

Introduction

THE SOCIOLOGIST who begins a history of social theories is at once very tempted to stop. To write such a history, he has first to decide what social theories are. If he suggests a sociological answer, he both presupposes the validity of what he is trying to explain and undermines his claim to have taken a decision at all. If he suggests another, he seems oddly to exempt himself and his subjects from the determinations he claims to detect in others. He is caught between circularity, self-cancellation and bad faith. This is a history written in bad faith. It accepts that sociologists have asked important questions. It doubts that they have so far provided very good answers. And this distance between question and answer is nowhere greater than on the issue of what social theories themselves are. None of the existing answers to this question will do, although why they will not makes it clear what might.

One of the first and always the most direct set of answers are the Marxists'. Marxists essentially claim that all social theories, except of course their own, are ideologies, false beliefs promoted and sustained by the extra-intellectual interests of the class which holds them, and explained by those interests. The claim has two obvious difficulties. The first, as Goldmann put it in *The Hidden God*, is that the truth of Marxism, upon which its entire position rests, is no more than a wager on the outcome of history, on the economic, political and intellectual victory of the exploited who by virtue of their exclusion from bourgeois society are uniquely privileged with a total comprehension of that society and its necessary supersession. It has become more difficult to make that wager. Second, however, even if one does, the fact that the variety of ideologies in approximately capitalist societies has been and continues to be so great requires the Marxist either to postulate an equally large variety of bourgeoisies or to concede that the beliefs of the one essential bourgeoisie are so various that the ideological obstacles to that

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class's supersession are themselves various. Either concession badly weakens the force of the initial wager. It might be said that this is too crude, that the nature of class conflicts, of their infrastructural and superstructural characters, is complex and shifting. But this, while meeting what non-Marxists might regard as more nearly the true state of affairs, concedes too much. If societies do differ so, little or nothing is left of the Marxist philosophy of history but a diffuse teleology, an interesting set of concepts, and a rather roughshod notion of intellectual production.

A second set of answers are those suggested by Durkheim and by those in some way inspired by him. They are implied in accounts of religious belief and of primitive classifications more generally. They point to the similarity of structure between a society and its ideas. Durkheim himself never offered more than a very simple account of how one was to assess such similarities, but even his more sophisticated followers have failed satisfactorily to answer the three obvious questions. First, why should the structure of an idea, the arrangement of its premises and of the contentions derived from or otherwise built upon them, be more fundamental than its content? Second, what is to count as a society? And third, how exactly may one explain the connections between social structures and intellectual ones? Durkheim himself was very high-handed with the heterogeneity of complex societies, which he regarded as a passing phase, and he also exaggerated the homogeneity of simpler ones. This is not a simply scholastic point. If one regards each idea or set of ideas as distinct, which the approach requires, one is faced with the problem of explaining why the heterogeneity of ideas is so great and socially so untidy, of how groups can relate in some ways and not in others, of why a society in one respect is not a society in another. If the reply is that this is exactly the virtue, that the approach sorts analytical order from empirical chaos, then one is back with the first question: why should one sort order in this particular way? It cannot be that it promises more satisfactory explanations. Durkheim's own account of how the homology comes about is notoriously feeble. Where he does not confuse the very faculty of intellectual organisation with particular orderings in particular societies (as in *Primitive Classification*), he resorts (in *The Elementary Forms of the Religious Life*) to the view

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that the the sentiments engendered in a particular pattern of social relations will cause men to have notions that replicate, reinforce and celebrate that pattern. Others resort to more plausible accounts. Lévi-Strauss takes the more directly Kantian line of suggesting a human mind with certain universal habits of organisation, the outcome of which will depend upon the exigencies of the society in question. Mary Douglas appears similarly to work from a suppressed psychological premise of a need for cognitive order, and also assumes a causal connection between such order and tight social control. But such devices steer an extremely unsteady course between arbitrary declaration and *ad hoc* invention which although in many ways attractive as what Lévi-Strauss himself would call *bricolage* cannot really be sustained as a proper theory of belief, and thus as a plausible starting point for a theory of social theories themselves.

