

THE SOCIAL STUDIES AS SCIENCE

1. *J. S. Mill's 'Logic of the Moral Sciences'*

I TRIED to show in the last chapter how the view of philosophy presented in Chapter I leads to the discussion of the nature of human activities in society. I want next to consider some of the difficulties which arise if we try to base our understanding of societies on the methods of natural science. I start with John Stuart Mill for two reasons: first, because Mill states naively a position which underlies the pronouncements of a large proportion of contemporary social scientists, even if they do not always make it explicit; second, because some rather more sophisticated interpretations of the social studies as science, which I shall examine subsequently, can be best understood as attempts to remedy some of the more obvious defects in Mill's position. (Though I do not want to suggest that this represents the actual historical genesis of such ideas.)

Mill, like many of our own contemporaries, regarded the state of the 'moral sciences' as a 'blot on the face of science'. The way to remove this was to generalize the methods used in those subjects 'on which the results obtained have finally received the unanimous

assent of all who have attended the proof". (18: Book VI, Chapter I.) For this reason he regarded the philosophy of the social studies as just a branch of the philosophy of science. 'The methods of investigation applicable to moral and social science must have been already described, if I have succeeded in enumerating and characterizing those of science in general.' (*Ibid.*) This implies that, despite the title of Book VI of the *System of Logic*, Mill does not really believe that there is a 'logic of the moral sciences'. The logic is the same as that of any other science and all that has to be done is to elucidate certain difficulties arising in its application to the peculiar subject-matter studied in the moral sciences.

That is the task to which the main part of Mill's discussion is addressed. I want here to examine rather the validity of the thesis which his discussion takes for granted. To understand it we need to refer to Mill's conception of scientific investigation generally, which is based on Hume's ideas about the nature of causation. (See 12: Sections IV to VII; and 18: Book II.) To say that A is the cause of B is not to assert the existence of any intelligible (or mysterious) nexus between A and B, but to say that the temporal succession of A and B is an instance of a generalization to the effect that events like A are always found in our experience to be followed by events like B.

If scientific investigation consists in establishing causal sequences, then it seems to follow that we may have a scientific investigation of any subject-matter about which it is possible to establish generalizations. Indeed, Mill goes further: 'Any facts are fitted, in themselves, to be a subject of science, which follow

one another according to constant laws; although these laws may not have been discovered, nor even be discoverable by our existing resources'. (18: Book VI, Chapter III.) That is, there may be science wherever there are uniformities; and there may be uniformities even where we have not yet discovered them and are not in a position to discover them and formulate them in generalizations.

Mill cites the contemporary state of meteorology as an example: everybody knows that changes in atmospheric conditions are subject to regularities; they are therefore a proper subject for scientific study. This has not got very far owing to 'the difficulty of observing the facts on which the phenomena depend'. The theory of the tides ('Tidology') is in somewhat better shape in that scientists have discovered the phenomena on which the movements of the tides depend in general; but they are unable to predict exactly what will happen in particular circumstances owing to the complexity of local conditions in the context of which the gravitational effects of the moon operate. (*Ibid.*)

Mill supposes that the 'science of human nature' could at least be developed to the level of Tidology. Owing to the complexity of the variables we may be unable to do more than make statistical generalizations about the probable outcome of social situations. 'The agencies which determine human character are so numerous and diversified . . . that in the aggregate they are never in two cases exactly similar.' Nevertheless,

an approximate generalization is, in social inquiries, for most practical purposes equivalent to an exact one; that

which is only probable when asserted of individual human beings indiscriminately selected, being certain when affirmed of the character and collective conduct of masses. (*Ibid.*)

Just as the irregularity of the tides as between different places on the globe does not mean that there are no regular laws governing them, so in the case of human behaviour. Individual divergences are to be explained by the operation of laws on highly diversified individual situations. So broad statistical generalizations are not ultimately enough: they must be 'connected deductively with the laws of nature from which they result'. These ultimate laws of nature are the 'Laws of Mind' discussed in Chapter IV of the *Logic*; they differ from 'empirical laws' not in kind but in their much greater degree of generality and exactitude. Like all scientific laws they are statements of uniformities, namely 'uniformities of succession among states of mind'. Mill raises the question whether these should be resolved into uniformities of succession between physiological states and states of mind and concludes that, though this may one day be possible to a significant degree, it does not vitiate the possibility of establishing autonomous psychological laws which do not depend on physiology.

'Ethology, or the Science of the Development of Character' can be based on our knowledge of the Laws of Mind. (18: Book VI, Chapter IV.) This comprises the study of human mental development, which Mill conceives as resulting from the operation of the general Laws of Mind on the individual circumstances of particular human beings. Hence he regards Ethology as 'altogether deductive', as opposed to

Psychology which is observational and experimental.

The laws of the formation of character are . . . derivative laws, resulting from the general laws of mind, and are to be obtained by deducing them from those general laws by supposing any given set of circumstances, and then considering what, according to the laws of mind, will be the influence of those circumstances on the formation of character. (*Ibid.*)

Ethology is related to Psychology as is mechanics to theoretical physics; its principles are '*axiomata media*', on the one hand derived from the general Laws of Mind and on the other hand leading to the 'empirical laws resulting from simple observation'.

The discovery of these lowest-level empirical laws is the task of the historian. The social scientist aims to explain the empirical laws of history by showing how they follow, first from the *axiomata media* of Ethology, and ultimately from the general laws of Psychology. This leads Mill to his conception of the 'Inverse Deductive Method'. Historical circumstances are so exceedingly complex, owing to the cumulative effect of 'the influence exercised over each generation by the generations which preceded it' (18: Book VI, Chapter X), that nobody could hope to achieve a sufficiently detailed knowledge of any particular historical situation to predict its outcome. So, in dealing with large-scale historical developments, the social scientist must, for the most part, wait and see what happens, formulate the results of his observations in 'Empirical Laws of Society', and finally 'connect them with the laws of human nature, by deductions showing that such were the derivative laws naturally to be

expected as the consequences of those ultimate ones'. (*Ibid.*)

Karl Popper has indicated some of the misconceptions in this account of the social sciences. In particular he has criticized what he calls Mill's 'Psychologism': the doctrine that the development of one social situation out of another can ultimately be explained in terms of individual psychology. He has also shown the confusions involved in describing the findings of history as '*empirical laws of society*', rather than as statements of *trends*. (See 25: Chapter 14; and 26: Section 27.) Here I want to concentrate on some of the other elements in Mill's view; I hope thus to be able to show that Mill's conception of the social studies is open to much more radical objections even than those which Popper has brought forward.

2. *Differences in Degree and Differences in Kind*

Mill regards all explanations as fundamentally of the same logical structure; and this view is the foundation of his belief that there can be no fundamental logical difference between the principles according to which we explain natural changes and those according to which we explain social changes. It is a necessary consequence of this that the methodological issues concerning the moral sciences should be seen as *empirical*: an attitude involving a wait-and-see attitude to the question of what can be achieved by the social sciences and, incidentally, ruling the philosopher out of the picture.

But the issue is not an empirical one at all: it is

conceptual. It is not a question of what empirical research may show to be the case, but of what philosophical analysis reveals about *what it makes sense to say*. I want to show that the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences.

Both the rhetorical strength and the logical weakness of Mill's position revolve round the phrase 'just very much more complicated'. It is true, so the line of thought runs, that human beings react differently to their environment from other creatures; but the difference is just one of complexity. So the uniformities, though more difficult to discover in the case of humans, certainly exist; and the generalizations which express them are on precisely the same logical footing as any other generalizations.

Now though human reactions are very much more complex than those of other beings, they are not *just* very much more complex. For what is, from one point of view, a change in the degree of complexity is, from another point of view, a difference in kind: the concepts which we apply to the more complex behaviour are logically different from those we apply to the less complex. This is an instance of something like the Hegelian 'Law of the Transformation of Quantity into Quality' which I mentioned in connection with Ayer in the first Chapter. Unfortunately, Hegel's account of this, as well as Engels's gloss on Hegel, commits a mistake closely analogous to Mill's, in failing to distinguish physical changes from conceptual changes. They include, as instances of one and the same principle, the sudden qualitative change of

water into ice following on a series of uniform quantitative changes of temperature, and on the other hand the qualitative change from hirsuteness to baldness following on a series of uniform quantitative changes in the number of hairs. (See I: Chapter II, Section 7. For a detailed application of the principle to a particular sociological problem see 27, *passim*.)

By how many degrees does one need to reduce the temperature of a bucket of water for it to freeze?—The answer to that has to be settled experimentally. How many grains of wheat does one have to add together before one has a heap?—This cannot be settled by experiment because the criteria by which we distinguish a heap from a non-heap are vague in comparison with those by which we distinguish water from ice: there is no sharp dividing line. Neither, as Acton mentions, is there any sharp dividing line between what is and what is not alive: but that does not make the difference between life and non-life 'merely one of degree'. Acton says that 'the point at which we draw the line is one that we have to choose, not one that the facts press upon us in unmistakable fashion'. But though there may be a choice in borderline cases, there is not in others: it is not for me or anyone else to *decide* whether I, as I write these words, am alive or not.

The reaction of a cat which is seriously hurt is 'very much more complex' than that of a tree which is being chopped down. But is it really intelligible to say it is only a difference in degree? We say the cat 'writhe's' about. Suppose I describe his very complex movements in purely mechanical terms, using a set of space-time co-ordinates. This is, in a sense, a description of what

is going on as much as is the statement that the cat is writhing in pain. But the one statement could not be substituted for the other. The statement which includes the concept of writhing says something which no statement of the other sort, however detailed, could approximate to. The concept of writhing belongs to a quite different framework from that of the concept of movement in terms of space-time co-ordinates; and it is the former rather than the latter which is appropriate to the conception of the cat as an animate creature. Anyone who thought that a study of the mechanics of the movement of animate creatures would throw light on the concept of animate life would be the victim of a conceptual misunderstanding.

Similar considerations apply to my earlier comparison between the reactions of a dog who is taught a trick and those of a man who is taught a rule of language. Certainly the latter are very much more complex, but what is more important is the logical difference between the concepts which are applicable. Whereas the man learns to understand the rule the dog just learns to react in a certain way. The difference between these concepts *follows but cannot be explained in terms of* the difference in the complexity of the reactions. As indicated in the earlier discussion, the concept of understanding is rooted in a social context in which the dog does not participate as does the man.

