions*. Logically these forms of speech mutually presuppose each other. The

descriptive usage is in no way limited to a certain class of ‘facts’ [e.g. the facts about the ‘objects’ of empirical-analytic science]. The postulatory usage covers the *establishment of norms, standards, criteria and definitions of all kinds, no matter whether practical, logical or methodological rules* are involved. The *critical* usage employs *arguments for considering, evaluating, judging and justifying the choice of standards*; it includes therefore language-transcendent approaches and attitudes in its discussion.

What, then, is the relationship between self-reflection (embodying critical argument), the emancipatory interest, and the nature of a critical theory of society? To answer this absurdly complex question, we must note how Habermas treats Freud’s theory of psycho-analysis. He regards this theory as especially important as the only tangible example of a science incorporating methodical self-reflection.

Habermas claims that psycho-analysis involves a kind of ‘depth hermeneutics’, in which the ‘distorted texts’ of the patient’s behaviour become intelligible to the patient through the analyst’s interpretations, and the patient’s self-reflection. As a result of successful psycho-analysis, the previously concealed and causally determinative motives are revealed to the patient, and at the same time lose their power as unconscious determinants of the patient’s actions. For Habermas, psycho-analytic theory involves both empirical-analytic and historical-hermeneutic forms of knowledge: the former in, for example, the discovery of the past episodes that lead to repressions, and the latter in, for example, the interpretation of dreams. Further, Habermas (1971, p. 226) claims that psycho-analysis presupposes a specific norm or ideal standard of human relations:

In the methodically rigorous sense, ‘wrong’ behaviour means *every deviation from the model of the language-game of communicative action*, in which motives of action and linguistically expressed intentions coincide... . This model, however, could be generally applicable only under the conditions of a non-repressive society. Therefore deviations from it are the normal case under all known social conditions.

In two more recent articles (Habermas, 1970a, 1970b), Habermas extends his account of the norms involved in communicative action, by outlining a ‘general theory of communicative competence’. A central feature of this is the specification of an ‘ideal speech situation’, which is presupposed by the actual communicative competence that all language-users display. This ideal situation involves, in addition to the norm of motive-and-expressed intention coincidence, others which Habermas suggests are related to the traditional ideals of truth, freedom and justice. And he puts forward two hypotheses. First, that the degree to which actual speech-situations deviate from the ideal one is dependent upon

the degree of repression that characterizes the institutional system within a given society. Second, that the degree of repression depends on the developmental stage of the productive forces and the system of political and economic power (Habermas, 1970b, p. 374).

We conclude by stating the implications of Habermas's position for the nature of a critical theory of society, and our own critical response to these. For Habermas, a critical theory of society is not itself a distinctive form of knowledge. Thus the relationship between such a theory and the emancipatory interest, is not symmetrical with the relationships between empirical-analytic and historical-hermeneutic sciences, and their knowledge-constitutive interests. Rather, a critical theory of society is one which involves both of these other forms of knowledge; which is methodologically self-conscious of the distinctive nature of these forms; and which is oriented towards the emancipatory values embodied in the activity of that philosophical self-reflection which makes methodological self-consciousness possible. More specifically, such a theory would include the following types of enquiry. First, the interpretive understanding, by the methods of historical-hermeneutic science, of the systems of belief and modes of communication present in a given society. Second, their critical evaluation by reference to the norms of an ideal speech-situation. Third, the investigation, by the methods of empirical-analytic science, of the causal determinants of those modes of communication and belief, and of their departure from the norms that can be justified by self-reflection and critical argument. Finally, a specification of the kind of non-repressive society in which these values can be realized.

We believe that a critical theory of society of this kind has many desirable features. But it also has at least two serious defects. First, Habermas's account of the nature of the empirical-analytic-sciences is essentially positivistic. This is revealed partly by the way he characterizes the objects of these sciences, in terms of what is open to detection and manipulation in controlled experiments; partly by the way he describes their aim as the discovery of nomological knowledge; and partly by his inclusion of what he calls the 'systematic sciences of social action', namely economics, sociology and political science, as examples of empirical-analytic science (Habermas. 1971, p. 310). We suggested, in part 2, that most social theories have been based upon positivist conceptions of science. Further, we noted that a major exception to this, Marx's structural theory, involves the adoption of a non-positivist, realist conception of science. And we argued in part 1 that realism, at least in the natural sciences, is preferable to both positivism and conventionalism. Thus we regard Habermas's analysis of the empirical-analytic sciences as failing to challenge the positivist understanding of scientific knowledge, its view of theories and explanations, and its ontological and epistemological restrictions.