A third answer to the question of what social theories are and how they may be explained has not been proposed as a strictly sociological answer at all, but its ambitions are identical and its promise, at least, is to resolve difficulties raised by each of the first two. It is suggested by Toulmin. Toulmin's thesis rests upon an extension into intellectual history of the Darwinian account of species change. He suggests that one can at any time identify what he calls 'transmits', populations of concepts which are subject always to wide variation and which are constantly being selected, or not, according to their applicability. This has its attractions. It concedes, which most Marxists and Durkheimians do not, that in any society at any time there is a great variety of incompatible or ill-fitting ideas. It also allows the irresistible truth that the ways in which ideas change owe much to what it suits people to believe. But there are difficulties. One is raised by the analogy between mutation and the variety and variability of ideas. The first can be described by models of chance occurrence and explained mechanistically. The second cannot. Another lies in the extension of 'applicability'. In biology, fitness is conventionally and narrowly defined as reproductive success, and although there is in itself nothing wrong with defining the fitness, or success, or applicability of an idea in the same way, such a definition tends to overlook the wholly different ways in which animals reproduce and ideas survive. Indeed, Toulmin's superficially seductive scheme falls apart at every point. At best,

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it can only provide the basis of a purely statistical description of changing populations of ideas.

Marx and Durkheim and most of those who have looked at beliefs in the ways they suggested have in general looked only at non-scientific beliefs or, in the case of the Durkheimians, at what from a western point of view may be regarded as primitive proto-sciences. Toulmin apparently intends his account to cover beliefs of all kinds. Two others, however, a French philosopher, Bachelard, and an American historian of physics, Kuhn, have talked only of science, and although neither seems to have thought that what he said could also be extended to the social sciences (Kuhn has deliberately denied that it can), others have demurred. Bachelard has been appropriated by Althusser to explain the development of Marxism in Marx himself and in Lenin, and Kuhn has been more or less thoroughly appropriated by others to commend, condemn or merely describe the similarities as well as the differences between natural science and social science. Bachelard wrote from within an uncompromising (but also unorthodox) rationalism. Kuhn writes from within a conventionalism which like the scientists he describes is compromised upon a recalcitrant world. But the outcomes, as history, are remarkably alike. They are also sociological, and unless one presumes that there are no good reasons at all for thinking that the practice of understanding society bears any resemblance to that of understanding nature, they are thus both at least *prima facie* candidates for a sociology of sociology itself.

For each, science is essentially social. It consists of a community within which there is a common view, what Kuhn calls a 'paradigm', a conceptual scheme which defines the objects of investigation and the ways in which they are to be investigated. Bachelard is the philosophically more extreme of the two, appearing to believe that scientists quite literally construct the objects of their inquiry, and equating this at once socially and intellectually disciplined activity with reason itself. Kuhn does grant an external world, and also grants that it can face any paradigm with eventually intolerable anomalies, so causing a change. Each implies that what scientists believe is the effect of a strictly social pressure, a pressure against imagination and fancy, a pressure which achieves its effect in what are at least partly psychological ways. The paradox of science in their view,

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therefore, is that its rational constructions are if not always then certainly for long periods independent of the properties of the world and themselves a function of non-rational forces. But for Bachelard, the history of science is necessarily an *histoire raisonnée*, a rational history of a rational activity for which because it is entirely exhaustive of reason there can be no rational explanation outside itself. The other history, which *ex hypothesi* cannot be written as a rational and so intelligible history at all, is an *histoire périmée*, a history of the confused and shifting *rêveries* of those who have not yet entered the *cit  scientifique*. For Kuhn, the history of science is the history of scientific revolutions, a history of transitions from one paradigm to another, a history which is explained by the recurring fact that rational men, men who are rational by virtue of their being men and not by virtue of their being scientists, encounter facts which their paradigm cannot explain. By extension, and eliding their philosophical differences, the history of sociology implied by each is a history of the systematisation and closure of explanations of social life in clearly demarcated and tightly controlled communities.

As such, it has three difficulties. The first and most obvious, as Kuhn himself has said, is that it is simply not true. The social sciences, and certainly sociology, have rarely been as well institutionalised as the natural sciences, and even where they have, social scientists have seemed much more able to resist the pressure of their peers. There does indeed seem almost to be a qualitative difference. In the one, the deviant is ignored and unrewarded. In the other, at least in Europe, he is praised and respected. It is no doubt indicative of the exceptional professionalisation of modern American sociology that the one thoroughly Kuhnian account of the subject, Friedrichs' *A Sociology of Sociology*, is an account of it as it has recently appeared in the United States. Second, and related to this, there is the difficulty raised by Kuhn's weak suggestion and Bachelard's quite definite insistence that what is not social is not scientific. Bachelard indeed goes as far as to say that the pre-scientific is not only non-social but also non-rational, the result only of unconscious *r veries*. One could, of course, recast the history of social theorising in this way, but only by stipulation, and it would produce a most bizarre account, an account, indeed, that would itself be vitiated if it were not the product of communal delibera-

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tion and official sanction. Third, and most severely, there is in each the difficulty that the rational, whether also social or not, is partly but to that extent paradoxically explained by the non-rational.

