ASPECTS OF MODERN SOCIOLOGY

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INTRODUCTION

The relationship between philosophy and what we now refer to as the social sciences has a long history bearing some resemblance to the parable of the prodigal son. Like petulant adolescents, the social sciences, having been born and nurtured within the familial fold of philosophy, reject their parentage, squander their inheritance, only to return for refuge and succour when the 'going gets tough'. Since developing as relatively autonomous disciplines, the social sciences have tended to re-examine, and seek support from, their philosophical foundations only during periods of crisis; when tried and hitherto trusted methods no longer seem to justify the faith originally invested in them, when researchers lose confidence in the significance of their findings, and when obvious and taken-for-granted principles no longer seem quite so clear and obvious. It is in such periods that warnings about the 'coming crisis' go out, or pleas for a re-examination of basic principles are voiced. Such periods – and, for many, the social sciences seem to consist almost entirely of these lacunae – force scholars to look again at fundamentals and especially the philosophical bases of their disciplines.

Although philosophical issues perhaps become more salient in periods of intellectual crisis, this is not to say that philosophical matters are relevant only at such times. Indeed, as far as sociology is concerned, the founding trinity of Marx, Weber and Durkheim spent a considerable amount of intellectual effort, the result of which still massively affect styles of sociological thinking, in establishing and refining the philosophical bases of their more substantive enquiries. Though Weber, for one, was less than enthusiastic about methodological disputations, regarding them as a 'pestilence', much of his early writing was devoted to taking to methodological task some of the scholars of his day.[1] For them, and this is perhaps more typical of the European traditions of social science than it is of the British and the American, philosophical questions had to be settled in advance of empirical enquiries.

What, then, is the relationship between philosophy and the social sciences? Why do the prodigals return when times are bad? What does philosophy offer that the social sciences, seemingly,

cannot provide out of their own resources? Obviously, these questions, however they may be phrased, are beyond simple answer and are still unclear. And nor, it is necessary to add, need there be only *one* answer to this question. Nonetheless, it is necessary to sketch out some of the issues involved before discussing some facets of the relationship between philosophy and the social sciences in more detail.

The relationship between philosophy and the social sciences involves historical as well as logical and conceptual dimensions. Indeed, historically speaking, it is only relatively recently that the relationship has come under scrutiny in ways that make it articulable as an issue. A long period of intellectual development is presupposed and it is only at the end of this that the very idea of a social science becomes conceivable. Plato, for obvious example, spoke of society, of the relationship between the collective and its individual members, of stratification, of ways of designing and reconstructing society so that it better exemplified certain values, and so on, but had different aims and spoke under different auspices, than did Marx, Weber and Durkheim and later social theorists. Between Plato and the major theorists of the late nineteenth and early twentieth centuries have been the major successes of the natural sciences that have had a portentous influence on the ways in which we now think about and study human social life. We can no longer think about society, about human behaviour, as if the natural sciences had never existed. This is true not only of sociology, of course, but of economics, psychology, political science, anthropology, and even history. Indeed, most of these disciplines – the exception being history, but even this was not immune from the ensuing debates – owe their origins to the desire to create *sciences* of human behaviour. This is not to say that these disciplines have slavishly followed the method of the natural sciences; far from it. It is simply to point out that they have the natural sciences as an inevitable feature of their intellectual background and one to which they feel it necessary to respond either by rejection of the natural science model or by embracing it. Neutrality is not, seemingly, an option.

The question of whether the study of social life could be like the scientific study of nature was the outcome of a long philosophical debate; a debate that is of continuing importance. But, then, we might ask, what is it about philosophy that gives it this seemingly vital role in human intellectual affairs? Is this simply a contingent fact of our intellectual history, or is there something distinctive about philosophy itself which gives it this authoritative place?

THE NATURE OF PHILOSOPHY

There have been many definitions of philosophy and as many different philosophical styles as definitions; and, from the point of view of securing a definition of philosophy, matters are made worse by the fact that there are special difficulties about defining philosophy that we shall not be in a position to understand until we examine philosophical problems about definition in general.[2] This is not untypical of the way in which philosophical questions start to assume an interminably circular quality by depending upon so many other matters before we can even begin to see what an answer to what looked like an inoffensive and straightforward question could be like. 'What is reality?', 'Are there other minds?' rarely get answers of the form, 'Reality is such-and-such' or 'Yes, there are other minds – sometimes'. More often than not such questions will invite other questions: 'What is meant by . . .? 'How could we determine whether or not there are other minds?', 'What criteria could we use to distinguish the real from the unreal?', and so on.

It is this indirectness which is no doubt responsible for much of the sense of wonder we feel when faced with philosophical questions for, though they can look simple enough, it becomes very hard just to know quite what kind of answer is required. In addition, there is the feeling that they are about the most fundamental and general kinds of things: Reality, Other Minds, the Nature of Knowledge, Matter, Truth, and more. They are, of course, about these things, but in a special way. Philosophical questions about the nature of matter are not the kind of questions physicists, say, can answer. Philosophical questions about 'other minds' are not the kind of questions on which psychologists might devise experiments to explore. Philosophical questions about truth are not reducible to the manipulation of logical formulae or to the advice of lawyers. It is this style of questioning and the almost childlike and innocent way it has of producing confusion in our established and taken-for-granted ideas about the world that seem special about philosophy. Not that there is any clear and unambiguous answer to what makes a question a philosophical one. It is not so much its form *as a question*; not all questions are philosophical, after all. It is more, as said earlier, a matter of the uncertainty about whether a serious question is being asked at all that helps make questions philosophical. With most queries, such as 'what is a gearbox?', we generally know what form an answer might take even if we ourselves are incapable of providing a satisfactory one. With philosophical questions, on the other hand, we are not sure what kind of answer will suffice which, in turn, makes us doubtful about the character of the question itself. The other features I have mentioned, like a sense of wonder, a feeling

that philosophical questions are about the most general kinds of things, about fundamentals, seem to hang upon this quality. Let me try to illustrate these points from a mundane example.

Occasionally, when driving around the British countryside, one comes across lorries with the word 'Milk' painted on their rear and sides. A fairly obvious conclusion to draw on seeing such a lorry is that this is a vehicle designed to carry milk going about its business picking up milk from the farms to be delivered to the dairy. But what is the basis of this inference? The fact that 'Milk' appears on the lorry? More than likely, but what does this presumption depend on? It depends, for one thing, on presuming that 'Milk' refers to what the lorry carries. Yet, as we well know, lorries can have names or words on them which do not refer to what they carry. Sometimes the name of the firm or owner is blazoned on the side, or the name of some product. So, how do I know that the lorry referred to carries milk? 'Milk' may have been the owner of the lorry, or a firm, or even the make of a lorry. How can I be sure about my claim? What kind of claim is it? Is it a claim about what I believe or about what I know? There are, of course, lots of reasons I could provide to substantiate my claim: it was a tanker lorry; 'Milk' is not a usual surname; it is not, as far as I know, the name of a firm, and it would be strange to use it as a petname, etc. And, perhaps, an accumulation of such reasons might 'add up' to a conviction that I am right: this lorry does carry milk. But why?

The reasons just adduced include reference to my personal experience, my personal knowledge, the practices of vehicle manufacturers, lorry drivers, and more. How far do I need to go before the link between the sign 'Milk' and the function of the lorry is established beyond doubt? It could be argued that no amount of personal beliefs and reasons are sufficient; what I need to do is look inside the lorry. Again, what makes looking any more certain or corroborative than the reasons I have already offered? I may still be deceived. What should I conclude if the lorry was full of whisky instead of milk? Accuse the driver of smuggling? Conclude that I had misunderstood the label all along and that milk refers to a bright brownish liquid that comes from Scotland and not to a white thick liquid that comes from cows?

But whatever conclusion I came to, the point is that I would be embroiled in questions about the nature of evidence – how we know certain things, believe others, how we know things to be true or false, what inferences can legitimately be made from various kinds of experiences, what inferences consist in, and so on. Of course, in doing so we begin to lose something of our sense of direction; familiar experiences become doubtful and even the most self-evident, certain, commonsensically true features of our everyday world begin to take on a puzzling air.

Note that these questions arose out of an everyday ability of human beings. No esoteric knowledge is required to make the sort of connection between the label on the lorry and its function of carrying milk. We make such connections routinely as part and parcel of our daily lives. The ability to read road signs, labels on packets or bottles, headlines, street names, and so on, is an essential part of our everyday competence. In which case, why raise philosophical questions about it? Of course, at one level there is no reason whatsoever why we should do so. Certainly it is unlikely that philosophical discussion of this ability will have very much bearing upon the way in which it enters and affects our lives. However, at another level, philosophical questions are important and this has to be with questions of intellectual authority.

ONTOLOGY, EPISTEMOLOGY AND INTELLECTUAL AUTHORITY

The above kind of philosophical probing, albeit of a very modest kind, stands as an example of a philosophical issue which has been dominant in Western philosophy for centuries, namely, 'What is the character of our knowledge of the world?': a question which motivates that aspect of philosophy known as epistemology. Related to this is the equally vital question, 'What kinds of things are there in the world?': a question of ontology. Epistemology is, to put it briefly, concerned with philosophical claims about the way in which the world is known to us or can be made known to us and, as such, clearly involves issues about the nature of knowledge itself. Such questions are not about techniques or matters of fact, such as 'How do you measure IQ?' or 'What was the suicide rate in the United Kingdom in 1973?' since such technical questions presume, it can be argued, philosophically justified epistemological positions. In short, epistemological questions are questions, among other things, about *what are to count as facts*.

Quite clearly ontological and epistemological issues are not unconnected. Claims about what exists in the world almost inevitably lead to questions about how what exists is made known. It is important to emphasise, however, that ontological and epistemological questions are not to be answered by empirical enquiry since they are concerned with, among other things, the nature and significance of empirical inquiry. We cannot empirically inquire into the question of whether or not there are empirical facts. We can do so in respect of particular facts but not whether the world is factual or not. The latter is not an empirical question but one requiring philosophical and logical argument and debate in which

the very presuppositions of knowledge, as a general issue, are of concern. Philosophical questions are to be resolved by reason, not by empirical inquiry.

Philosophical issues are not resolved by citing evidence since much of the argument is about what is to be allowed to stand as evidence or stand as fact. Instead, what is required to resolve a philosophical dispute is an argument which shows how a set of conclusions follow, step by step, from some agreed-upon premises. If the premises are agreed and the steps consistently and rigorously followed, then the conclusions must follow *as a matter of logical argument*, no matter how outrageous they may seem commonsensically.[3] Of course, securing agreed-upon premises, following the steps strictly through to their conclusions, are neither easy nor straightforward.

In making a knowledge claim, whatever it may be, one is also indicating a preparedness to justify that claim by pointing to the ways in which one knows. Such ways may include reference to experimental methods, correct procedures of analysis, authoritative sources, spiritual inspiration, age, experience, and so on: that is, by reference to those procedures collectively accredited as, in general, good reasons for knowing. It is this public collective licensing from which the intellectual authority of our knowledge derives.[4] Receiving such a licence is not, of course, always a sufficient guarantee that one does know. What is being stressed here is the reasoned nature of our knowledge claims and the way in which particular reasons have an authoritative status, but, like all reasons, they are defeasible. To put it another way, there may be, in the case of a particular knowledge claim, reasons why normally 'good reasons' are not acceptable or, alternatively, it may simply turn out to be wrong. Nevertheless, seeing if our 'good reasons' are, ultimately, good is one of the aims of philosophical inquiry.

But why, if we recall the example of the milk tanker, should there be any doubt about the facts of the matter, that it carried milk, or doubts about how we could find out what the facts are? In the practical sense already mentioned, there is no reason at all, except in the cases where, for example, there is suspicion of smuggling, deceit or whatever – cases which are, again, also very practical ones. Of course, from a philosophical point of view, cases like these can only be resolved, one way or the other, within a framework which allows them to be dealt with in terms of factual evidence and such like. Claims, it is true, which require the assemblage of evidence, argument, and so on, to establish, but claims and evidence, nonetheless, only articulable once there is in place some framework for underpinning them as claims and evidence and about which it is reasonable to ask, 'Why this framework and not others?' and, further, 'What is the nature of this framework?'

In an important practical sense, we learn frameworks as we learn about the world. Philosophically, however, this really gets us nowhere because it is possible that what we learn may be wrong, and systematically so. We might be dreaming, deluded, blinded by personal prejudice or have learned cultural practices and beliefs that are false. In other words, it is reasonably possible to be sceptical about whether what we learn about the world is, in fact, the way the world is. One can simply point to the variety of views and conceptions held by various historical societies about the world – beliefs in witchcraft, gods sitting on mountain tops, procreation as the result of jumping over fires, magic, crossed fingers as inducing luck, and much more – to suggest that the way the world is underdetermines human conceptions of it. In which case, then, questions arise about the manner in which discriminations can be made between the way the world is and the way in which we might believe it is. Moreover, such a distinction would have to be secured by other than empirical evidence since appeal to this would not escape the arguments of that crucial philosophical character, the persistent sceptic. After all, the gods of ancient Greece, or whatever, were real, facts if you like, to the members of those societies, who might well regard the facts of our world, such as the internal combustion engine, television, or aircraft, as some species of magic. But, just what this difference might imply for the essential character of knowledge is by no means clear. Were the ancient Greeks deluded, and if they were, on what grounds can we secure our claim that gods on mountain tops do not exist? What entitles us to assert against the ancient Greeks given that, to all intents and purposes, the facticity of gods was, so to speak, something it was difficult for them to doubt? What secures our knowledge that they did not exist, against their equally fervent belief that they did?

Thus, epistemology is concerned to find arguments against persistent scepticism; an endeavour directed towards, to put it simply, finding arguments for the objectivity of certain forms of belief in order to better secure an authoritative distinction between knowledge and those beliefs which simply masquerade as knowledge. Indeed, one of the major activities of philosophical theories of knowledge has been, and still is, to give what Quinton calls, 'a critical account of the logical order of justification'.[5] This has often taken the form of a search for the indisputable foundations of human knowledge; that is, a search for those beliefs upon which other sets of beliefs rest and are justified by. If such beliefs, which it would be impossible to doubt, could be formulated then all beliefs could be arranged in a hierarchical order at the bottom of which are those which, while justifying those above, do not themselves require support. These beliefs, those of absolute epistemological priority, would

be the foundations of human knowledge and, hence, the source of intellectual authority.

Unfortunately, and as we shall see, candidates for such beliefs have not received universal assent and the philosophical search goes on. Moreover, as said before, conceptions of the world have changed historically. Anyone with even a cursory knowledge of history and anthropology quickly realises, as said earlier, that in many respects our forefathers had ideas about the world very different to our own, and these have influenced philosophy itself.

What this means is that we need, perhaps, to heed Toulmin's advice not to treat epistemology as if it were without roots in the thought of a period, or unrelated to the practical procedures and problems of historically conceived disciplines.[6] For example, the methodological debates within the social sciences cannot be understood independently of the wider cultural setting of the findings produced by earlier research and based on different epistemological assumptions, and, as indicated earlier, such debates have been massively influenced by the rise of natural science. No epistemology can be composed of self-evident truths, firm for all time. Indeed, as we shall see, and have cause to criticise later, the present-day 'commonsense' conceptions of the nature of the world and the ways in which it is possible to know it themselves derive from seventeenth-century debates when they were extremely radical ideas.

Descartes and Locke, two of the major figures in Western philosophy, despite their genius, were men of their age and discussed the principles of human knowledge against the background of the then current ideas about the order of nature and man's place within it. According to Toulmin, they took three 'commonplaces' for granted, 'commonplaces' that were felt to be in need of philosophical justification: that nature was fixed and stable, to be known by principles of understanding equally fixed, stable and universal; that there was a dualism between mind and matter, the latter being inert, while the mind was the source of reason, motivation and other mental functions; and, finally, that the criterion of knowledge, of incorrigible certainty, was provided by geometry against which all other claims to knowledge were to be judged. We can see how such a conception provided both a basic ontological description of the world and epistemological prescriptions about how that world could be investigated. It directed scientists' and philosophers' attention and, through time, became established as the authoritative version of the world rather like a set of instructions about how the world should be sensibly assembled. This arose because it became a view, a conception, widely held by scientists and philosophers. More detailed theoretical work within various disciplines was given intellectual validity by the extent to

which it was seen as consistent with this conception, and, at the same time, reflexively established its own validity. There were many different theoretical schools, even within a single discipline – rationalists, empiricists, corpuscularians, vorticists – taken as consistent with the ontological and epistemological principles put forward. Nonetheless, these principles set the context of debate within which the different schools fought their disagreements and their selected versions of the world. In short, it was these principles which had intellectual authority.

An awareness of the social and historical contexts of claims to knowledge does raise a problem, again one that will be addressed more fully later, which has to do with the relativity of knowledge arising, as one might say, from its social determination. Although the 'commonplaces' of the seventeenth-century view of the world – a view, incidentally, that was specific to learned groups in Europe – retained a strong influence throughout the succeeding two centuries, none of them has the same meaning, or is held with the same conviction, today. The ideas of evolution and a changing universe no longer support the conception of a fixed and unalterable universe in quite the same way. Similarly, the distinction between mind and matter, so 'commonsensically' true, no longer has the clear bright force it once had. The invention of new non-Euclidian geometries, too, went a long way towards questioning the geometrical ideal as the frame of the universe, allowing that discipline, paradoxically, more room as a human creation, useful and powerful for particular purposes but by no means the source of certainty as a universal standard of knowledge. But, if such 'evidently true' principles of our own culture and our past have come to be questioned, what is to take their place? Further, is this change a progression, an evolution of our knowledge towards better forms, or can systems of knowledge only be judged in their own terms as the product of particular social and historical societies? What are we to make of forms of knowledge alien to our own, such as beliefs in witchcraft, for example, or in medicines which rest on very different conceptions of disease and, yet, have a remarkable efficacy at least in the cultures they serve?

These examples, and there are many more, sharply pose the issue of the relativity of the criteria of knowledge or, to put it another way, the sources of our intellectual authority. How do we judge between different systems of knowledge? Are there clear and unambiguous criteria, as Plato and Descartes felt geometry represented, by which we can determine whether or not what we know is true? Is there, in short, any universal source of intellectual authority, or is all our knowledge simply relative to the society and the period in which we happen to live? Questions such as these, abstract though they may seem, are important in helping

us to understand what we are doing when, among other things, we engage in social research to produce knowledge.

This brings us to another feature of philosophy, namely, that it arises in that area of human thought where our ideas and concepts are stretched to their limits. I spoke earlier of the prodigal social sciences returning to philosophy when there arises a deep uncertainty about what they are about, when human thinking seems to be getting out of hand, when barely articulable questions seem to undermine our most cherished and securely based conceptions. It is at times like these that social scientists, or at least some of them, begin to speak of 'epistemological breaks' and 'paradigm shifts' or, more prosaically, developments in human thought.

PHILOSOPHY AND THE RESEARCH PROCESS

To round off this introductory chapter I want to try to relate these general remarks about the nature of philosophy to the process of social research.

Broadly speaking, research is carried out in order to discover something about the world, a world conceived, albeit loosely and tentatively, in terms of concepts that characterise a discipline, whatever it might be. The popular image of the researcher emphasises what one might term the manipulative aspects of the role, the tangible, the 'mucking about' with things, whether they be chemical compounds, test tubes, microscopes and slides, particle accelerators, wires and transistors, microchips, and so on. This imagery derives, in large part, from the salience of natural science within our culture, and if 'mucking about' were all that was necessary to research it would have little philosophical interest. Although many of the major discoveries of our age and others have been unintended, accidental even, they have been established and accredited as discoveries through the application of a method, a corpus of procedures vested with the power to produce knowledge we would call 'scientific' which are, in effect, collective agreements as to how specific versions of the world can be arrived at. How they relate to actual scientific practice is another matter.[7]

Having raised the epistemological issue it is less easy to say exactly what the procedures are. We could easily point to things like experiments, hypothesis-testing, the public scrutiny of method and so on, as composing at least some of the methods important in producing scientific knowledge. However, for any set of procedures it is open to us to ask 'Why these procedures and not others?' 'What sort of guarantees, if any, do these methods provide that others cannot?' To pose these questions in the context of social science, it is necessary to ask what it is about the procedures and methods used by social researchers, of

whatever social science discipline, that makes them superior (and gives them greater intellectual authority) to those used by, say, the man or woman in the street, the journalist, the racial bigot, the politician, the revolutionary, or a Trobriand Islander. Or, to put it even more fundamentally, what is the basis of their claim to intellectual authority?

It will be no surprise to find that answers to these questions are not straightforward. The difficulties grow if we take even a cursory look at what social researchers do when they say they are engaged in research. The training of a social researcher will normally consist in his or her being required to master questionnaire techniques, the principles of survey design and analysis, the intricacies of statistics, maybe even computer programming and modelling, and so on. Of course, the emphasis given to different techniques would depend on the discipline involved: the sociological researcher might also have to know about participant observation as well as statistical techniques, the economist about even more sophisticated mathematical and statistical tools, while the historian would probably be more concerned with developing skills in the interpretation of various kinds of documentary evidence. The point is that these skills can be learned and used as if they were the skills of a craft. Researching a problem is a matter of using the skills and techniques appropriate to do the job required within the limits set: a matter of finely judging the ability of a particular research tool to provide the data required. In short, it is to treat research methods as a technology; and, make no mistake, without this attitude 'normal science', to borrow Kuhn's phrase, would not be possible.[8]

The relevance of the philosophical issues mentioned arises from the fact that every research tool or procedure is inextricably embedded in commitments to particular versions of the world and to knowing that world. To use a questionnaire, to use an attitude scale, to take the role of a participant observer, to select a random sample, to measure rates of population growth, and so on, is to be involved in conceptions of the world which allow these instruments to be used for the purposes conceived. No technique or method of investigation (and this is as true of the natural sciences as it is of the social) is self-validating: its effectiveness, i.e. its very status as a research instrument making the world tractable to investigation, is, from a philosophical point of view, ultimately dependent on epistemological justifications. Whether they may be treated as such or not, research instruments and methods cannot be divorced from theory; as research tools they operate only within a given set of assumptions about the nature of society, the nature of human beings, the relationship between the two and how they may be known. It is at this level that one begins to meet the philosophical questions and issues mentioned earlier.

However, what is not so clear despite the kind of account given earlier of philosophy being concerned with providing intellectual authority is whether, indeed, philosophy *can* provide such authority and, even so, what *its* basis of authority is. What is true is that for most researchers, be they in the natural or the human sciences, philosophical inquiry is largely an irrelevance to their activities, and it can be suggested that the extent to which they are concerned about what philosophy has to say, is motivated more by a search for security, like Linus and his blanket, than it is for practical guidance.[9] Moreover, the kind of status that philosophy has had in our own culture for a very long time as *the* learned pursuit, may well be a reflection of the contingencies of our culture rather than due to any intrinsic and necessary feature of the character of philosophy itself as a form of knowledge. Not surprisingly, the nature and status of philosophical knowledge has been, and still is, a contentious issue in philosophy itself. Locke, for example, held to a modest 'underlabourer' conception of the philosophical task, as one concerned to clear up confusions, muddled thinking, unclarities, and the like, in knowledge. For him the new and developing science was of much greater significance. The great metaphysicians, on the other hand, such as Descartes, Kant, Hegel and, more recently, the Phenomenologists, Husserl and Heidegger, propounded much mightier views on the nature of philosophy as the arbiter of knowledge itself; a role also embraced by the Logical Positivists but without the systemic earnestness of the great metaphysicians to build great systems of philosophical rumination. Yet other philosophers, and here perhaps Wittgenstein is the most salient figure, questioned whether metaphysics said anything at all and, in doing so, challenged the very notion that has motivated much of Western philosophy, namely, that knowledge needed philosophical foundations.[10]

There is, of course, no reason why there should be only one valid conception of the relationship between philosophy and social research, especially since the nature of philosophy, and its relationship to other forms of knowledge, is itself contentious within philosophy. What can be acknowledged, however, is the fact that philosophy had, and still does have, a strong relationship to social science and, through this, to social research even though this may be unclear and arguable. We can also accept that philosophical issues can arise from within any activity, though not with equal virility at all times. Professional philosophers are not the only ones capable of raising philosophical issues although they may be rather better at it than those not so trained.