Some social scientists have acknowledged the difference in concept between our currently accepted descriptions and explanations of natural and of social processes respectively, but have argued that the social scientist need not adhere to this non-scientific

conceptual framework; that he is at liberty to frame such concepts as are useful for the kind of investigation he is conducting. I shall consider some of the fallacies in this line of thought in the next chapter; but Mill does not follow it. He takes for granted the scientific legitimacy of describing human behaviour in terms which are current in everyday discourse. The Laws of Mind are high-level causal generalizations setting out invariable sequences between 'Thoughts, Emotions, Volitions, and Sensations'. (18: Book VI, Chapter IV.) And his argument against Libertarianism in Chapter II is couched in terms of such conventional categories as 'character and disposition', 'motives', 'purposes', 'efforts', and so on. I have next then to discuss the attempt to interpret explanations of behaviour in such terms as based on generalizations of the causal type.

3. *Motives and Causes*

It will not do simply to dismiss Mill as antediluvian, for his approach flourishes still at the present time, as can be seen by studying the discussion of motives in T. M. Newcomb's prominent textbook of social psychology. (19: Chapter II). Newcomb agrees with Mill in regarding explanations of actions in terms of the agent's motives as a species of causal explanation; but differs from him in regarding motives as physiological, rather than psychological, states. A motive is 'a state of the organism in which bodily energy is mobilized and selectively directed towards part of the environment'. Newcomb also speaks of 'drives': 'bodily states felt as restlessness, which initiate tendencies to

activity'. Clearly a mechanical model is at work here: it is as if the actions of a man were like the behaviour of a watch, where the energy contained in the tensed spring is transmitted *via* the mechanism in such a way as to bring about the regular revolution of the hands.

Why does Newcomb abandon Mill's caution about admitting Comte's claim that explanation in terms of motives should be reducible to physiological explanations? Is it that the once problematic physiological states have now been identified? Not at all for, as Newcomb says, 'nothing akin to a motive has ever been seen by a psychologist'. No, the identification of motives with 'states of the organism' is the action of a drowning man clutching at a straw. Newcomb thinks himself forced to this conclusion by the unacceptability of the only alternatives he can envisage: *viz.* that 'motives are merely figments of the psychologist's imagination' or else that the motive ascribed to a sequence of behaviour is simply a synonym for that behaviour itself.

He also imagines that there is compelling, though necessarily circumstantial, positive evidence. 'First, a behaviour sequence may show varying degrees of strength, or intensity, while its direction remains more or less constant.' 'The only way to account for such facts is to assume that a motive corresponds to an actual state of the organism.' Newcomb weights the scales heavily in his own favour by relying largely on examples which involve obviously physiological drives like hunger, thirst and sex; and by appealing mainly to experiments with *animals* (to whose behaviour the concept of a motive is not obviously appropriate), he ensures that only the physiological

aspects of those drives shall be taken into account. But would it be intelligent to try to explain how Romeo's love for Juliet enters into his behaviour in the same terms as we might want to apply to the rat whose sexual excitement makes him run across an electrically charged grid to reach his mate? Does not Shakespeare do this much better?

Moreover, unless and until the 'actual state of the organism' is actually identified and correlated with the appropriate mode of behaviour, this type of explanation is as vacuous as those which Newcomb rejects. And the facts which he adduces certainly do not constitute *evidence* for the desired conclusion; the most one can say is that if there were good independent reasons for regarding motives as bodily states, those facts would not be incompatible with such a view. This is particularly obvious in connection with the 'experimental evidence', to which Newcomb appeals, provided by Zeigarnik in 1927. In these experiments a set of people were each given a series of twenty tasks and were told that there was a strict (though unspecified) time-limit for each task. But each subject was in fact allowed to complete only half his allotted tasks, irrespective of the time he had taken, and was given to understand that his permitted time had expired. Subsequently it was found that the subjects were inclined to remember the nature of the uncompleted tasks more readily than the others and to manifest a desire to be allowed to finish them. Newcomb comments:

Such evidence suggests that motivation involves a mobilization of energy earmarked, as it were, for achieving a specified goal. The experimental data do not provide final

'proof' for such a theory, but they are consistent with it and are difficult to explain in any other way. (19: p. 117.)

Now this evidence only 'suggests' such a conclusion to someone who is already predisposed to believe it; and the necessity for any special explanation is not in fact obvious. The behaviour noted by Zeigarnik is perfectly intelligible in such terms as the following: that the subjects' interest had been aroused and they were irritated at not being allowed to finish something which they had started. If that sounds insufficiently scientific to anyone, he should ask himself just how much is added to our understanding by Newcomb's way of talking. There is in fact a very simple, but nonetheless cogent, argument against the physiological interpretation of motives. To discover the motives of a puzzling action *is* to increase our understanding of that action; that is what 'understanding' means as applied to human behaviour. But this is something we in fact discover without any significant knowledge about people's physiological states; therefore our accounts of their motives can have nothing to do with their physiological states. It does not follow, as Newcomb fears, that motive explanations are either mere tautologies or are an appeal to figments of the imagination. But before I try to give a positive account of what they do involve, there are some further misconceptions to be removed.

Mill, as we have seen, rejects the physiological account of motives, but he still wants to make motive explanations a species of causal explanation. The conception he wishes to advocate, though he is not very explicit, seems to be something like this.—A

motive is a specific mental occurrence (in a Cartesian sense of 'mental' implying that it belongs wholly to the realm of consciousness). A toothache, for instance, is mental in this sense, whereas the hole in the tooth which gives rise to the ache is physical. It makes sense to say that someone has a hole in his tooth, of which he is unaware, but not that that he has a toothache of which he is unaware: 'unfelt ache' is a self-contradictory expression. The issue between Mill and Newcomb can now be phrased as follows: whereas Newcomb wants to assimilate motives (toothaches) to states of the organism (holes in the teeth), Mill insists that these are different and argues that it has yet to be shown whether to every motive (toothache) there corresponds a specific kind of organic state (dental decay). But what we can do, Mill argues, is to study the causal relation between motives, considered as purely conscious events, and the actions to which they give rise. This involves careful observation of what specific mental occurrences are associated with what actions—just as we might discover that certain kinds of stoppage in a motor engine are associated with a blocked carburettor and certain others with a defective sparking plug.

Mill's account does fit moderately well certain kinds of fact which we can discover about ourselves. For instance, I might come to associate a certain kind of headache with an incipient attack of migraine; every time I experience that kind of headache I can then predict that, within an hour, I shall be lying in bed in great discomfort. But nobody would want to call my headache the *motive* for the migraine.—Neither, of course, should we as a matter of fact be justified in

calling the headache the *cause* of the migraine; but this raises general difficulties about the validity of Mill's account of scientific method which it would be out of place to discuss here.

4. *Motives, Dispositions and Reasons*

Gilbert Ryle argues, against the kind of account advocated by Mill, that to speak of a person's motives is not to speak of any events at all, either mental or physical, but is to refer to his general dispositions to act in the ways in question. 'To explain an act as done from a certain motive is not analogous to saying that the glass broke, because a stone hit it, but to the quite different type of statement that the glass broke, when the stone hit it, because the glass was brittle.' (29: p. 87.) There are a number of objections to this. For one thing, there seems to be a danger of reducing motive explanations to the sort of vacuity feared by Newcomb. (An analogous point is made by Peter Grech; See 10: p. 5.) Again, Ryle's account runs into difficulties where we assign a motive to an act which is quite at variance with the agent's previously experienced behaviour. There is no contradiction in saying that someone who never before manifested any signs of a jealous disposition has, on a given occasion, acted from jealousy; indeed, it is precisely when someone acts unexpectedly that the need for a motive explanation is particularly apparent.

But for my present purposes it is more important to notice that though Ryle's account is different from Mill's in many respects, it is not nearly different

enough. A dispositional, just as much as a causal, statement, is based on generalizations from what has been observed to happen. But a statement about an agent's motives is not like that: it is better understood as analogous to a setting out of the agent's *reasons* for acting thus. Suppose that *N*, a university lecturer, says that he is going to cancel his next week's lectures because he intends to travel to London: here we have a statement of intention for which a reason is given. Now *N* does not *infer* his intention of cancelling his lectures from his desire to go to London, as the imminent shattering of the glass might be inferred, either from the fact that someone had thrown a stone or from the brittleness of the glass. *N* does not offer his reason as *evidence* for the soundness of his prediction about his future behaviour. (Cf. Wittgenstein; 37: I, 629 ff.) Rather, he is *justifying* his intention. His statement is not of the form: 'Such and such causal factors are present, therefore this will result'; nor yet of the form: 'I have such and such a disposition, which will result in my doing this'; it is of the form: 'In view of such and such considerations this will be a reasonable thing to do'.

This takes me back to the argument of Chapter II, Section 2, which provides a way of correcting Ryle's account of motives. Ryle says that a statement about someone's motives is to be understood as a 'law-like proposition' describing the agent's propensity to act in certain kinds of way on certain kinds of occasion. (29: p. 89.) But the 'law-like proposition' in terms of which *N*'s reasons must be understood concerns not *N*'s dispositions but the accepted standards of reasonable behaviour current in his society.

The terms 'reason' and 'motive' are not synonymous. It would, for instance, be absurd to describe most imputations of motives as 'justifications': to impute a motive is more often to condemn than it is to justify. To say, for example, that *N* murdered his wife from jealousy is certainly not to say that he acted reasonably. But it is to say that his act was *intelligible* in terms of the modes of behaviour which are familiar in our society, and that it was governed by considerations appropriate to its context. These two aspects of the matter are interwoven: one can act 'from considerations' only where there are accepted standards of what is appropriate to appeal to. The behaviour of Chaucer's Troilus towards Cressida is intelligible only in the context of the conventions of courtly love. Understanding Troilus presupposes understanding those conventions, for it is from them that his acts derive their meaning.

I have noted how the relation between *N*'s intention and his reason for it differs from the relation between a prediction and the evidence offered in its support. But somebody who knows *N* and his circumstances well and who is familiar with the type of consideration which he is prone to regard as important, may on the basis of this knowledge predict how he is likely to behave. '*N* has a jealous temperament; if his emotions in that direction are aroused he is likely to become violent. I must be careful not to provoke him further.' Here I adduce *N*'s motives as part of the evidence for my prediction of his behaviour. But though this is possible, given that I already possess the concept of a motive, that concept is not in the first place learned as part of a technique for making predictions (unlike the

concept of a cause). Learning what a motive is belongs to learning the standards governing life in the society in which one lives; and that again belongs to the process of learning to live as a social being.

5. *The Investigation of Regularities*

A follower of Mill might concede that explanations of human behaviour must appeal not to causal generalizations about the individual's reaction to his environment but to our knowledge of the institutions and ways of life which give his acts their meaning. But he might argue that this does not damage the fundamentals of Mill's thesis, since understanding social institutions is still a matter of grasping empirical generalizations which are logically on a footing with those of natural science. For an institution is, after all, a certain kind of uniformity, and a uniformity can only be grasped in a generalization. I shall now examine this argument.