This is indeed the most fundamental difficulty in any supposed 'sociology of knowledge'. If beliefs are externally caused, it seems impossible to see how they can also be rational. And if they are rational, it seems impossible to see how they can be externally caused. The solution of arguing that what is rational is also determined, because what is rational is a correct appreciation of the tendency of the external world itself, a world tending towards non-contradiction, universality and so reason, the solution suggested by Hegel and with different emphasis by Marx after him, is, as I suggest in chapters two and three, invalid. Without it, one is forced either to the view that beliefs are sentimental responses to external stimuli, a view that threatens to vanquish them (and itself) as rational, or to the view that rational autonomy exists in a world in which sociologists, above all, insist that it does not. One has to choose.

The choice is clear. If the history is to be rational, it must presume rationality in its subjects, and forswear sociology. It must simply ask what any past thinker thought he was right about, and what his reasons were for thinking he was. But these questions, although necessary, are not sufficient. For in order properly to understand why any thinker thought it important to be right about the things that he thought it important to be right about, and not something else, the historian has also to recover his intentions. This point was made in a general way by Collingwood, and has recently been made again by Dunn and secured by Skinner on the basis of the analytical philosophies of action developed from Austin in England in the 1950s and 1960s. The contention, simply put, is that political philosophies and by extension social theories are to be seen neither as disembodied abstractions nor as the epiphenomenal and implicitly mechanical consequences of social conditions, but instead, as actions. That is to say, they are to be seen as being in part actually constituted by the intentions within them. Thus, properly to understand what a social theorist was saying is not only to understand what he actually said, or his motives for saying it, but also what he thought he was saying in saying it. This is simple,

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powerful and attractive. Ideas are not detached from their authors and attached instead to social groups or to nothing at all, although it is clear that any author's intentions, in this sense, may be unintelligible without a prior understanding of the social conventions within which he expressed them. The intentions themselves are not prejudged. And the sheer empirical complexity of the provenance of ideas receives its proper due.

As I have said, this does not in any way solve the fundamental problem of the 'sociology of knowledge'. It simply but skilfully avoids it. The intentions and the conventions within which they are expressed have still to be explained, and unless this is done by invoking a potentially endless regress of other intentions and other conventions, any such explanation will still have to face the problem of accommodating apparently external causes. But unlike the other 'sociologies of knowledge' that I have described, this approach no more prejudices these causes than it does the intentions and conventions which they might explain. It ironically allows the most sensitive sociology of knowledge.

It also allows the historian to criticise. Unlike other approaches, of course, and most obviously unlike those which derive from Hegel's Objective Idealism, it does not require him to, and it does not generate any criteria by which he may do so except those of rationality itself, which it presupposes. But because I believe that it is difficult to understand the history of social theorising in western Europe and the United States since the eighteenth century without understanding the extent to which it has been a history of failure, I have taken advantage of this. The approach's final virtue is that it allows one to in a way which does not necessarily interfere with the history itself. My own most firm intention has been to write the history so that others may draw their own conclusions.

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Enlightenment and doubt

IN MEDIEVAL EUROPEAN THOUGHT, the epistemological authority was the word of God as revealed through the teachings of the Roman Church. There was in this period, it is true, a recognised distinction between natural and divine law, but although the first was held to be distinct from the second and accessible to reason, it was not generally considered to be independent of it. Divine law alone was considered capable of restoring to man that true knowledge lost in his Fall. Reason, therefore, could do more than lead towards, prepare the ground for, revelation.