My concern here is with philosophical issues arising from social research. Inevitably many of the issues to be discussed will be of a wider concern than simply social research methods. Accordingly much of the discussion will cover the ground of the philosophy

of science. There is no avoiding this, but I shall try to point more directly to questions about the nature of social research practice itself. This is more than just of technical interest, as it might be in an underlabourer conception of philosophy. The lack of consensus within and between the various social sciences, reaching chronic proportions in some of them, as to whether they are sciences, pseudo-sciences, immature sciences, multiparadigm sciences, moral sciences, or whatever, makes the issues all the more important, if still difficult to resolve. Since their appearance on the intellectual scene, the social sciences have been accompanied by a continual sense of failure over their inability to produce analyses of social life as convincing as those produced by the natural sciences of the natural world. In spite of Economics we still have economic crises, a fact sometimes blamed on politicians for not listening to their economic advisors who, in any event, speak with very different voices. In turn, politicians blame social scientists for not dealing with the 'problems of our time', and so it goes. The status of the social sciences is not settled. Within sociology, for example, debates range over whether it can be scientific in the manner of the natural sciences which has led, in its turn, to an examination of what natural science is as a form of knowledge. There is a disquiet, too, about whether the optimism of a decade or so back was really justified, as many eminent methodologists begin to question the direction of social research.[11] Whether philosophical rumination on these and other problems will resolve them is doubtful, since the problems are so widespread and multifarious. Nevertheless, what can be said is that some effort at clearing some of the philosophical ground would not go amiss.

It could be said that my concern is with the methodology of social research; that is, with an examination of the means of obtaining knowledge of the social world. As far as methods of research are concerned, I shall endeavour to approach them through what claims can be made about the knowledge they produce. This involves looking at the theories of knowledge on which they are based and coming to some conclusions about their philosophical plausibility. I shall begin, in the next two chapters, by discussing what I have termed the 'positivist orthodoxy' since, as a theory of knowledge, it has been, and still is, a major influence in the social sciences. I shall then go on to examine an alternative view which implies rather different conclusions about the nature of the social sciences and the forms of knowledge to which they can, or ought, to aspire.

One final word. By training I am a sociologist so, on the principle that an author should write according to his strengths, such as they are, most of the examples and ideas are derived from this particular social science. However, it should not be thought that

other social sciences do not experience the issues I shall discuss; on the contrary. Throughout, unless accurate exposition dictates otherwise, I have used the term 'social science' for convenience, and would remind the reader that the scientific status of these disciplines is an issue in what follows.

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7. This is one of the concerns of the new sociology of science. See Chapter 4. See also Yearley, S., *Science and Sociological Practice.* Milton Keynes, Open University Press, 1984.
8. Kuhn, T., *The Structure of Scientific Revolutions,* 2nd edn, enl., Chicago, University of Chicago Press, 1970.
9. As hinted at earlier, the issue of philosophy's own intellectual authority has recently come under attack from scholars who argue for the social construction of knowledge, a form of relativism in which philosophy, too, is culturally shaped and, hence, no more absolute than any other form of knowledge, including that of science. See, for example, Bloor, D., *Knowledge and Social Imagery,* London, Routledge and Kegan Paul, 1976. Hazelrigg, L., *Social Science and the Challenge of Relativism.* Vol. 1: A Wilderness of Mirrors: On Practices of Theory in a Gray Age, Gainsville, Florida State University Press, 1989, is an extensive review of the issue of relativism.
10. See Wittgenstein, L., *Philosophical Investigations,* Oxford, Blackwell, 1958, and for a summary, Anderson, R. J. *et al.,*

   Philosophy and the Human Sciences, London, Croom Helm, 1986, ch. 8.
11. See, for example, Lieberson, S., *Making It Count: The Improvement of Social Research and Theory,* Berkeley, University of California Press, 1985.

CHAPTER 2

The positivist orthodoxy

A word of caution is in order about the title of this chapter. The critics of positivist social science, among whom I wish to be counted, like all critics have a tendency to present a picture of the opposition as if it were not only stupid but without subtlety and variety. Although it is necessary to give a summary picture of positivism, the reader is warned that it is neither a stupid position, though it is more than possible to argue that it is wrong, nor some monolithic school of thought. It also needs to be pointed out that what I am referring to as 'positivism' also goes by other names, 'empiricism', 'behaviourism', 'naturalism', even 'science', some of which, to make matters worse, are labels used on occasion to refer to anti-positivist viewpoints. It is also a term, as mentioned, associated with a number of rather disparate philosophical schools. Nevertheless, on the admittedly hazardous rose principle that it would be the same by any other name, I shall use the label I have chosen since it is the most common, and shall draw attention to differences as and when necessary.

I refer to positivism as the orthodoxy because, in some of its versions, it is the philosophical epistemology that currently holds intellectual sway within the domain of social research though, these days, this hold is weakening, sometimes significantly. Further, since it has been attacked most vehemently in the last decade or so, there are few brave enough to now embrace the label with any gusto. Nevertheless, despite this ebbing of positivism, its influence has inspired much of social research's most used research instruments, such as the survey, the questionnaire, statistical models, the idea of research as hypothesis-testing and theory corroboration, to mention but a few. As one commentator says of the relationship between positivism and sociology, 'even if in its simpler philosophical forms it is dead, the spirit of those earlier formulations continues to haunt sociology, in a full range of guises . . .'[1] Also, while in some social sciences, such as sociology, its authority is less than absolute and probably always has been so, in economics it is not seriously challenged. Political science had its 'behavioural movement' rather later than its compatriot disciplines and it still holds a strong position in various branches of that subject. In psychology, too, its hold is weakening but is still immensely strong. History is beginning to make more use

of statistical methods classically associated with social research and, to this extent, developing what could be described as a more positivistic orientation. The growth of such fields as educational research, management studies, marketing – as endeavours within higher education institutions, and closely associated with the human sciences – have revived positivism's fortunes in some ways.[2] So, it is still worth looking at the philosophical character of positivism not out of some archaeological interest in a decayed civilisation but because it is very much alive.[3]

However, the authority of positivism did not arise overnight, but grew out of an intellectual debate spanning many years.

AN INTELLECTUAL BACKGROUND

Although it is customary to trace philosophical ancestors back to some early Greek philosopher, the more proximate origins of positivist epistemology lie in that blooming of European thought in the sixteenth and seventeenth centuries. Even though the Renaissance and Enlightenment picture of the intellectual darkness of the Middle Ages was overdrawn, even caricatured, later centuries did see tremendous changes in ways of thinking, particularly in what we would recognise as the beginnings of modern science and also in social and political thought. In brief, European thought was gradually freed from the theological cage erected by an alliance between political Absolutism and the Roman Catholic Church. Although 'natural philosophers', and Newton is a good example of this, often saw their endeavours as primarily religious rather than narrowly scientific, as a means of better understanding the mind of God and his creation, the allegorical world picture of medieval times was replaced by a scepticism over whether nature could so easily be explained by reference to the Bible or to religious dogma. Though religious elements were still strong, the groundwork was being laid for a secular vision of the traditional theological images of the natural and the social worlds.[4]

Two figures stand out sharply: Bacon (1561–1626) and Descartes (1596–1650). The former represented the Aristotelian legacy of empiricism as the account of the foundations of human knowledge, while the latter continued the Platonic rationalist tradition. Both were looking for an intellectual method that would overcome scepticism and, in doing so, provide a new certainty for knowledge of the world. Bacon argued for the authority of experience, experiment, induction and painstaking observation as the way towards providing a reliable basis for scientific ideas rather than the *a priori* method of medieval scholasticism. For him, a theory

of knowledge had to emphasise the methodical accumulation of experientially tested knowledge. True knowledge of nature required the scrupulous design and conduct of experiments, patiently working to the 'most general axioms, ridding the mind of false notions', opinions and received tradition. Descartes, on the other hand, placed his faith in the certainty of mathematics, especially geometry, as the fundamental instrument of scientific knowledge. For him, mathematical principles were timeless and unchanging and, therefore, the most suitable language for expressing the laws of nature. Though the doctrines each of these figures represented were in many respects very different, they did have in common the search for the foundations of human knowledge. Descartes, along with other rationalist philosophers, such as Spinoza and Leibnitz, while not denying the value of sensory experience, experiment and observation, stressed the role of logical deduction from self-evident premises, while Bacon, Locke, Hume and other empiricist philosophers gave pride of place to sensory experience, and it was this branch of the epistemological divide that was carried forward by positivist philosophy.

In the social sciences the first self-conscious voice proclaiming the positivist view was to be heard in the writings of Auguste Comte in the early nineteenth century. It was Comte who followed the optimistic impulses of Diderot and other '**philosophes**' in extending Bacon's conception of the study of nature to the social. It was Comte, too, who coined the term 'positivist philosophy' and, incidentally, 'social physics' or 'sociology'.[5] Comte's work was influenced by the major philosophical attacks on metaphysics made by Hume, and others, in the eighteenth century, and by the new ideas of progress and order emerging in and after the French Revolution. Indeed, Comte's positivism is also very much a theory of history in which progress in knowledge is the motor of history itself. Comte saw the task of philosophy as attempting to express the synthesis of scientific knowledge in which all the sciences would be integrated into a single system. His own theory of knowledge stressed that science consisted of precise and certain method, basing theoretical laws on sound empirical observation. For him the social sciences were kin to the natural sciences, sharing the same epistemological form and free from the speculative dross of metaphysics. Though Comte was sufficiently a child of the Enlightenment to reject religious criteria for knowledge, he also rejected much of traditional metaphysical philosophy's claim that knowledge could be derived by rational thought alone, and, instead, pressed the claim that knowledge is derived only from empirical evidence.

Though Comte's explicit doctrines have little more than historical interest these days, his spirit was carried forward in the

work of J. S. Mill, Herbert Spencer and Emile Durkheim, and is diffusely represented in the style and manner of the social sciences today.[6] Of all Comte's claims, perhaps the most important is his assertion that society, including values and beliefs, could be studied using the same logic of inquiry as that employed by natural science. Comte's explicit espousal of a unity of method between the natural and the social sciences was both timely and fateful. For one thing it gave impetus and strength to the view that the explanation of social phenomena, and by this is meant all that is studied by the human sciences, was not, in principle, different from the explanation of natural events: a view endorsed by Mill. Indeed, for Comte, the development of all sciences followed from mathematics, through astronomy, the physical and the biological sciences, and reach their apogee in the rise of the social sciences.[7] Phenomena in both the human and the natural worlds were subject to invariant laws. Though there were differences between the human and the natural sciences arising from their respective subject matters, the development of appropriate research methods in the former would remove these irritants so that the social sciences could take their rightful place at the head of the hierarchy of human knowledge. As indicated earlier, Comte himself stressed the importance of indirect experimentation, observation, and the comparative method. More deeply than this, his view encouraged a deterministic conception of man and society by effectively underplaying those factors normally regarded as uniquely human: free will, choice, chance, morality, emotions and the like. Human social life was simply the result of a coalescence of forces interacting so as to produce a particular sequence of behaviour. History, too, was simply a theme with variations in which human and other factors combined to work themselves out through time.[8]

Throughout the nineteenth century this conception gained an authority continually reinforced by the seemingly amazing progress of the natural sciences and their applications. The landmarks of this progress are familiar, the most famous being the publication in 1859 of Darwin's, *The Origin of Species*, which gave the world a systematic statement of the idea that mankind was irretrievably part of nature and subject to the same laws of process, adaptation and change. It was not long before the social sciences began to use these insights to develop theories of human society. Herbert Spencer, for example, explicitly borrowed from Darwin's work as a vindication of his own theory and method.[9] By the end of the century the scientific-deterministic view of positivism was firmly entrenched as the ambition of the social sciences. However, although, as far as the human sciences were concerned, it was positivist systems which became dominant, there were rivals. There was, in philosophy, towards the end of

the nineteenth century, a revolt against positivist thought and a resurgence of idealism and romanticism, a movement particularly strong in Germany.[10] More recently, positivist thought has come to be associated with statistics as the manipulation of quantified social facts: a tradition, by the way, to which Comte was strongly opposed.

The questions to address now are 'What did this view of science and knowledge involve in practice for the social sciences as opposed to exhorting an overarching ambition?' 'What procedures and rules of investigation did it justify and authorise?' 'What kind of knowledge did it claim was the proper goal of social science?'

THE ELEMENTS OF POSITIVISM

According to Giddens, in its widest sense, 'positivist philosophy' refers to those perspectives that have made some or all of the following claims.[11] First, that reality consists in what is available to the senses. Second, philosophy, while a distinct discipline, is parasitic on the findings of science. Associated with this is an aversion to metaphysics as having any rightful place in philosophical inquiry proper. As a philosophy, therefore, it is as much concerned to establish the limits of knowledge as well as its character. Hume's petulant outburst against metaphysics captures this and its general spirit well:

If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion.[12]

Third, that the natural and the human sciences share common logical and methodological foundations. This is not to say that they share precisely the same methods since their respective sub-ject-matters require rather different methodological approaches, but this is a pragmatic not a logical or principled difference. Fourth, that there is a fundamental distinction to be made between fact and value, science dealing with the former while the latter belonging to an entirely different order of discourse beyond the remit of science. However, as we shall see later, this did not imply that all human qualities were beyond the reach of scientific understanding. While scientific knowledge has its limits these do not exclude knowledge of the mental or 'inner' life of human beings. What science is neutral about are the values to which human beings ought to aspire.

This resumé of the main elements of positivist thought as it ap-plies to the human sciences cannot, obviously, do justice to the

various and important nuances represented by its many schools.[13] From the perspective of social research the important questions turn on what positivism implies for the methods of studying society; what it claims about the proper knowledge to be gained from such study and, equally important, the criteria necessary to assess that knowledge and distinguish this from beliefs and understanding which cannot qualify as knowledge. These are wide–ranging questions, and there are many styles of social research consistent with the broad domain precepts reviewed earlier. However, as a system of thought with pretensions to authorising particular versions of the world, both the natural and the social (and positivism is particularly strident, not to say intolerant, in its views of what knowledge is), we need to look closer at its various postures.

The revulsion against metaphysics was bound up with a strong commitment to knowledge which dealt with facts, systematically discovered, rigorously supported and which could serve to ground adequate theories. In order to make and maintain the distinction between empirically grounded knowledge and mere speculation, demarcation criteria were needed. Positivism recognised only two forms of knowledge as having any claims to the status of knowledge, the empirical and the logical: the former represented by natural science and the latter by logic and mathematics. By far the greater importance was attached to the empirical. In this it took its inspiration from that philosophical tradition which claimed that all our ideas come in one way or another from our sensory experience of the world; any idea that cannot be shown to be derived from this was not a genuine idea. Clearly, such a view is dependent on a presumption that the external world acts on our senses and, in this way, is made known to us at least in a 'brute' form. The knower contributes very little to this experience and the knowledge it provides and what organisation there is to this knowledge is itself provided by experience. This view served as a criterion by which to determine what was knowledge and what was mere speculative dross; ideas only deserved the appellate of knowledge if they could be put to the test of empirical experience. There was no knowledge *a priori* of experience which, at the same time, was informative about the world. As we shall see later, mathematics was a problem for this view.

Though this view of the source of knowledge had some plausibility as an account underpinning natural scientific know-ledge, there were difficulties in applying it to human life. The notion of fact, especially when posed in opposition to value and the kind of entities conjured up by metaphysicians, had strong connotations of the material world, the world of fixed, tangible, permanent *matter*. To this extent, positivism had to overcome a distinction expressed in a number of ways between 'things

material' and 'things human'; a distinction massively important in the history of thought involving, as it did, legal, religious and ethical as well as political implications. Given the imperial ambitions of positivism, one position it had to invalidate was that the human and the material world were different orders of phenomena and had to be understood differently. Some positivists denied the distinction altogether by reducing what we would regard as human to manifestations of material nature. Reducing, that is, human activities to the outcome of physiology, chemistry or a particularly behaviouristic psychology. Others, however, were not reductionists in this sense claiming instead that human and material phenomena were both real, if different in important respects, but could be made knowable by using the same methods of scientific inquiry.

The difficulties of showing the latter were manifold. For one thing phenomena in the material world, as a matter of commonsense alone – which, in effect, provided positivism with the problem in the first place – seemed to have a nature and character independent of the observer, while so much of human phenomena seemed entirely the product of human beings. How could one reach an understanding of beliefs, systems of magic, emotion, morality, legal codes, legends, public opinion, and the like, in the same way that one could of the moon, the stars, skeletons, gases, chemical compounds, and so on? Do they possess the same attributes of permanence, durability, independence of human volition and perception as phenomena of the external material world? These were the questions that had to be answered before positivism could successfully claim that the human world, like the physical, operated according to natural laws which could be discovered by a scientific method taken from the natural sciences.

So, the questions were: 'What in the human world corresponded to the "hard facts" of nature?' 'What procedures were appropriate for discovering and studying these facts?' And, assuming these questions were satisfactorily answered, 'What were the laws corresponding to the laws of nature?' By the early nineteenth century there were some hopeful straws in the wind. Some scholars were beginning to take seriously the observation, now a rather self-evident one, that human action is not random but conforms to certain predictable patterns. One of the great insights, late in the eighteenth century, was Adam Smith's formulation that individuals acting on their own self-interested preferences could, as if controlled by an 'invisible hand', produce large-scale social regularities.[14] The very notion of society, it was realised, strongly implies a set of phenomena which, though involving individuals with all their uniqueness, whims and fancies, nevertheless, exhibits large-scale regularities in some sense as real and as predictable as

individuals are unique and different. In short, there were ideas around which made it plausible to conceive of society as a level of reality *sui generis*. The problem was to say how.

There were, and still are, many puzzles here and it is opportune at this juncture to look in more detail at one attempted answer to these and related questions, that of Durkheim, one which illustrates some of the major issues confronted by a positivist social science and which was profoundly influential in setting the terms of such an approach. This is not to argue that Durkheim's work is without its problems let alone provided all the answers. It was enough that he did at least do much towards identifying many of the issues. As is perhaps inevitable, the corpus of Durkheim's work displays many contradictions, inconsistencies, doubtful reasonings and other difficulties, but he does exemplify well the spirit of positivist social science.[15]

DURKHEIM'S POSITIVISM

Durkheim was the first sociologist since Comte, to whom he owed a considerable intellectual debt, and in a serious sense carried on Comte's vision in French sociology, zealously to justify sociology as an autonomous discipline characterised by the application of the scientific method. Durkheim shared Comte's empiricism, his views on the unity of science, his devotion to rational social reform, and his distrust of psychology, but rejected as bordering on the metaphysical many of Comte's pronouncements about the historical progress of knowledge. Durkheim's own work encompassed philosophical discussions on the nature of sociology as well as his more substantive enquiries into the division of labour, suicide, religion and education. In significant respects his work is a bridge between the nineteenth century and the twentieth. Many of his ideas – including the centrality of the division of labour for social organisation, the recognition that society represented a level of reality in its own right, that society was fundamentally a moral order – had their roots in Comte and his contemporaries. Other scholars, too, particularly J. S. Mill, Spencer, Tönnies, had their influence on Durkheim's ideas. However, although Durkheim was very much a child of nineteenth-century thought, he was to modify that tradition in consequential ways, and none more so than in his stress on social science as dealing with quantifiable data.

Durkheim insisted that society was a moral phenomenon in that collective ways of thinking, perceiving, acting, included elements of constraint and obligation and, therefore, constituted a collective moral consciousness. This, he held, was expressed in religion, in law, in the division of labour, in institutionalisation itself. Yet, like a true child of positivism, he wanted to show how the methods of

science were provably superior to other methods of conjecture and speculation, including those of social philosophy, for studying the moral association of society; an endeavour that tried to forge a new unity between idealism and materialism. The former group of philosophies argued for a strict duality between nature and human life rejecting, in other words and among other things, the positivistic idea of a unity of method between the natural and the social, or human, sciences. For his part, Durkheim wished to retain a distinctive, and moral, conception of humankind but to study this by using the methods of natural science without their materialistic implications which, unfortunately in his view, leads to a reduction of the distinctively human to the material. Herein lies the importance of his efforts to establish sociology as an autonomous discipline defined by its object of study and to avoid the tendency in much of nineteenth-century thought to reduce the moral and the social to an epiphenomenon of material forces; a tendency most marked, though not simply so, in Marx. Moral phenomena, such as law, religion, morality itself, and so on, could be the object of a natural science of man if they were studied in the correct way. 'The aim is to bring the ideal, in various forms, into the sphere of nature, with its distinctive attributes unimpaired.'[16] These aspirations left Durkheim with two related problems to solve and to do so within the framework of positivism: first, to establish the reality of the social and, second, to discover ways in which it may be scientifically investigated.

Science, for Durkheim, was the study of 'things' and was concerned, in the first instance, to describe and classify these accurately, and, in the second, to explain the ways in which they were connected. The notion of 'things' is contrasted with ideas:

Things include all objects of knowledge that cannot be conceived by purely mental activity, those that require for their conceptions data from outside the mind, from observations and experiments, those which are built up from the more external and immediately accessible characteristics to the less visible and more profound.[17]

A most important characteristic of 'things' is that they are not subject to our will, but resist our subjective attempts to change them, proving, according to Durkheim, that their existence is independent of our ideas about them; they belong, in brief, to the external world.

Sciences deal with 'things' and sociology and the social sciences should be no exception to this. So, turning from the properties of 'things' in general we must now examine the way in which Durkheim tries to establish the facticity, the 'thingness', of the social. 'Social facts' take on properties of 'things' in general: they are external to us, are resistant to our will, and constrain us. By way of illustration he cites the French language, moral rules,

economic organisations, laws, customs: all social phenomena but which are independent of and constrain individuals.

Here, then, is a category of facts with very distinctive characteristics: it consists of ways of acting, thinking, and feeling, external to the individual, and endowed with a power of coercion, by reason of which they control him . . . the term 'social' applies to them exclusively, for it has a distinct meaning only if it designates exclusively the phenomena which are not included in any of the categories of facts that have already been established and classified. These ways of thinking and acting therefore constitute the proper domain of sociology.[18]

These facts are not reducible to other disciplines, for example, to biology or to psychology, which have their own order of facts. 'Social facts', nonetheless, are 'things' in that they possess externality, constraint, diffuseness and generality, and are distinctive to sociology belonging to no other discipline or science. This, in sum, is his argument for the autonomy of sociology.

Durkheim's conception of society is a realist one claiming that there exists, within the realm of nature, an entity defined in terms of a system of relations responsible for generating collectively shared norms and beliefs. Society is a reality 'in itself' and 'social facts' exist 'in their own right' quite apart from manifestations of them in and by individuals. It is individuals who commit suicide but the suicide rate indicates a 'social fact' independent of individual suicides. It is the interaction and association of individuals which gives rise to the emergent phenomena of the social, and which is not reducible to psychology (a fate Durkheim particularly wanted to avoid) or biology. For Durkheim this means that the explanation of 'social facts' has to be in terms of other social facts.

Society is not a mere sum of individuals . . . the system formed by their association represents a specific reality which has its own characteristics . . . It is, then, in the nature of this collective individuality . . . that one must seek the immediate and determining causes of the facts appearing therein.[19]

The task of the sociologist, according to Durkheim, is to describe the essential characteristics of social facts, demonstrate how they come into being, enter into relationships with one another, act on each other, and function together to form social wholes.

In this way Durkheim attempted to reject the dualism between ideas and matter, but in a way that would preserve the qualities of ideas and not reduce them to merely material productions. Social relations and the phenomena engendered by them are facts, they possess a reality though not a material one. They exist neither apart from individuals nor in any single individual, but in and among associated individuals. By acting together individuals produce linguistic symbols, religious beliefs, moral

History of Sociological Analysis, London, Heinemann, 1979, pp. 187–236.

16. Durkheim, E., *Sociology and Philosophy,* trans. Pocock, P. F., London, Cohen and West, 1953, p. 96.
17. Durkheim. E., *The Rules of Sociological Method,* ed. Catlin, G., New York. Free Press, 1966, p. xliii.
18. *Ibid.,* pp. 3–4.
19. *Ibid.,* pp. 103–4.
20. *Ibid.,* p. 27.
21. *Ibid.,* p. 15.
22. *Ibid.,* p. xiv.
23. Durkheim, E., *Suicide,* trans, Spaulding, J. and Simpson, G., London, Routledge and Kegan Paul, 1952, p. 42.
24. *Rules, op. cit.,* p. 42.
25. *Suicide, op. cit.,* p. 44.
26. *Rules, op. cit.,* pp. 130–1.
27. See Shaw, M. and Miles, I., 'The social role of statistical knowledge' in Irvine, J., Miles, I. and Evans, J. (eds), *Demystifying Social Statistics,* London, Pluto Press, 1979, pp. 27–38; Halfpenny, *op. cit.,* ch. 2.