A regularity or uniformity is the constant recurrence of the same kind of event on the same kind of occasion; hence statements of uniformities presuppose judgements of identity. But this takes us right back to the argument of Chapter I, Section 8, according to which criteria of identity are necessarily relative to some rule; with the corollary that two events which count as qualitatively similar from the point of view of one rule would count as different from the point of view of another. So to investigate the type of regularity studied in a given kind of enquiry is to examine the nature of the rule according to which judgements of

Identity are made in that enquiry. Such judgements are intelligible only relatively to a given mode of human behaviour, governed by its own rules.¹ In a physical science the relevant rules are those governing the procedures of investigators in the science in question. For instance, someone with no understanding of the problems and procedures of nuclear physics would gain nothing from being present at an experiment like the Cockcroft-Walton bombardment of lithium by hydrogen; indeed even the description of what he saw in those terms would be unintelligible to him, since the term 'bombardment' does not carry the sense in the context of the nuclear physicists' activities that it carries elsewhere. To understand what was going on in this experiment he would have to learn the nature of what nuclear physicists do; and this would include learning the criteria according to which they make judgements of identity.

Those rules, like all others, rest on a social context of common activity. So to understand the activities of an individual scientific investigator we must take account of two sets of relations: first, his relation to the phenomena which he investigates; second, his relation to his fellow-scientists. Both of these are essential to the sense of saying that he is 'detecting regularities' or 'discovering uniformities'; but writers on scientific 'methodology' too often concentrate on

¹ Cf. Hume: *A Treatise of Human Nature*, Introduction—" 'Tis evident, that all the sciences have a relation, greater or less, to human nature; and that however wide any of them may seem to run from it, they still return back by one passage or another.' " Hume's remark is a further reminder of the close relation between the subject of this monograph and one of the most persistent and dominant motifs in the history of modern philosophy.

the first and overlook the importance of the second. That they must belong to different types is evident from the following considerations.—The phenomena being investigated present themselves to the scientist as an *object* of study; he observes them and notices certain facts about them. But to say of a man that he does this presupposes that he already has a mode of communication in the use of which rules are already being observed. For to notice something is to identify relevant characteristics, which means that the noticer must have some *concept* of such characteristics; this is possible only if he is able to use some symbol according to a rule which makes it refer to those characteristics. So we come back to his relation to his fellow-scientists, in which context alone he can be spoken of as following such a rule. Hence the relation between N and his fellows, in virtue of which we say that N is following the same rule as they, cannot be simply a relation of observation: it cannot consist in the fact that N has noticed how his fellows behave and has decided to take that as a norm for his own behaviour. For this would presuppose that we could give some account of the notion of 'noticing how his fellows behave' *apart from* the relation between N and his fellows which we are trying to specify; and that, as has been shown, is untrue. To quote Rush Rhees: 'We see that we understand one another, without noticing whether our reactions tally or not. *Because* we agree in our reactions, it is possible for me to tell you something, and it is possible for you to teach me something'. (28.)

In the course of his investigation the scientist applies and develops the concepts germane to his

particular field of study. This application and modification are 'influenced' both by the phenomena to which they are applied and also by the fellow-workers in *participation with* whom they are applied. But the two kinds of 'influence' are different. Whereas it is on the basis of his observation of the phenomena (in the course of his experiments) that he develops his concepts as he does, he is able to do this only in virtue of his participation in an established form of activity with his fellow-scientists. When I speak of 'participation' here I do not necessarily imply any direct physical conjunction or even any direct communication between fellow-participants. What is important is that they are all taking part in the same general kind of activity, which they have all *learned* in similar ways; that they are, therefore, *capable* of communicating with each other about what they are doing; that what any one of them is doing is in principle intelligible to the others.

6. *Understanding Social Institutions*

Mill's view is that understanding a social institution consists in observing regularities in the behaviour of its participants and expressing these regularities in the form of generalizations. Now if the position of the sociological investigator (in a broad sense) can be regarded as comparable, in its main logical outlines, with that of the natural scientist, the following must be the case. The concepts and criteria according to which the sociologist judges that, in two situations, the same thing has happened, or the same action

performed, must be understood *in relation to the rules governing sociological investigation.* But here we run against a difficulty; for whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientist's investigation itself, here *what the sociologist is studying*, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity.

An example may make this clearer. Consider the parable of the Pharisee and the Publican (*Luke*, 18, 9). Was the Pharisee who said 'God, I thank Thee that I am not as other men are' doing the same kind of thing as the Publican who prayed 'God be merciful unto me a sinner'? To answer this one would have to start by considering what is involved in the idea of prayer; and that is a *religious* question. In other words, the appropriate criteria for deciding whether the actions of these two men were of the same kind or not belong to religion itself. Thus the sociologist of religion will be confronted with an answer to the question: Do these two acts belong to the same kind of activity?; and this answer is given according to criteria which are not taken from sociology, but from religion itself.

But if the judgements of identity and hence the generalizations of the sociologist of religion rest on criteria taken from religion, then his relation to the performers of religious activity cannot be just that of observer to observed. It must rather be analogous to the participation of the natural scientist with his

fellow-workers in the activities of scientific investigation. Putting the point generally, even if it is legitimate to speak of one's understanding of a mode of social activity as consisting in a knowledge of regularities, the nature of this knowledge must be very different from the nature of knowledge of physical regularities. So it is quite mistaken in principle to compare the activity of a student of a form of social behaviour with that of, say, an engineer studying the workings of a machine; and one does not advance matters by saying, with Mill, that the machine in question is of course immensely more complicated than any physical machine. If we are going to compare the social student to an engineer, we shall do better to compare him to an apprentice engineer who is studying what engineering---that is, the activity of engineering---is all about. His understanding of social phenomena is more like the engineer's understanding of his colleagues' activities than it is like the engineer's understanding of the mechanical systems which he studies.

This point is reflected in such common-sense considerations as the following: that a historian or sociologist of religion must himself have some religious feeling if he is to make sense of the religious movement he is studying and understand the considerations which govern the lives of its participants. A historian of art must have some aesthetic sense if he is to understand the problems confronting the artists of his period; and without this he will have left out of his account precisely what would have made it a history of *art*, as opposed to a rather puzzling external account of certain motions which certain people have been perceived to go through.

I do not wish to maintain that we must stop at the unreflective kind of understanding of which I gave as an instance the engineer's understanding of the activities of his colleagues. But I do want to say that any more reflective understanding must necessarily presuppose, if it is to count as genuine understanding, at all, the participant's unreflective understanding. And this in itself makes it misleading to compare it with the natural scientist's understanding of his scientific data. Similarly, although the reflective student of society, or of a particular mode of social life, may find it necessary to use concepts which are not taken from the forms of activity which are investigating, but which are taken rather from the context of his own investigation, still these technical concepts of his will imply a previous understanding of those other concepts which belong to the activities under investigation.

For example, liquidity preference is a technical concept of economics: it is not generally used by business men in the conduct of their affairs but by the economist who wishes to *explain* the nature and consequences of certain kinds of business behaviour. But it is logically tied to concepts which do enter into business activity, for its use by the economist presupposes his understanding of what it is to conduct a business, which in turn involves an understanding of such business concepts as money, profit, cost, risk, etc. It is only the relation between his account and these concepts which makes it an account of economic activity as opposed, say, to a piece of theology.

Again, a psychoanalyst may explain a patient's neurotic behaviour in terms of factors unknown to

the patient and of concepts which would be unintelligible to him. Let us suppose that the psychoanalyst's explanation refers to events in the patient's early childhood. Well, the description of those events will presuppose an understanding of the concepts in terms of which family life, for example, is carried on in our society; for these will have entered, however rudimentarily, into the relations between the child and his family. A psychoanalyst who wished to give an account of the aetiology of neuroses amongst, say, the Trobriand Islanders, could not just apply without further reflection the concepts developed by Freud for situations arising in our own society. He would have first to investigate such things as the idea of fatherhood amongst the islanders and take into account any relevant aspects in which their idea differed from that current in his own society. And it is almost inevitable that such an investigation would lead to some modification in the psychological theory appropriate for explaining neurotic behaviour in this new situation.

These considerations also provide some justification for the sort of historical scepticism which that under-estimated philosopher, R. G. Collingwood, expresses in *The Idea of History*. (6: *passim*.) Although they need not be brought to the foreground where one is dealing with situations in one's own society or in societies with whose life one is reasonably familiar, the practical implications become pressing where the object of study is a society which is culturally remote from that of the investigator. This accounts for the weight which the Idealists attached to concepts like 'empathy' and 'historical imagination' (which is not to deny that these concepts give rise to difficulties of their own). It

is also connected with another characteristic doctrine of theirs: that the understanding of a human society is closely connected with the activities of the philosopher. I led up to that doctrine in the first two chapters and shall return to it in the last two.

7. *Prediction in the Social Studies*

In my discussion of Oakeshott in the last chapter I noticed the importance of the fact that voluntary behaviour is behaviour to which there is an alternative. Since understanding something involves understanding its contradictory, someone who, with understanding, performs *X* must be capable of envisaging the possibility of doing not-*X*. This is not an empirical statement but a remark about what is involved in the concept of doing something with understanding. Consider now an observer, *O*, of *N*'s behaviour. If *O* wants to predict how *N* is going to act he must familiarize himself with the concepts in terms of which *N* is viewing the situation; having done this he may, from his knowledge of *N*'s character, be able to predict with great confidence what decision *N* is going to take. But the notions which *O* uses to make his prediction are nonetheless compatible with *N*'s taking a different decision from that predicted for him. If this happens it does not necessarily follow that *O* has made a mistake in his calculations; for the whole point about a decision is that a given set of 'calculations' may lead to any one of a set of different outcomes. This is quite different from predictions in the natural sciences, where a falsified prediction always implies some sort of

mistake on the part of the predictor: false or inadequate data, faulty calculation, or defective theory.

The following may make that clearer. To understand the nature of the decision confronting *N*, *O* must be aware of the rules which provide the criteria specifying for *N* the relevant features of his situation. If one knows the rule which someone is following one can, in a large number of cases, predict what he will do in given circumstances. For instance, if *O* knows that *N* is following the rule: 'Start with 0 and add 2 till you reach 1,000', he can predict that, having written down 104, *N* will next write 106. But sometimes even if *O* knows with certainty the rule which *N* is following, he cannot predict with any certainty what *N* will do: where, namely, the question arises of *what is involved* in following that rule, e.g. in circumstances markedly different from any in which it has previously been applied. The rule here does not specify any determinate outcome to the situation, though it does limit the range of possible alternatives: it is made determinate for the future by the choice of one of these alternatives and the rejection of the others—until such time as it again becomes necessary to interpret the rule in the light of yet new conditions.