Second, though Habermas is correct to distinguish the forms of knowledge involved in interpretive understanding and causal explanation, there is a danger that critical social theory will be split into two irreconcilable components: the investigation of causal relations that do not involve the subjective states of human agents; and the interpretive understanding of human actions and beliefs. Against this, we maintain the necessity of a social theory which, whilst involving both interpretive and explanatory understanding, unifies these in the analysis of structural relations, and of the way in which these affect, and are affected by, the subjective meanings of human agents. For example, we need interpretive understanding to identify the intentions of individual agents, and the contents of the systems of belief and value that are present in a given society. But this must be combined with an analysis of how an agent's acceptance of such beliefs and values is causally operative in his or her actions; and of how systems of belief are causally related to the structural relations and mechanisms present in specific social formations.<sup>21</sup>

## Notes

1 We will not discuss a third concept that Weber employs, 'value-interpretation': see especially 'The Logic of the Cultural Sciences', and pp. 22 and 33, in Weber (1949). This concept has been ignored by most commentators, but is important in relating his views about values and understanding: on the latter, see section 1 of chapter 7. Our account here is drawn mainly from the first two essays in Weber (1949). Useful discussions of Weber and value-freedom are Runciman (1972), sections 3 and 4; and Lessnoff (1974), ch. 6.

2 On Weber's political values, see Giddens (1971), pp. 190-5. On the intellectual context of the dispute about value-judgments in teaching, see Dahrendorf (1968), ch. 1. See Strauss (1953), for an attack on Weber's philosophical theory of value-judgments, as leading to 'moral nihilism'.

3 Our account of Weber's position about the relation of values to causal explanation departs from that of most commentators, e.g. Ashcraft (1972). For examples of how values have, in fact, distorted social scientific work, see Myrdal (1958). On Weber's concepts of rationalization and disenchantment, see Giddens (1971), pp. 214-16, and 178-84. On idealization and abstraction, see n. 9 to chapter 2, and section 5 of chapter 5. For a stimulating discussion of the Kantian influence on Weber's position, see Goddard (1973).

4 Taylor also argues (1967) that the rejection of the value-judgments supported by a social theory entails the rejection of the theory's explanatory framework. But this argument depends, we believe, on mistakenly assuming that a variable that is significant for value-judgments is necessarily causally significant also. Taylor's moral theory seems to ignore issues about the distribution of satisfactions: see Lessnoff (1974), pp. 139-40. For an interesting discussion of the values may be involved in setting appropriate levels of 'statistical significance' in theory-testing, see Leach (1968).

5 For Gouldner's later development of the idea of 'reflexive sociology' which contain many of the elements in the two papers we have discussed, see Gouldner (1971), ch. 13. For a critical review of this book, see Urry (1972).

6 Cf. our remarks about Strauss and MacIntyre, with Nagel's distinction between 'characterizing' and 'appraising' value-judgments. We should note that several definitions of 'justice' make use of the concept of human needs: the precise status of this concept, and thus its use in explaining social phenomena, is problematic.

7 On the denial of universal standards for assessing competing theories in the natural sciences, see chapter 3, sections 3 and 4. For a useful discussion of the relations between determinism and the free-will problem, see Honderich (1971), ch. 5. Lukes (1973b) examines many arguments about truth and social determination. The idea that only 'odd' or 'abnormal' phenomena require causal explanation is common in many ordinary language  
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philosophers: for a decisive rebuttal of one such view, see Mandelbaum (1958). We return to arguments against determinism and relativism at the end of this section.

8 On Lukács's position, see section 2 of chapter 8. See Cohen, G. (1968), for an interesting defence of the claim that the proletariat, unlike the bourgeoisie, can develop true theories. Plamenatz (1970), ch. 5, criticizes the way 'class-interest' is typically used in making such claims. Note that causal explanations of beliefs must concern not only their origins, but also their *continued acceptance*.

9 For discussion of different criteria of rationality, see Schutz (1943), and Lukes (1967). On the difference between reasons, good reasons, and sufficient reasons, see Alexander (1962). MacIntyre emphasizes the importance of distinguishing truth from rationality: (1971), p. 248. On Newton's theology, and his beliefs about space and time, see Burt (1932), ch. 7. On sixteenth- and early seventeenth-century astronomy, see Kuhn (1959). For support of the legitimacy of causally explaining non-defective beliefs, see Peel (1969), and Barnes (1972b).

10 Cf. Popper (1970), who claims that Kuhn concentrates too much on what is, for Popper, 'bad' science (i.e. 'normal' science). For an assessment of Kuhn as a sociologist of knowledge, see Urry (1973c). A useful collection of papers on the sociology of science is Barnes (1972a). Ravetz (1971) contains a stimulating discussion of the problem of 'quality-control' in science, and the deleterious effects of various external influences on the scientific community.