C H A P T E R 3

Positivism and the language of social research

The first half of this century saw the flowering of positivist philosophy as the orthodox philosophy underpinning empirical social science. This is not meant to imply that its doctrines were unanimously agreed; simply that it served to set the lines of debate, in so far as this took place, about the nature of the social sciences. It was positivist philosophy of social science that had to be argued against. It became the dominant methodological justification for what Kuhn referred to, though in another context, as 'normal science': a science practised without constant reference back to fundamental philosophical premises.[1] It is science characterised by 'puzzle solving' empirical research rather than debates about fundamental theories and approaches. Most social scientists agreed that the social sciences should model themselves on the natural sciences, especially physics, since it was these disciplines that represented the peak of achievement in human knowledge. Accepting this ambition, however was one thing, there was rather less agreement on the precise nature not only of the social sciences but of the physical sciences, too, and it is important to remember that although the social sciences took the natural sciences as their yardstick they did so with respect to particular *philosophical* interpretations of natural science of which positivism was the major one. What was rarely done, if ever, was compare social scientific *practice* with that of physical science. It was philosophy that was the interpreter and mediator of the method of science.

Within positivistically inspired social science there were debates, and still are, about, for example, the nature of scientific explanation, whether social science theories could attain the certainty of theories in natural science or had to be satisfied with probabilistic ones, whether falsification or verification was the fundamental criterion distinguishing scientific statements from non-scientific ones, and so on. These debates, among others, formed the core issues in the philosophy of social science.[2] The concern in this chapter, however, is mainly with philosophical issues arising out positivism's stress on the sovereignty of the empirical, and, in this respect, is focused rather closer to the research process itself. This is not to say that such issues are not unrelated to the wider issues of the philosophy of science, though such connections can be

overrated. However, in so far as one is dealing with questions about the nature of knowledge, one cannot avoid them. As we saw in the previous chapter's discussion of Durkheim's attempt to establish a social science of sociology, the explicit use of what is regarded as the correct philosophical account of natural science, namely, positivism, has been the traditional mode of demonstrating that, despite their different subject-matters, the social sciences use the same epistemological principles as the natural sciences. However, some of these wider issues will be dealt with in the following chapter. This division is somewhat artificial but expedient in view of the nature of the issues and the lengthy exposition required of a number of different if related themes.

THE LANGUAGE OF OBSERVATION

One of the important features of positivist philosophies of science was the pre-eminence accorded to empirical research in the production of knowledge. All the major scientific advances, it was claimed, had resulted from the patient accumulation of facts about the world to produce the generalisations known as scientific laws. Science, above all else, was an empirical pursuit and its basis lay in the observation of what we can term 'brute data': that is, data which are not the result of judgement, interpretation, or other subjective mental operation.[3] In the same manner as natural scientists describe and classify phenomena by noting such observables as shape, size, motion, and so on, so should social scientists define and chart their phenomena of interest.

Positivists argued, then, that the basis of science lies in a theoretically neutral observation language which is both ontologically and epistemologically primary.[4] That is, statements made in this privileged language are directly verifiable as true or false simply by looking at the 'facts' of the world. In this it espoused a correspondence theory of truth, namely, that the truth of a statement is to be determined by its correspondence with the facts. If it did correspond then it was true; if not, then false. Later this would become, in modified form, a criterion of the meaningfulness of a statement and a way, thereby, of distinguishing scientific statements from non-scientific ones.

The clearest and most influential version of positivism in this century was that propounded by a group known as the Logical Positivists, which began in Vienna in the late 1920s under the leadership of Mach, Schlick and Carnap.[5] They were to give positivistic philosophy of science a shape and system which served to make it the predominant view of the first half of this century.[6]

As in other forms of positivism they rejected metaphysics by recognising only two kinds of proposition: the analytic, e.g. those of mathematics and logic, and the synthetic, e.g. those verifiable by empirical observation. Statements belonging to neither kind were not propositions and were meaningless. Religious, moral and aesthetic statements along with metaphysical ones were consigned to the dustbin of meaninglessness or, a slightly better fate, statements about personal taste or preference, since they were verifiable neither by empirical observation nor by logical deduction. The principle of verification, as it was called, served as a criterion for deciding whether a statement was meaningful or not.

Logical Positivism also differed from nineteenth century versions of positivism by stressing the *logical* character of the scientific method as well as the empirical. Logic had always been a problem for positivist and empiricist philosophies given their emphasis on the empirical as the source of knowledge and, from this, their rejection of rationalist doctrines as little better than metaphysics. Some, J. S. Mill for one, put forward an empiricist interpretation of logic and mathematics. For him logic and pure mathematics consisted of propositions that were generalisations from experience: a view which rendered mathematical statements, such as $2 + 2 = 4$, susceptible to empirical refutation. It was developments in formal logic from the middle of the nineteenth century onwards that offered a resolution of the suspicions with which empiricist philosophies held logic and mathematics. Logic, and mathematics as a branch of logic, came to be regarded as a collection of formal rules for constructing propositions and stipulating the conditions under which, within the formal system, they could be taken as true or false. Formal logic, in other words, elaborates the relational structure of terms within a symbolic system but is empty of empirical content. Thus, although beyond experience, logic and mathematics, unlike metaphysics, expressed *analytical* truths; that is, their statements were true or false by virtue of the rules for manipulating the symbols. Mathematical and logical truths are *a priori* not, as many rationalists thought, because they mirror the way in which the human mind works, but because they are analytic and tell us nothing about experience. As analytical truths they could be incorporated into science without fear of metaphysical contagion.

As far as Logical Positivism was concerned, these developments in the reconceptualisation of the nature of logic and mathematics spelt the end of traditional metaphysical philosophy. Logical analysis, as a method, could resolve philosophical problems and paradoxes by reconstructing philosophical statements in the language of formal logic. They also helped to reformulate the notion of empiricism. Since Hume, empirical knowledge had

been conceived in terms of ideas, or concepts, which were the remains of sense impressions. These were the source, and the only source, of our knowledge of the external world. As said earlier, *contra* rationalists, such as Descartes, there were no innate ideas. However, the notion of a primary observation language as the bedrock of scientific knowledge was a troublesome if powerful one.

While many of the more extreme positions of the Logical Positivists were found to be untenable, there is little doubt that their influence was profound. Their view was taken by many philosophers, and by many scientists, as the philosophically authoritative version of science's epistemology. Though difficulties remained, these were not considered to be of such fundamental importance as to invalidate the positivist tradition itself.

THE LANGUAGE OF OBSERVATION AND SOCIAL SCIENCE

The idea that knowledge is based upon a primary, or 'protocol' language was intended to ground science as an empirical discipline, giving it an objective character by providing, in principle at any rate, a publicly available, emotionally, ideologically and theoretically neutral mode of expression, providing a clear criterion of truth independent of human whim and prejudice, and privileging its status as knowledge of the highest order. However, the very difficulties of formulating an adequate basic observation or protocol language suggested that observation was a complex matter. Indeed, there were radical empiricists, including Mach, who were suspicious of the powerful theoretical concepts of even physics such as 'atom' or 'absolute vacuum', since they were beyond experience. But for the more moderate empiricists the idea of a sensory experiential language proved, in the end, a difficult idea to establish. Facts did not just appear. They were not just lying around waiting to be picked up by some wandering scientist; they had to be discovered, assembled and made informative. All the 'facts' Darwin used as evidence for this theory of evolution were 'known' before he used them. Fossils had been noticed by other naturalists many years before Darwin, most of the flora and fauna, too, had been discovered or seen by other travellers. What Darwin contributed was a profoundly radical way of rearranging these materials.[7] There was, then, more to scientific observation than 'simply looking at the facts', however basic these so-called facts appeared to be. So as far as the Logical Positivist position was concerned while most members of the school saw this language as consisting of the direct non-inferential reports of experience, exactly what the 'protocol' terms in the observational language

referred to was a matter of much inconclusive debate. Some argued for a sense data interpretation of the non-inferential reports of experience, others for 'physical objects', and yet others for 'atomic facts'. But, for our purpose, the point is that whatever the characterisation of these protocol terms, it was the observational language that was ontologically and epistemologically privileged as beyond reasonable doubt. So far as scientific practice was concerned, it was not suggested that all descriptive terms and concepts be couched in this basic observational language. All that was necessary was that if they were to be meaningful, then they should, in principle, be translatable into, or reducible to, statements in the observational language. As with the nature of the basic entities themselves, there was considerable debate as to how such a translation or reduction could be carried out.

So, while the formulation of a primary observation language proved philosophically elusive, if not illusory, other criteria or principles of observation for determining facts were required. To an extent these were already implicit in the positivist theory of knowledge. The world, whether natural or social, operated according to strict laws and therefore possessed a deterministic structure which it was the business of science to discover; a structure which could be described formally and quantitatively. Methodologically, then, empirical research, and here one might as well say scientific research, amounted to discovering those regular and invariant properties of the phenomena of the world and the relationships between them; the properties being described, as far as possible, in terms of what is rigorously observable. Thus, the physicist does not deal with billiard balls, falling feathers, crashing cars, boiling water, but with bodies of a particular shape, size, mass, motion, wavelength or whatever. Correlates among such attributes constitute the basic ingredients of scientific theories. Many such attributes may not be observable unaided by instrumentation, but the principle is there nonetheless.

Carried over into the human sciences this kind of conception faced a number of problems. One of these had to do with so-called 'mental states'. Human beings are not simply external shells of shape, size and motion: they have an inner life not accessible to observation in the normal way, unless private introspection is counted as a publicly available form of observation. Some argued that the inaccessibility of mental phenomena implied that they could not be dealt with objectively, that is, scientifically. Physical objects, physical events, physical processes could be described in more rigorous versions of the five senses and were, accordingly, publicly available. Mental states or states of consciousness, on the other hand, could only be experienced, and truly known, by one person, namely, the person undergoing the experience. Some Logical Positivists, Neurath's 'physicalism' being perhaps

the most prominent of these, claimed that science could only deal with 'mind' in so far as its products could be described in the language of physics; that is, as spatio-temporal phenomena, such as speech, facial expressions, etc.[8] The social sciences were, in other words, the study of behaviour with this being construed extremely narrowly. Few were, however, quite prepared to go this far.

A more typical strategy was to argue that though mental states were not observable in any direct fashion, nonetheless particular mental states were associated with specific outward bodily displays. For example, we see a person clenching his fists, gnashing his teeth, glaring wide-eyed from a red face, and reasonably infer that the mental state the person is experiencing is that of anger; indeed, that the cause of all this dramatic posturing is the internal experience of anger and rage. Accordingly, the argument was adduced that all statements referring to mental states could be analysed into a further set of statements referring to overt bodily signs or displays. Mental phenomena, then, could be observed, for all intents and purposes, by studying the corresponding outward behavioural display, the latter being an index of the former.

This sort of account of the relationship between mental states and overt behaviour was comfortable to many empiricists since it, seemingly at least, brought the 'mind' into a scientific frame of reference in which its features could be publicly observed, charted, quantified and correlated. The epistemological principle of sensory experience as the foundation of scientific knowledge was preserved and no unsystematic introspection involved. However, though this account had some plausibility with reference to anger, pleasure or pain, human beings experience more sophisticated mental states, to call them that, than these. They can desire wealth, status or power, can believe in democracy or the divine right of kings, determine the moral worth of actions, admire the beauty of the Giaconda, despise Dynasty, fall in love, and many more. Could these emotions, beliefs, morality, judgements be interpreted in the same way? Do these mental states correlate with determinate bodily displays in the same way that it might be said of anger? For the positivists the answer was in the affirmative. The beliefs people hold, the values they subscribe to, the judgements they make, their tastes and their preferences, are all publicly verifiable since they issue in, or result in, publicly observable behaviour, artifacts of various kinds, and so on. Values are objective in the sense that they are held by persons who can report their values and beliefs. The social scientist does not have to agree or disagree with such expressions; he simply has to report on them or use them as primary data. In short, the values people hold are as factually brute as geological strata, atoms, gas flows, velocities and the like.

By using carefully constructed instruments, such as questionnaires, attitude scales or interviews, subjects can provide responses which are indicative of mental states and in this way provide objective access to important aspects of human mental life.

Developing a methodology for investigating the mental aspects of human life was itself part of a larger issue mentioned earlier of formulating principles of social scientific observation. It was felt that to conform to what natural scientists were able to achieve, the language of social science observation had to consist of terms objectively defined, and had to be generalisable and, if possible, quantifiable. Since the aim was, in effect, to achieve the Comtean vision of discovering general laws of social life, the basic terms of the scientific language had to express general rather than particular qualities. One of the important moves in this respect was the adoption of quasi-mathematical terms in which to talk about data, namely, the language of variables. This represented a way of talking about social phenomena within an apparently neutral framework in terms of their attributes and properties and how they varied between and among one another.

THE LANGUAGE OF VARIABLES

It is difficult today to recapture the revolutionary impact of this formulation of the character of social research and its phenomena, since the language of variables is so much taken for granted in empirical social research.[9] These developments owed much to the work of Paul F. Lazarsfeld (1901–76) – an occasional participant in the Vienna Circle prior to the Second World War – and his colleagues. His major efforts were devoted to developing research techniques and designs in the context of research on, for example, the effects of the mass media and on the determinants of voting choice; both areas in which he did pioneering work. His work was inspired by a particular, though not unique, conception of science and how this could be translated into making social research more scientific in its quest for adequate empirically based theories.

The notion of a variable has a long tradition in mathematics, statistics and, importantly, symbolic logic. It is essentially a simple idea. A variable as opposed to a constant can vary in value within a range of values, even if this is of the order of 0 or 1, where 0 indicates the absence of a variable and 1 its presence. The innovative move was to use this idea as the pivot around which a whole way of thinking about social research could revolve. 'No science', declared Lazarsfeld, 'deals with its objects of study in their full concreteness.'[10] Certain properties are selected as the special province of study of each science, among which each tries to discover empirical relationships, the ultimate being those of

a lawlike character. Thus, physics is interested in its objects not in their full concreteness but in abstracted properties of them, such as their mass, length, force, velocity, molecular composition, and so on. Science's connection to the world is an abstracted one dealing with the properties or qualities of things, not with things in themselves. In this, Lazarsfeld is thoroughly Kantian espousing the position that empirical knowledge can never penetrate to the 'essence' of phenomena but deal only with their 'surface' appearances or indications. This means that the first task of any science is to identify those properties which are its concern: no simple task as the history of science attests to. It is particularly difficult for the social sciences where there is no standard terminology. Nevertheless, for Lazarsfeld this was not an insuperable problem nor, indeed, one that needed to be solved by epistemological or ontological rumination. What he proposed was an empirical strategy for pursuing this goal for social science by treating properties as variables; that is, using variables as 'devices by which we characterise the objects of empirical social investigations'.[11]

Briefly, Lazarsfeld saw the research process as one of translating concepts into empirical indicators; that is, indicators based upon what is observable, recordable, measurable in some objective way. The first step was the creation of a 'vague image', or construct, that results from the immersion of a reseacher in a theoretical problem. The real work begins by 'specifying' its components, aspects or dimensions, and selecting 'indicators' which can 'stand for' them. Thus, 'prejudice' could be measured in a number of ways, for example, by responses to particular items on a questionnaire, attitude scales, voting choice, avoidance behaviour, etc., which depend upon the practical exigencies of the research and the tools of investigation. Most concepts will, likely as not, prove to be complex combinations of indicators rather than measures of a single indicator variable. As most social research will be interested in more than one construct, it is by finding patterns among indicators in terms of their covariation and interrelationships that empirical descriptions are built up, and out of which empirically grounded theories can be devised to explain the patterns found. Quantification, according to Lazarsfeld, is possible through using the idea of variables if only at the relatively crude level of frequency counts of the presence or absence of some property, since even this modest level allows for the identification of covariations between variables.

As hinted at earlier, it is perhaps better to regard Lazarsfeld's endeavours as methodological rather than philosophical; that is, as seeking for a way of making social research an empirically based science. Nevertheless, there is a metaphysics of ontological realism there to the extent that it only makes sense to talk of indices if it

can be claimed that they 'stand for' something. However, although Lazarsfeld talked of an abstracted connection between scientific concepts and the world, in practice his strategy is effected by means of correlations among indices and the strength and stability they display, if they do, across studies. Theoretical validity is owed to the sovereign position given to the empirical in that the adequacy of a construct is determined by patterns found among measured variables. Special importance is given to operationalising concepts into observable measured indicators. For Lazarsfeld, indicators are what social science research works with, and they indicate something, to varying degrees, if they show detectable patterns of association with each other.

For empirical social science, then, the language of variables offered a way of expressing relationships in data and, as such, a way of describing phenomena objectively and quantitatively. All phenomena that are of interest to social research, including subjective states, could be conceptualised, measured at some level at least, correlated and variously manipulated by the formal techniques of variable analysis. Hypotheses could be formulated and tested. Although few, if any, of the social sciences could emulate psychology in being able to carry out laboratory experiments, fair approximations in non-experimental settings of social research to the logic of experimental design could be achieved using statistical partitioning methods.

However, despite the fact that the Lazarsfeldian conception has virtually become the orthodox style of social research, in some quarters regarded as **the** method of empirical social research, it has not been without its critics. Some objected to the way in which the reality of social phenomena and processes, in all their fullness, richness, complexity and flux, were concealed behind what was, in effect, a descriptive apparatus whose character owed more to the requirements of quantification and measurement than it did to reflecting the underlying phenomena it was supposed to describe. Some of these matters will be taken up in a subsequent chapter.

A further difficulty was that variable analysis was intendedly atheoretical; a ubiquitous method for pattern searching in data as a route towards theory formulation. Theories explained the patterns, but the patterns were needed first in order to obtain better theories. That is, although 'vague' theoretical ideas will inform the kinds of variables that will be investigated, or will be regarded as independent, as dependent, as mediating variables, and so on, their significance is to be determined by empirically confirmed patterns and correlations shown in the data.[12] The method is, in short, an empirical strategy for theory construction.[13] However, theoretical conceptions could not be ignored altogether in the business of constructing indices. Let us take one example for extended discussion.

SOCIAL WHOLES VERSUS METHODOLOGICAL INDIVIDUALISM

As Durkheim had taken great pains to argue, the social sciences were not concerned with individual phenomena as such but with collective phenomena including, of course, those individual states of consciousness that were due to the operation of social processes. The social sciences dealt with groups of various kinds, institutions, cultures, whole systems of interaction and processes which are, so to speak, more than just the sum of individual phenomena but have, again as Durkheim would put it, a reality in their own right. Economics deals with institutions concerned with the production and distribution of goods; sociology with classes, groups, institutions, even whole societies; political science with governments, political parties, voting patterns, and so on. Yet, as we saw earlier in reference to mental states, such are not observable in any direct fashion. One cannot, for example, observe social classes, the economic system, capitalism, and the like, so what ontological status could such concepts possess? Again, as Durkheim asserted most forcibly about the reality of collective phenomena: 'Society is not a mere sum of individuals. Rather, the system formed by their association represents a specific reality which has its own characteristics.'[14] Social reality, in short, transcends that of individuals. As in nature, there occur in the social world, and definitive of it, wholes which are not simply aggregates of the individual elements composing them but are organic unities, more than the sum of their parts. Such emergent wholes cannot be reduced to the parts composing them.

A successful substantiation of this kind of claim, it could be argued, is necessary to the viability of the social sciences, and Durkheim did so argue for example, for without it the proper study of human behaviour, whether deemed to be social or otherwise, would be psychology or one of its branches. Philosophically, and as Lukes points out, the issue is an ontological one concerning the reality of social entities.[15] As we have seen, Durkheim claimed that social entities were real 'things' even though they were not material 'things'. However, operationally matters were not so easily resolved. The empirical evidence adduced for social facts was, primarily, derived from individuals. Only individual behaviour could be observed in any direct way, whether this be in the form of responses to questionnaires, attitude tests, participant observations, recorded rates of the frequency of criminal activity, rates of suicide, voting preferences, share buying, or whatever. In short, 'nothing about social facts is *observable* except their individual manifestations'.[16]

The paradox here seems firm enough: on the one hand, the claim that social wholes were real depended on it not being possible to reduce completely statements about them to statements about individuals; on the other hand, the evidence for the reality of social wholes seemed to depend almost entirely on evidence derived from observable individual manifestations of behaviour. Even though Durkheim, among others, had claimed to show that individual characteristics and behaviour varied with, or were determined or caused by, social contextual factors such as religion, social class, social status, and so on, the data on which such conclusions were based were always traceable back to their origins with individuals. Nor is the problem one of research technique since, after all, multivariate analysis, contextual analysis, correlation, etc., are all powerful enough *given* the assumptions that the chosen indicators do reflect, to put it crudely, the reality of collectivities in the first place; the problem is to license this interpretation.

It is undoubtedly the case that properties can be predicated of social wholes which cannot be predicted of an individual. A society or a group can be said to be stratified, hierarchical, democratic, class ridden, and so on, while the same characteristics cannot be said of an individual, at least not the same way. Groups, for example, can be said to maintain their identity in spite of the replacement of members. The character of the groups, too, can be shown to influence the behaviour of its members. In many legal systems some associations are treated as if they were persons having rights and obligations distinct from their members. Economists speak of, and even have theories about, the firm. However, these observations are, to a degree, beside the point. Though in legal and in ordinary language we can and do speak in this way, the issue is whether this is legitimate *scientifically*, and, if so, what ontological and epistemological justifications can be offered for so speaking? Answers to this affect the interpretations that can be plausibly offered for research operations supposedly measuring or indicating collective phenomena. There is one additional restraint for the positivist: an acceptable answer must eschew any implication of a 'group mind' or other metaphysical entities – the trap of reification into which Durkheim nearly fell.

Of course, the problem as set out does not require that a choice be made between the reality of social wholes or the reality of individuals; it is not, or need not, be a question of one or the other. To maintain the view that there are both individuals and social wholes, while accepting at the same time that the latter are not observable in any direct way, we need also to claim that, if anything is to be truly predicated of a social whole, this must imply the truth of at least several descriptions of individuals. Without this condition it would be impossible to test statements about social wholes by observation since these are not observable, though individuals are.[17] But, equally, the

description of social wholes, though implying several descriptions of individuals, must involve more than this; that is, it must mean that the set of relevant individual descriptions does not exhaust what may be predicated of the social whole. Thus, for example, 'British society' may be offered as the name of a social collective and a number of properties predicated of it, such as 'is governed by the Conservative Party', 'is a monarchy', 'has a low crime rate compared with X, Y, Z societies', 'has a rate of inflation of X%', and so on. The question is, however, whether each of these statements, while implying the truth of a host of statements about individuals – their behaviour in elections, in the market place, their obedience to the law, their attitudes and beliefs, and many more – is reducible simply to a listing of such individual statements however large in number? If not, then what is left that is not so reducible?

According to the doctrine of 'methodological individualism' nothing is left since all so-called collective facts are, in principle, explicable in terms of facts about individuals. References to social wholes or collectivities are, in this view, essentially summary references to the characteristics and properties of individuals, and the latter could replace the former without residue. The 'real', in other words, is restricted to what can be observed and these are the characteristics and properties of individuals. The most that can ontologically be claimed of social wholes, since they are never concretely given in observation, is a status as theoretical entities having explanatory convenience only.[18] Ontological reality is attributable only to individuals while social wholes are regarded as abstract or theoretical entities not observable but having an explanatory usefulness rather like similar kinds of theoretical concepts in physics and the other natural sciences.

For some this interpretation was tremendously important, since it seemed to bring the social sciences even closer to the practice of natural science in which a principle of reduction seemed to operate through a hierarchy of explanation from fundamental physical processes up to larger more global ones. Moreover, it appeared, too, to avoid the metaphysical lapses to which the social sciences seemed heir, such as reifying collectivities and attributing to them qualities which, properly speaking, could only belong to individuals and their relationships with each other. In so far as recourse was made in ordinary language to things like the 'spirit of the people', 'the racial memory', 'the mind of an age', 'class consciousness', 'the people', and so on, then either this was a careless way of speaking done for effect, or, at best, a conveniently summary way of referring to large numbers of individuals in some capacity, or at worst, unscientific and ignorant.

For others, however, 'methodological individualism' was too timorous and, furthermore, seemed to lead to a psychological

reductionism in which all so-called social facts, including those social properties and attributes of individuals, were ultimately reducible to explanations in terms of psychological dispositions. Durkheim would certainly have made this objection. Social wholes needed to be given a less ephemeral character than that of mere theoretical entities and given, instead, a conception more consistent with a view of them as real causal factors.