This may throw some light on what is involved in the idea of a developing historical tradition. As I remarked earlier, Mill thought of historical trends as analogous to scientific laws and Popper wished to modify that conception by pointing out that the statement of a trend, unlike that of a true law, involves a reference to a set of specific initial conditions. I now want to make a further modification: even given a specific set of initial conditions, one will still not be

able to predict any determinate outcome to a historical trend because the continuation or breaking off of that trend involves human decisions which are not determined by their antecedent conditions in the context of which the sense of calling them 'decisions' lies.

Two words of caution are necessary in connection with my last remark. I am not denying that it is sometimes possible to predict decisions; only that their relation to the evidence on which they are based is unlike that characteristic of scientific predictions. And I am not falling into the trap of saying that historical trends are consciously willed and intended by their participants; the point is that such trends are in part the *outcome* of intentions and decisions of their participants.

The development of a historical tradition may involve deliberation, argument, the canvassing of rival interpretations, followed perhaps by the adoption of some agreed compromise or the springing up of rival schools. Consider, for instance, the relation between the music of Haydn, Mozart and Beethoven; or the rival schools of political thought which all claim, with some show of reason, to be based on the Marxist tradition. Think of the interplay between orthodoxy and heresy in the development of religion; or of the way in which the game of football was revolutionized by the Rugby boy who picked up the ball and ran. It would certainly not have been possible to predict that revolution from knowledge of the preceding state of the game any more than it would have been possible to predict the philosophy of Hume from the philosophies of his predecessors. It may help here to recall Humphrey Lyttleton's rejoinder to

someone who asked him where Jazz was going: 'If I knew where Jazz was going I'd be there already'.

Maurice Cranston makes essentially the same point when he notices that to predict the writing of a piece of poetry or the making of a new invention would involve writing the poem or making the invention oneself. And if one has already done this oneself then it is impossible to predict that someone else will make up that poem or discover that invention. 'He could not predict it because he could not say it was going to happen before it happened.' (8: p. 166.)

It would be a mistake, though tempting, to regard this as a piece of trivial logic-chopping. One appears to be attempting an impossible task of *a priori* legislation against a purely empirical possibility. What in fact one is showing, however, is that the central concepts which belong to our understanding of social life are incompatible with concepts central to the activity of scientific prediction. When we speak of the possibility of scientific prediction of social developments of this sort, we literally do not understand what we are saying. We cannot understand it, because it has no sense.

CHAPTER FOUR

THE MIND AND SOCIETY

1. *Pareto: Logical and Non-Logical Conduct*

WHAT I tried to show in Chapter III was that the conceptions according to which we normally think of social events are logically incompatible with the concepts belonging to scientific explanation. An important part of the argument was that the former conceptions enter into social life itself and not merely into the observer's description of it. But there is a powerful stream of thought which maintains that the ideas of participants must be discounted as more likely than not to be misguided and confusing. To this stream belongs, for instance, the quotation from Durkheim at the end of Chapter I. I propose now to examine the attempt made by Vilfredo Pareto, in *The Mind and Society*, a title in which Pareto's translator has most admirably caught his main preoccupation, to show empirically that the ideas which people have, in behaving as they do, influence the nature and outcome of their behaviour far less fundamentally than is usually thought; and that, therefore, the sociologist must develop his own concepts *de novo* and pay as little attention as possible to the ideas of participants. My examination is designed to bring out two main

points: (1) that Pareto mistakes what is essentially a philosophical issue for an empirical, scientific, one; (2) that the conclusion of his argument is in fact false.

Pareto starts by considering what is involved in a scientific approach to sociology. His answer is, roughly, that it consists in using only concepts which have a strictly empirical reference. In subjecting one's theories always rigorously to the control of observation and experiment, and in ensuring that one's inferences always follow strict logic. This he calls the 'logico-experimental' approach. The sociologist's data are the actions of human beings living together, and from these Pareto singles out, as requiring special attention, that behaviour which expresses an intellectual content.

(Current in any given group of people are a number of propositions, descriptive, preceptive or otherwise. . . . Such propositions, combined by logical or pseudo-logical nexuses and amplified with factual narrations of various sorts, constitute theories, theologics, cosmogonies, systems of metaphysics, and so on. Viewed from the outside without regard to any intrinsic merit with which they may be credited by faith, all such propositions and theories are experimental facts, and as experimental facts we are here obliged to consider and examine them. (23: Section 7.)

We are here concerned with Pareto's views on how the propositions and theories which people embrace are related to their other behaviour. How, for instance, are the propositions of Christian theology related to the practice of Christian rites? Now Pareto rightly points out that this question is ambiguous. It may

mean: Do these theories really constitute good reasons for the actions they purport to justify? Or it may mean: Is people's behaviour really governed by the ideas they embrace in the way they would claim, or would they go on behaving like that even if they ceased to embrace such ideas? Pareto conceives it to be the function of a scientific 'logico-experimental' sociology to answer both these questions; for this purpose he introduces two important distinctions: (i) that between *logical and non-logical* action; (ii) that between *residues and derivations*.

① is designed to throw light on the question how far the theories people embrace really constitute good reasons for the actions they perform.

There are actions that use means appropriate to ends and which logically link means with ends. There are other actions in which those traits are missing. The two sorts of conduct are very different according as they are considered under their objective or their subjective aspect. From the subjective point of view nearly all human actions belong to the logical class. In the eyes of the Greek manner sacrifices to Poseidon and rowing with oars were equally logical means of navigation. . . . Suppose we apply the term *logical actions* to actions that logically conjoin means to ends not only from the standpoint of the subject performing them, but from the standpoint of other persons who have a more extensive knowledge - in other words, to actions that are logical both subjectively and objectively in the sense just explained. Other actions we shall call *non-logical* (by no means the same as 'illogical'). (23: Section 150.)

A logical action then is one that fulfils the following conditions: (a) it is thought of by the agent as having

a result and is performed by him for the purpose of achieving that result; (b) it actually does tend to have the result which the agent envisages; (c) the agent has (what Pareto would regard as) good (ie. 'logico-experimental') grounds for his belief; (d) the end sought must be one that is empirically identifiable. The diversity of these criteria means that an action can also be non-logical in a variety of different ways, of which the following are among the most important. It may be non-logical because the agent does not think to achieve any end by it at all; this seems to correspond to what Max Weber meant by actions that are *zweckrational* as opposed to *wertrational*. But Pareto thinks these are few and far between because, he says, 'human beings have a very conspicuous tendency to paint a varnish of logic over their conduct' (Section 154). (It is interesting and important that he is unable to conceive of any way in which an action may have even the appearance of being logical except in terms of the category of means and ends.) Again, an action may be non-logical because, although the agent performs it for the sake of an end, it either achieves some quite different end or none at all. This may be because, as Pareto puts it, the end envisaged is not in fact a real one at all but is 'imaginary', because 'located outside the field of observation and experience' (Section 151): he several times mentions the salvation of the soul as an example of an 'imaginary' end of this sort. Or it may be because, although the end envisaged is a perfectly real one, it is not gained in the way the agent thinks it is; to this class Pareto assigns both operations in magic (Section 160) and also 'certain measures (for example, wage-cutting) of business men

(entrepreneurs) working under conditions of free competition' (Section 159).

Now the inclusion of all these different types of action (and many more besides) within a single category is obviously going to give rise to serious difficulties. I should like here to concentrate on one such difficulty: that of making any clear distinction between 'non-logical' and 'illogical' conduct. In the above quotation from Section 150 of *The Mind and Society* we saw that Pareto maintained that these are 'by no means the same'; and he is making the same point when he writes, much later, that 'a mistake in engineering is not a non-logical action' (Section 327). Nevertheless, Pareto holds that the mistake of an entrepreneur under free competition, who thinks that by cutting his employees' wages he will increase his own profits is a non-logical action. How does a mistake in engineering differ relevantly from that of the entrepreneur (whose idea, Pareto says, may no longer be a mistake in conditions of monopoly)? And is the entrepreneur's mistake really comparable at all to the performance of a magical rite? Surely it ought rather to be compared to a *mistake* in a magical rite. The entrepreneur's mistake is a particular act (of which there may, nevertheless, be a great many similar examples) within the *category* of business behaviour; but magical operations themselves constitute a category of behaviour. Magic, in a society in which it occurs, plays a peculiar role of its own and is conducted according to considerations of its own. The same is true of business activity; but it is not true of the kind of *misguided* business activity to which Pareto refers, for that can only be understood by reference to the

aims and nature of business activity in general. On the other hand, to try to understand magic by reference to the aims and nature of scientific activity, as Pareto does, will necessarily be to misunderstand it.

The distinction between a general category of action—a mode of social life—and a particular sort of act falling within such a category, is of central importance to the distinction between non-logical and illogical behaviour. An illogical act presumably involves a mistake in logic: but to call something non-logical should be to deny that criteria of logic apply to it at all. That is, it does not make sense to say of non-logical conduct that it is either logical or illogical, just as it does not make sense to say of something non-spatial (such as virtue) that it is either big or small. But Pareto does not follow through the implications of this. For instance, he tries to use the term 'non-logical' in a logically pejorative sense, which is like concluding from the fact that virtue is not big that it must be small. A large part of the trouble here arises from the fact that he has not seen the point around which the main argument of this monograph revolves: that criteria of logic are not a direct gift of God, but 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 is another; and each has criteria of intelligibility peculiar to itself. So within science or religion actions can be logical or illogical: in science, for example, it would be illogical to refuse to be bound by the results of a properly carried out experiment; in religion it would be illogical to suppose that one

could pit one's own strength against God's; and so on. 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. (This is, of course, an over-simplification, in that it does not allow for the overlapping character of different modes of social life. Somebody might, for instance, have religious reasons for devoting his life to science. But I do not think that this affects the substance of what I want to say, though it would make its precise expression in detail more complicated.) Now what Pareto tries to say is that science itself is a form of logical behaviour (in fact *the form par excellence* of such behaviour), whereas religion is non-logical (in a logically pejorative sense). And this, as I have tried to show, is not permissible.