The Renaissance modification of this epistemology was a radical one, but the radicalism was still one to which the Church could at least in principle accommodate itself, adapting yet maintaining its authority. In medieval thought nature was seen as the creation of God, a realm that was in part intelligible through the use of reason but only in part so intelligible, since a complete understanding of it entailed an understanding of God's purpose in having created it, and that was accessible only to revelation. But towards the Renaissance it came to be asserted, as Bruno put it, that 'it is more worthy for [God] to be the internal principle of motion, which is his own nature, his own appearance, his own soul than that as many entities as live in his bosom should have motion'. That is, there appeared a shift of emphasis, not always clear at the time but nevertheless, and especially in retrospect, fundamental, a shift from a view of God as creator of nature to a view of him as expressed in it. The corollary of this was a shift in the view of the importance of natural law and the power of reason. In the medieval view, reason was always and necessarily subservient to revelation, which alone could reveal God's purpose. But as God came to be seen as expressed in rather than as distinct from and anterior to nature, so the importance of reason grew. God was expressed in nature; nature was accessible through reason; God therefore was accessible through reason. Reason was, perhaps, sufficient.

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Once this conceptual transformation was accomplished, and despite the persistence of all kinds of variation inevitably disguised in a simple account such as this, it was accomplished long before that period now described as the Enlightenment, it is easy to see how pressure built up towards the state of intellectual affairs that does truly characterise that period. This pressure was the need to know nature better, and it produced a gradual shift from reliance on faith to a reliance on reason and experience. It was this pressure that the Church recognised, ironically, where for example Galileo did not. Galileo strove to reconcile his findings with a framework acceptable to the post-medieval Church and there seems no good theological reason why this should have been impossible or even difficult. Given sufficient will, no theologian would have had any problem in arguing that the Copernican system was any less marvellous an indication and vindication of God than the Ptolemaic, which Galileo's work helped to replace. The Church's hostility rested rather upon what one might call the politics of epistemology. It could see all too clearly that any claim it might hitherto make to a privileged access to the ultimate, divine final cause would be fatally weakened by an alternative institution, science, being able to reveal that divine purpose in nature by describing nature in what we would now call straightforwardly naturalistic ways. It could not tolerate that.

In general, then, it is not difficult to see how the distinctiveness of the European Enlightenment arose and in what it consisted. First, the long-established claims of reason as a means of knowing natural law were strengthened. And second, since it had always been clear that nature was not in itself a wholly metaphysical realm but was an at least superficially physical one, and since an investigation of nature was now of paramount importance, so more attention could and it was thought should be paid to the methods suitable to understanding physical phenomena. Hence the insistence on supplementing the much vindicated faculty of reason by experience and experiment.

If it is clear that nothing in the gradual intellectual evolution from the medieval period to the eighteenth century necessarily implied a challenge to religious faith, it has nevertheless become a convention in the past two hundred years to think of the eighteenth century as the period in which reason triumphed over faith and experience over intuition, a convention that

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perhaps owes much to the exaggerated reaction of the early nineteenth-century Romantics, a reaction to which I shall return. It is misleading, if not false. Certainly, there was a direct challenge to ecclesiastical authority, in social, political and moral matters as much as in intellectual ones. But this should not be confused, as the Church so long confused it, with faith. Indeed, and ironically, it is largely to differences of religious as well as political tradition within Europe that one can attribute the different courses the Enlightenment took there. These differences have proved crucial to the history of social thought since the eighteenth century.

The Enlightenment is ordinarily thought of as a French affair, with the English having accomplished much of their intellectual revolution with their political one in the seventeenth century, and other people, such as the Germans, remaining in a dogmatic slumber until the next century, when they took somewhat different paths, often in direct reaction to the French. This is an exaggeration. Certainly, two of the architects of the new intellectual radicalism in England, Newton and Locke, did much to inspire the French *philosophes*, and Hume in Scotland and Rousseau in France did much to arouse Kant and so give direction to the train of German Idealism at the end of the eighteenth century. But Hume's scepticism is sufficient evidence of the fact that Scots were not resting in that century, and Kant cannot be described as being in opposition to the ideals of the Enlightenment without grossly distorting his whole philosophy. Much of what followed in Germany was the product, at least to begin with, of a great enthusiasm for the ideals of the French, ideals that were at first thought to have been realised in the revolution of 1789. Nevertheless, there were striking differences between the three countries, and these require explanation.

France was much more rationalist than it was empiricist, in that the *philosophes* tended to rely more upon the deliverances of reason than the evidence of their senses. A corollary of this was that they were more inclined to intellectual systems. D'Alembert asserted in 1759 that 'the true system of the world has been recognised, developed and perfected' and eleven years later D'Holbach described the universe, 'that vast assemblage of everything that exists', as presenting us everywhere 'only with