Of course, as hinted at earlier, methodological reductionism does not necessarily imply a psychological reductionism: that the only valid explanations of social life are those couched in terms of human psychological dispositions. Homans, for example, argued that sociology can be 'reduced' to psychology in the sense that its laws can be logically derived from those of psychology, just as the laws of chemistry can be deduced from the more general ones of physics.[19] For one thing, it can and often does claim that human action is, at least in important and irreducible respects, the outcome of interaction with others. That is, it acknowledges that there are 'emergent properties' arising out of individuals *interacting* with other individuals, properties that are not present in the individual alone. Interaction itself is one such emergent property, and all that flows from this, such as the possibility of power between two or more people, exchange, social status, cooperation, conflict, and many more. Indeed, in describing the actions of individuals we often have to make reference to their institutional status in order to understand the actions they are performing. The actions of a person towards his children cannot be understood without the relational description 'father', arrest by a person unless we understand the institutional identity 'policeman'. In short, the whole relational context that is social life is not reducible to psychological dispositions.[20] This does not, of course, dispose of psychological explanations as relevant to the explanation of human social behaviour; but nor is it meant to do so, it is merely to reserve places for the respective disposition of social and psychological explanations.

What does all this amount to methodologically? What are the implications of these views for social research? The problem occurs for the social sciences in the following way: 'individuals' and 'social wholes' are not discrete, separate phenomena, the latter being defined and conceptualised in large part in terms of the former, because only individuals, their attributes and behaviour are observable. If this is correct, then it is extremely difficult to establish, theoretically and empirically, the reality of social wholes independently of the reality already accepted for individuals. But, for the positivist, if an observational basis cannot be provided for social wholes then they are little more than metaphysical entities, and data presumably about such entities are masquerading as scientific data.

From the point of view of variable analysis, such debates seem beside the point. All that is needed are ways of indicating the properties of 'objects', be they individuals, collectivities, aggregates or even whole societies. Then it is an empirical matter of determining which of these indications, in any particular case, are the important determinative ones. After all, we can calculate rates for regions, for example, develop indices for group properties, and so on.[21] Unfortunately, the ontological claim is prior to choices of indicators since, presumably, the indicator has to reflect the properties of the phenomenon it is designed to 'stand for'. It is not that indicators cannot be produced but, having done so, what inferences does this entitle one to make about the character of the underlying phenomenon? If we lean towards methodological individualism, then the interpretation of the patterns produced will lead to one kind of theoretical interpretation than if we are persuaded of other types of conceptions. The empirical patterns of variable analysis will not resolve matters such as these. This is a problem to which we shall return.

THE STATUS OF THEORY AND THE NATURE OF GENERALISATIONS

It is fair to say that in the empiricist tradition of positivism the philosophical treatment of theory lagged behind the formulation and development of methods of empirical research. This is perhaps not surprising given the emphasis placed on empirical observation as the prime ingredient of science. Both Bacon and Mill, for example, and years apart, eager to exploit and advocate the method of experiment, regarded nature and its laws as there waiting to be discovered by the correct empirical methods.

It was widely regarded that the aim of science was to produce generalisations or laws stating the causal relationships which held between phenomena in the universe. Natural science had progressed by discovering invariant and necessary connections between phenomena in an orderly and lawful universe. Galileo, Newton, Darwin, later Einstein, and others, had each contributed a precise and universal statement as to how certain phenomena operated and, using these statements, scientists had an ability to predict events in the natural world with astounding accuracy. Such statements, it seemed, were universal in the sense that they specified that all events of a particular kind were invariably connected with other events and having the basic logical form of 'all A is B'. The problem was how to regard these statements. However, the idea that laws involved both invariance and necessity began to look less than straightforward. Invariance or regularity were less of a problem since departures from laws

could be explained by special circumstances. The real problem was necessity. As Outhwaite points out, the most obvious way was to see the source of necessity as inherent in the nature of things; yet others saw this as anthropomorphic or trivial.[22] Molière expressed the latter criticism in his mockery of the claim that opium makes people sleep because of its 'dormative power'. The positivist tradition, with its presupposition that empirical knowledge was the most fundamental knowledge in the foundation of science, was to give laws an empirical interpretation. In this it owed much to Hume and other philosophers of the British empirical tradition.

Hume argued that the idea of cause is no more than the outcome of repeated observations of one object following another, or one event following another event. For Hume ideas were impressions gained from the senses and his interpretation of cause was consistent with this point of view. Knowledge of causes was the result of sensation and habit. Reason alone, for example, could not arrive at the idea of heat causing water to boil, or of gravity causing bodies to fall, without experience to work on. To say that A causes B is to say that A and B are 'constantly conjoined' in our sensations; the causal connection being attributed to, but not observed in, nature. Through repeated observations of similar conjunctions one comes, by habit, to expect that they will always be related.

The idea of cause and effect is deriv'd from experience, which informs us, that such particular objects, in all past instances, have been constantly conjoin'd with each other: And as an object similar to one of these is suppos'd to be immediately present in its impression, we thence presume on the existence of one similar to its usual attendant.[23]

In some respect, however, this did not seem to go far enough. Universal laws were regarded as exactly that: universal both in time and place applying to the past, the present *and* the future wherever. Hume's reasonings though, by making causal generalisations the result of sensory experience, could provide no guarantee that the generalisations would hold in the future since they were based on evidence which could only be gathered in the past and the present. By definition, constant conjunction could not be observed in the future, and water might, in the future, boil at 80° rather than 100° centigrade. Hume's reply to this would be that, indeed, there could be no guarantee that such generalisations, even the well-established ones of science, would hold in the future as past experience would itself show. Nevertheless, we only have past experience on which to base future expectations, so this is all we can use. Accordingly, knowledge of empirical connections, of causes and their effects, is never certain but only probable; that is, we can never have absolute confidence in their repeated connection in the future.

A general causal statement, on this view, was a summary of our sensations of two sets of phenomena and constituted what is normally called an empirical generalisation. To determine causes we formulate categories of objects or events on the basis of their respective similarities. The relationship between them is observed, naturally or experimentally, and the sequence noted. If we find that in a large enough number of cases there is a constant conjunction of the putative cause followed by its corresponding effect, then we expect that this association will hold for the future, though there is no guarantee that it will. Thus we have our causal generalisation.

Later, J. S. Mill was to provide further arguments for the empiricist interpretation of laws. He defined concepts as referring to classes of objects which demonstrate a likeness with respect to some property. Man, woman, cow, girl, temperature, energy, catholic, etc., would all be concepts in Mill's terms because each word stands for a group of objects having similar characteristics. The method of relating concepts within synthetic propositions, (that is, propositions which are empirical as opposed to *a priori*) – and the only one relevant to science as far as Mill was concerned – he called 'induction'; that is,

that operation of the mind by which we infer that what we know to be true in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects.[24]

Whereas Hume justified generalising from particular instances on the pragmatic grounds that the future will not, likely as not, be unlike the past, Mill argued that the inductive inference could be made that the knowledge we have of some cases will be true of all cases at all times, past, present *and* future. This he justified by an appeal to the uniformity of nature, itself arrived at through an inductive process of reasoning in which the accumulations of inductions of individual uniformities in nature are the basis of the all-encompassing induction that nature is uniform. Induction was justified by induction.

Mill did, however, recognise that life was not quite so simple. In nature things did not appear related to each other in simple fashion. Small empirical regularities would overlap and give the appearance of irregularity, some would appear regular only because they were commonly produced by another not-so-visible causal agent, and so on. The various absolute causal regularities could only be found by systematically sorting out one uniformity from another using experimental methods of manipulation. These methods were his famous 'canons of proof', reasoning procedures that could be used to identify causal relationships from the mess in which the world often appeared. Briefly, the canons were the 'method of agreement', the 'method of difference', the

'joint method of agreement and difference', the 'method of residues' and the 'method of concomitant variation'. The end result of the application of these methods should be absolute causal generalisations, according to Mill.

However, even to the thoroughgoing empiricist this interpretation of the nature of laws had its weaknesses. Mill's methods were firmly based on the supposition that nature is uniform, lawful and causally interrelated and, therefore, the language to describe it had to be a causal one. There was little need to speak of theories. Although there are hierarchies of laws, at the pinnacle standing those of Newton, the ultimate ones are discovered, like any other generalisation, by application of empirical methods of inquiry. The source of all scientific law is empirical generalisation, a conclusion built upon the presupposition that nature is lawful and uniform. Lawfulness being, in other words, a characteristic of nature itself.

Modern positivistic and empirical thinking, however, has been critical of the naive interpretation of causal laws as typified by Mill's philosophy of science. Knowledge in science is certain, not probable. Accordingly, while admitting of the essentially empirical nature of laws, it was argued that certainty arose from the employment of the rigorous connections of deductive inference in mathematics and logic, rather than from induction. Thus, 'all swans are white', if interpreted as an empirical generalisation, has to be tested again and again on each new observation of swans. Such an inference cannot license inferences to the future, just as the statement, 'All British prime ministers are male' merely reports on past experience up and until Mrs Thatcher became prime minister, and could not have said anything about the future as a scientific law would. Pure empiricism cannot generate the universal laws of science. These, it was argued, can only be provided by logic where the determinativeness, the necessity, is a consequence of formal structure. The conclusion of a logical argument must follow from the general premises if the deductive rules are followed. This interpretation of scientific explanation, as a marriage between empirical conception and the certainties of deductive logic, became known as the 'hypothetico-deductive model' of scientific explanation.

On this view a scientific theory consisted of a set of statements connected by logical rules. The law was expressed as a universal statement of the form 'all A's are B's'. From this and other statements of 'initial conditions' a hypothesis was deduced which could be tested against empirical observation. An event was considered to be explained if it could be shown to be a logical consequence of the theoretical statements. This interpretation seemed to solve a number of problems, not least those involved in induction as the basis for the universality of scientific laws.

While statements of the form 'all A's are B's' cannot logically be conclusively proved or verified, they can be falsified by one counter instance of an A not being a B. Popper, for example, denied the possibility of using induction to arrive at a general law. The universality of the law cannot be a matter of probability either, since this would, in effect, say that the law was sometimes true, sometimes not. Nonetheless, scientific laws are empirical laws and subject to empirical confirmation, and involved in the method of testing is deduction. Scientific explanation is causal explanation in which 'the explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws; together with certain singular statements, the initial conditions'.[25] Scientific laws are causal statements describing events in nature and are capable of being true or false, their truth or falsity being determined by observation.

Another issue the combination of empiricism and logic seemed to resolve was one discussed earlier in connection with the observability, or lack of it, of social wholes. A theory, interpreted in the way just discussed, was clearly more complex than 'all A's are B's' would seem to imply. The theory may contain postulates and concepts which are themselves not subject to observational testing. Such concepts served a heuristic purpose within the theoretical language. So, although theories were still given an empirical interpretation, more room was allowed for non-observables, concepts not directly depending for their truth on a correspondence with the world. The formal structure of a theory was so complex and detailed that 'theoretical concepts' were often necessary for the convenience of logical and mathematical manipulation. It was no longer considered necessary for all concepts in a theory to have empirical meaning. One way of expressing this was to speak of a theoretical language and an observational language linked together by correspondence rules which interpreted some of the theoretical concepts empirically.[26] In this way the theory was still subject to empirical test through hypotheses deductively arrived at.

These moves away from the rather naive empirical interpretation of theoretical explanation as propounded by Mill and his followers did not, however, destroy the empiricist spirit: reinterpretation merely amended it to conform more closely with was what seen as natural scientific practice. For the social sciences this was a helpful development in that it licensed what are now orthodox research methods. The distinction between a theoretical and an observational language was important. So, too, was the account of the supposed certainty of science. The empiricist interpretation of scientific laws had claimed that they were only probable in the sense of being tentative and open to revision.

How, then, could the certainty be accounted for? According to the hypothetico-deductive account of scientific explanation it was the combination of mathematics-cum-logic and the essentially empirical interpretation of laws that gave laws their 'certainty'. This 'certainty' was a fiction, a convenient and helpful one to be sure, but a fiction nonetheless in that it could not conceal the tentative nature of scientific discovery. After all, it was a matter of historical record that scientific laws had been found wanting only to be replaced by newer and more effective ones. The history of science was a history of wrong theories. For the social sciences this was all to the good since their lack of success in formulating laws of even moderate probability could be attributed to the far greater complexity of social phenomena compared with those of inanimate nature. Social phenomena were also more difficult to measure with the kind of precision achieved in the natural sciences. All of which was taken as a sign that positivistic social science was at least on the right track by emphasising the development of more and more sophisticated *methods* of research and paying less attention to the questions of the theoretical basis of disciplines.

In this connection it is worth noting that Pearson, one of the founders of modern inductive statistics, argued that the precise, pristine laws of science are idealisations: the products of averaging and not descriptions of the real universe where all kinds of 'contaminations' are present.[27] Even in the most advanced of the natural sciences, all kinds of factors are present which affect the causal relationship of interest. The result is that data are prone to variability due to error of all kinds. Accordingly, the distinction between a causal relationship as expressed in a law and a correlation is a spurious one. Causation is simply the conceptual limit of correlation. In which case and on this argument, one distinction between natural and social science – that the former deals with causal relationships, the latter with correlations – falls down since all that this reflects are the conditions in which errors can be estimated.

Problems remained, however. Earlier it was pointed out that the hypothetico-deductive model of explanation required that theory be related to the world through transformation rules translating some of the concepts in the theory into observational concepts. The theory was dependent for its truth or falsity, irrespective of a verificationist or a Popperian falsificationist position, on the facts of the world. The world was 'external' to the theory; the theory did not shape the world but could only be responsive to it. The importance of a neutral observation language was precisely in this, despite the fact that the idea of such a language proved troublesome. The transformation rules also proved equally refractory and boiled down to what came to be widely known as the 'measurement problem'.[28]

Among the positivist solutions to this were various measurement models presumed to apply to social research data and the contexts in which they were gathered. One influential doctrine was 'operationalism' which was based on the assumption that the categories used in empirical research were best defined in terms of the operations used to measure them.[29] Thus, on this doctrine, the concept of IQ is defined as that property measured by IQ tests; similarly concepts such as class, status, power, authority, and so on, would be defined by the indicators used to measure them. Such measures could be, and indeed are, used in statistical analyses of data. Once again, operationalism embodied an empiricist conception of the nature of concepts which did not meet the hopes invested in it. One difficulty was that, as strictly conceived, operationalism created acute problems of validity. Though, strictly speaking, one could not ask what a test *really* measured since the measure *was* the concept, questions of validity did arise. For one thing, different measures of phenomena, such as IQ, could be said to be measuring different things since they were different measures. Similarly, different measures of social class or social status would be measuring different things. Clearly this was not a satisfactory situation since often measures had to be different for very good practical reasons and, yet, researchers still wanted to generalise to all instances of the phenomenon, whatever it might be, despite problems of having to use different measures. Also, even a weak operationalism, that is, one which did not claim that concepts were the measurement operations themselves but, instead, took the doctrine as a useful imperative to guide social research, still led to the problem of relating empirical concepts to theoretical ones.[30]

While measurement procedures in a number of the social sciences are extremely sophisticated, as are those methods of quantitative data analysis, there remains relevant the important question about the theoretical relevance of such techniques.[31] Most have been designed to exploit the principle of association or correlation very much in the tradition of Mill's canons of inquiry, the aim being to measure concepts at a sufficiently high level to meet the assumptions of correlational techniques first developed in genetics at the turn of the century. While the use of such techniques have resulted in any number of empirical generalisations, none has been so far offered as a causal law. Social science has produced a catalogue of associations between any number of variables; between, for instance, class and educational attainment, educational attainment and mobility, class and voting choice, class and mental illness, religion and voting choice, the degree of industrialisation and domestic political violence, and so on and so on.[32] All range from weak to strong, none is perfect, a fact attributed to various kinds of measurement error

and the difficulty of controlling for all possible factors. But, some questions arise: What do such generalisations amount to? Are they 'proto-laws' from young and immature disciplines which, nonetheless, could serve as a basis for sounder laws?

Let us first take the question of the nature of such generalisations. Such associations are normally derived from a sample of some population, the measures of association summarising the relationships among the variables in that sample. In any sample any number of such associations could be produced between all kinds of 'disparate' phenomena, so they summarise relationships among those variables felt to be important enough to be considered. So, how is the decision reached as to what to include within a study, given that it is impossible to include everything? The hypothetico-deductive model would suggest that the theory dictates what should be included, what variables should be examined, what variables should be controlled, and so on. Mill himself, although as thoroughgoing an empiricist as one is very likely to find, did not dismiss the importance of hypotheses as necessary if one was to apply any of his methods of inquiry and derive verifiable consequences of the laws themselves. But for Mill, all hypotheses were suggested by experience and capable of being true or false. If we accept this it is still not quite clear how associations between variables could be said to be theoretically relevant. What are we to do about a less than perfect association or correlation? Does it prove or disprove a theory? Alternatively, should we want to say something a little weaker: that it 'lends support to' or 'is not entirely consistent with'? In fact, the interpretation of such associations is usually a *post hoc* affair in spite of the obeisance made to the hypothetico-deductive model's espousel of the test of prediction. All kinds of rationalisations, some more plausible than others but still many plausible enough, are entered into to make the associations theoretically interesting. That classic of positivistic social science, Durkheim's study of suicide, contains many generalisations summarising the correlations between marriage and suicide, religion and suicide, urban living and suicide, and more, while the remainder of the analysis consists of interpretations and arguments, many of them shrewd, ingenious and insightful, elaborating *post hoc* rationales to explain what it is about the correlated phenomena that leads to suicide.

Can such associations be regarded as proto-laws? An affirmative answer to this questions looks remote since what has been said so far points to the conclusion that no empirical generalisation can logically ever be a law. Indeed, not all statements of the logical form 'all A's are B's' can be treated as lawlike in the sense required by science. 'Nomological generalisations', for example, support subjunctive and counterfactual conditional

statements whereas empirical, or 'accidental generalisations' to use a phrase of Brown's, do not.[33] For example, the law concerning the effects of dissolved solids on the boiling point of a liquid entitles a subjunctive conditional such as, 'If this solid salt were dissolved in this pan of boiling water, then the boiling point would be raised.' The law, along with statements about the initial conditions stating that the law is applicable in this particular case, entitles us to make such a statement. Similarly, it lends support to counterfactuals such as, 'If this piece of solid salt had been dissolved in water – though in fact it had not been – the boiling point of the water would have been raised.' In short, 'nomological generalisations' or laws allow us to make inferences about cases that do not now occur, have not occurred in the past and may not occur in the future. They state hypothetical relationships of invariable connection irrespective of whether or not the relationships is actually exemplified.

None of these characteristics applies to 'accidental generalisations'. The generalisation that all the people in this room are under 6 feet tall does not entitle the inference that any future incomer to the room will be under 6 feet tall. Although a number of 'accidental generalisations' may have always held in fact, at all times and places, this will still be, as Brown puts it, 'a happy accident and not a consequence of there being a law-like connection between the properties in question or, more basically, of there being a scientific theory from which the generalisation can be derived'.[34] That is, in the absence of any scientific theory to preclude the appearance of anyone over 6 feet tall entering this room, we have no basis for the kind of inferences we can make using nomological generalisations.

The question is, however, whether 'accidental generalisations' – and the word 'accidental' is perhaps unfortunate here since they are certainly not trivial or unimportant – or nomological ones are the kind of generalisations produced by the Lazarsfeldian-type methods of social science. Brown claims that no clear line can be drawn between 'accidental generalisations' and empirical–universal generalisations since both are based on observational processes rather than theoretical ones. They are generalisations based on observed regularities, unlike theoretical laws, and, as a consequence, their explanatory scope is limited.

Suppose, for example, after intensive studies of samples of individuals we find a high positive correlation between the number of siblings in a family and poor educational performance. What kind of generalisation would this be? An 'accidental' one or an empirical one, or what? It is hard to say since the case could be made for both. This, however, is not really the issue. If we wanted to use the generalisation to *explain* why little Johnny down the road with twelve brothers and sisters is not doing very well at

school, it might indeed be offered as an explanation. But is this good enough? What about other factors which may play a part? How do we know that it is the number of siblings that causes the poor performance rather than, say, the poor school, little Johnny's passion for fishing, his dyslexia, or whatever else might characterise little Johnny's life and circumstance? Could, to be brief, little Johnny's poor educational performance be deduced from the generalisation? The answer is no, and for two major reasons. First, unlike the laws offered in natural science, the *ceteris paribus* conditions under which the applicability of the law is judged, in this example and in most real-life social science examples, are indeterminate – to say the least. Second, the lack of a theory from which to derive the generalisation, along with some statement of the conditions under which the theory will apply, means that any application will have to be determined *post hoc*. Though the mechanisms involved here have intuitive plausibility – for example, large families means less time for study, less parental attention for any one child, sibling rivalry, and so on – this *ad hocing* process is not quite what is to be expected from a scientific theory and observations that might be deduced from it. Moreover, there are, in fact, any number of theories that could explain little Johnny's poor educational performance, some consistent with the generalisation but many not so, and for which the empirical connection between number of siblings and educational performance is an irrelevance. Third, since the generalisation is drawn from samples, all we have is a statistical generalisation stating that a property (number of siblings) is associated with another property (educational performance) in a particular direction and size. From this, nothing follows about any *particular* instance.[35] A deductive conclusion cannot be found, only an inductive one. Premises made up of such generalisations cannot logically imply a conclusion, only lend support.

In this respect Lieberson offers an illuminating example.[36] He asks: how might social researchers go about studying the question of why objects fall? He visualises a study, based on an analogy with the typical type of social research study, in which a variety of objects is dropped from a height without benefit of strong controls such as a vacuum: a condition, to repeat, which parallels most circumstances in social research where controls, such as they are, enter *post hoc* at the data analysis stage. If the objects differ in the time they take to reach the ground, the question becomes What characteristics of the objects determines this difference? Air resistance in the absence of a vacuum, the size and density of the objects will, on the face of it, affect the speed of the fall. Assume that these factors, even including others, taken together account for all the differences in velocities of fall between the objects.[37] In a social research context, likely as not, it would be concluded that

a complete understanding of the phenomenon had been achieved since all the differences had been accounted for. But, of course, the point of the example is that we would not have come up with the idea of gravity. What is wrong here? As Lieberson puts it, data on the *phenomenon* of interest are not necessarily relevant to the *question* of interest. So, an analysis of the rate of fall of various objects might tell us why they differ in the rate of their fall, not why they fall. What we would not have available is the power of the theory of gravity and its statement of the constancy of the rate of acceleration of falling objects to deal with many of the applications for which it is employed.

An important consideration here, and one fully acknowledged by Lazarsfeld in pursuing the ideas of variable analysis, is the non-experimental character of social research. Without the ability to effectively make *ceteris paribus* assumptions about the effects of unwanted factors, identifying causal relationships where 'contamination' by multifarious influences is ever present, is likely to present fundamental difficulties to researchers. As said earlier, in variable analysis controls are normally employed at the data analysis stage, the aim being to see how much of the variability of the values of the dependent variable is accounted for by one or more of the independent variables. Again, as indicated earlier, for such as Pearson this is entirely what the problem is, namely, finding those variables which account for *most* but not *all* of the variance. For him there is simply no point in trying to add causes together until all the variation has been explained. The complete elimination of variability in real world observation is a chimera. It is only highly correlated variables that matter.

Unfortunately for this kind of conception there are serious technical flaws, quite apart from the ones suggested by Lieberson's example. Turner points out, for example, that both the metaphysics underlying this conception of social research and the statistical techniques used to implement it, fail to realise that theories remain underdetermined.[38] Not only is there no attainable goal of the complete elimination of variability, there is usually more than one way of adding or combining variables to the point of redundancy, assuming that this is capable of plausible definition, irrespective of the fact that there is more than one choice about the way in which the variables can be measured. Turner points out that no logical relationship can hold between theoretical claims and generalisations based on statistical data largely because of the assumptions about the order of variables, their completeness, their linearity, or otherwise, and their independence; all essential to the mathematics of statistical modelling, and which always make the generalisations assumption-relative.[39] It is this feature which directs us back to theoretical considerations.

POSITIVISM AND SCIENTIFIC THEORY

The positivist conception of scientific knowledge with its emphasis on observation and empirical method to the relative neglect of theory turns out to be a less than adequate reflection of the natural science logic which it extolled. Certainly in social science the generalisations produced by most current empirical methods do not begin to look like the laws offered by natural science: a fact more often than not attributed to the relative immaturity of the social sciences and the relatively greater complexity of social phenomena compared with the subject matters of the natural sciences. However, what is also clear is that there was much wrong with the positivist conception of science itself.