11 See Trigg (1973), ch. 7, for a typical defence of the claim that determinism supports relativism, and is self-refuting: in ch. 1 he also argues that relativism is self-refuting. It seems that Mannheim (1960) was led to support a version of relativism by his belief in social determination: but this is unclear, as is the precise nature of his relativism. Some form of conceptual relativism is probably implied by the relations between concepts and reality derivable from Marx's early writings — see section 4 below.

12 For Evans-Pritchard's account of the Azande, see Evans-Pritchard (1937): a useful summary is in Gluckman (1944), who emphasizes that witchcraft explanations are typically invoked in cases of coincidence, and where the Azande's technical knowledge (important to an adequate analysis of their witchcraft beliefs) provides no explanation. We will not examine the many other interpretations of witchcraft by anthropologists: see Skorupski (1973), for a helpful discussion of these, and their philosophical relevance. Marwick (1970) contains a number of important papers on witchcraft and magic.

13 See Polanyi (1958), pp. 286-94, on witchcraft, science and falsification; and Horton (1967), for a seminal discussion of witchcraft and magic on the basis of a generally realist view of science. Note that our account of Evans-Pritchard's evaluation of Zande witchcraft is constructed from his comments in several different publications. Our quotations are taken from those used by Winch (1964): the bibliographical data are in that paper.

14 Cf. this argument of Gellner's against Winch, with the 'self-refuting' argument against relativism outlined at the end of section 2. On the concept of ideology, see chapter 8, *passim*. Note that the identification of inconsistencies between beliefs is made difficult by the problem of the hermeneutic circle: see chapter 7, section 4, and n. 21. (I sometimes wonder what the Azande would make of their misfortune if they read all this — R.N.K.)

15 See Sherry (1972), and Mounce (1973), for criticisms of the way Winch interprets Wittgenstein's (1963) views on language-games and forms of life. On the relations between Kuhn, Winch and Wittgenstein, see Trigg (1973). Kuhn discusses the question of what, on his views, counts as 'scientific progress', in the Postscript to (1970a), and in (1970e).

16 Our account of early critical theorists owes much to Jay (1973), ch. 2. He notes their rejection of certain features of Hegel's philosophy, especially the subject-object identity — cf. our discussion of Lukács, in section 2 of chapter 8. We have ignored the differences between, e.g. Horkheimer and Marcuse. Many of Marcuse's early papers are translated in Marcuse (1968): Horkheimer's, in Horkheimer (1972).

17 On the broader sense of 'positivism', see the Introduction to part 1, and the discussion of Habermas later in this chapter. For Horkheimer's attacks on positivism, see 'Traditional and Critical Theory', 'The New Attack on Metaphysics', and 'The Social Function of Philosophy', in Horkheimer (1972). For Marcuse's, see his (1937),



and 'The Concept of Essence', in Marcuse (1968). For Marcuse's defence of hedonism, see 'On Hedonism', in (1968); the attack on capitalism's generation of false wants is continued in his later (1964). See also our discussion of Marx's essence-appearance distinction in chapter 8, section 1.

18 The pessimism of critical theorists is related to the later studies of the 'authoritarian personality', by Adorno and others. These are discussed by Jay (1973), who also notes the ambivalence of Horkheimer on the relations between true theories, reason, and social change: see pp. 63-4.

19 For discussions of American pragmatists, see Habermas (1971), chs 5-6, and Bernstein (1972), part III. On the idea of language producing 'different dissections of nature', see Whorf (1956): Hoijer (ed.) (1954) contains several important discussions of his ideas, especially in the papers by Hoijer and Hockett. Lawton (1968), ch. 4, gives a useful summary of work on the relations between language, thought and perception. On Marx's early epistemology, see Rotenstreich (1965).

20 On Habermas's conception of interests, see especially ch. 9 of (1971); his (1974), p. 203, is helpful on what he means by 'constitutive'. Both these, and other features of his position have generated considerable discussion. A useful summary of the main criticisms is Dallmayr (1972) for Habermas's replies, see his (1973). Our own account is based mainly on Habermas (1971) and (1974), which is part of an important exchange with Albert (1974).

21 We should note that Habermas, in the writings we consider, is not explicitly concerned with the nature of a critical *social* theory: our view of the implications of his position for this are based on his general standpoint, and occasional specific remarks — e.g. the analogy he suggests between Freudian 'rationalizations' and the concept of ideology. We believe his analysis of Freud, in chs 10-11 of (1971), indicates that he regards only unconscious motives and intentions as causal determinants, and that he thus rejects the causal interpretation of reasons we defended in chapter 7, sections 2 and 3. See McCarthy (1973), on Habermas's theory of communicative competence.

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