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INTRODUCTION: SOCIAL SCIENCE IN PRACTICE

I Preamble

The thirteen papers reprinted in this book were written over a period of nearly forty years, in response to a variety of requests and opportunities, and draw on a very mixed bag of empirical data. Some are my contributions to symposia I was asked to join, others were written for *Festschriften*, and a couple were delivered as addresses to conferences. Yet although none was designed as a component in an orderly exposition of some gradually specified theoretical stance, they do all imply, some more strongly than others, a view of the enterprise of social science that differs from the views held by many of my colleagues. This view has gradually become clearer to me as I have argued with them. The time has come to make explicit the many assumptions that I have silently, and to a large extent unconsciously, made in writing these papers. In this introduction I try to meet the challenge; in particular I endeavour to present a picture of social inquiry that takes account of the inherent differences between the natural and the social branches of science, and that distinguishes social science from the humanities.

In building up this picture, my starting-point is the practice of social science, rather than the philosophy of social theory from which so many other views have been derived. I am concerned with 'the social sciences, as actually practised and identified in contemporary societies', as Gellner (1984: 567) puts it. In this way I am following my own intellectual development, for, like many social scientists of my generation, I began making empirical inquiries about the social world blissfully untroubled by questions of ontology and epistemology. I was content to shelter behind Fortes and Evans-Pritchard's (1940a: 4) bald and misunderstood assertion that they considered the theories of political philosophers to be 'of little scientific value'. Only later, when I found myself obliged to defend what I

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was doing against critics from many quarters – notably natural scientists, historians and philosophers – did I attempt to work out in coherent fashion the hitherto somewhat muddled analytic framework that I had been taking for granted. I am aware that philosophers of social science like Richard Rorty (1980: 315) can proclaim ‘the demise of epistemology’ but the continuing flood of books and articles on the philosophical foundations of social science indicates that, even if buried, the corpse of epistemology is still kicking. More importantly, the significant, and in my view largely deleterious, impact of theoretical orientations such as ethnomethodology and reflexivity on the praxis of social inquiry shows that the philosophy of social science is too important a matter to be left to the philosophers. Given the limitations of possible human achievement and the diversity of human preferences, we cannot hope to avoid a division of labour between those who enjoy learning about the real world – whether this be the fantasies of Neasden grandmothers or the bargaining in Melanesian markets – and those who concentrate their attention on discussing whether and how anyone can learn anything about the real world. Each activity has its own autonomy, and each set of practitioners pays at most only intermittent attention to the pronouncements and discoveries of the other. In the natural sciences the divide is even sharper. The contempt in which many practising natural scientists hold the philosophy of science is notorious; they regard the subject as a soft option that good students should be encouraged to avoid in favour of *real* science courses. With natural science in mind Medewar (1969: 12) comments that

Science, broadly considered, is incomparably the most successful enterprise human beings have ever engaged upon; yet the methodology that has presumably made it so, when propounded by learned laymen, is not attended to by scientists, and when propounded by scientists is a misrepresentation of what they do. Only a minority of scientists have received instruction in scientific methodology, and those that have done so seem no better off.

In most branches of social science there is more open recognition that some amount of theoretical, and even philosophical, sophistication is necessary to prevent curiosity about the real world leading only to low-level empiricism and mindless number crunching. Indeed Henri Poincaré is said to have commented ‘The natural sciences talk about their results. The social sciences talk about their methods’. Nevertheless the divide between the empirical investigators and the theoreticians exists even in social science; in Chapter 13 I indicate why, in my view, it is a permanent feature and why advances in understanding are made sometimes mainly on one side of the divide and sometimes mainly on the other.

But that is an Olympian account of the scene. For the moment, if I am to defend the view that those of us whose primary interest is in ‘what really happens’ need to have some understanding of the extent to which we can

possibly satisfy that interest, I have to set out the broad outlines of my own understanding of the matter. In other words, I have to construct a model of social science.