One persuasive view on this matter takes issue with the predominantly empirical interpretation of scientific laws and theories to be found in positivism. As a system of knowledge empiricism, of which positivism is an important part, is to be found in many diverse activities of widely varying content, from primitive magic to modern technology. Its main and distinguishing feature is that it relates observable to observable.[40] Effectively it is a system of trial and error and no less important in science for this since it can, and does, lead to effective results which, in their turn, lead to routine procedures in investigative work. By contrast, rational thought deals solely with the theoretical, connecting idea to idea, and is characteristic of logic, mathematics and also of some metaphysical systems of thought. Science shares characteristics of both but in a very different way to that presupposed in positivism. Science is concerned with empirical connection and, like logic and mathematics, with the rational connection of idea to idea, but it is through the 'abstractive connection' of theoretical concepts with observations that these concepts are given empirical import. A scientific explanation uses determinative laws and not laws interpreted as general causal statements as positivist thought had it. The determinativeness of its rational connections is what gives greater precision to scientific knowledge.

To illustrate.[41] A relationship between cold weather and cracked car radiators can easily be established using empirical methods. In such a case the connection is made as a result of repeated observations and, Hume would add, habit. Such an explanation, making use of the empirical connection between cracked radiators and freezing weather, can be adequate for its purpose, especially if the aim is to avoid cracked car radiators. A scientific explanation, on the other hand, might begin with the idea that under perfect elasticity, stress is equal to strain. Then an attempt would be made to determine a value for the limit of elasticity for radiators by measuring the amount of force applied before the radiator cracks. By measuring air temperature and that of the water at night the

point at which the water freezes to produce enough ice to apply the stress that would bring the radiator to its strain limit can be determined. A stress greater than the strain limit would break the radiator. In this case a law is being used in which stress is equal to strain under conditions of perfect elasticity. The purpose of the law is to derive a measure for the strain limit by applying stress, and to compare the calculated stress at the time of breakage with that limit. In so far as the value of the strain limit was determined from a calculation of stress, it is difficult to see how the laws could be proved false in such an application. The exact calculation of a limit could not have been arrived at empirically. Even though temperature could have been measured as precisely as possible using empirical methods, and the generalisation offered that the colder it gets the more likely the radiator is to break, this could not result in the calculation of a limit. It may result in a probability distribution, but this will not tell us whether or not the radiator will crack. The scientific laws can point to a measurable condition under which breaking will occur; an empirical generalisation only that it will break with a certain probability.

Science and empiricism differ, too, in the way in which they transcend particular instances. The latter does so by generalisation, that is, applying a name to a set of similar objects so forming a particular category through the observational process; car, tree, society, male, female, and so on. These are then related to other empirical categories by means of such methods as correlations. Science, on the other hand, transcends the particular case by abstraction, by a process of selection and not by the summation of similar characteristics. Indeed, observational phenomena abstracted in this way may bear little obvious similarity to each other. Billiard balls are not like rockets, to use another example from Willer and Willer, but both may be abstractively connected to the concepts of the same laws of motion.

The meaning of abstracted concepts is derived not from the similar appearance of objects but from their relationship to other concepts within the theory. The process of abstraction is, in effect, one of conceptualising observations so that they may be deterministically related to other concepts. At once an infinite universe is provided as a conceptual framework 'for the theory. The rational connection between the concepts in the theory is not like a causal connection at all. We may well use the relationship $d = vt$, and use it often to build speedometers, measure distance travelled, and so on, but we do not observe distance to discover if it is, in fact, velocity multiplied by time. *vt tells* us what distance is in terms of time and velocity.

Abstraction in science moves back and forth between the empirical and the theoretical expounding and sharpening the scope of application of the theory and its explanatory power: a

matter of establishing an isomorphism between theoretical terms and empirical observables. This may be aided by manipulation under laboratory conditions, constructing models to fit particular cases, changing empirical conditions and varying models, and so on. As far as the abstraction process is concerned, and unlike in the case of generalisations, there is no problem about how similar is similar since the theory and its model(s) are deliberate constructions, even inventions, to fit and translate the theory to apply to particular cases. Indeed, theories may be applied to a large number of cases, as the law of falling bodies applies to anything that falls or flies. This is not the case with empirical generalisations. Lack of success in the case of a theory does not mean that the theory was false: it may instead indicate a limit to its scope, or mean that an abstractive error has been made.

A very different conception of measurement is embodied in this view of scientific explanation. For the positivist, measurement is effectively a matter divorced from theory. Accordingly, the so-called 'measurement problem' in social science has largely been seen as an effort to scale all kinds of variables, from the macro structural variables to the affective, trying to give them the kind of precision and exactness felt to be characteristic of measurement in science.[42] Energy was devoted to constructing 'indices' for theoretical concepts, the aim being to connect the theory to the empirical world of observables by the use of essentially empirical techniques. However, on the abstractive view of the connection of theory to empirical observables, it is measurement which gives a theoretical concept empirical interpretation. Measurement orders data, not the other way around, and is very much a consequence of theory. 'Length', for example, in a scientific theory has a purely theoretical meaning determined by the postulates and laws of the theory. The concepts that are measured are chosen as a consequence of these postulates and laws and can be empirically interpreted in many different ways according to circumstances. The application of a theory to a broad range of phenomena gives rise to very different empirical interpretations. As Pawson points out, in science the

objective of measurement is to incorporate and embody within an instrument, principles derived from theoretical science. Instrumentation is thus seen as a branch of engineering and engineering is nothing other than the application of the laws, theories, hypotheses and principles of theoretical physics . . . the incorporation of theory into the observational domain is seen not as the problem, but as the true justification of measurement.[43]

Temperature can be measured, for example, using an ordinary mercury thermometer or, with very cold objects, by means of the resistance to an electric current. In both cases, the measurement is

the direct result of the laws of thermodynamics applied to different domains, the expansion of liquids in the one case and electrical conductivity in the other. Measurement of a rigorous scientific character is impossible without a strict theory specifying the strict numerical relationship between concepts.

If the views just summarised are correct then the positivist conception of scientific knowledge must go by the board. It emphasised some aspects of science, particularly its empirical character, at the expense of failing to see the significance of others, especially that of theory. Science is empirical but it is also profoundly theoretical; indeed, perhaps a more convincing case could be made that science is more interested in theory than it is in the empirical. Laws, the aim both of positivism and science, are not causal empirical generalisations but rationally connected statements. True enough, in their infancy, some sciences may well proceed in a more empirical manner by correlating observables with other observables, but matters do not end there if it is to progress. Positivism did suspect that rational connection might be more important than some of its adherents would allow and the hypothetico-deductive model of explanation, for example, was an attempt to rationalise the importance of logic and mathematics, but firmly within the empiricist framework.

However, although it may well be accepted that the positivist view of science was misconceived, this is not to say that the methods it authorised as the methods of social science are also entirely useless. It may be that they are not scientific, either in the way that positivism understood or, indeed, in terms of the view just outlined, but does this imply that they are useless as a form of knowledge? Further, it has also been argued that the hypothetico-deductive, or covering law, model of explanation is not a useful one for social science to follow given that, very often, they are more concerned with discovery than with explanation.[44] Genetic explanations, typical of history for example, are concerned to show how certain events came about and, here, there is no explicit reference to laws as such but the deployment of an explanatory narrative.[45] Nor, of course, need it be the case that the hypothetico-deductive model is an accurate rendering of the scientific method. It may be, too, that there is no one method for science in the way in which positivist philosophy understands this. These are, obviously, large questions, some of which I want to take up more directly in the final chapter. For now, one or two remarks are in order. One implication that could be drawn is that the intellectual authority for such methods can no longer be in terms of a positivistic conception of science, or that by using such methods the social sciences are aping the natural sciences. In so far as such methods deal with the production of empirical generalisations, they will be subject to the kind of logical

constraints pointed out earlier, but, to reiterate, this is not to say that such generalisations are of no interest.

There are also implications for interpretations of the nature of social scientific theory, most of which become less than scientific. Even within a positivistic framework the relationship of theory to data was a troublesome one. Theory was supposed to be dependent for its truth on the 'facts' of the world which were external to the theory itself. The theory did not shape the world, but was responsive to it. The importance placed on developing a theory-neutral observation language lay precisely in this. However, many of the candidates for theory in social science were, and still are, rejected on extra-empirical grounds. In the 1960s, for example, the great theoretical debate in sociology was between conflict theories and functionalism.[46] Functionalism was attacked because it seemed to ignore the fact of conflict in social life, whereas one of its major aims was to examine the causes and consequences of conflict within a set of concepts stressing the systemic nature of society. Each side of the debate, however, effectively talked past each other. Something other than the scientific status of the respective theoretical positions was at issue, having much to do with what the connotations of concepts like 'conflict', 'stability', etc., carried with them about familiar events and processes in historical societies. Such debates might better be seen as quarrels about how the social world ought to be looked at and less to do with the scientific value of such theories.[47] This brings us to another general point about social scientific theory and one which will be discussed more fully in the next chapter.

Positivism, with its stress on the idea of a neutral observation language, empirical generalisation, and so forth, was disinclined to concern itself with the origin and source of theories. This is illustrated by the relative lack of interest shown in the matter of scientific discovery which was relegated to a sideshow beyond serious philosophical concern. Of much greater importance was the matter of verifying theories once formulated. The discovery of theories was a matter of conjecture on the part of scientists and their imagination, fancy, even induction and speculation, but certainly beyond formal logical description. What could be described as a logical process, it was argued, was the confirmation and testing of theories. To this extent theories had to conform to certain formal criteria in order to be capable of test against the 'facts' of the world. However, although this emphasis might have seemed excusable or justifiable in connection with natural science theories, it is less so with reference to the social sciences. The very notion of a domain of inquiry, whether it be sociology, economics, physics, chemistry, history or whatever, presupposes some conceptual schema ordering the world as a prelude to the

observation of the relevant facts. This is what Durkheim, for example, was insistent on establishing, namely, the conceptual distinctiveness of sociology as an autonomous discipline with its own domain of facts, facts which gain their importance and significance because they are distinctively social. In other words, the conceptual ordering necessary to identifying a species of facts begins to challenge the idea that observation is entirely a theoretically neutral affair. It suggests that the knower is an active constituent in the construction of knowledge. On this view scientific theories become like inventions actively engaged in creating a reality, not passively waiting for their substantiation by the facts of the external world. Indeed, much of social scientific theory is underdetermined by the facts of the social world in the sense that no 'strategic experiment' is conceivable that could decide between them. Rather, such theories are better seen as conceptual schemes stipulating, even legislating, what the domain of fact might be.

One final point. Although the positivist conception of science has been shown to be seriously flawed, this is not the same as saying that the social sciences cannot be scientific within another interpretation of science. This issue will have to be dealt with, but before doing so it is necessary to bring some of the debates about the nature of science up to date.

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1. Kuhn, T. S., *The Structure of Scientific Revolutions*. 2nd edn, Chicago, University of Chicago Press, 1970. Kuhn was referring to natural science and it is a moot point as to whether the social sciences are, or could ever be, 'normal' in the sense offered. Nevertheless, positivistically inspired social science is, perhaps, as near as could be got to 'normal science' in the last few decades. See next chapter for fuller discussion of some of Kuhn's ideas.

2. See, for example, Ryan, A., *The Philosophy of the Social Sciences*, London, Macmillan, 1979; Lessnoff, M., *The Structure of Social Science*, London, Allen and Unwin, 1974; Popineau, D., *For Science in the Social Sciences*, London, Macmillan, 1978; Doyal, L. and Harris, R., *Empiricism, Explanation and Rationality*, London, Routledge and Kegan Paul, 1986. See also Bernstein, R. J., *The Restructuring of Social and Political Theory*, Oxford, Blackwell, 1976, for an excellent review of some of the philosophical influences on major social theories. See Anderson, R. J. *et al.*, *Philosophy and*

the Human Sciences, London, Croom Helm, 1986, for a review of most of the major philosophical schools of importance in the social sciences.

3. See Anscombe, G. E., 'On brute facts', *Analysis*, 18, 1957–8, pp. 69–72; Taylor, C., 'Interpretation and the sciences of man' in Beehler, R. and Drengson, A. R. (eds), *The Philosophy of Society*, London, Methuen, 1978, p. 160.

4. Carnap, R., *The Logical Structure of the World*, London, Routledge and Kegan Paul, 1967 (first appeared in German, 1928), for example, gives an account of the whole apparatus of empirical discourse in terms of remembered similarity between sense impressions. These are the basic elements out of which are constructed, with the help of logic, the concepts of material things, other minds, social institutions. Topics of thought stand in various levels each reducible to the one preceding it. Higher level statements are justified by induction from statements of levels below; basic lowest level statements need and can have no inferential justification. At this point the system of beliefs makes contact, through observation, with the world of empirical fact.

5. Many members of the Vienna Circle, as it was known, went to the United States prior to the Second World War and had a major impact on the philosophy of science there, as well as philosophy generally.

6. See the selections in Ayer, A. J. (ed), *Logical Positivism*, New York, Free Press, 1959.

7. Toulmin, S. and Goodfield, J., *The Discovery of Time*, London, Hutchinson, 1963, gives a good discussion of Darwin's contribution to geology and biology.

8. Neurath, M., 'Empirical sociology; the scientific content of history and political economy' in Neurath, M. and Cohen, R. S. (eds), *Empiricism and Sociology*, Dordrecht, Reidel, 1973, pp. 319–412.

9. As Smelser wrote of the language of science: '. . . the language of the ingredients of science; independent variables, dependent variables, theoretical frameworks, and research methods' (*Essays in Sociological Explanation*, Englewood Cliffs, Prentice Hall, 1968, p. 43). It is important to note that he is, in fact, referring to the language of social science not physical science where talk of variables is rare. One textbook gives the following instruction: 'it is necessary to translate your ideas . . . into the language of variables . . . The experienced sociologist develops the habit of routinely translating the English he reads and hears into variables, just as a bilingual person can read one language while thinking in another' (Davis, J. A., *Elementary Survey Analysis*, Englewood Cliffs, Prentice Hall, 1971, p. 16).

are known and measurable factors not assumptions necessary
to deploy statistical models.

40. Willer, D. and Willer, J., *Systematic Empiricism*, Englewood
Cliffs, Prentice Hall, 1973, p. 16.

41. *Ibid.*

42. Duncan, O. D., *Notes on Social Measurement: Historical and
Critical,* New York, Sage, 1984, has much to say on the
so-called exactness of measurement in the physical sciences
and, at even greater length, on the efforts of the human
sciences to emulate this. However, the point being made is
not affected by a modest scepticism about exactness.

43. Pawson, *op. cit.*, pp. 106–7.

44. Brown, *op. cit.*; also Outhwaite, *op. cit.*

45. But see Nagel, *op. cit.*, on the nature of historical explanation
for an alternative view.

46. See, for example, Rex, J., *Key Problems of Sociological
Theory,* London, Routledge and Kegan Paul, 1961.

47. Anderson *et al., The Sociology Game, op. cit.*, for a criticism
of perspectivalism in sociology.

C H A P T E R   4

Positivism, theory and science

In this chapter I want to take a rather wider perspective on issues in
the philosophy of science, but one which will return to some of the
themes raised in the previous chapter. Once again, the beginning is
that ambition bequeathed by nineteenth-century positivism to the
social sciences, namely, to become scientific in the same fashion
as the natural sciences. As said previously, the vision of science
was very much a philosophically inspired one generating no little
philosophical argument.

I shall begin from a summary recapitulation of the hypothetico-
deductive model of explanation.

THE HYPOTHETICO–DEDUCTIVE MODEL OF SCIENTIFIC EXPLANATION AGAIN

On this view a scientific theory consisted of a set of statements
connected by logical rules. The law was expressed as a universal
statement of the form 'all A's are B's'. From this and other
statements of 'initial conditions' a hypothesis was deduced which
could be tested against empirical observation. An event was
considered to be explained if it could be shown to be a logical
consequence of the theoretical statements. In other words, the
truth of the explanandum is, in crucial part, guaranteed by logic,
– as long as the explanadum is logically deducible from empirically
true statements about the initial conditions and the general laws.
If the scheme is used retrospectively it gives explanations; used
prospectively it provides predictions. If the universal law is true
then the prediction will be confirmed; if not, then the prediction
will fail. This interpretation seemed to solve a number of prob-
lems, not least those involved in proposing induction as the basis
for the universality of scientific laws. Scientific laws are empirical
laws subject to empirical confirmation, and involved in the method
of testing is deduction. Scientific explanation is causal explanation
in which 'the explanation of an event means to deduce a statement
which describes it, using as premises of the deduction one or more
universal laws; together with certain singular statements, the initial
conditions'.[1] Scientific laws are causal statements describing events

in nature and are capable of being true or false, their truth or falsity being determined by observation. There was no need for any inductive process or, indeed, for any metaphysical refuge in appeals to the uniformity of nature.

The hypothetico-deductive model seemed to offer a characterisation of scientific reasoning that social science could live with and emulate. Further, the adoption of such a mode of reasoning, even at the modest levels the social sciences could realistically achieve, would place the social sciences firmly within the science camp. In other words, the schema served as a criterion definitive of scientific forms of knowledge. However, in the hands of Karl Popper, matters did not turn out quite so straightforwardly.

POPPER'S FALSIFICATIONISM AND THE ROAD TO THE SOCIOLOGY OF SCIENCE

Earlier I drew attention to arguments showing that inductivism failed as a justification for the truth of scientific theories and generalisations. Popper agreed. The classical model of induction could not logically escape the uncertainty of succeeding observations. Scientific knowledge cannot proceed by the verification of theories by means of empirical testing but, instead, has to rely on a critical method of 'bold conjectures' and attempts at refutation. His philosophy of science, however, is more than just a criticism of inductivism; he was also vitally interested in looking for what was distinctive about the scientific method. Not all forms of knowledge are scientific and, like Logical Positivism, he was interested in developing a demarcation criterion that could distinguish 'science' from 'pseudo-science'. Inductivism failed to do this since many activities laying claim to scientific status – such as astrology, which Popper did not want to count as science – relied upon induction. Not only had it failed to provide an adequate justification for the truth of scientific generalisations, but it had run the risk of allowing into the collection of scientific disciplines such activities as astrology, psycho-analysis and Marxism, to mention but three which earned Popper's disapproval. Unlike the Logical Positivists, however, the issue for Popper was not one between verifiable science and nonsensical metaphysics, but between science and pseudo-science.[2]

The 'demarcation criterion' Popper offered was that of falsification. Though no amount of observations could ever confirm a generalisation of the form 'all A's are B's', one counter instance of an A not being a B would disconfirm the generalisation and this, Popper argued, is in fact the characteristic method of science; that is, to seek the disconfirmation of a theory's predictions. Scientific

theories state the conditions for their failure as theories, whereas the theories of pseudo-sciences do not. Astrological theories, for example, are unfalsifiable and, therefore, not scientific.

In this way Popper revised the orthodox positivist conception of science. The object of science is not to infer from specific instances to generalisations but to search for ways of rejecting what he calls 'conjectural hypotheses'. Science is not a body of accumulated and accumulating true theories but a collection of conjectures which have yet to be refuted: science is a 'system of guesses or anticipations which in principle cannot be justified, but which work as long as they stand up to tests . . .'[3] Further, it is those theories which make very precise predictions, and accordingly are more likely to fail with one crucial experiment or test, which are the best.[4] The ability of theories to withstand tests, their 'corroboration', is related to the improbability of their predictions. The best theories, like Einstein's General Theory of Relativity, provide for very precise predictions across a range of tests and, therefore, have a high empirical content. It is those theories, which are unfalsifiable-in-principle, which are virtually devoid of empirical content. Science is, as a consequence, ruthlessly competitive forever seeking to destroy or refute its conjectures, even its best ones. It is by critical trial and error that science proceeds, discarding those theories that fail to match up to the tests and hanging on to those, at least for the time being, that have passed the best tests that can be currently devised for them. As long as 'we admit that there is no authority beyond the reach of criticism to be found within the whole province of our knowledge . . . then we can retain . . . the idea that truth is beyond human authority'.[5] This 'evolutionary epistemology' is no different, for Popper, to the way that all forms of life adapt, or fail to adapt. Of course, there is always the risk of holding on to an unsound theory or, for that matter, abandoning prematurely a good one. But these risks science has to live with since, as Popper admits, there are non-logical criteria involved in the selection and promotion of scientific theories. As studies in the sociology and the history of science have shown, there are many reasons why theories are often held on to or thrown over, other than for strictly scientific criteria, including such prosaic matters as personal preference, career advancement, religious conviction, etc.;[6] but, for Popper, although such things are an ineradicable feature of the social history of science, they are not part of its logic, and it is this with which Popper sees himself as primarily concerned. The only defensible concern of epistemology as the theory of scientific knowledge is with regard to the actual procedures and products of science. Science aims at truth in the sense of correspondence with reality, yet we can never conclusively demonstrate that our conjectures are true. Rather truth is tested

by eliminating falsehood; 'we are seekers of truth but we are not its possessors'.[7]

Nonetheless, though Popper's criterion of falsifiability is intended as a logical one, reservations have to be expressed as whether its point is descriptive or prescriptive. If the former, then as an account of how science works it is clearly deficient. If the latter, then falsificationism does not only rule out well-known and well-respected theories in the human sciences, it has the same effect on a number of natural scientific theories including, for example, Darwin's theory of evolution. As far as Popper is concerned, theories must be predictivist: it is the predictions that lend theories to falsification. Heuristic theories are not allowable. Further, no notice is taken of the immense amount of taxonomic work fundamental in many sciences. These issues apart, even as an account of the logic of science, Popper's is idealised paying little attention to the other than strictly logical reasons scientists might have for accepting and rejecting hypotheses. This matters if Popper's criterion is invoked prescriptively since it no longer just describes the difference between 'proper' science and pseudo-science, but begins to stipulate how science ought to be done.

As far as the social sciences were concerned, the first impact of Popper's work was devastating. The requirements of falsification effectively ruled out of the court of science many tried and trusted social science theories because they could not state the grounds on which they could stand refuted. As far as Popper was concerned, all they offered was ways of looking at, or perspectives on, social life; they were not *scientific* theories. This aspect of his work was developed in his vehement arguments against collectivist views of society, such as those of Marxism, as inviting not more freedom for the individual but less. Any attempt to impose equality as a major social organising principle would, likely as not, produce tyranny.[8] The arguments are connected, and strongly so, to Popper's sense of the limitations of human knowledge and, in this respect, his suspicions of those inspirations of social science, of which Comte was a precursor, which see it as a way of enhancing the rational intervention in human society to ameliorate its evils. For Popper such an ambition, if conceived holistically, is to invite tyranny. Scientific knowledge is a matter or trial and error and this can only be institutionally realised in an 'open' society where a plurality of points of view can compete with each other. Such a process requires that criticism flower, argument thrive, dissent flourish, and these cannot do so in 'closed' societies.

The hypothetico-deductive schema, whether interpreted in verificationist or falsificationist terms, has been a powerful idea in the philosophy of science, though not without its critics. It was intended to avoid the philosophical difficulties of inductivism but

also, sometimes inadvertently, while concerned to maintain the rationality of the *method* of science did, at the same time, highlight the importance of the history and sociology of science if only, to put it in Popperian terms, to understand which theories entered the evolutionary race.

Popper's intervention, however, raised a number of issues which transformed the debate on the nature of science and the scientific method. Though Popper himself rejected the charge of relativism, the claim that science could only, at best, obtain 'successive approximations' to truth along with the view that observations are invariably theory impregnated, do invite relativist conclusions. Popper made two important claims. First, that the logical method of science is falsification. Second, that science progresses through trial and error, by an evolutionary epistemology incorporating a logic of criticism. Kuhn, however, claimed that neither of these assertions is supported by the history of science.[9] Far from the history of science displaying a steady continuity in which theories, subjected to steady but unrelenting criticism, are weeded out leaving only the best conjectures holding the field, conformity and conservatism seem to rule. For most of the time scientists exhibit a strong attachment to general frameworks, or 'paradigms', within which 'normal science' proceeds its uneventful and cautious way. Such prolonged periods are punctuated by upheavals in which 'revolutionary science' overthrows the orthodoxy only to establish itself as a new orthodoxy. Upheavals such as this are relatively rare, however.