There is a still deeper source for Pareto's failure to distinguish adequately between 'non-logical' and 'illogical'; it is connected with his belief that the appropriate way to produce a completely impartial, uncommitted theory of the workings of human societies is to be governed solely by 'logico-experimental' criteria, which he conceives on the analogy of what he takes to be the practice of the natural sciences. From this point of view he is clearly quite justified in evaluating rival theories about social existence (i.e. alternative sociological theories) by reference to those criteria. But he is constantly trying to do more than this: to evaluate by reference to the same criteria the ideas and theories which belong to the subject-matter he is studying. But this involves him in a fundamental confusion: that of taking sides in just the sort of way which the application of the logico-experimental

technique was supposed to preclude. The embarrassment in which he is thus placed illustrates what I wanted to emphasize in maintaining that the type of problem with which he is here concerned belongs more properly to philosophy than it does to science. This has to do with the peculiar sense in which philosophy is *uncommitted* enquiry. I noted in the first chapter how philosophy is concerned with elucidating and comparing the ways in which the world is made intelligible in different intellectual disciplines; and how this leads on to the elucidation and comparison of different forms of life. The uncommittedness of philosophy comes out here in the fact that it is equally concerned to elucidate its own account of things; the concern of philosophy with its own being is thus not an unhealthy Narcissistic aberration, but an essential part of what it is trying to do. In performing this task the philosopher will in particular be alert to deflate the pretensions of any form of enquiry to enshrine the essence of intelligibility as such, to possess the key to reality. For connected with the realization that intelligibility takes many and varied forms is the realization that reality has no key. But Pareto is committing just this mistake: his way of discussing the distinction between logical and non-logical conduct involves setting up scientific intelligibility (or rather, his own misconception of it) as the norm for intelligibility in general; he is claiming that science possesses the key to reality.

Science, unlike philosophy, is wrapped up in its own way of making things intelligible to the exclusion of all others. Or rather it applies its criteria, *unself-consciously*, for to be self-conscious about such matters

is to be philosophical. This non-philosophical unself-consciousness is for the most part right and proper in the investigation of nature (except at such critical times as that gone through by Einstein prior to the formulation of the Special Theory of Relativity); but it is disastrous in the investigation of a human society, whose very nature is to consist in different and competing ways of life, each offering a different account of the intelligibility of things. To take an uncommitted view of such competing conceptions is peculiarly the task of philosophy; it is not its business to award prizes to science, religion, or anything else. It is not its business to advocate any *Weltanschauung* (in the way Pareto offers, inconsistently, a pseudo-scientific *Weltanschauung*). In Wittgenstein's words, 'Philosophy leaves everything as it was'.

In this connection it is worth while to recall Collingwood's allegation that some accounts of magical practices in primitive societies offered by 'scientific' anthropologists often mask 'a half-conscious conspiracy to bring into ridicule and contempt civilizations different from our own'. (7: Book I, Chapter IV.) A classic example of this corrupt use of 'scientific objectivity' is to be found in R. S. Lynd's *Knowledge for What?* (15: p. 121, footnote 7.) The philosophical confusions in Lynd's argument should be evident to anyone who has followed the argument of this monograph.

2. Pareto: Residues and Derivations

To develop this point further I now turn to the second of Pareto's distinctions: between *residues* and

derivations. This distinction is supposed to perform two functions. In the (first) place it is supposed to provide *recurring features* in our observation of human societies, which will be a suitable subject for scientific generalization. Pareto argues that if one looks at a wide variety of different societies at different historical periods, one is struck by the fact that whereas certain kinds of conduct occur again and again with very little variation, other kinds are very unstable, changing constantly with time and differing considerably from one society to another. He calls the constant, recurring element 'residues'; they are what remains when the changeable features are left out of account. The variable elements are 'derivations', a term which refers to a fact about such kinds of conduct which Pareto claims to have discovered empirically: namely, that the main occupants of this category are the theories in terms of which people try to explain why they behave as they do. The derivation 'represents the work of the mind in accounting for [the residue]. That is why [it] is much more variable, as reflecting the play of the imagination'. (23: Section 850.) Because the derivations are so unstable and variable in comparison with the residues, Pareto urges, we must accept that the ideas and theories which people embrace have little real influence on the way they otherwise behave; embracing the theories cannot be a valid explanation of why people act in the given way, for that behaviour goes on even after the theories have been abandoned. The concept of a derivation obviously offers many points of comparison with, for example, the Marxian concept of an 'ideology' and the Freudian concept of a 'rationalization'. The

point I should like to emphasize here, however, is that it is only by way of this conceptual distinction that Pareto succeeds in finding common features of different societies of a sort which appear suitable as a subject for scientific generalization. That is, the claim that there are sociological uniformities goes hand in hand with the claim that human intelligence is much overrated as a real influence on social events.

I shall now quote an example of Pareto's detailed application of the distinction.

Christians have the custom of baptism. If one knew the Christian procedure only one would not know whether and how it could be analysed. Moreover, we have an explanation of it: we are told that the rite of baptism is celebrated in order to remove original sin. That still is not enough. If we had no other facts of the same class to go by, we should find it difficult to isolate the elements in the complex phenomenon of baptism. But we do have other facts of that type. The pagans too had lustral water, and they used it for purposes of purification. If we stopped at that we might associate the idea of water with the fact of purification. But other cases of baptism show that the use of water is not a constant element. Blood may be used for purification, and other substances as well. Nor is that all; there are numbers of rites that effect the same result. . . . The given case, therefore, is made up of that constant element, *a*, and a variable element, *b*, the latter comprising the means that are used for restoring the individual's integrity and the reasonings by which the efficacy of the means is presumably explained. The human being has a vague feeling that water somehow cleanses moral as well as material pollution. However, he does not, as a rule, justify his conduct in that manner. The explanation would

be far too simple. So he goes looking for something more complicated, more pretentious, and readily finds what he is looking for. (23: Section 863.)

Now there are well-known philosophical difficulties which arise from the attempt to reject as nugatory whole classes of reasonings as opposed to particular appeals to that kind of reasoning within an accepted class. Consider, for instance, the often discussed difficulties involved in casting *general* doubt on the reliability of the senses, or of memory. But Pareto would no doubt maintain that his thesis is saved from this kind of vacuity by the mass of empirical evidence on which it rests. However, his thesis concerning the relative variability of derivations and constancy of residues is not, as he thinks, a straightforward report of the results of observation; it involves a conceptual misinterpretation of those results. The constant element, *a*, and the variable element, *b*, are not distinguished by observation, but only as the result of an (illegitimate) abstraction. In the example quoted of the purification residues, the unvarying element is not just a straightforward set of physical movements for it may take a multitude of different physical forms (as Pareto himself is at pains to point out). The mere act of washing one's hands would not be an instance of it; it would become one only if performed with *symbolic* intent, as a sign of moral or religious purification. This point is so important that I will illustrate it with another example, the 'sex residues'. Pareto does not, as might be expected, mean to refer to the common factor of simple biological sexual intercourse which is found amidst all the multifarious social customs and moral ideas connected with sexual

relations at different times and in different societies. He explicitly rules this out. To qualify as a residue a form of behaviour must have a quasi-intellectual, or symbolic content. 'Mere sexual appetite, though powerfully active in the human race, is no concern of ours here. . . We are interested in it only in so far as it influences theories, modes of thinking'. (23: Section 1,324.) For example, one dominant residue which Pareto discusses is the ascetic attitude to sexual relations: the idea that they are to be avoided as something evil or at least morally debilitating. But this constant factor, as in the previous example, is not something that Pareto has *observed* separately from the highly various moral and theological systems of ideas in terms of which sexual ascetism is justified or explained in different societies. It is something that he has analysed out of those systems of ideas by means of a conceptual analysis.

But ideas cannot be torn out of their context in that way; the relation between idea and context is an *internal* one. The idea gets its sense from the role it plays in the system. It is nonsensical to take several systems of ideas, find an element in each which can be expressed in the same verbal form, and then claim to have discovered an idea which is common to all the systems. This would be like observing that both the Aristotelian and Galilean systems of mechanics use a notion of force, and concluding that they therefore make use of the same notion. One can imagine the howl of rage which Pareto would send up at the philistinism of such a proceeding; but he is guilty of exactly the same kind of philistinism when, for instance, he compares the social relation between 'an

American millionaire and a plain American' to that between an Indian of high caste and one of low caste. (See Section 1,044.) And this sort of comparison is essential to his whole method of procedure.

The same point may be expressed as follows. Two things may be called 'the same' or 'different' only with reference to a set of criteria which lay down what is to be regarded as a relevant difference. When the 'things' in question are purely physical the criteria appealed to will of course be those of the observer. But when one is dealing with intellectual (or, indeed, any kind of social) 'things', that is not so. For their *being* intellectual or social, as opposed to physical, in character depends entirely on their belonging in a certain way to a system of ideas or mode of living. It is only by reference to the criteria governing that system of ideas or mode of life that they have any existence as intellectual or social events. It follows that if the sociological investigator wants to regard them as social events (as, *ex hypothesi*, he must), he has to take seriously the criteria which are applied for distinguishing 'different' kinds of actions and identifying the 'same' kinds of actions within the way of life he is studying. It is not open to him arbitrarily to impose his own standards from without. In so far as he does so, the events he is studying lose altogether their character as *social* events. A Christian would strenuously deny that the baptism rites of his faith were really the same in character as the acts of a pagan sprinkling lustral water or letting sacrificial blood. Pareto, in maintaining the contrary, is inadvertently removing from his subject-matter precisely that which

gives them sociological interest: namely their internal connection with a way of living.

Miss G. E. M. Ancombe has remarked, in an unpublished paper, how there are certain activities—she mentions arithmetic as an example—which, unlike other activities, such as acrobatics, cannot be understood by an observer unless he himself possesses the ability to perform the activities in question. She notes that any description of activities like arithmetic which is not based on arithmetical (or whatever) capacities is bound to seem pointless and arbitrary, and also compulsive in the sense that the steps no longer appear as meaningful choices. This is precisely the impression of social activities which is given by Pareto's account of them as residues; but the impression is not a well-founded one, it is an optical illusion based on a conceptual misunderstanding.