2 Sciences and scientists

Let us begin by considering the relation between ideas and the individuals who propose, oppose or defend them. This relation is sometimes crucial for understanding ideas, and sometimes quite irrelevant. For example, the history of scientific ideas may be handled in many ways depending, roughly speaking, on the relative weight given to the interactions of disembodied ideas and to the histories of the individuals who propagated them. Notably, the history of mathematics can be presented satisfactorily in terms of the sequence of the discovery of its theorems, with minimal reference to the people who discovered them; the history of graph theory, a branch of mathematics becoming of interest to social scientists, has been written in this way (Harary 1973). The natural sciences, which are less austere than is mathematics, can be viewed historically from either internalist or externalist standpoints, giving less or more weight respectively to the wider social environment within which scientific discoveries were made and paradigms discarded and adopted. However, when we come to the social sciences, we have to recognize that the social context in which theories are propounded and inquiries undertaken is not merely a background factor which, at the whim of the analyst, can be either incorporated into the analysis or omitted from it. To assess the rationale, the utility and the success of a social theory we are forced to take account of the social context within which the theory was developed and the political, religious, economic and even demographic characteristics of those propounding and opposing the theory. Nowhere is the significance of the broad social context made clearer than in communist-led countries, where drastic changes in the theoretical interpretations that are allowed, and in the topics that can be investigated, and indeed the very establishment or abolition of social science disciplines have all depended on political vicissitudes (e.g. Cheng and So 1983). These considerations apply clearly to the history of ideas but have to be heeded also in discussing the present status of ideas, at least when the ideas under scrutiny are those of social science.

For many of the features that distinguish social scientists from, say, natural scientists and humanists and from the lay public derive directly from the specific differentiae which mark off the social sciences from other branches of systematized knowledge. The epistemological status of the social sciences has been controversial in the past and remains controversial today, and in like fashion the status of their practitioners, the social

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scientists, has been and continues to be a matter of debate not only among the practitioners themselves but also in the wider public arena. The roots of this latter debate are to be found, at least in part, in the unsettled status of the social sciences themselves. There is thus a dialectical relation between the intellectual characteristics of the social sciences and the social attributes of their practitioners, each influencing the other. Indeed, the existence of this dialectical or two-way relation is one of the features that mark off social sciences and social scientists from the natural sciences and natural scientists. For example, the speed of light is the same, whether it is measured in Washington or Moscow. Atomic reactors, not to mention atom bombs, work according to the same principles on both sides of the Iron Curtain. In the Soviet Union Marxist physicists enjoy a monopoly of research opportunities, whereas in the United States they are denied them; yet Marxist and bourgeois physicists are in agreement about the inventory of elementary particles. On the other hand, in social science the same differential treatment of Marxist and bourgeois practitioners in the two countries is paralleled by a lack of agreement between them about, say, the class structure of advanced societies and the nature of the state. To discuss what differentiates the social from the natural sciences we must therefore pay attention also to the differences between social scientists and natural scientists.

3 The attributes of social science

The strength of the two-way relation between science and scientist is not the only attribute of social science that particularly bears upon the social status of its practitioners, the social scientists. The next that should be mentioned is the fact that the subject-matter of the social sciences is made up of phenomena that are familiar to the general public. This is a point that has been repeatedly emphasized by Giddens, as for instance when he writes that 'the production and reproduction of reality . . . has to be treated as a skilled performance on the parts of its members' (Giddens 1976: 160). Giddens would certainly not claim to be the first to make this point, and the same viewpoint was adopted earlier, and in a more uncompromising form, by young and enthusiastic ethnomethodologists who, in the early '70s, tended to claim that 'members' were at least as skilled in the analysis of their own social environment as any social scientist who tried to study them (cf. Mehan and Wood 1975: 37). This stance of epistemological populism was particularly marked in sociology, but an analogous viewpoint was found also in social anthropology where, under the rubric of 'cultural relativism', it has had a longer history and a wider theoretical underpinning. In both disciplines this viewpoint has had serious consequences for the debate about whether, or to what extent, social scientists

are professionals and not merely verbose citizens. Substantial numbers of practitioners in the two disciplines nowadays embrace the many-headed hydra of 'reflexivity' and risk becoming engulfed in the black hole of an infinite regress (e.g. Rose et al. 1987). There is no corresponding phenomenon in the natural sciences, for most citizens are not skilled performers in any of them, and notions of epistemological populism are nowhere entertained. Many routine inquiries in social science, of course, do not depart substantially from the natural science paradigm, in which, among other things, there is a clear distinction between the observer and the observed, and where whatever is observed supplies only data, not analysis (cf. Barnes 1980a: ch. 2). Nevertheless, we cannot overlook the significance of the fact that social scientists are the only scientists who gather most of their data by talking to people and asking them questions.