Here Kuhn is drawing upon sociological ideas and using them against philosophical conceptions of science, including those of positivism. In brief, science is a social institution into which scientists are socialised: learning how to work and think within the idioms of particular scientific communities. In doing so they become committed to a paradigm which, though it is not always clear just what Kuhn means by this, contains, first, a constellation of values and beliefs, cognitions, rules of order and techniques of procedure shared by a given scientific community, and, second, a collection of exemplary work within a discipline that serve as recipes for problem-solving activity. Paradigms involve a shared set of symbols, metaphysical commitments and values, as well as criteria of judgement and the worth of work done. So, becoming a member of a scientific community involves enculturation into the paradigm. 'Normal science' characterises the kind of attitudes and practices that go on within a discipline for most of the time, in which scientists patiently and undramatically work to elaborate the theories and accumulate findings shaped by the orthodox paradigm. However, such a process always creates puzzles and problems which, though for a time can be put on one side, eventually accumulate until they become so serious that the

orthodox paradigm is increasingly seen as inadequate. The search for a new paradigm starts; a search best done by younger scientists with reputations and careers to establish. Out of this turmoil a new paradigm emerges.

For some, Kuhn's version of the development of science overemphasises the irrational and non-rational factors. The change in paradigms amounts to a *gestalt* switch in which things can never be the same as before. A new paradigm is a new way of seeing the 'same' things differently, and the kind of phenomena with which a discipline deals change fundamentally. Paradigms are incommensurable. When the phlogiston theory of combustion was refuted and Lavoisier discovered oxygen, the universe was different for science.[10] In which case, if Kuhn is right there can be no theory-independent view of the world, and if a change from one paradigm to another is a movement between incommensurables, then it is hard to see the development of science as progress towards truth. The history of science is simply a history of changes and science unequivocally a social process and the selection of competing theories dependent on this context. Moreover, such a view seemed to reject a correspondence theory of scientific truth. Theories are radically underdetermined by the facts of the world; the world, to put it slightly differently, is capable of bearing a very wide variety of theories, none of which could be said to be absolutely superior to another. Kuhn himself claimed that he was not a relativist and expressed unease at the apparent abandonment of the idea that sensory experience was fixed and neutral, but did also despair of securing this through the pursuit of a neutral observation language.[11]

The dispute between the Popperian and Kuhnian views is one over the character of scientific logic and its place in the development of science itself, in particular as to whether it is possible, or sensible, to describe science's development as a progress towards truth.[12] Popper wants to say that despite local vagaries and perturbations, the choice between theories, between paradigms even, is, or can be, made on rational scientific criteria. It is the effort to deploy these criteria in a process of criticism, trial and error, that results in the slow progress towards truth as weaker offerings are discarded. Kuhn, on the other hand, seems to suggest that such choices are not rational in this sense, but the outcome of non-rational, extra-scientific considerations and factors, such as the distribution of power and reputation within disciplines, within society itself, personal commitments, wider cultural and political circumstances, and so on. The 'facts' cannot decide the matter because what the 'facts' are is dependent upon the particular paradigm they belong to, as do the standards in force for judging which theories are better than others. Facts, methods and standards are internal to paradigms and there is

no independent position from which to judge them – least of all by an appeal of a world independent of any theoretical position whatsoever. Such a thing is a chimera.

Although Kuhn's views provoked no little excitement in social science, it is not clear just what their implications might be, except as a way of writing the history of social science in terms of paradigm changes.[13] In other words, it is not clear just what philosophical or methodological consequences would flow from Kuhn's conception. Are the social sciences at a pre-paradigmatic phase, or do they exhibit a plurality of paradigms which, though incommensurable, can be ignored until a better paradigm emerges? But what follows from any answer to these and other questions of this order? As far as the sociology of science was concerned, however, Kuhn's work proved to be a liberation. Hitherto a relatively minor branch of sociology concerned with studies on, for example, the background of scientists, the social history of science, the rise of science as an institution, and so on, began to see itself as able to make inquiries into the cognitive aspects of science, scientific knowledge itself, and so encroach, it was claimed, upon territory hitherto reserved for philosophy. For some, it meant that philosophical questions about knowledge were at least open to empirical solution. The Strong Programme in the Sociology of Knowledge, for example, saw itself as banishing forever the philosophy of science and all the epistemological and ontological questions that went with it. Science was through and through a social construction and, therefore, a concern of sociology rather than philosophy.[14] It is social, political as well as wider moral attitudes that determine the theories which are held and sustained and those which are rejected. Boyle's atomic theory of matter, for example, crucial to the origins of modern chemistry, had a strong affinity with the corpuscular philosophy that shaped the political opinions of the 'establishment' groups to which Boyle belonged in post-Civil War England. Corpuscular philosophy was the ideology of an establishment class and corresponded with the requirements of their social, political and economic interests.

The claim is that all knowledge, including scientific knowledge, is social. Although knowledge can be analysed and studied as if it were asocial, that is, independent of the social circumstances which produced it, this is a very limited conception and one which will not be able to explain why some theories, some beliefs, are held and others not. If one looks at the history of science one can find many theories, including some that were just as plausible in terms of the evidence available, but which were not accepted while others were. This cannot be explained purely in terms of rational criteria. A proper footing for the examination of knowledge is the sociology of knowledge rather than philosophy. According to the Strong Programme, what such an examination should seek

to do is specify the causal connections between social conditions and knowledge, irrespective of whether or not these bodies of knowledge are true or false. In other words, it should not simply seek to explain why false beliefs are held – for example – why some people still believe that the Earth is flat – but also try to explain why true beliefs are accepted. Nor is the sociology of knowledge to be exempted from its own strictures; it, too, is capable of explanation in terms of its causal social conditions. One implication of such a view is to render meaningless the quest for intellectual authority, as positivism did, for example, through a philosophically secure conception of the foundations of human knowledge. For philosophy, too, as a body of knowledge is socially caused and, hence, dependent on the social conditions which produced it. There are no secure foundations for human knowledge; it is relative.

However, what this represents is a mistake traceable back to Kuhn, or at least to some interpretations of Kuhn, of confusing the history and the sociology of science with the philosophy of science. For, even accepting that the boundaries between disciplines are not always clear, it can be argued that history, sociology and philosophy represent different ways of being interested in the world and, as such, are incommensurable in respect of their problems and procedures. In which case, the claims of the Strong Programme, for example, to answer philosophical questions empirically is simply mistaken, for not only is philosophy not an empirical discipline, its problems and interests are not of the kind to be resolved empirically. Its interest in the world is independent of whatever empirical conclusions history or sociology might provide. This is an issue that will surface again; but as far as Kuhn's work is concerned, if he was doing no more, as suggested earlier, than describe the development of natural science in a particular period of European history, then it is debatable as to whether his analysis has any methodological consequences for the social sciences as to how they might meet the requirements of scientificity.[15] Rightly or wrongly, however, one of the implications that has been drawn is that Kuhn's arguments deny the possibility of scientific progress. Science does not grow, it simply changes. As Laudan points out with respect to Kuhn's conception, 'scientific revolutions are regarded as progressive because the "victors" write the history . . .'.[16] This, as said earlier, is, for many, an absurd conclusion. Whatever scientific discipline we choose to take, be it physics, chemistry, biology, mathematics, history, even any of the social sciences, our knowledge has not merely changed but grown, though not always in a straight linear fashion. However, this is not quite the problem. We could still accept that scientific knowledge has grown and still deny that this is solely the result of the rational accumulation of

knowledge. Lakatos turned to this in an effort to reconcile some of Kuhn's insights into the historical development of science with the view that science is a rational activity.[17] Falsificationism, as far as Lakatos was concerned, failed as a demarcation criterion between science and non-science because it underestimated, even ignored, the tenacity with which theories were clung to despite disconfirmation; a point which Kuhn dwells upon. However, Kuhn's own conclusions about the incommensurability of theories was far too relativistic for Lakatos' taste. Science, for him, is a rationally accumulating body of knowledge; but it does not progress, as Popper claimed, through trial and error. The key notion for Lakatos is not, as it is for both Popper and Kuhn, the theory but the 'research programme'.

Newton's theory of gravitation, Einstein's relativity theory, Marxism, Freudianism, among many more, would qualify as 'research programmes' in Lakatos' sense. They are characterised by a 'hard core' of definitive propositions protected by a belt of auxiliary theories and hypotheses which connect the 'core' to the domain of facts to which they pertain. Thus, for Marxism, the theory of value formation and the creation of surplus value would be the core, and the theories of alienation, diminishing return to capital and of revolution, would be the auxiliary theories. However, a 'research programme', as its name implies, is not some dead, fixed collection of ideas but a living thing directed by scholars working within it at the problems it poses, suggesting ways in which they can be tackled, exploring its ideas, indicating problems that are best avoided, and so on. It is the last kind of problems wherein lie the dynamics of 'research programmes' since, by eventually confronting them, progress can be achieved. Of course, the problem is knowing which problems are likely to prove promising and which not. For Lakatos, as for Popper, the important criterion is the ability of a research programme to predict novel facts or facts ruled out to be impossible by other research programmes. So, if a theory is running ahead of the facts, or is predicting new facts, then it is a progressive one. If, on the other hand, the theory constantly needs repair and patching up to stay in business, then it is degenerating or, at best, static.

Lakatos' 'rationalist history' of science tries to merge the philosophy of science's traditional concerns for the logic of the scientific method with those of the history of science. The tendency of science to persist with disconfirmed theories is rational in that it delays judgement until a research programme has matured. However, whatever the merits of the views just discussed, there is no doubt that bringing historical and social considerations to bear upon discussions on the logic of science has cast serious doubts on the traditional view that science is a paragon of rational-cum-empirical knowledge.

This is most pronounced in the work of Feyerabend who argues that scientific change and progress is really a conversion from one myth to another. Rejecting the distinction between observation and theory, as well as philosophical rumination as having any relevance for the operation of science, he regards science as a social institution located within a specific set of cultural, political and social concerns, just like any other institution in society. Thus, scientific changes do not simply arise from the application of a scientific method, but from the influences of 'interests, forces, propaganda brainwashing techniques' of 'professional socialization'.[18] In this respect, science is not different to any other form of knowledge; it is part and parcel of 'forms of life'. The conclusion that Feyerabend draws from this familiar relativist argument is that 'anything goes' in science. There is no scientific method. There is certainly no superiority attaching to scientific knowledge. For Western society, science has become an idol, a dogma, and its conception as a progressive rational activity little more than an obsession without foundation. In this he is not claiming the need to 'correct' the actual practices of science, but to bring its ideology more into accord with these practices.

His own examination of the Copernican revolution in astronomy during the sixteenth and seventeenth centuries suggests that Copernicus' theory did not succeed because it was 'obviously' more rational and more progressive than Ptolemaic astronomy. Indeed, Copernicus' theory did not fit many of the widely accepted astronomical 'facts' and made use of some of Aristotle's theories about the harmony of the universe. It was not until the use of the telescope that the majority were eventually persuaded to accept Copernicus' heliocentric theory of the solar system. Other aspects of the theory depended upon Galileo's new theory of motion. But Feyerabend's point is that conversions such as these are not the products of reason, evidence or method, but have much to do with self-interest, ideology and wider cultural beliefs. Although Feyerabend's anarchism is well known and fits well with his rejection of the notion that there is any superiority to the method of science, he is not against science, only against its pretensions and idolisation.

Popper, Kuhn, Lakatos and Feyerabend represent, although in different ways, a response to the epistemological problems posed by induction as the basis of scientific knowledge. Popper revised the scope of the problem by proposing that it was the scientific method that was rational, not necessarily any particular scientific theory. Science is a human activity and, consequently, prone to mistakes, confusion and error. Nonetheless, the rationality of science and the cut and thrust of scientific debate ensures that, in the end, true theories will prevail. This apart, Popper's reflections on science had the further consequence of making

science's history and its social context highly visible, eventually leading to the prominence of views which gave science little credence as the epitome of reason. Science did not progress rationally. As Feyerabend would put it, change in science is simply the replacement of one myth by another. Relativism is loosed.

However, although the social and historical nature of science, indeed of any form of knowledge, is well accepted by most, what are less palatable are precisely the kind of relativist conclusions that seem to follow. For one thing, although it might be accepted that observation is theory-laden and there is no theory independent way of observing the external world, and that theories might well be incommensurable, surely nature must play some role in determining which particular theories, categories, methods, etc., are the right ones? Surely we just cannot determine what the world is like in whatever way we choose? If we cannot then a fundamental requirement is the independent existence of an external world which has a character independent of human conceptions of it.

REDEFINING EMPIRICISM

However, positing this requirement for an adequate philosophical empiricism is one thing; demonstrating, as we have seen in the discussion of positivism, is quite another, especially after the forceful attacks made by arguments concerning the social construction of knowledge and, importantly as a consequence, varying standards of truth and validity. If science is a social construct, then any claims it might have to a unique accessibility to the nature of the external world has to go. Our conception of science, its methods, and its findings, are a consequence of our history, our society, and not the result of some privileged method for describing and explaining the nature of reality. Science becomes, at best, simply another way in which the world may be described. As Quine argues, our experience of the world, of facts, does not impose any single theory on us. Theories are underdetermined by facts, and the factuality of the external world, to call it that, is capable of sustaining many different interpretations of it.

This Quine accepts with equanimity, but does not conclude that we should abandon science. While we can have no firmer knowledge than science can give us, this knowledge is always revisable and contingent. What has to be abandoned is the epistemological goal of trying to discover those principles which would guarantee certain knowledge. Such an endeavour is futile. Epistemology is really an inquiry into how we come to know the world in the way in which we do, and not an inquiry into whether

we can acquire certain knowledge. Indeed, Quine is prepared to consign epistemology to psychology; that is, as part of an empirical rather than a philosophical discipline.[19] For his own part, his interests are ontological rather than epistemological and begin from the position that there is nothing that can be more certain than science and no philosophy that can be foundational to it. This is not, as said earlier, to claim that science is certain. Philosophy depends upon the foundations of science for this is the best guide as to what kinds of things there are in the world. So what Quine is offering is a limited scepticism about science unlike, say, that offered on some interpretations of Feyerabend's remarks. We do not have to accept or reject science *in toto*, but we should recognise that some of the theories and the findings of science will be wrong, as they have been in the past. This is the best we can have even though they may be the products of *our* culture.[20] For Quine, science and philosophy, though not the same, are joint endeavours distinguished by the generality of their respective concerns. Nevertheless, philosophy must take its lead from science. What Quine seeks to provide is an economical, not to say austere, account of what there is; an ontology that postulates as few entities as possible. There is one important difference between science and philosophy, however. Philosophy does not investigate the world directly, but through language invoking what Quine calls the principle of 'semantic ascent'. Instead of examining 'things' like science, philosophy investigates how things are talked about and, through this, investigates the nature of the world.[21] Quine is a relativist to the extent that, although the aim of both philosophy and science is to discover what there is, neither can claim to do this in any theory independent way.

However, instead of regarding this as the conclusive result of philosophical rumination, Quine, in effect, comes at the issue from another direction. The answer to the question, what exists? can only receive the answer, what exists is what theories posit. Since there are different theories, these will posit different things. Accordingly, Quine is happy to accept some of the implications of the Kuhnian type of view, which argues that different theories postulate different existents and that there is an incommensurability involved in this. But, for Quine, this is to take an 'externalist' view of theories. However, we look at the world *through* theories and though we can accept, from an external point of view, that there can be alternative accounts of the world and what exists, we judge their adequacy from the point of view of our 'home' theory and that is, for us, science.

Quine is claiming that certainty cannot be sought from the places philosophy has traditionally looked for it; that is, what we can know independently of all experience, the *a priori*, or that which is certain because it arises directly from experience, the *a*

posteriori. These are, for Quine, the two 'dogmas' of empiricism.[22] What he intends is nothing less than eroding the distinction that has long been central to philosophy between analytic and synthetic statements. Philosophers have tended to regard the truth of sentences as a matter to be settled for each sentence separately when, in actuality, sentences are part of whole languages. The same is true of sentences within theories. It is language, or the theory, which is the unit of meaning and, accordingly, the truth and the meaning of any one sentence in that language, or in that theory, has to be answered in terms of its relationship within the whole. The sentences that make up theories, for example, are like a spider's web, anchored at some points, but connected by filaments of thread such that perturbations at one point will affect others. Thus, at some points there will be sentences that are directly related to our experience, others will be more remote. Others we may be prepared to readily abandon, others less so. Still, all sentences are linked together and it is this organisation which has much to do with our conception of the world rather than those points anchored to a more direct experience of the world.

The structure can be revised, of course, though there will be some statements we might be more reluctant to give up than others. Some will have more consequence than others and their abandonment involving major revisions to the structure, while others might have only minor effects. The reason, therefore, why the analytic–synthetic distinction cannot be sustained is that questions of meaning and those of fact are thoroughly intertwined within the structure. The discovery of black swans does not suddenly render the statement 'all swans are white' synthetic, for we could choose not to regard black 'swans' as swans at all.[23] In other words, the facts do not necessarily impose either solution on us. The configured web of our beliefs, theories, sentences can be altered to cope in various ways with any changes we might be forced to make: 'a statement about the world does not always or usually have a separable fund of empirical consequences that it can call its own'.[24] Thus, and this is the important point, our experience of the world does not impose any single theory on us, nor any particular response we need to make in adjusting theories to meet new facts. It is this which sets limits to the certainty of our knowledge: theories are radically underdetermined by evidence.

Accordingly, the hope of positivist empiricism that certain knowledge of the world could be provided by sensory experience is rejected by Quine. Even the sentences which report on our direct sensory experiences are also part of the web of sentences and, as a result, revisable as necessary. Quine is not here rejecting the idea that the evidence of our senses is the evidence for the theories we have: indeed, this is the only evidence we have. But theories go beyond that evidence and cannot be limited by it. There will

always be more than one logically equivalent theory (note, not *any* theory) consistent with the evidence we have. This is not because that evidence may be insufficient, but because the same facts can be accommodated in different ways by alterations in the configuration of the theory. Of course, there may be lots of good reasons why we should prefer one theory or another logically and evidentially equivalent theory, but these cannot be on the grounds of evidence alone.

Similar problems emerge in translating one language, or a theory, into another. As Kuhn seems to have suggested, theories are incommensurable and that, as a result, changes in scientific theories represent fundamental changes in our conception of the world and, indeed, in the ontology of the world. For Quine, translation between two theories is a matter of aligning two systems, not simply trying to match up the meaning of separate words, concepts or sentences. So, attempts to match, say, separate sentences between two systems will involve making assumptions about how the bits fit into separate but respective wholes; and, as before, we can provide different solutions for particular sentences depending upon the adjustments and compensations we wish to make. Translating involves guesswork, assumptions about the ontologies referred to by the respective theories and, for Quine, there is no right way of deciding which translation might be the correct one. There is even logical room for doubt that even the speakers of a common language hold the same ontology. This, however, makes no practical difference to social relationships. It is the pattern of behavioural dispositions that is crucial and there is no way of telling from these, with absolute certainty, whether a person has the same ontology as we do. In logic there is no compelling reason why our ontology should be chosen over another.[25]

The sort of revisions Quine, and others, envisage for empiricism are major revisions against positivism. In trying to obtain an ontology for science, what we cannot do, as positivism did, is to look to the nature of the world independently of our theories, our language. As one contemporary philosopher of science expresses it, it is 'generally agreed . . . that the idea of a descriptive vocabulary which is applicable to observations, but which is entirely innocent of theoretical influences, is unrealizable'.[26] But looking towards theories simply brings us up against their incompatibility and incommensurability, and their indeterminacy, and the spectre of relativism. Once again, we seem to lose any possibility of justifying scientific knowledge over other forms.

Others, however, such as Putnam, while agreeing with Quine that we can have no knowledge stronger than science, nevertheless want to reintroduce the notion of 'essence' through a theory of 'direct reference'. Thus, although an object might manifest all

kinds of appearances, what is essential to this is the nature of the stuff. Gold, for example, can vary in appearance in relation to light, heat, etc., but its physio-chemical constitution cannot vary, cannot become, say, like that of water and still remain gold.[27] What links a word and an object is an act of 'dubbing', and what the name is linked to is whatever it is that makes it the kind of stuff that it is. So, when scientists discover 'essences', what kind of stuff a thing is, then they discover necessary relations; that is, discover what it is that makes something what it is. Such a theory is intended to avoid the Kuhnian and Feyerabend claims that there is no continuity between theories. Even though, before and after a scientific revolution, beliefs about the things theorised may have changed, this is irrelevant since these do not 'fix the reference' of the terms.

One further consequence of these efforts to revise empiricism is embodied in Hacking's recommendation that philosophy turn its attention to the ways in which scientists intervene in the world to produce their theories in order to see what ontologies their methods of experimentation, observation and measurement are committed to.[28] In other words, philosophical interests in science should have less concern with the question of how scientific theories *represent* the world, and more in how they *intervene* in the world in order to investigate it. Such a conception does not require that science has a single unified ontology. Realism for theories only causes us problems when we try to imagine that we can effect a match between the theory and the world independent of the theory. Without theories we have no idea what the external world is like. Realism belongs *within* our theories; what Putnam refers to as 'internal realism'.[29] Propositions are true within a theory, or within a given language, but we can cope with the diversity of the conceptions of the world implied if we regard ontologies as allowing us to make experiments, observations, and so on, to give organised and systematic descriptions of what is found, rather than requiring us to match theories with how the external world *really is* organised. In this there is no need for a unified theory, a unified method, or a unified ontology. The theories of the various scientific disciplines are descriptions of what is observed, measured, experimented on, counted, and so on. The 'phenomenological laws', as Cartwright calls them, are the outcome of many different premises, assumptions, interests, exigencies, and problems that are peculiar to particular disciplines.[30] These laws are correct within their respective domains but do not add up to a theoretical or ontological unity. Any attempt at unification by connecting them to more 'fundamental laws' is bound to distort them since they can only be approximations to the concepts deployed in the original theories. Various orders of observations, measurements and the phenomena displayed

in the investigations of different disciplines cannot be reduced to each other without 'lying'. In which case, science has to be committed to multiple ontologies, multiple realities, rather than the myth propounded by the likes of positivism of a single, unified description of the ontology of the external world.

The implications of the new philosophy of science for the social sciences are radical in some ways, inconsequential in others. What is rejected is positivism's effort to build a view of science stressing the unity of its method, its search for laws, and so on, which the social sciences, if they were to become scientific, would have to emulate. However, the views reviewed here cast doubt, in various ways, on the idea that there could be a unified science united by its commitment to a single ontology of the external world. The work of Kuhn, and the sociologists of science, as well as the arguments of Feyerabend, showed that scientific change has little to do with the shape science obtains through the application of a rational method, and has more to do with the fact that it is a social institution. Arguments about the incommensurability of theories raised questions about the truth of theories and provoked damaging doubts about the correspondence theory of truth espoused by positivism. Nevertheless, rather than abandon science to anarchy, as Feyerabend is alleged to want, contemporary realists and empiricists, such as Quine, Putnam and Hacking, have revised the conception of science and knowledge in light of the arguments raised against positivist conceptions of science. What was rejected was not science, or indeed its eminence as a form of knowledge, but the view of science requiring epistemological and ontological unity. What is emphasised is the diversity and the disunity of science. After all, scientists do not worry about epistemology and ontology but about the particular problems they confront from their theories and investigations. And, indeed, as Pawson reminds us, the theory-laden nature of observation is a feature of scientific work that natural scientists find unremarkable and obvious.[31]

A further implication of this kind of view is that the intellectual authority of philosophy is eroded. If all that matters is that scientists go about their business in the ways that they are taught, learn and acquire, using methods appropriate to the problems they have to deal with, then philosophical worries about ontology and epistemology are an irrelevance. Quine, for example, argued for the 'naturalisation' of epistemology by reducing it to one of the sciences of knowledge, such as behavioural psychology or brain physiology, to discover those laws of cognition which determine why we accept and hold the theories and the beliefs that we do. Ontology, too, becomes the business of the respective sciences and their investigations. In which case, as far as the social sciences are concerned, if they want to emulate the natural sciences what they

should do, like them, is disregard philosophical versions of science and get on with tackling their problems as they see fit. There is certainly no reason to feel bound by stipulations about a unified method or a unified ontology for science, for on these arguments no such creature exists.