This shows, I think, that the whole presupposition of Pareto's procedure is absurd: namely that it is possible to treat propositions and theories as 'experimental facts' on a par with any other kind of such fact. (See 28: Section 7.) It is a presupposition which is certainly not peculiar to him: it is contained, for instance, in Emile Durkheim's first rule of sociological method: 'to consider social facts as things'. Pareto's statement, and the others like it, are absurd because they involve a contradiction: in so far as a set of phenomena is being looked at from the outside, 'as experimental facts', it cannot at the same time be described as constituting a 'theory' or set of 'propositions'. In a sense Pareto has not carried his empiricism far enough. For what the sociological observer has presented to his senses is not at all people holding

certain theories, believing in certain propositions, but people making certain movements and sounds. Indeed, even describing them as 'people' really goes too far, which may explain the popularity of the sociological and social psychological jargon word 'organism': but organisms, as opposed to people, do not believe propositions or embrace theories. To describe what is observed by the sociologist in terms of notions like 'proposition' and 'theory' is already to have taken the decision to apply a set of concepts incompatible with the 'external' 'experimental' point of view. To refuse to describe what is observed in such terms, on the other hand, involves not treating it as having social significance. It follows that the understanding of society cannot be observational and experimental in one widely accepted sense.

What I am saying needs qualification. I do not mean, of course, that it is impossible to take as a datum that a certain person, or group of people, holds a certain belief—say that the earth is flat—without subscribing to it oneself. And this is all Pareto thinks he is doing; but actually he is doing more than this. He is not just speaking of particular beliefs *within* a given mode of discourse, but of whole modes of discourse. What he misses is that a mode of discourse has to be *understood* before anyone can speak of theories and propositions within it which could constitute data for him. He does not really consider the fundamental problem of what it is to understand a mode of discourse. In so far as he thinks anything about it he regards it as simply a matter of establishing generalizations on the basis of observation; a view which was disposed of in Chapter III.

There is, unfortunately, no space available to discuss further examples of attempts, like Pareto's, to eliminate human ideas and intelligence from the sociologist's account of social life. But readers may find it instructive to re-read Durkheim's *Suicide* in the light of what I have been saying. It is particularly important to notice the connection between Durkheim's conclusion—that conscious deliberations may be treated as 'purely formal, with no object but confirmation of a resolve previously formed for reasons unknown to consciousness', and his initial decision to define the word 'suicide' for the purposes of his study in a sense different from that which it bore within the societies which he was studying. (9.)

3. Max Weber: *Verstehen and Causal Explanation*

It is Max Weber who has said most about the peculiar sense which the word 'understand' bears when applied to modes of social life. I have already referred to his account of meaningful behaviour and propose in the next two sections to say something about his conception of sociological understanding (*Verstehen*). (See 33: Chapter 1.) The first issue on which I mean to concentrate is Weber's account of the relation between acquiring an 'interpretative understanding' (*deutend verstehen*) of the meaning (*Sinn*) of a piece of behaviour and providing a causal explanation (*kausal erklaren*) of what brought the behaviour in question about and what its consequences are.

Now Weber never gives a clear account of the

logical character of interpretative understanding. He speaks of it much of the time as if it were simply a psychological technique: a matter of putting oneself in the other fellow's position. This has led many writers to allege that Weber confuses what is simply a technique for framing hypotheses with the logical character of the evidence for such hypotheses. Thus Popper argues that although we may use our knowledge of our own mental processes in order to frame hypotheses about the similar processes of other people, 'these hypotheses must be tested, they must be submitted to the method of selection by elimination. (By their intuition, some people are prevented from even imagining that anybody can possibly dislike chocolate).' (26: Section 29.)

Nevertheless, however applicable such criticisms may be to Weber's vulgarizers, they cannot justly be used against his own views, for he is very insistent that mere 'intuition' is not enough and must be tested by careful observation. However, what I think can be said against Weber is that he gives a wrong account of the process of checking the validity of suggested sociological interpretations. But the correction of Weber takes us farther away from, rather than closer to, the account which Popper, Ginsberg, and the many who think like them, would like to substitute.

Weber says:

Every interpretation aims at self-evidence or immediate plausibility (*Evidenz*). But an interpretation which makes the meaning of a piece of behaviour as self-evidently obvious as you like cannot claim *just* on that account to be the causally *valid* interpretation as well. In itself it is

nothing more than a particularly plausible hypothesis. (33: Chapter I.)

He goes on to say that the appropriate way to verify such an hypothesis is to establish statistical laws based on observation of what happens. In this way he arrives at the conception of a sociological law as 'a statistical regularity which corresponds to an intelligible intended meaning'.

Weber is clearly right in pointing out that the obvious interpretation need not be the right one. R. S. Lynd's interpretation of West Indian voodoo magic as 'a system of imputedly true and reliable causal sequences' is a case in point (15: p. 121); and there is a plethora of similar examples in Frazer's *The Golden Bough*. But I want to question Weber's implied suggestion that Verstehen is something which is logically incomplete and needs supplementing by a different method altogether, namely the collection of statistics. Against this, I want to insist that if a proffered interpretation is wrong, statistics, though they may suggest that that is so, are not the decisive and ultimate court of appeal for the validity of sociological interpretations in the way Weber suggests. What is then needed is a better interpretation, not something different in kind. The compatibility of an interpretation with the statistics does not prove its validity. Someone who interprets a tribe's magical rites as a form of misplaced scientific activity will not be corrected by statistics about what members of that tribe are likely to do on various kinds of occasion (though this might form part of the argument); what is ultimately required is a philosophical argument like, e.g., Collingwood's in *The Principles of Art*. (6:

Book I, Chapter IV.) For a mistaken interpretation of a form of social activity is closely akin to the type of mistake dealt with in philosophy.

Wittgenstein says somewhere that when we get into philosophical difficulties over the use of some of the concepts of our language, we are like savages confronted with something from an alien culture. I am simply indicating a corollary of this: that sociologists who misinterpret an alien culture are like philosophers getting into difficulties over the use of their own concepts. There will be differences of course. The philosopher's difficulty is usually with something with which he is perfectly familiar but which he is for the moment failing to see in its proper perspective. The sociologist's difficulty will often be over something with which he is not at all familiar; he may have no suitable perspective to apply. This may sometimes make his task more difficult than the philosopher's, and it may also sometimes make it easier. But the analogy between their problems should be plain.

Some of Wittgenstein's procedures in his philosophical elucidations reinforce this point. He is prone to draw our attention to certain features of our own concepts by comparing them with those of an imaginary society, in which our own familiar ways of thinking are subtly distorted. For instance, he asks us to suppose that such a society sold wood in the following way: They 'piled the timber in heaps of arbitrary, varying height and then sold it at a price proportionate to the area covered by the piles. And what if they even justified this with the words: "Of course, if you buy more timber, you must pay more"?'?

(88: Chapter I, p. 142-151.) The important question for us is: in what circumstances could one say that one had *understood* this sort of behaviour? As I have indicated, Weber often speaks as if the ultimate test were our ability to formulate statistical laws which would enable us to *predict* with fair accuracy what people would be likely to do in given circumstances. In line with this is his attempt to define a 'social role' in terms of the probability (*Chance*) of actions of a certain sort being performed in given circumstances. But with Wittgenstein's example we might well be able to make predictions of great accuracy in this way and still not be able to claim any real understanding of what those people were doing. The difference is precisely analogous to that between being able to formulate statistical laws about the likely occurrences of words in a language and being able to understand what was being said by someone who spoke the language. The latter can never be reduced to the former; a man who understands Chinese is not a man who has a firm grasp of the statistical probabilities for the occurrence of the various words in the Chinese language. Indeed, he could have that without knowing that he was dealing with a language at all; and anyway, the knowledge that he was dealing with a language is not itself something that could be formulated statistically. 'Understanding' in situations like this is grasping the point or meaning of what is being done or said. This is a notion far removed from the world of statistics and causal laws; it is closer to the realm of discourse and to the internal relations that link the parts of a realm of discourse. The notion of *meaning* should be carefully distinguished from that

of *function*, in its quasi-causal sense, the use of which in social anthropology and sociology I shall not explore further here.

4. *Max Weber: Meaningful Action and Social Action*

I can best bring out the implications of this by considering another aspect of Weber's view: his distinction between behaviour which is merely meaningful and that which is both meaningful and social. Now it is evident that any such distinction is incompatible with the argument of Chapter II of this book: all meaningful behaviour must be social, since it can be meaningful only if governed by rules, and rules presuppose a social setting. Weber clearly recognizes the importance of this issue for sociology even though he comes down on what I must regard as the wrong side. What is interesting is that in so doing he at the same time begins to write of social situations in a way which is quite incompatible with what he has said about *Verstehen*; this is just what one would expect in so far as *Verstehen* implies *Sinn* and *Sinn*, as I have argued, implies socially established rules. I am thinking here of the important paper: *R. Stammers "Ueberwindung" der materialistischen Geschichtsauffassung* (34), where he connects together the following pair of assertions: first, that there is no logical difficulty in supposing a man to be capable of following rules of conduct in complete abstraction from any sort of social context; second, that there is no logical difference between the technique of manipulating natural objects (e.g. machinery) in order to achieve

one's ends and that of 'manipulating' human beings as he suggests; does the owner of a factory his employees. He says: 'that in the one case "events of consciousness" enter into the causal chain and in the other case not, makes "logically" not the slightest difference'; thus committing the mistake of supposing that 'events of consciousness' just happen to differ empirically from other kinds of event. He does not realize that the whole notion of an 'event' carries a different sense here, implying as it does a context of humanly followed rules which cannot be combined with a context of causal laws in this way without creating logical difficulties. Weber thus fails in his attempt to infer that the kind of 'law' which the sociologist may formulate to account for the behaviour of human beings is logically no different from a 'law' in natural science.

In trying to describe the situation he is using as an example in such a way as to support his point of view, Weber ceases to use the notions that would be appropriate to an interpretative understanding of the situation. Instead of speaking of the workers in his factory being paid and spending money, he speaks of their being handed pieces of metal, handing those pieces of metal to other people and receiving other objects from them; he does not speak of policemen protecting the workers' property, but of 'people with helmets' coming and giving back the workers the pieces of metal which other people have taken from them; and so on. In short, he adopts the external point of view and forgets to take account of the 'subjectively intended sense' of the behaviour he is talking about: and this, I want to say, is a natural result of his

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attempt to divorce the *social relations* linking those workers from the *ideas* which their actions embody: ideas such as those of 'money', 'property', 'police', 'buying and selling', and so on. Their relations to each other exist only through those ideas and similarly those ideas exist only in their relations to each other.