A third intellectual feature that affects the social position of social scientists is the quality of the language used in social science. On the whole, the language of natural science is incomprehensible to the untutored lay person. Incomprehensibility arises in two ways, from the words that are used and from the meanings attached to the words. Chemistry seems to constitute the extreme case, for many of the words in its special vocabulary are so long and segmented that they cannot be assimilated into ordinary discourse, and the meanings attached to these words also seem hard for the lay person to grasp. Physics makes much greater use of words that are drawn from ordinary speech, as for example the so-called 'colour' of quarks, but gives these words meanings that are both precisely defined and significantly different from their meanings in lay speech (Hey and Walters 1987: 156–7). Social science also uses ordinary words and continually makes attempts to give these words precise and technical meanings, as for example with 'class', 'honour' and 'status'. But there is a striking difference between, say, physics and sociology in the way in which their use of language is seen by outsiders. No one complains because physicists nowadays use, for example, the word 'atom' in a way different from its Shakespearean usage; there is no lay assumption that there is a *real* meaning to the word 'atom', and that any departure from this usage by physicists is preposterous jargon. However, when social scientists use words like 'power' and 'authority', the lay public is always ready to protest that these words are not being used properly, and not being given their *real* meanings. These protests occur despite explicit statements by social scientists that these words are, in the context concerned, to be understood not in their everyday usage but in some other, usually more restricted, and analytically more useful, sense. Yet at the same time the wish, or need, to make some impact on the wider world, and in particular to make a contribution to the formulation of public policy, constrains social scientists to use, as far as they can, the language of the market-place and the

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political arena. As Rorty (1981: 574) remarks, 'Predictions will do "policy-making" no good if they are not phrased in the terms in which policy can be formulated'.

Nevertheless one defence against this kind of lay criticism is to concoct technical terms that initially do not have any clear lay meaning at all. An example of this ploy is the invention of the term 'socio-economic status'. The adjective 'socio-economic' was first used as early as 1883 (Ward 1883: 525) and the expression 'socio-economic status' was used later to avoid some of the confusion over the diversity of lay and scientific meanings attached to the word 'class'. Forty years ago the former term was still seen by the laity as a typical instance of unpleasant sociological gobbledegook, but nowadays it raises no eyebrows. For with this and similar technical terms a process of diffusion operates which is fundamental for understanding the relation between social science and the wider public. Robert Merton notes that some sociological technical terms drift into ordinary speech (Merton 1981) but there is more to this process than simply drifting. Would-be trendy writers in high-class literary magazines and Sunday newspaper supplements take up new technical terms from social science for their own purposes, and thus begin the gradual modification of the denotation of these terms. The precise definitions with which these terms may have been first introduced are soon forgotten, and by the time the words have passed into ordinary everyday speech they have acquired all the polysemic attributes that make lay speech so vivid and ambiguous at the same time. 'Socio-economic status' can now be heard on lay lips all over Australia, and I guess elsewhere in the English-speaking world, along with 'J-curves', 'charisma' and 'relative deprivation' (cf. p. 144).

The process of transformation may in some instances go a stage further, whereby a technical term acquires a lay meaning far removed from the meaning intended for it by the social scientists who first used it. A good example is provided by the term 'network'. The word has been part of the English language since Shakespearean times but I think it was not used in social science in a technical sense until 1954, when I published an article which hinged on a technical use of the word (Chapter 4). Some twenty-five years later interest in the topic among social scientists had grown sufficiently for the launch of a new international journal with the title *Social Networks*. This title conforms to the 1954 definition, but by that time the idea of a social network had already passed into ordinary speech and had lost the precise denotation I had given it. This did not surprise me, but I must admit to being surprised at the emergence of 'networking' as a new English verb, and to the appearance of journals and other publications advocating 'networking' as a new form of happiness, as Muller (1986) claims in a poem used as promotional material by the American-based

Networking Institute Incorporated. On the other hand, the publication of whimsical in-jokes, based on the 1954 concept of network, and incomprehensible to the general public, in the increasingly technical pages of the journal *Social Networks* (Keul and Freeman 1987), is strong evidence for professional closure in this sector of quantitative sociology.