For some realists, especially those concerned with the social sciences, this kind of conclusion is unsatisfactory. While recognising that positivism has been found wanting, they still want to assert that science is concerned to describe real structures, entities and processes which constitute the external world. In this, the regularity that is required by the orthodox notion of laws is less important than identifying and describing the real, operative causal mechanisms which have real effects. It is not required that these operating mechanisms and their entities be observable, *contra* empiricism, but by isolating their causal effects in suitably designed studies, their existence can be plausibly postulated. Many of the more powerful mechanisms postulated by natural science theories, as said before, are not directly observable, though their effects can be. Laws need not be universal in the sense required by positivism but should represent recognisable tendencies.[32] As Bhaskar says,

The citation of a law presupposes a claim about the activity of some mechanism but not about the conditions under which the mechanism operates and hence not about the results of its activity, i.e. the actual outcome on any particular occasion.[33]

The consequences stated by laws happen only in special circumstances, that is, when its operation is not 'impeded' by complicating tendencies and the *ceteris paribus* conditions are in place. All heavier than air objects fall, for a simple example, unless 'impeded' by things that do not allow them to 'realise' the law of falling bodies, so to speak. In the natural sciences, the ability to set up 'closed systems' often, but not always, experimentally, allows for the more detailed specification of the *ceteris paribus* conditions for a law. This is the major difference between the natural and the social sciences. Accordingly, it should not be expected that the degrees of precision attainable in most of the natural sciences should be found in the causal statements of the social sciences. In addition, the view also stresses realism for theories in that entities have their meaning and significance from the theories of which they are a part.

This realist conception of the nature of social science – although in many ways in accord with much of the new philosophy of science and, in this sense at least, tries to avoid many of the problems of earlier positivist and empiricist philosophies of science – contains little of direct guidance to social research itself. It is still a language of causation and in this respect attractive to materialists. But,

in addition, it represents a serious relaxation of the criteria for determining causal relationships as exhibited in natural science. Others, however, recognise that any realist social science would have to take into account the fact that social actors themselves have their own theories about the way in which the world operates, and taking this seriously raises the question of whether any causalist conception of the business of social science can be sustained. This is one of the issues to be taken up in the next chapter.

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CHAPTER 5

The interpretative alternative

In the previous chapter I mentioned a distinction which, historically speaking, is of fundamental importance in Western intellectual development, namely but rather grossly, that between mind and matter. While admitting of the distinction in some sense or another, different philosophical schools have, as we have seen, interpreted it in different ways. For materialists the aim is to reduce mental phenomena to epiphenomena of the material. Extreme idealists, on the other hand, argue that the so-called material world is simply the resultant of ideas. Both positions, of course, are more detailed and more plausible than this summary statement might indicate. For our immediate purposes in this chapter, what is important is the acceptance of the dualism as representing different orders of phenomena and, accordingly, to be known in different ways.

SOME INTELLECTUAL FORERUNNERS

As with the origins of positivistic thought, the tremendous debates of the seventeenth century form the more immediate intellectual background for the view that the proper study of human society could not be scientific in the manner of the natural sciences. Where the social sciences are concerned, the important figures are Vico and, much later, Dilthey and the development of hermeneutic philosophy. More contemporaneously, the Phenomenologists, among others, have expanded this tradition. The earliest mentioned, Giovanni Batista Vico (1668–1744), saw human history as a process reflecting the maturation of the human mind in its understanding of God's nature. He also stressed that the study of man and society in history was very different from the study of inanimate nature in the sense that the former involved subjective understanding, a theme that was to be developed more fully later, especially by German scholars of the nineteenth century. Important to this particular phase of the debate were considerations arising from Biblical philology. Translating texts which had themselves gone through a number of different translations and modifications from their original language did not simply involve linguistic considerations, but also required them to be related,

in order to discover their original meaning, to the wider social context in which they were originally produced. So, making sense of textual materials required a union of philology and history and, one might add, sociology and anthropology. It was this which provided what has become known as hermeneutics with its abiding question: how is an understanding of the past to be gained through its texts and other remains? Schleiermacher, who was responsible for drawing hermeneutics away from its original home in philology and applying it to the problems of historical knowledge, took this as *the* problem of history. To understand the past one has to identify with it. By complementing grammatical interpretation with psychological identification, hermeneutics was introduced into the study of human activities more generally and the possibility of interpretative understanding more particularly.[1] Dilthey (1833–1911), building on Schleiermacher and as part of a widespread romantic reaction against positivism, held that the positivist methodology of the natural sciences was inadequate to the understanding of human phenomena except as natural objects. It left no room for the idea that history and society were human creations and that this constituted the essence of all social forms. The study of human history has to be based on the fact that humans were purposive creators whose lives were bounded by a reality which has meaning for them. The duality of the subjective and the objective was irreducible. History was not simply the succession of events one after another, but expressed the spirituality of social life as expressed in social institutions, law, literature, government, morality, values, and more.

This required a wholly different but still well-grounded method of inquiry to that of natural science. The method had to recognise the actions, events and artifacts from *within* human life not as the observation of some external reality. Knowledge of persons could only be gained through an interpretative procedure grounded in the imaginative recreation of the experiences of others. History, society, art, indeed all human products, were the objectifications of the human mind and not at all like material things. Accordingly, understanding such phenomena required that the lived experiences of others be grasped through the apprehension of their inner meaning; the meaning that led to their production. The socio-historical world is a symbolic world created by the human mind and cannot be understood as simply a relationship of material things.

So for Dilthey and others of like mind, nature and culture were inherently different and required different methods of study. Natural science, conceived mainly in positivistic terms, studied the objective, inanimate, non-human world. Society, a product of the human mind, was subjective, emotive as well as intellectual. What we would refer to as causal, mechanistic and measurement oriented models of explanation were inappropriate, since human

consciousness was not determined by natural forces. Human social behaviour was always imbued with values, and reliable knowledge of a culture could only be gained by isolating the common ideas, the feelings, or the goals of a particular historical society. It was these that made each social act subjectively meaningful. The observer, as a human being studying other human beings, has access to the cultural world of others through some form of 'imaginative reconstruction' or 'empathy'.

Others, notably Rickert, did not accept Dilthey's dichotomous view of reality as split, in effect, between nature and culture, but, instead, argued that reality was indivisible. However, unlike the positivists who held a similar view, this did not imply that the methods of natural science were thereby applicable to the world of society, culture and history. Differences between the natural and the social, or cultural, sciences were based on logic rather than a metaphysical ontology. For Rickert, human beings could have no knowledge of the world independently of what was in their minds. They had no way of finding out whether their knowledge faithfully reproduced a reality existing outside the mind and independent of it. They could only know things as they appear as phenomena, never as things as they are as such.[2] Facts, so to speak, are constituted out of the phenomena and given both form and content by the mind. This is a volitional act and its performance an intentional activity. All human knowledge, therefore, is selective, involving abstraction according to particular interests. Objectivity, therefore, is achieved not by matching ideas to some external reality, as the positivists would have it, but by the intersubjective establishment of those facts by those who have an interest in knowing them. Accordingly, if the knowledge of laws of nature is the only knowledge that anyone wants, then the legitimate method leading to their discovery is the method of natural science. If, on the other hand, the interest is in knowing different things, then the basis of knowledge, too, is different.

Empirically, according to Rickert, there are two basic principles of selection at work, each making it possible to arrive at one of two different kinds of representations of reality, namely, the nomothetic and the ideographic. The former, characteristic of natural science, is an interest in discovering general laws, while the latter, more characteristic of history, is concerned to understand the concrete and unique case. This dichotomy represents no fundamental difference in the ontology of the world but in the kind of knowledge required by different interests. Human products embody values and it is these which need to be understood by the social scientist in order to make sense of the unique constellations that make up human history. So, while natural science is interested in forming general concepts by abstracting from the concrete case those features which are in common with other phenomena,

historical inquiry is concerned to form individual concepts by focusing on the unique combination of elements that represent a culturally significant phenomenon. Both forms of inquiry use their own principles of selection for the purpose of isolating the elements of empirical reality which are essential to their respective cognitive purposes. The ideal of objective knowledge requires both methods, as any one of them gives only a one-sided picture of reality. The same reality, however, can be presented either as history or as natural science.

Although Dilthey and Rickert differed as to the reasons for the employment of different methodologies with respect to the natural and social worlds, they did concur that positivistic natural science method could not be used to gain adequate knowledge of the social. Weber, much influenced in a selective way by Rickert, accepted the distinctive character of the social sciences but not the implication that they were unscientific in being unable to meet the rigorous standards of objectivity. In Diltheyian fashion Weber accepted the importance of 'interpretative understanding' as the distinctive form of knowledge for the socio-historical sciences but only as a means towards objective knowledge. In Rickertian fashion he supported the view that the essential distinction between the natural and the social sciences was methodological rather than ontological. Indeed, the possibility of 'interpretative understanding' in the social sciences was, for Weber, a tremendous opportunity and not something to be apologised for. By its means human action could be studied in greater depth than a natural scientist could ever penetrate into the nature of the inanimate world.[3] There was, however, a price to be paid in objectivity, precision and conclusiveness. For his own part Weber tried to reconcile the advantages of 'interpretative understanding' with the demands of scientific criteria.

However, to understand quite what this meant it is important to understand something of the road that led Weber to this conclusion. At the time, two general positions dominated the debate over the social scientific method: one we have already reviewed at some length, positivism, and the other, intuitionism. Weber rejected both of these. Any socio-cultural science must use a method distinct from that deployed in natural science, but this is not characterised, as the intuitionists wanted, by any allegedly unique stance. Both forms of knowledge, the natural scientific and the socio-cultural, are 'invariably tied to the instrument of concept formation'.[4] In other words, the problems of the logic of concept formation are the same for both despite the fact of practical differences in the manner in which intellectual inquiry is pursued. The crucial difference lies in the 'theoretic interest' or 'purpose' of understanding which, for the socio-cultural sciences, is understanding subjectively meaningful phenomena. Thus, we

understand and expect the historical, the socio-cultural, sciences to be distinctive in their objective of interpreting meaning because of our own historically shaped, exiological interests. It is the values in our own culture which determine the kinds of interests that we take in history and in the social world as subjectively meaningful. By the same token, we take the 'theoretic interest' of the natural sciences to be in the production of universal-general concepts and propositions, or laws. But each of these different kinds of 'theoretic interests' cannot be reduced to the other. This is not for ontological reasons, as the intuitionists argued, but because of differences in the axiological or 'theoretical purpose' of the enquiry, and this does have methodological consequences for the socio-cultural sciences. A different method of inquiry is called for, given the theoretic interest of understanding or interpreting meaningfulness, and this is the method of *verstehen*; that is, attempting to reconstruct the subjective experience of social actors.

To this end Weber advanced two major methodological principles, both of which are still part of the contemporary language of social science, namely, value neutrality and the method of ideal types. So far as the the first is concerned, Weber held that social scientists should never abuse their scientific authority by passing off value judgements as scientific truths. About conflicting values scientists can have nothing to say as to which is to be preferred, but can only review the likely outcome of the various value alternatives. Science deals only with the rational, and is a technically oriented, instrumental activity.[5] The second methodological device, the ideal type, was offered as a means of grasping subjectively held meanings more objectively. All irrational and emotive aspects of human behaviour are to be seen as deviations from a conceptually pure type of rational action. This ideal type is both clear and free from ambiguity. Understanding, then, was transformed into the construction of rational models. Weber felt that the natural science method, transplanted to the study of social behaviour, would produce valid knowledge but of largely irrelevant and unimportant activities, at least as far as the subjective perspective was concerned. The contrast between the natural and the social sciences occurs because, in the latter, human beings are both the subject and the object of inquiry, which means that knowledge of society is a form of self-knowledge. *Verstehen* or interpretative understanding, gives social observers a method of investigating social phenomena in a way that does not distort the social world of those being studied. Since the essence of social interaction lies in the meanings agents give to their actions and environment, all valid social analysis must refer back to these. However, the insights gleaned in this manner must be supported by data of a scientific and statistical kind. All phenomena, no matter how unique and particular, are the products of antecedent,

causally related conditions. By this Weber does not mean that social acts are to be reduced to single all-embracing laws, but rather that, from the complex whole of social reality, limited and unique antecedents and consequences are abstracted and related to observed phenomena. Such 'adequate causation' provides probabilistic explanations.

This tradition of thought reacting against positivist conceptions of science and their importation into social science had a powerful impact especially in Europe but, while not ignored, less so in the United Kingdom and the United States. For our purposes one feature above all stands out, namely, the view that the social sciences involve radically different methods from those of the natural sciences. Admittedly, the arguments for this did not always take on an ontological form but pointed instead to the different kinds of knowledge required by the respective disciplines. Either way, different methodologies were involved. It is to an examination of some of the issues here that I now turn.

SOCIAL ACTION AND MEANING

In part, the 'interpretavist' or 'humanistic' programme is a reaction against the very strident claims of positivism and its 'scientised' conception of the social actor which they see as embodied in orthodox social science of a positivist persuasion. The accusation is that those features which make social life a distinctively *human* product are analysed out and reduced to the interaction of variables.[6]

At times such accusations are little more than understandable frustration at the apparent trivialisation of the problems of social science and the betrayal this represents of the moral concerns that motivated the founding fathers. Exactly what positivistic social science had left out was a matter of some debate; was it freewill and choice, moral and political concerns, a regard for human fate, values, the self, the subjective dimension, or what?[7] On a less elevated plane the argument is about the character of the objects of social scientific inquiry. Even though it is more than possible to describe empirically patterns of social action by using all the elegant correlational apparatus of positivist social science, this would fail to get at the proper subject-matter of social science. It would fail, in short, to give an adequate account or interpretation of why the pattern of interaction occurred as it did, when it did and where, in terms faithful to its status as a human product. It would give no account of the fact that human beings of flesh and blood produced the interactions and, accordingly, would at best be only a partial analysis. The argument could go much further than this and claim that positivist methods not only give a partial account

of social life but that they distort its nature in profound ways.

The manifold issues here are encapsulated in Weber's famous definition of 'social action': an action is social when a social actor assigns a certain meaning to his or her conduct and, by this meaning, is related to the behaviour of other persons.[8] Social interaction occurs when actions are reciprocally oriented towards the actions of others. Actions are reciprocally oriented not in any mechanistic fashion of stimulus and response, but because actors *interpret* and give meaning both to their own and to others' behaviour. Weber himself devoted considerable efforts to elucidating the implications of this formulation of the central tenet or, as for Weber the objective, of sociology. The important point here has to do with the idea of meaning and its relationship to the kind of knowledge we require in order to understand and explain social phenomena. To speak of meaning is to begin to point to that most important fact, that human beings have a rich and highly varied mental life reflected in all the artifacts by which, and institutions in which, they live. In sociological and anthropological terms this is referred to as 'culture' and includes all that social actors can talk about, explain, describe to others, excuse or justify, believe in, assert, theorise about, agree and disagree over, pray to, create, build, and so on.

One way of regarding meaning is to see it as a subjective or internal component of behaviour. This would be to draw a contrast between the objective features of social action and its subjective elements. The regularities we discover by studying society are only the external appearances of what the members of a society understand and, thereby, act upon. This point can be illustrated using Hart's famous example of traffic behaviour.[9] A stream of traffic controlled by traffic lights displays regularity. If it were to be regarded purely as the product of causal factors, then to explain the patterns we would have to specify the necessary and sufficient conditions which produce a given pattern, and go on to formulate a theory linking the traffic signals to the movement of the traffic stream. We would have to postulate the causal mechanism involved in effecting the connection between the different coloured lights and the movement of the vehicular units. However, as it happens, we do know that there are regulations governing traffic lights which order the drivers of the cars and other vehicles to behave in particular ways in accordance with the pattern of the lights. Thus, the connection between the lights and the movement of traffic is one which can be explained in terms of the *meaning* the lights have within the culture.

An important issue arising from this example is whether an explanation in terms of meaning is compatible with a causal explanation. If the answer is negative then this would seem to indicate a fundamental difference between the social and the

physical sciences. The claim would be that the relationships between the traffic lights and the behaviour of the road vehicles is not of the same logical order as, say, that between sunlight and plant growth, between thunder and lightning, or between colliding billiard balls. Though classically regarded causal elements are involved in the traffic lights and the behaviours they produce – for example, in the mechanisms which activate the lights and in the control systems of the vehicles – these are irrelevant to understanding the relationship between the lights and the patterns of traffic. The relationship is a meaningful one, and what we have uncovered is a custom- or rule-governed practice rather than a causal law. The drivers could give *reasons* why they stopped when the light shone red, moved on when it changed to green. In short, they themselves could account for what they did: 'because the red light signalled "stop"', 'The green light allowed me to proceed', 'If you don't stop at a red light you can get in trouble with the police', 'You have to obey traffic lights otherwise the roads would be in chaos', and so on. Such reasons would invoke intentions, purposes, justifications, rules, conventions, and the like, rather than impersonal causal mechanisms.

There are a number of problems here to do with the ontological status of reasons and rules, the status of social science theories versus those accounts offered by the members of society, the nature of social action and its description, among others, all intertwined in complex ways. However, let me begin by trying to establish some preliminary positions.

One task of the social scientist is to give some theoretical account of social life. This requires empirical research in order to bring data to bear on the theory. These data must derive in some way from the lives of the social actors being studied, but unlike physical phenomena, social actors give meaning to themselves, to others and to the social environments in which they live. They can describe what they do, explain and justify it, give reasons, declare their motives, decide upon appropriate courses of action, try to fit means to ends, and so on. As Schutz expresses it:

It is up to the natural scientist and him alone to define, in accordance with the procedural rules of his science, his observational field, and to determine the facts, data, and events within it which are relevant for the problems or scientific purposes at hand . . . The world of nature, as explored by the natural scientist, does not 'mean' anything to the molecules, atoms, and electrons therein. The observational field of the social scientist, however, namely the social reality, has a specific meaning and relevance structure for the human beings living, acting, and thinking therein. By a series of commonsense constructs they have preselected and preinterpreted this world they experience as the reality of their daily lives.[10]

The social scientist, then, must come to terms with these meanings for, as we shall see later, in a fundamental sense the origins of the researcher's data lies in these meanings. The starting point for empirical social science research is the observation of what the members of society do or have done. These observations may be in the form of records, statistical rates, tape-recordings, writings, questionnaires or interviews, archeological remains, diaries, and so on. An essential part of observation is the description of the phenomenon. Actions and behaviours must be classified and categorised. Decisions must be made, for example, about whether a man carving a piece of wood is doing something economic, religious, political, artistic, or whatever. What is also certain is that the man himself would have views on what he is doing. What, then, is the relationship between his account and any that the social scientist might offer? What, if any, should the link be? More generally, what difference does the fact that social actors assign meaning to their social reality make for the study of social life?

Since positivistically inspired social science has not exactly ignored what might loosely be termed, the 'meaningful components' of social behaviour, and since the philosophical positions being discussed in this chapter involve a critique of this treatment, it is as well, perhaps, to begin with some statement of the traditional ways in which reasons, motives, intentions, rules and conventions have been regarded in traditional social scientific theorising.

RULES, MOTIVES AND THE DESCRIPTION OF SOCIAL ACTION

In the traffic light example used earlier two sorts of phenomena were identified as important in a 'meaningful' account of behaviour; rules and dispositional concepts such as reasons, intentions, or motives. These, as it were, point to the 'internal' character of the relationship between the lights and driver behaviour; that is, to the subjective meaning which leads to the sequence of actions we would describe as 'obeying the rules of traffic signals'. The idea that social action is rule-governed is not, of course, new or surprising. Some of the basic concepts of social science, such as norms, institutions, deviance, rationality, authority, profit-seeking, exchange, legitimacy, and many more, all pay more than just passing homage to the idea that social behaviour, whatever else it consists in, involves rules. In its various forms, the notion of rules is used to explain social conduct and is able to do so because rules, even if imposed, are part of the system of meaning actors use to make sense of their reality. Similarly, and in a related fashion, motives, intentions, and so on, point to another aspect of

meaning, namely, that social actors pursue goals, have reasons for doing things, explain their conduct in such terms, and so on. Let us look at how these elements are normally treated within positivistic science.

The common mode of explanation is predicated on the notion that interaction is both rule-governed and motivated. Patterns of action are explained by reference to two groups of factors: dispositional ones, such as attitudes, motives, feelings, beliefs, personality; and sanctioned expectations, or norms, to which the actor is subject. These latter are sometimes referred to as 'role expectations' attaching to the incumbent of a particular position within a network of social relationships. The occupants of managerial positions, for example, are expected by others to behave in particular ways as are, though in different ways, mothers, fathers, prime ministers, ministers of religion, bank clerks, and so on. These expectations can be seen as rules guiding or even dictating the appropriate mode of behaviour for someone in one of these positions. A newly employed teacher, for illustration, has to learn the rules, both official and unofficial, that shape what others with whom he or she comes into contact will expect. Moreover, the incumbent of any particular position will be expected to occupy that position authentically by having the right motivations to perform the role properly.

These expectations or rules are, as it were, 'external' to the individual. They exist prior to whomever occupies a position and, moreover, act as coercive elements producing appropriate behaviour. In Durkheim's terms they have a 'thing-like' quality to them. Their 'externality' in this sense produces social patterning because similar rules apply to the same positions; managers are all subject to much the same kinds of expectations, as are mothers, fathers, and all the rest. This is much of what is meant by the idea of a normative order. There is presumed to be a more or less stable linkage between the role-performance expected of position occupants and the situations in which they find themselves because of the normative rules governing behaviour in that situation. It is further presumed that actors have been socialised into a common culture so that there is some substantive cognitive consensus among them enabling them to identify situations, actions and rules in more or less the same way.[11] The regularly and routinely occurring patterns enable social scientists to speak of such stable societal elements as 'social structure', 'institutions', 'the political', or the 'economic system'.

For the sake of completeness, it is important to make the point there may be significant subgroup differences within a society in terms of the expectations and normative definitions attaching to particular positions, but these do not modify the general picture. Indeed, such differences pose problems of some interest as the

studies of such phenomena as role conflict, marginality, social change, deviance and minorities illustrate.

In a similar vein, motives, reasons, intentions, and so on, are seen as causal antecedents and, therefore, 'external' to action, which impinge upon or coerce persons into certain behaviours. Behaviour, in short, has a motivated character. To ascribe a motive to someone, on this view, is to identify an 'inner' causal mechanism that produces an 'outward' display of behaviour. To say that workers strike because they have anti-management dispositions or attitudes is to say that the 'inner' picture of their working world produces, or causes, their intransigence *vis-à-vis* management. It is to give their striking behaviour a purpose or goal, and offers an explanation in terms of the ends the action is designed to meet. Weber's analysis of the economically innovative behaviour of ascetic Protestants ascribes a particular set of religious motivations which caused the persons holding such beliefs to work harder, be thrifty in their ways, endeavour to succeed in all that they did, and so on.[12] Of course, motives, though regarded as 'internal' and private states, are not considered to be randomly distributed among the population. As with rules, socialisation into a common culture means that motives are patterned, typical to particular socially defined persons and, in this way, produced by the social structure. Thus, occupancy of a particular social position 'leads to' the development of certain socially relevant and consequential dispositions which, in their turn, cause conduct or behaviour of a particular kind. The motivated character of such actions can be said to arise from the interest embodied in the occupancy of particular positions; voting for reasons of class advantage, joining certain associations in order to improve one's career prospects, or striking to improve the earning position of oneself and fellow workers are such examples.

This, then, is the basic model of social scientific accounts using those elements of meaning we have referred to as rules and dispositions. Although I have relied on sociology for the lineaments of this account, it is by no means restricted to that discipline. The assumption of homo economicus in economic theory is to postulate an actor with disposition to act rationally; the sense of history is provided by imputing motives to personages acting within specified historical circumstances, and so on.[13] Nor is it claimed that the accounts are as simple as this; simply that they follow, more or less, this basic mode. What must be added to it are the elements required for a positivist account of social action, though it would perhaps be more accurate to say that the mode of explanation outlined already owes a great deal to the attempt to make the explanation of social life scientific in a positivistic fashion.

The additional elements, stated more explicitly, are, first, that the explanation must be couched in a deductive form showing how

the observed behaviour can be deduced from a set of premises containing the theory plus stated empirical conditions. This means, second, that the behaviour to be explained must be defined independently of the so-called predisposing factors or causes; that is, social action must be defined or described independently of the rules or norms supposedly governing that action and independently of whatever motives might be said to cause it. Further, any descriptions entering into the deductive argument – and they enter when specifying the empirical conditions and the facts to be explained as well as in statements of the theory – must have stable meanings independent of the circumstances of their use. They must, in short, be 'literal descriptions'.[14] A deductive argument cannot logically work if the expressions used in it shift their meaning according to circumstance and occasion.

Given that the mode of explanation outlined earlier satisfies these methodological conditions, then the framework is coherent. The job of empirical research is to discover precisely the pattern of the contingent relationships between rules, motives, situations, social relationships and behaviour and formulate them as regularities bringing them under a theory which explains why they have the form that they do. To see just how far this is justified, let us examine the relationship between motives and the description of social action a little more closely.