I am not denying that it may sometimes be useful to adopt devices like Weber's 'externalization' of his description of this situation. It may serve the purpose of drawing the reader's attention to aspects of the situation which are so obvious and familiar that he would otherwise miss them, in which case it is comparable to Wittgenstein's use of imaginary outlandish examples, to which I have already referred. Again, it may be compared with the *Verfremdungseffekt* which Berthold Brecht aimed at in his theatrical productions, or to Caradog Evans' use of outlandishly literal translations from the Welsh in his sinisterly satirical stories about West Wales.¹ The effect of all these devices is to shake the reader or spectator out of the complacent myopia which over-familiarity may induce. What is dangerous is that the user of these devices should come to think of *his* way of looking at things as somehow more real than the usual way. One suspects that Brecht may sometimes have adopted this God-like attitude (as would be consistent with his Marxism); it is certainly involved in Pareto's treatment of 'residues'; and although it is an attitude which is on the whole very uncharacteristic of Weber, it nevertheless follows very naturally from his methodological account of the way in which social relations and

¹ This last example was suggested to me by conversations with my colleague, Mr. D. L. Sims.

human ideas are related and from any attempt to compare sociological theories with those of natural science. The only legitimate use of such a *Verfremdungseffekt* is to draw attention to the familiar and obvious, not to show that it is dispensable from our understanding.

Moreover, if this mistake in Weber's account is corrected, it becomes much easier to defend his conception of *Verstehen* from a persistently reiterated criticism. Morris Ginsberg, for instance, writes:

It appears to be a basic assumption of *verstehende Soziologie* and *verstehende Psychologie* that what we know within our minds is somehow more intelligible than what is outwardly observed. But this is to confuse the familiar with the intelligible. There is no inner sense establishing connexions between inner facts by direct intuition. Such connexions are in fact empirical generalizations, of no greater validity than the similar generalizations relating to outward facts. (11: p. 155.)

It must be said very firmly here that the case for saying that the understanding of society is logically different from the understanding of nature does not rest on the hypothesis of an 'inner sense' (a notion trenchantly criticized by Peter Geach.—10: Section 24.) In fact it follows from my argument in Chapter II that the concepts in terms of which we understand our *own* mental processes and behaviour have to be learned, and must, therefore, be *socially* established, just as much as the concepts in terms of which we come to understand the behaviour of other people. Thus Ginsberg's remark that the disgust induced by certain foods in someone who is subject to a taboo 'is

not directly intelligible to anyone brought up in a different tradition', so far from being a valid criticism of the sort of view which I have tried to present of *Verstehen*, follows immediately from that view. I have already dealt, in Chapter III, with the idea that the connections embodied in our concepts of human behaviour are just the result of empirical generalizations.

CHAPTER FIVE

CONCEPTS AND ACTIONS

1. *The Internality of Social Relations*

TO illustrate what is meant by saying that the social relations between men and the ideas which men's actions embody are really the same thing considered from different points of view, I want now to consider the general nature of what happens when the ideas current in a society change: when new ideas come into the language and old ideas go out of it. In speaking of 'new ideas' I shall make a distinction. Imagine a biochemist making certain observations and experiments as a result of which he discovers a new germ which is responsible for a certain disease. In one sense we might say that the name he gives to this new germ expresses a new idea, but I prefer to say in this context that he has made a discovery within the existing framework of ideas. I am assuming that the germ theory of disease is already well established in the scientific language he speaks. Now compare with this discovery the impact made by the first formula-
tion of that theory, the first introduction of the concept of a germ into the language of medicine. This was a much more radically new departure, involving not merely a new factual discovery within an existing way of looking at things, but a completely new way of

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looking at the whole problem of the causation of diseases, the adoption of new diagnostic techniques, the asking of new kinds of question about illnesses, and so on. In short it involved the adoption of new ways of doing things by people involved, in one way or another, in medical practice. An account of the way in which social relations in the medical profession had been influenced by this new concept would include an account of what that concept was. Conversely, the concept itself is unintelligible, apart from its relation to medical practice. A doctor who (i) claimed to accept the germ theory of disease, (ii) claimed to aim at reducing the incidence of disease, and (iii) completely ignored the necessity for isolating infectious patients, would be behaving in a self-contradictory and unintelligible manner.

Again, imagine a society which has no concept of proper names, as we know them. People are known by general descriptive phrases, say, or by numbers. This would carry with it a great many other differences from our own social life as well. The whole structure of personal relationships would be affected. Consider the importance of numbers in prison or military life. Imagine how different it would be to fall in love with a girl known only by a number rather than by a name; and what the effect of that might be, for instance, on the poetry of love. The development of the use of proper names in such a society would certainly count as the introduction of a new idea, whereas the mere introduction of a *particular* new proper name, within the existing framework, would not.

I have wanted to show by these examples that a new way of talking sufficiently important to rank as a new

idea implies a new set of social relationships. Similarly with the dying out of a way of speaking. Take the notion of friendship: we read, in Penelope Hall's book, *The Social Services of Modern England* (Routledge), that it is the duty of a social worker to establish a relationship of friendship with her clients; but that she must never forget that her first duty is to the policy of the agency by which she is employed. Now that is a debasement of the notion of friendship as it has been understood, which has excluded this sort of divided loyalty, not to say double-dealing. To the extent to which the old idea gives way to this new one social relationships are impoverished (or, if anyone objects to the interpolation of personal moral attitudes, at least they are *changed*). It will not do, either, to say that the mere change in the meaning of a word need not prevent people from having the relations to each other they want to have; for this is to overlook the fact that our language and our social relations are just two different sides of the same coin. To give an account of the meaning of a word is to describe how it is used; and to describe how it is used is to describe the social intercourse into which it enters.

If social relations between men exist only in and through their ideas, then, since the relations between ideas are internal relations, social relations must be a species of internal relation, too. This brings me into conflict with a widely accepted principle of Hume's: 'There is no object, which implies the existence of any other if we consider these objects in themselves, and never look beyond the ideas which we form of them.' There is no doubt that Hume intended this to apply to human actions and social life as well as to the

phenomena of nature. Now to start with, Hume's principle is not unqualifiedly true even of our knowledge of natural phenomena. If I hear a sound and recognize it as a clap of thunder, I already commit myself to believing in the occurrence of a number of other events—e.g. electrical discharges in the atmosphere—even in calling what I have heard 'thunder'. That is, from 'the idea which I have formed' of what I heard I *can* legitimately infer 'the existence of other objects'. If I subsequently find that there was no electrical storm in the vicinity at the time I heard the sound I shall have to retract my claim that what I heard was thunder. To use a phrase of Gilbert Ryle's, the word 'thunder' is theory-impregnated; statements affirming the occurrence of thunder have logical connections with statements affirming the occurrence of other events. To say this, of course, is not to reintroduce any mysterious causal nexus *in rebus*, of a sort to which Hume could legitimately object. It is simply to point out that Hume overlooked the fact that 'the idea we form of an object' does not just consist of elements drawn from our observation of that object in isolation, but includes the idea of connections between it and other objects. (And one could scarcely form a conception of a language in which this was not so.)

Consider now a very simple paradigm case of a relation between actions in a human society: that between an act of command and an act of obedience to that command. A sergeant calls 'Eyes right!' and his men all turn their eyes to the right. Now, in describing the men's act in terms of the notion of obedience to a command, one is of course committing oneself to saying that a command has been issued. So

far the situation looks precisely parallel to the relation between thunder and electrical storms. But now one needs to draw a distinction. An event's character as an act of obedience is *intrinsic* to it in a way which is not true of an event's character as a clap of thunder; and this is in general true of human acts as opposed to natural events. In the case of the latter, although human beings can think of the occurrences in question only in terms of the concepts they do in fact have of them, yet the events themselves have an existence independent of those concepts. There existed electrical storms and thunder long before there were human beings to form concepts of them or establish that there was any connection between them. But it does not make sense to suppose that human beings might have been issuing commands and obeying them before they came to form the concept of command and obedience. For their performance of such acts is itself the chief manifestation of their possession of those concepts. An act of obedience itself contains, as an essential element, a recognition of what went before as an order. But it would of course be senseless to suppose that a clap of thunder contained any recognition of what went before as an electrical storm; it is our recognition of the sound, rather than the sound itself, which contains that recognition of what went before.

Part of the opposition one feels to the idea that men can be related to each other through their actions in at all the same kind of way as propositions can be related to each other is probably due to an inadequate conception of what logical relations between propositions themselves are. One is inclined to think of the laws of logic as forming a given rigid structure to

which men try, with greater or less (but never complete) success, to make what they say in their actual linguistic and social intercourse conform. One thinks of propositions as something ethereal, which just because of their ethereal, non-physical nature, can fit together more tightly than can be conceived in the case of anything so grossly material as flesh-and-blood men and their actions. In a sense one is right in this; for to treat of logical relations in a formal systematic way is to think at a very high level of abstraction, at which all the anomalies, imperfections and crudities which characterize men's actual intercourse with each other in society have been removed. But like any abstraction not recognized as such, this can be misleading. It may make one forget that it is only from their roots in this actual flesh-and-blood intercourse that those formal systems draw such life as they have; for the whole idea of a logical relation is only possible by virtue of the sort of agreement between men and their actions which is discussed by Wittgenstein in the *Philosophical Investigations*. Collingwood's remark on formal grammar is apposite: 'I likened the grammarian to a butcher; but if so, he is a butcher of a curious kind. Travellers say that certain African peoples will cut a steak from a living animal and cook it for dinner, the animal being not much the worse. This may serve to amend the original comparison'. (7; p. 259.) It will seem less strange that social relations should be like logical relations between propositions once it is seen that logical relations between propositions themselves depend on social relations between men.

What I have been saying conflicts, of course, with

Karl Popper's 'postulate of methodological individualism' and appears to commit the sin of what he calls 'methodological essentialism'. Popper maintains that the theories of the social sciences apply to theoretical constructions or models which are formulated by the investigator in order to explain certain experiences, a method which he explicitly compares to the construction of theoretical models in the natural sciences.

This use of models explains and at the same time destroys the claims of methodological essentialism. . . . It explains them, for the model is of an abstract or theoretical character, and we are liable to believe that we see it, either within or behind the changing observable events, as a kind of observable ghost or essence. And it destroys them because our task is to analyze our sociological models carefully in descriptive or nominalist terms, viz. *in terms of individuals*, their attitudes, expectations, relations, etc. — a postulate which may be called 'methodological individualism'. (26; Section 29.)

Popper's statement that social institutions are just explanatory models introduced by the social scientist for his own purposes is palpably untrue. The ways of thinking embodied in institutions govern the way the members of the societies studied by the social scientist behave. The idea of war, for instance, which is one of Popper's examples, was not simply invented by people who wanted to explain what happens when societies come into armed conflict. It is an idea which provides the criteria of what is appropriate in the behaviour of members of the conflicting societies. Because my country is at war there are certain things which I must and certain things which I must not do. My behaviour is governed, one could say, by my

concept of myself as a member of a belligerent country. The concept of war belongs *essentially* to my behaviour. But the concept of gravity does not belong essentially to the behaviour of a falling apple in the same way: it belongs rather to the physicist's *explanation* of the apple's behaviour. To recognize this has, *pace* Popper, nothing to do with a belief in ghosts behind the phenomena. Further, it is impossible to go far in specifying the attitudes, expectations and relations of individuals without referring to concepts which enter into those attitudes, etc., and the meaning of which certainly cannot be explained in terms of the actions of any individual persons. (Cf. Maurice Mandelbaum: 17.)