Thus we have a somewhat paradoxical situation. In general, natural scientists are not criticized by lay writers for writing incomprehensibly in their technical journals, and for making extensive use of technical jargon, whereas the publications of social scientists are continually subjected to this kind of criticism from lay reviewers, or indeed from fellow social scientists belonging to rival factions (cf. Becker 1983: 578). But at the same time there is a continual trickle of words from the technical language of social science into ordinary speech, with changes in meaning occurring along the way. Commenting on this process, Runciman (1965: 47) remarks that 'jargon is not jargon if it succeeds: like treason, jargon is by definition a failure'.

Social science thus not only lacks a vocabulary of technical terms with stable meanings; in addition its persistent attempts to create such a vocabulary are derided by the general public as insufferable goobledookery. Maybe this criticism would not seriously impair the aspirations of many social scientists towards public recognition as professionals and as scientists, were it not for another feature of the relation between them and the public. For not only do the technical terms of social science pass into the wider arena of ordinary speech, albeit with modified meanings, but the findings of social inquiry are also taken up into the corpus of general knowledge and common sense. In part, of course, they are there already, for a great deal of empirical inquiry in social science is directed towards discovering which popular assumptions about the workings of society are true and which are false. Fortunately many are true. At least it is fortunate for society that many citizens are substantially correct in the assumptions they make about the social environment in which they have to operate and survive; but at the same time it is unfortunate that social scientists are often criticized for wasting time and effort on discovering what everyone knows already.

There are, however, other instances where empirical inquiry demonstrates that a popular assumption is wrong and where, therefore, social scientists might expect praise from the laity for their efforts in revealing the truth. But, alas, the praise, if it is accorded at all, does not last long. After a lapse of time the research finding becomes incorporated into popular wisdom. The stimulus for the change is forgotten, so that in retrospect the inquiry may well be seen as yet another instance of wasting money to tell us what we know already. The research conducted during the Second World War by Stouffer and his colleagues (1949) on morale in the United

States Army is an example of this process. Their finding, that morale was higher in army units where promotions were comparatively rare, was seen at the time as unexpected and contrary to popular belief, but forty years later is part of the accepted wisdom of every personnel manager, if not also of members of the wider public. For these reasons I have argued elsewhere that, at least for sociology:

Building an edifice of professional achievement is like building on quicksand; the scientific achievements of one moment are buried as common sense in the next.

I go on to describe 'the continual denigration of sociology in the popular media, in literary journals and by colleagues in other disciplines' as 'a rational defence by the laity against the claims of sociologists to know best' (Barnes 1981c: 22). Because so much of sociological inquiry is directed parochially, at the society to which the sociologists concerned themselves belong, the discipline is particularly vulnerable to this type of criticism; social anthropology is on safer ground when its inquiries are directed at exotic others, but when it tackles the local scene it encounters the same denigration.

The absorption of social science findings into the pool of common sense explains a contrast to which Gellner calls attention. He notes, somewhat over optimistically in my view, that on the one hand the social sciences are characterized by

- The presence of well-articulated hypotheses and their systematic testing.
- Precise quantitative measurement, and the operationalization of concepts.
- Careful observation by publicly checkable methods.
- Sophisticated and rigorous conceptual structures, and great insights.
- Shared paradigms, at any rate over sizeable communities of scholars, and persisting over prolonged periods.

On the other hand, in terms of 'the impact on our cognitive world', the social sciences are quite unlike the natural sciences, for the former obviously lack 'a generally overall consensual cognitive activity, radically discontinuous from the insights and techniques of ordinary thought, and unambiguously cumulative at an astonishing and unmistakable rate' (Gellner 1984: 584). I would doubt whether cognitive activity in the natural sciences is, or ever has been, 'radically discontinuous' from ordinary thought, but I agree with Gellner that the discontinuity is greater for most natural sciences than it is for most social sciences. Common sense absorbs quickest those scientific findings that form part of the common way of life.