As pointed out, in the typical form of explanation some internal and private characteristic of persons is offered, often implicitly, as a causal antecedent that predisposes the actor to behave in a particular manner. The motive and the behaviour are regarded as independent, the internal and private state being the mainspring, as it were, for the external behavioural display, the action. However, this formulation of the relationship gives rise to all kinds of methodological problems for social science. Being conceived as internal and private, and therefore not open to direct inspection, the problem is to devise methods of assessing such internal states to which effect a number of techniques, such as attitude scales, questionnaires, interviews, personality inventories, etc., have been devised. The results provided by these are usually correlated with 'objective' indices, such as level of education, social class, ethnic identity, associational participation, voting, spending patterns, etc., to mention but a very few of the kinds of variables employed.

With methods such as these, which rely for attributions as to 'mental states', to use a catch-all term for the moment, on what respondents say, there has always been the problem of relating what people say to what they do.[15] Prior to this problem, considerable effort has been expended improving the validity of such methods so that they can provide more accurate assessments of what is really 'in people's minds'. However, in other cases,

motives are inferred less from what people say about themselves and more from what they do or have done. From the fact that a person has tried to commit suicide inferences are drawn as to the state of mind of the person concerned. From the fact that dustbins are increasingly full of empty food packages it could be inferred that people are motivated to create more leisure time by using convenience foods. From the fact that the earliest capitalists were members of ascetic Protestant sects it can be inferred that their religious membership motivated them to engage in behaviour appropriate to capitalist accumulation.

In all the cases the problem has been to formalise the seemingly inevitable inferences that must be made in order to gauge the appropriate mental states of a person or group of persons. For the positivists, such inferences, unless rigorously controlled and measured, could lead to unwarrantable imputations on the part of the researcher. The situation led some to almost despair, seeking salvation in neurophysiology or some other method which would 'permit us to observe what was going on in a person's head in the same way that we can observe stomach contractions or nerve discharges in a hungry organism'.[16] Be this as it may, the contention here is that the conception of the relationship between so-called 'internal states', such as motives, intentions, and reasons, and the behaviour presupposed in the traditional approach outlined earlier is fundamentally misconceived.

Consider the following description of fairly mundane acts: 'I raised my arm', 'I raised the glass', 'I toasted the happy couple', 'I assuaged my thirst', 'I decided that the only thing to do was get drunk'. All these statements describe what could be said to be different actions and yet could also be said to consist of, or involve, much the same bodily movement. This one 'behavioural display' is capable, then, of being part of many different 'actions' and, generalising from this, we can say that there is no necessary one-to-one matching of an action description with a behavioural display. Pitkin puts the point rather well:

With the same physical movement, the stroke of a pen or the shake of a head, a man can break a promise or make one, renounce his birthright, insult a friend, obey a command, or commit treason. The same movement can, in various circumstances and with various intentions constitute any of these actions: so in itself it constitutes none of them.[17]

So an observer seeing me raise my arm and a glass of beer could describe my action in a number of ways. Any of those offered earlier could be appropriate, though 'I raised my arm' does seem singularly uninformative. The observer cannot, of course, see directly into my mind to inspect my intentions or bodily states. Nonetheless, noting the circumstances – it was a wedding, a hot day, I had just been jilted, etc. – some description could have

been supplied without much trouble or anguish about what was really the case. Some of these descriptions might well impute a motive or purpose to my behaviour, such as a desire to get drunk, be sociable, wish luck to the happy couple, assuage a thirst, and so on. In such cases what the motive does is tell us more about the action that is being performed, tells us what the person was doing, 'getting drunk', 'toasting the happy couple', 'assuaging a thirst' or whatever.[18]

In describing many actions we are unavoidably involved in imputing motives of one sort or another. The analytic force of motives, reasons, lies not so much in their being 'internal' and private mainsprings to action or behaviour, but in their being tantamount to rules for formulating a piece of behaviour *as* action of a particular kind. Motives, reasons, and other dispositional concepts can be seen as rules, or embedded instructions, if you will, for seeing behaviour in such-and-such a way, for explicating action further, for giving an account of that action. It follows that any particular behavioural display can be described and explained in a variety of different, and often competing, ways; that is, as several kinds of motivated action. As Austin expresses it:

It is in principle always open to us, along various lines, to describe or refer to 'what I did' in so many different ways . . . should we say, are we saying, that he took her money, or that he sank a putt? That he said 'Done', or that he accepted an offer? How far, that is, are motives, intentions, and conventions, to be part of the description of actions?[19]

The issue here is, perhaps, most apparent where the motivated character of an event is equivocal as in a case reported by Atkinson.[20] An 83-year-old widow was found gassed in her kitchen. She had lived alone since the death of her husband. Rugs and towels had been stuffed under doors and around windows. Neighbours testified at the inquest that she had always seemed a happy and cheerful person. The coroner returned an open verdict on the grounds that there was no evidence to show how the gas taps had been turned on. In this case, the circumstances of the death, which occurred during the winter, were insufficient to lead to a definitive verdict. For example, it was difficult to establish whether the rugs and towels had been used to keep out the cold and the draughts rather than the gas in, and, accordingly, to establish whether the turning on of the gas was intentional or whether unmotivated and due to absentmindedness. If the death had occurred in the summer, the motivated character of the events might have been less ambiguous. The fact that it happened in winter meant that the motivated character of the scene could not be clarified without recourse to circumstantial evidence regarding the widow's state of mind. Different assumptions regarding her state of mind would have instructed those responsible for reaching

a verdict to compose an account for the scene in particular ways or, vice versa, assumptions about the scene instruct them to make inferences about the victim's state of mind, and so on.

To argue or presume, as the typical mode of social scientific explanation would have us do, that behaviour can be described as a kind of 'brute fact' independent of motives or reasons, is seriously to misrepresent the relationship these have to the description of action. To describe my action referred to earlier as 'raising a glass to my lips' as if this were somehow more real than other descriptions which involve imputations or inferences about motivations, leaves out those very elements which make it a *social action*, though, it must be said, for some purposes such a description may well be adequate. However, such a description, treated as a description of indisputable 'brute fact' or fundamental 'observational datum', allowing meaning or the imputation of motive, reason or intention, merely as a subjective component, is to misconceive the process of action description.[21] Further, motives can be arguable, indeterminate and doubtful as a matter of course. Conjecture as to motive does not arise out of the absence of evidence we might have but do not – as the despairing positivist spoken of earlier might have it – but is a review of a range of possibilities where behaviour is just ambiguous, though for parties to those activities they may not necessarily be so.

As with motives, so with rules. Any piece of behaviour could be consistent with a vast number of rules, though in practice only some would be deemed relevant on any occasion. We normally think of rules as commandments to do, or not do, something we could engage in whether or not the rule existed. In this sense, rules are seen as independent of and external to the behaviour to which they apply. The Ten Commandments, for example, forbid various kinds of behaviours which, presumably, the framer of the rules regarded as less than wholesome, such as adultery, thieving, envy, worshipping false idols, and so on. There is, however, an aspect of rules which is not entirely separable from behaviour. Many rules can be said to be 'constitutive' of action in the sense that they tell us *how* to do something. It would be hard, for obvious example, to imagine playing chess without the rules of chess. Suspend rules like this and the activity in question ceases to exist. There would, of course, still be the behaviour of pushing pieces of wood or plastic around a chequered board, but this would hardly be playing chess.[22] In the same way it would be hard to conceive of 'obeying traffic lights' without the notion of traffic rules.

A relevant distinction here is that between a process being in accordance with a rule and a process involving a rule; between action in accord with a rule' and 'action governed by a rule'.[23] Any observed agent, process or action can be brought under

the auspices of many rule-like formulations, none of which is unambiguously *the* rule governing the process or event. As Coulter expresses it, the 'rules which make an action what it *is* are not reducible to any (set of) descriptions of physical or physiological transformations, since virtually any "action" or "activity" can be realized through *different* transformations . . . and the converse is also true . . .'[24] An activity accords with a rule if it exhibits the regularities expresses by the rule. It involves a rule if agents actually *use* the rule to guide or assess their actions. Rules, however, do not determine their own application but have to be used, and one of their more important uses is to bring a set of events, processes, persons or conduct, or all of these together, into some scheme of interpretation. In this sense the notion of rule is tied to that of 'making a mistake' and it is the possibility of this which helps distinguish being 'rule-governed' from mere regularity. That is, it enables us to evaluate what is being done, to attribute fault, to be subject to criticism. Invoking rules is a way of depicting or describing action, of pointing out what it is we are doing, of making our actions accountable. Used in this way, rules are part of our resources for making the world understandable.

The upshot of these remarks strongly suggests a very different sort of relationship between action and its description, and the rules or the motives which could be said to govern the action, from that envisaged in the positivistic approach. For one thing it claims that action and their descriptions are conceptually tied to reasons and motives, neither being describable as if they were separate and independent; on the contrary, they inform each other reflexively.

This discussion of rules, motives and other intentional concepts – let us call them action concepts – presupposes that these are major means through which members of society meaningfully construct their social world. It is making the point, too, that the vocabulary of action displays very different properties to those presupposed in a causal one. Action is predicated on the idea of an agent, specifically a human agent. The vocabulary of action is used by human beings in speaking to each other about what it is they are doing. An agent differs from a causal process because he or she can be said to make a choice, be held responsible for, initiate, do something, and so on. An action can be praised or condemned, commanded or forbidden, because the person performing the action can be praised, condemned, commanded or forbidden.

The use of causal expressions in action contexts should not entice us into thinking of invariant relationships or into thinking that these are somehow more real than non-causal ones. To say something like 'The fact that it was dark caused me to trip over the stool' is to make use of a causal type relationship between the amount of available light and the ability to see, but its import is

offering an excuse, suggesting that I was not just clumsy, but could not help myself, and could not be blamed for what happened. Actions do not come along conveniently labelled as 'suicide', 'clumsiness', 'obeying traffic signals', 'walking the dog', 'voting for a party of the working class', 'being motherly', and so on, but have to be described, and doing this is an action. It involves more than just looking at 'concrete behaviour', if this makes any sense at all, but paying attention to circumstances, reasons, motives, rules, and so on.

Of course, it is clearly not the case that intentions, motives, rules or conventions, are necessarily imputations in action descriptions. One can kill inadvertently, deceive without intending to deceive, and so on, while in other cases matters are not so clear; can one murder without the intention to murder, promise without intending to promise, for example? Events can also be described without motive implications: 'The gun happened to be loaded, the trigger knocked, the bullet hit her and she died of wounds received.' Whether or not such a description would be deemed accurate or adequate would depend on the purposes for which the description was formulated. The description of an action is an occasioned event, is itself an action, done for some purpose, informed by some interest, done in some context. The point is, however, that action descriptions are essentially defeasible; that is, it is always possible, in principle, to argue against any particular description by bringing in other particulars about the situation, the person, the event or the object. Let me illustrate with another homespun example.

Some time ago I was walking down a corridor and, as it happened, I stopped to hold open a door for a woman following behind. The woman stopped and made the remark that what I had just done was sexist. I apologised in some confusion and said that holding the door to allow her to precede me was a gesture of simple courtesy which I would have done for anyone, male or female. This did not carry a great deal of weight and the argument went on for some minutes. The point of this anecdote is not the by now familiar one of the same behavioural display – opening the door, standing back, etc. – being open to different interpretations, which it is, but that it is open to different descriptions as an action. The issue is not one of fitting the right description to an event as one might have to fit round pegs into round holes or the right words into the lines of a crossword puzzle. It has to do with justifying an action, describing it in socially consequential ways. To ask whether the right description of the act I performed was 'a courtesy' or 'male chauvinist piggery' is to miss the point. Neither description could be right in any absolute sense. The matter of description is bound up with justifying my action or my point of view with appropriate reasons and arguments, to do with

persuading, cajoling, threatening, coercing, etc., someone that what happened was of such and such a character. The woman and I could have argued our case sensibly. I could have pointed to my exemplary record of courtesy in all things, while she could well have taken this as more evidence for her case, arguing that such behaviour merely indicated a patriarchal attitude on my part and that sexism was part and parcel of this. As in the case of motives, various arguments could be invoked, reasons adduced, in order to support the claim that the scene should be looked at in a particular way. We could only have come to some agreement if we held, as it were, a framework in common whereby such disputes could have been resolved.[25]

However, the failure to find 'common methods', so to speak, is not some failure of our knowledge but a characteristic of our vocabulary of action. In pointing to the essential defeasibility of action descriptions it is being claimed that the vocabulary of action is part and parcel of moral discourse and, as such, is concerned with the appraisal of conduct. In this realm of discourse what we have done or are doing has no well-defined description in ways required by positivistic science, though such descriptions work well enough in the context of action. Knowing what it is you are doing, what you are going to do, what you have or have not done, cannot be fully explicated by looking at what in fact you do. To know what you are doing is to be able to elaborate the action, say why you are doing it, excuse or justify it if necessary, and so on.[26] What is at stake, in short, is what in fact was done. Was my opening the door a flagrant piece of chauvinist piggery or the last throw of knightly courtesy? What the dispute is about is precisely this and not the sort of issue that can be resolved by consulting some putative dictionary of social actions.

These arguments suggest that the description of social action is a problemmatic matter both for social actors and observers alike. Descriptions are, it has been pointed out, deeply sensitive to context and defeasible. They are social activities done for particular purposes and are deemed adequate or inadequate, as the case may be, in terms of these purposes. This leads on to another general property of descriptions, namely, that they are always, in principle, incomplete. Whatever is included in a description is always selective and cannot exhaust all that can be said about an object, event or a person. More could always be added: a person could be described, for example, as 'dark haired', 'tall', 'selfish', 'reticent', 'a worker', 'of higher than average intelligence', and so on, but these could not exhaust all that could be said about the person. Descriptions are selections from what could possibly be said and, depending on the occasion, be perfectly adequate for that occasion and that purpose. Although descriptions have a fringe of completeness about them, or, as

Wiseman puts it, an 'open-textured' quality, this does not impair their ability to do the job required since nothing like absolutely definitive completeness is ever attempted by speakers of a natural language.[27] As said before, often a single descriptor will provide an adequate description – 'this friend', 'my colleague', 'the landlord of the Plough', 'that stupid dog' – the remaining particulars being, as it were, bracketed away for present purposes or their sense 'filled in' using the specifics of the contexts in which uttered. It is always possible, however, to produce alternative descriptions of an object, event, action or person. Other properties may be added which modify the original description, or other aspects come along which provide additional elements to qualify, modify or even undercut the original. The relationship between the features of an object, an event, an act or a person and some description is not a determinate one. A speaker's selection of a descriptor from all that could be said or predicated of some phenomenon normally tells the hearer something about the practical purposes of the speaker in offering that particular description. It calls forth a host of possible elaborations, and this means that, on the occasions of its use, a description can only index what it might mean; a quality referred to, by Garfinkel, as 'indexicality'.[28]

The arguments reviewed here would seem to challenge many of the assumptions and claims of positivistic science. The social science tradition from which they derive takes meaning as central to social life and points to important differences between what I have termed the 'vocabulary of action' and that of science. The rather grand, and overworked, term 'meaning' more than hints at the intersubjective character of social life and, in its way, points to the fact that human action is not as predictable, as determined in its course, as the inanimate subject-matter of natural science.[29] Whereas positivism might perhaps attribute this lack of the paucity of good measurement, good theories and the infancy of the social sciences, or to the greater complexity of the social world compared to the natural, what is being claimed here is more fundamental, namely, that human life is essentially different, and that this difference requires another methodology to that required by a positivistic conception. It might also require a different kind of knowledge. (For the moment I shall beg the question of whether the fact that social life is meaningful can be reconciled with the alternative view of science outlined in Chapters 3 and 4.) Of course, matters hang heavily on the banal observation that human beings are capable of giving accounts of their own lives and their relationships to others. However, what is also being claimed is that this ability is essential to there being a social life at all. Giving reasons, justifications, explanations, making descriptions, are themselves profoundly social activities and, consequently, make social life what it is. What we have to

examine now is whether or not these considerations do imply that a social science is impossible.

REASONS VERSUS CAUSES

One major methodological consequence of binding reasons, motives and other dispositions to the notion of action is that it raises questions about whether social science can be concerned with causes of action. The conceptual tie between the imputation of reason and motive and the description of action argues that one of the major criteria for identifying a causal relationship is not met; that is, the logical independence of the antecedent factor, the reason, and the effect, the action. Instead a very different relationship is claimed in which the reason (or the motive) and the description of action are mutually informing, though not in any determinate way. A further objection to the causal account arises from issues addressed in connection with the description of action itself, and is an objection to the use of the hypothetico-deductive model of explanation. It is argued that such a method can only be used if literal description is possible; that is, description not dependent for its sense or meaning on the occasion of its use.[30]

As has been pointed out, descriptions enter into the hypothetico-deductive form of explanation in at least two places: in statements about the initial conditions and in the deduced prediction that constitutes the explanandum. The burden of the argument here, however, is that literal description is possible in the social sciences only by ignoring the interpretative nature of social action and forcing categories into a framework to satisfy the requirements of literal description. *Knowing what* people are doing (including oneself) is *knowing how* to identify what they are doing in the categories of a natural language, which requires *knowing how* to use those categories in discursive contexts . . .'[31] Thus, if I want to describe a piece of behaviour, which may be an utterance or a bodily movement, as, say, indicative of 'mental illness', neither the utterance itself nor the movement will indicate this without the use of some interpretative schema which enables me to compose this as an instance, an indicator, of mental illness.[32] It goes without saying that different schemas would prompt different descriptions, though not always inconsistent ones. Similarly, if I make use of someone else's descriptions of the same elements, to understand this I must use the same interpretative procedures in order to appreciate how the instances were gathered into the description used. Garfinkel refers to this as the 'documentary method of interpretation' in which a set of appearances, which may be objects, events, persons, or symbols, is taken as evidence for some underlying pattern,

while the postulated pattern serves as a guide for seeing how the appearances themselves should be read. Thus, the classification of the description of any piece of behaviour on a given occasion as an instance of a particular type of action 'is not based on a set of specifiable features of the behaviour and the occasion but, rather, depends on the indefinite context seen as relevant to the observer, a context that gets its meaning partly through the very action it is being used to interpret'.[33] The meaning, hence the action being performed, of a raised arm would depend on the context; similarly, the context itself would be partly made intelligible by the meaning or the description given to the movement. It follows that any interpretation is always both retrospectively and prospectively revisable in 'light of further evidence'.

These arguments, and we shall examine more in the next chapter, seriously question the idea of a social science based on the search for causes. Winch, among other critics, argues that action concepts are logically incompatible with the idea of causal necessity and, thus, with natural scientific causal explanation.[34] There have been attempts to deny the force of this distinction between reasons and causes. MacIntyre, for example, bothered by the fact that agents may offer many reasons for why they do something, wants to argue that an agent's possession of a reason may be a state identifiable independently of an agent's performance of an action and, accordingly, a candidate cause.[35] The difficulty here lies in specifying what is meant by an agent being in a state of possessing a reason. It would seem that this condition could only be based on the avowals of the agent concerned, although others, too, are equally free to impute what reasons they may to an agent without that agent necessarily having formulated that reason to himself or herself prior to the action. Reasons enter as justifications, as further elaborations of actions and are not necessarily formulated as prior antecedents to the action for which the reason is relevant. Nor does the argument destroy the conceptual link between reasons and the description of action: a relationship which is not one of independence or contingent invariance.

There are, nonetheless, other difficulties with the distinction between action concepts and causal ones. The fact is that we sometimes talk about the causes of actions and give causal accounts of actions. Some would want to say that this is merely a careless habit of speech, but this is hardly satisfactory. The whole issue here is bound up with an old philosophical problem to do with free will, a debate which can be briefly summarised as follows. On the one hand are arguments which say that we hold people responsible for what they do, blame them when they behave badly, and so forth. So, since there would be no point in blaming someone for doing something beyond his or her control, then at least some

of our actions must be the agent's doing and free. On the other hand, there is the view that what an agent does is the function of upbringing, personality, situation, and the like, and therefore, he or she is merely the helpless victim of all these factors. While we may all feel free to choose and act, this is, in fact, an illusion.

The conflict here, while easily stated, is not so easily resolved. The notion of cause itself is used in a variety of ways, not all of them accommodated within a Humean conception. Sometimes we give a causal account of action. Peters suggests that we are likely to do so when something has gone wrong, 'where there is some kind of deviation from the purposive rule-following model; when people, as it were, get it wrong'.[36] In such cases doubts are raised as to whether an action was fully performed. Also, we tend to give causal explanations of action where the actor's choice or responsibility is minimal or, alternatively, have no interest for us. We might do this, as Pitkin illustrates, in considering how to get a third party to do something.[37] Here causes are not incompatible with reasons, motives and intentions. In historical explanation, for example, we tend to be rather more interested in accounting for why a person did what they did than in holding him, or her, responsible or attributing blame.

One could say that this is all very well. The practices of ordinary language in respect of causal attributions versus the imputations of motives or reasons are well taken in connection with particular actions, but are not exactly relevant to social science which is concerned with the explanation of whole classes of actions. In reply, one could say that in trying to achieve explanation of this kind one runs the risk of stretching language until insoluble conceptual difficulties are created. 'Free', 'determined', 'cause', are concepts connected to many other concepts. If asked the question, 'what is a free action?' we could, with little difficulty, give many illustrations, synonyms, analogies, offer concepts roughly equivalent in meaning, and so on. If we denied that any actions were free then we would be involved in rejecting whole categories, and relationships, denying, in effect, whole regions of our language. This is something that we might wish to do but, in doing so, we would also ban a host of actions in the process. Terms such as 'free', 'cause', 'determined', and concepts associated with them, are used in particular contexts, used for assessing some particular action taken or contemplated. Whether a person has or has not a choice are questions partly dependent on the position adopted by a speaker in the situation at issue. I might say to a close friend, 'I can't come to the cinema with you because my parents are visiting' and intend that the force of the parental obligation means that I am not 'free' to go. If, on the other hand, my friend wanted me to accompany him to the hospital, it is possible that I might override parental obligations. I might not for an acquaintance, though even

that might be dependent on the seriousness of the reason for the visit to the hospital. The point is that in each of these situations I am taking a position with respect to others, and it is by this I will be judged. What is difficult is to generalise from these particular cases to distinguish criteria by which *all* actions are to be seen as causally determined.

In any event, it is difficult to see how one might *discover* whether all our actions are really causally determined or whether they are really all free. In fact, it looks as if the issue is hardly a matter about the facts of the world at all. If, as was suggested above, we seriously entertained the idea that all action was caused (or free) this would involve vast changes in the conceptual system in which and through which our lives are constituted. It would be difficult to speak of responsibility, blame, punishment, honour, achievement, generosity, valour, skill, quality, failure, conduct, and so on. We may, it is true, retain the use of these and like terms, but their point would be lost. One might still 'punish' but this would be the application of another causal mechanism designed to modify behaviour. One might still 'praise' but this would not be giving credit for some personal achievement and would only add another factor to induce a particular behaviour, one that we as 'blamer' or 'praiser' could ourselves taken no credit for since, we, too, could not help ourselves. The point is that though our language and our understandings do change and are conventional they are not arbitrary. They are shaped by our conduct as human beings. The determinist might argue that the distinction between actions and causes arises because we are ignorant of the causes of some actions, but this is to miss the point.

It begins to look as if what we are dealing with here are two different perspectives, Pitkin calls them 'that of the actor engaged in action, and that of the observer'. both deeply embedded in our language and form of life.[38] We cannot take either one alone without losing, in some way, crucial aspects of social reality. A purely observational science of society using a causal vocabulary independent of our action vocabulary might be possible but the question is: What would we then be observing? We would not be able to see promises, power, interests, war, worship, organisations, exploitation, deprivation, and the like, since these, definitionally, could not strike the action-concept free observer. In short, such a science 'could not answer the questions we can now formulate, for they are formulated in the concepts we have'.[39] In this connection, structuralist and post-structuralist theories seek to evade the dilemmas reviewed here by 'decentring the subject'; that is, to overcome subjectivity and individualism by rejecting any form of empiricist epistemology in favour of an analysis of the structural relations and realities underlying the surface appearances of social and cultural phenomena.[40]

It appears, then, that the old dichotomy of reasons versus causes is not as simple as either of the protagonists would have it. What is perhaps clearer is that it is inappropriate to use a purely causal vocabulary as the only one suitable for a social science. The arguments of this chapter, while not resolving many of the issues, do strongly suggest that the traditional manner in which this causal vocabulary has been used in much of social science is seriously flawed. In the next chapter I shall examine further arguments relevant to this matter.

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