2. *Discursive and Non-Discursive 'Ideas'*

In the course of this argument I have linked the assertion that social relations are internal with the ideas, suggesting that social interaction can more profitably be compared to the exchange of ideas in a conversation than to the interaction of forces in a physical system. This may seem to put me in danger of over-intellectualizing social life, especially since the examples I have so far discussed have all been examples of behaviour which expresses *discursive* ideas, that is, ideas which also have a straightforward linguistic expression. It is because the use of language is so intimately, so inseparably, bound up with the other, non-linguistic, activities which men perform, that it is possible to speak of their non-linguistic behaviour also as expressing discursive ideas. A part

from the examples of this which I have already given in other connections, one needs only to recall the enormous extent to which the learning of any characteristically human activity normally involves talking as well: in connection, e.g., with discussions of alternative ways of doing things, the inculcation of standards of good work, the giving of reasons, and so on. But there is no sharp break between behaviour which expresses discursive ideas and that which does not; and that which does not is sufficiently like that which does to make it necessary to regard it as analogous to the other. So, even where it would be unnatural to say that a given kind of social relation expresses any ideas of a discursive nature, still it is closer to that general category than it is to that of the interaction of physical forces.

Collingwood provides a striking illustration of this in his discussion of the analogy between language and dress. (7: p. 244.) Again, consider the following scene from the film *Shane*. A lone horseman arrives at the isolated homestead of a small farmer on the American prairies who is suffering from the depredations of the rising class of big cattle-owners. Although they hardly exchange a word, a bond of sympathy springs up between the stranger and the homesteader. The stranger silently joins the other in uprooting, with great effort, the stump of a tree in the yard; in pausing for breath, they happen to catch each other's eye and smile shyly at each other. Now any explicit account that one tried to give of the kind of understanding that had sprung up between these two, and which was expressed in that glance, would no doubt be very complicated and inadequate. We understand

it, however, as we may understand the meaning of a pregnant pause (consider what it is that makes a pause *pregnant*), or as we may understand the meaning of a gesture that completes a statement. 'There is a story that Buddha once, at the climax of a philosophical discussion . . . took a flower in his hand, and looked at it; one of his disciples smiled, and the master said to him, "You have understood me".' (7: p. 243.) And what I want to insist on is that, just as in a conversation the point of a remark (or of a pause) depends on its internal relation to what has gone before, so in the scene from the film the interchange of glances derives its full meaning from its internal relation to the situation in which it occurs: the loneliness, the threat of danger, the sharing of a common life in difficult circumstances, the satisfaction in physical effort, and so on.

It may be thought that there are certain kinds of social relation, particularly important for sociology and history, of which the foregoing considerations are not true: as for instance wars in which the issue between the combatants is not even remotely of an intellectual nature (as one might say, e.g., that the crusades were), but purely a struggle for physical survival as in a war between hunger migrants and the possessors of the land on which they are encroaching.¹ But even here, although the issue is in a sense a purely material one, the form which the struggle takes will still involve internal relations in a sense which will not apply to, say, a fight between two wild animals over a piece of meat. For the belligerents are *societies*

¹ This example was suggested to me by a discussion with my colleague, Professor J. C. Rees, as indeed was the realization for the necessity for this whole section.

in which much goes on besides eating, seeking shelter and reproducing; in which life is carried on in terms of symbolic ideas which express certain attitudes as between man and man. These symbolic relationships, incidentally, will affect the character even of those basic 'biological' activities: one does not throw much light on the particular form which the latter may take in a given society by speaking of them in Malinowski's neo-Marxist terminology as performing the 'function' of providing for the satisfaction of the basic biological needs. Now of course, 'out-group attitudes' between the members of my hypothetical warring societies will not be the same as 'in-group attitudes' (if I may be forgiven the momentary lapse into the jargon of social psychology). Nevertheless, the fact that the enemies are *men*, with their own ideas and institutions, and with whom it would be possible to communicate, will affect the attitudes of members of the other society to them—even if its only effect is to make them the more ferocious. Human war, like all other human activities, is governed by conventions; and where one is dealing with conventions, one is dealing with internal relations.

3. *The Social Sciences and History*

This view of the matter may make possible a new appreciation of Collingwood's conception of all human history as the history of thought. That is no doubt an exaggeration and the notion that the task of the historian is to re-think the thoughts of the historical participants is to some extent an intellectualistic distortion. But Collingwood is right if he is taken to

mean that the way to understand events in human history, even those which cannot naturally be represented as conflicts between or developments of discursive ideas, is more closely analogous to the way in which we understand expressions of ideas than it is to the way we understand physical processes.

There is a certain respect, indeed, in which Collingwood pays insufficient attention to the manner in which a way of thinking and the historical situation to which it belongs form one indivisible whole. He says that the aim of the historian is to think the very same thoughts as were once thought, just as they were thought at the historical moment in question. (6: Part V.) But though extinct ways of thinking may, in a sense, be recaptured by the historian, the way in which the historian thinks them will be coloured by the fact that he has had to employ historiographical methods to recapture them. The medieval knight did not have to use those methods in order to view his lady in terms of the notions of courtly love: he just thought of her in those terms. Historical research may enable me to achieve some understanding of what was involved in this way of thinking, but that will not make it open to me to think of *my* lady in those terms. I should always be conscious that this was an anachronism, which means, of course, that I should not be thinking of her in just the same terms as did the knight of his lady. And naturally, it is even more impossible for me to think of *his* lady as he did.

Nevertheless, Collingwood's view is nearer the truth than is that most favoured in empiricist methodologies of the social sciences, which runs somewhat as follows—on the one side we have human history which is a

kind of repository of data. The historian unearth[s] these data and presents them to his more theoretically minded colleagues who then produce scientific generalizations and theories establishing connections between one kind of social situation and another. These theories can then be applied to history itself in order to enhance our understanding of the ways in which its episodes are mutually connected. I have tried to show, particularly in connection with Pareto, how this involves minimizing the importance of ideas in human history, since ideas and theories are constantly developing and changing, and since each system of ideas, its component elements being interrelated internally, has to be understood in and for itself, the combined result of which is to make systems of ideas a very unsuitable subject for broad generalizations. I have also tried to show that social relations really exist only in and through the ideas which are current in society; or alternatively, that social relations fall into the same logical category as do relations between ideas. It follows that social relations must be an equally unsuitable subject for generalizations and theories of the scientific sort to be formulated about them. Historical explanation is not the application of generalizations and theories to particular instances: it is the tracing of internal relations. It is like applying one's knowledge of a language in order to understand a conversation rather than like applying one's knowledge of the laws of mechanics to understand the workings of a watch. Non-linguistic behaviour, for example, has an 'idiom' in the same kind of way as has a language. In the same kind of way as it can be difficult to recapture the idiom of Greek thought in a

translation into modern English of a Platonic dialogue, so it can be misleading to think of the behaviour of people in remote societies in terms of the demeanour to which we are accustomed in our own society. Think of the uneasy feeling one often has about the authenticity of 'racy' historical evocations like those in some of Robert Graves's novels: this has nothing to do with doubts about a writer's accuracy in matters of external detail.

The relation between sociological theories and historical narrative is less like the relation between scientific laws and the reports of experiments or observations than it is like that between theories or logic and arguments in particular languages. Consider for instance the explanation of a chemical reaction in terms of a theory about molecular structure and valency: here the theory *establishes* a connection between what happened at one moment when the two chemicals were brought together and what happened at a subsequent moment. It is only *in terms of the theory* that one can speak of the events being thus 'connected' (as opposed to a simple spatio-temporal connection); the only way to grasp the connection is to learn the theory. But the application of a logical theory to a particular piece of reasoning is not like that. One does not have to know the theory in order to appreciate the connection between the steps of the argument; on the contrary, it is only in so far as one can already grasp logical connections between particular statements in particular languages that one is even in a position to understand what the logical theory is all about. (This is implied by the argument of Lewis Carroll, which I referred to earlier.) Whereas in

natural science it is your theoretical knowledge which enables you to explain occurrences you have not previously met, a knowledge of logical theory on the other hand will not enable you to understand a piece of reasoning in an unknown language; you will have to learn that language, and that in itself *may* suffice to enable you to grasp the connections between the various parts of arguments in that language.

Consider now an example from sociology. Georg Simmel writes:

The degeneration of a difference in convictions into hatred and fight occurs only when there were essential, original similarities between the parties. The (sociologically very significant) 'respect for the enemy' is usually absent where the hostility has arisen on the basis of previous solidarity. And where enough similarities continue to make confusions and blurred outlines possible, points of difference need an emphasis not justified by the issue but only by that danger of confusion. This was involved, for instance, in the case of Catholicism in Berne... Roman Catholicism does not have to fear any threat to its identity from external contact with a church so different as the Reformed Church, but quite from something as closely akin as Old-Catholicism. (31: Chapter I.)

Here I want to say that it is not *through* Simmel's generalization that one understands the relationship he is pointing to between Roman and Old Catholicism: one understands that only to the extent that one understands the two religious systems themselves and their historical relations. The 'sociological law' may be helpful in calling one's attention to features of historical situations which one might otherwise have overlooked and in suggesting useful analogies. Here

for instance one may be led to compare Simmel's example with the relations between the Russian Communist Party and, on the one hand, the British Labour Party and, on the other, the British Conservatives. But no historical situation can be understood simply by 'applying' such laws, as one applies laws to particular occurrences in natural science. Indeed, it is only in so far as one has an *independent* historical grasp of situations like this one that one is able to understand what the law amounts to at all. That is not like having to know the kind of experiment on which a scientific theory is based before one can understand the theory, for there it makes no sense to speak of understanding the connections between the parts of the experiment except in terms of the scientific theory. But one could understand very well the nature of the relations between Roman Catholicism and Old Catholicism without ever having heard of Simmel's theory, or anything like it.

4. *Concluding Remark*

I have made no attempt, in this book, to consider the undoubted differences which exist between particular kinds of social study, such as sociology, political theory, economics, and so on. I have wanted rather to bring out certain features of the notion of a social study as such. I do not think that individual methodological differences, important as they may be within their own context, can affect the broad outlines of what I have tried to say. For this belongs to philosophy rather than to what is commonly understood by the term 'methodology'.

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