Cambridge provides good evidence of the extent to which those who are not social scientists expect the literature of social science to be written in language that all can understand, and also expect its findings to be in

conformity with their lay understandings. The Press Syndicate of the university has the fortnightly task of deciding what books the Press shall publish. Apart from two ex-officio members, the other sixteen Syndics are drawn from a range of disciplines intended to match the range of books currently published by the Press. While I was serving as a Syndic there was an obvious hierarchy in the way in which proposals for publication were discussed. In most cases proposals in mathematics were quite unintelligible to all Syndics except for a solitary mathematician, and only he spoke to these proposals. Natural science proposals were discussed more widely, not only by the Syndic who was expert in the discipline concerned but also by his fellow natural scientists. Proposals in the humanities were assessed mainly by the humanists, but with significant contributions from several other Syndics whose professional qualifications lay elsewhere but who were also well versed as amateurs in music, painting and literature. Proposals in the social sciences, particularly in sociology, were however seen as fair game by all Syndics; indeed there was, I think, a feeling that every well-educated person ought to have an opinion about the merits and demerits of social science proposals and that no Syndic, whatever his or her speciality, could escape sharing full responsibility for deciding whether or not to publish.

4 The dearth of social laws

In reviewing the relevant differences between the social and the natural sciences I have mentioned so far the dialectical relation between social scientists and the theories they have supported or contested; the competence of lay citizens in the subject-matter of social science, contrasted with their comparative lack of competence in the natural sciences; the impossibility of preserving a technical language in social science uncontaminated by lay connotations; the propensity of common sense to soak up the achievements of empirical inquiries. All these are features of social science that entail a social and intellectual status for social scientists that is significantly different from that occupied by natural scientists. There is, however, one more contrasting feature that many natural scientists, as well as philosophers of science, regard as inescapably fatal to the aspirations of social scientists for recognition as 'real' scientists. By and large, the corpus of accumulated social science understandings lacks propositions analogous to the laws, that is to say hypotheses that have survived attempts at refutation, that characterize natural science. Instead of logically interrelated scientific laws, social science understandings are made up of interpretations and models, together with a vast but only minimally interrelated array of empirical findings.

Most social scientists in earlier generations saw this state of affairs as a

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temporary condition, a shortcoming arising from the immaturity of the social sciences that would gradually be remedied with the passage of time, the accumulation of more evidence and the development of more powerful theories. But a later generation has abandoned this Whiggish prospect and views even Merton's famous tactic of concentrating on middle-range theories as barking up a blind alley. Giddens for instance argues that although there are valid generalizations in social science, 'they have a logical form substantially discrepant from laws in the natural sciences, whether these be universal laws or whether they be statistical-type laws . . . In social science to generalize about something means generalizing also about people's knowledge of the circumstances of the action involved' (Mullan 1987: 107). I am in broad agreement with Giddens on this point, though I would express it somewhat differently. As I see the difference, in the natural sciences law-like generalizations help us to understand how things work, whereas in social science law-like generalizations help us on the way towards making things work differently.

The main consequence of this difference in intellectual structure between the two branches of science is the existence of a plurality of models and interpretations in social science, in contrast to the prevalence of monolithic orthodoxy in natural science. The contrast is of course not absolute, for there are plenty of rival interpretative models in natural science also. But these are all to be found on the frontiers of inquiry, while behind the expanding frontier is an essentially orthodox terrain where scientific controversies have been resolved and are now only of antiquarian interest. In this light the whole of social science is a frontier zone, with rival interpreters contesting every square inch of territory. Hence we have not a unified and integrated set of social science propositions, supported by evidence, but rather a plethora of rival schools, appealing to partially overlapping bodies of empirical evidence or, in some cases, not bothering very much about empirical support at all. Consequently social scientists continually criticize one another's interpretations, contradict one another's assertions in courts of law, and offer conflicting advice to governments and other clients. Answers to examination questions in natural science can be easily sorted into right and wrong, whereas wrong answers in social science typically consist of, say, attributing to Weber the views held by Durkheim rather than asserting a false interpretation of events.

I realize that these contrasts fit some social sciences more closely than others. In some branches of psychology, scientific laws have indubitably been discovered and tested. But it can be argued that the label 'psychology' covers an area of inquiry that straddles the natural–social divide, and that scientific laws are to be found largely or solely on the natural science side of the subject. Indeed, recently in Britain a joint committee of the Royal