CHAPTER I

Introduction

This is a book about the history of sociological research methods. As such, it approaches the history of sociology from an unusual angle; history of sociology has most commonly been written as the history of theoretical ideas. This has sometimes included methodological [1] ideas, treated at an abstract and philosophical level, but has seldom given attention to practical research methods or, indeed, to empirical research. The history of theoretical ideas is an interesting and important area, but there has been proportionately too much of it for justice to be done to sociology as a whole. This emphasis is understandable in relation to the early ancestors of present-day sociology, since in their time such empirical research as took place was normally located outside the academy, and might not go under the name of sociology; it makes little or no sense for more recent times. It is also puzzling given the extent to which methods have been discussed within sociology, which has had more explicit concern with method than have most other disciplines. The time has come to shift the balance of historical concern further in the direction of empirical research and ideas about its methods.

That shift is needed not only to complete the picture of what has happened, but to combat the naive assumption, often implicit in writing on the history of theories, that theoretical positions determine or summarise the whole of sociological practice. If this were so, that would make additional historical work almost redundant. I

[1] English creates a terminological problem here, and at many other places through the book. This book is primarily concerned with method, rather than with its analytical logic and justification, methodology. The adjective 'methodical', however, does not have the right meaning, so one is compelled to use 'methodological' to correspond to both nouns; this is likely to mislead, but cannot be avoided. The reader is requested to bear this problem in mind, and to interpret the adjective as referring to method unless there are specific contextual clues implying that the other meaning is intended.

hope to show that it is not redundant, and that there is ample scope to investigate and describe research methods in their own right, as both theory and practice. This area is particularly interesting in that it provides both the opportunity and the obligation, as purely theoretical enquiries do not, to ask questions about the relationship between theory and practice, including ones about why it should be that some areas of practice have been more heavily theorised than others. This book is, therefore, very much interested in methodological thought, but does not take for granted that this either follows directly from general theory, or in its turn directly determines methodological practice. It also tries, without venturing onto the territory of the classic sociology of knowledge in its concern with diffuse social influences, to take into account, at the level of proximate causes, the practical social constraints which affect empirical research in ways which are not relevant to purely theoretical activity.

The book takes as its remit the period in American sociology from around 1920, when university sociologists started to carry out empirical research and to write about research methods, until around 1960. This is at present an unfashionable period, but that does not make it historically unimportant and, as is shown later, such fashions change. It was a period during which American sociology became dominant quantitatively and qualitatively; since then other national sociologies have grown, but the directions in which they have moved cannot be understood without understanding what happened in America, even if they have often reacted strongly against American influence in general, as well as particular American tendencies. Especially important in this has been the flow of migration created by Hitler and the Second World War, which led many European sociologists to the USA; there they made significant contributions drawing on their original intellectual backgrounds, but were also changed by the new experience. After the war, both the contacts which this established and the American political position in postwar Europe had considerable influence on European developments, particularly in diffusing survey method at the height of its novelty and vogue.[2] As European

[2] There have been a number of valuable studies of American influences and their reception in Europe. See, for instance, Chapoulie 1991, Mazon 1987, Münch 1991, Sulek 1994, van Elteren 1990. On the diffusion of the survey, see also Capecchi 1978, Pollak 1979.

sociology has expanded, American sociology has naturally become quantitatively less dominant. It has also, in what could be seen as part of the normal process of reaction against previous generations, been in some ways rejected by European sociologists – but even that rejection has often followed American oppositional models.[3] If we are concerned with influence and causes, the American empirical tradition cannot be ignored.

It is observable that much writing about the history of sociology (as no doubt also of other disciplines) starts from the moving frontier of the contemporary, and works forward to it from ancestors chosen for their perceived contemporary relevance. It is striking, thus, how the acknowledged theoretical ancestors of American sociology change from prewar accounts stressing early Americans such as Giddings, Ward, Ross and Sumner, to postwar accounts where suddenly the Europeans Marx, Weber and Durkheim figure (though Gumplowicz, Ratzenhofer and Tarde are now largely forgotten), and the only earlier American theorists mentioned prominently are Mead, Cooley and Thomas. (Of more recent figures, it has often been noted how Parsons moved from hero of the 1950s and 1960s to villain of the 1970s and 1980s, and is now being taken seriously as an ancestor again by at least some contemporary writers.) This sort of process also affects the treatment of whole national traditions, and of methodological strands within them. The shift, even within American sociology, away from interest in its own more direct ancestors, has led to a general downgrading of the historical significance of American sociology in relation to theory which is surely not justified. Least of all is it justified in relation to empirical research and its methods.

How have these figured in general histories? Far more has been written about theories, even when the title refers to 'thought'. Some standard works, chosen to exclude those with 'theory' in the title whose remit could make the question irrelevant, have been examined. Those of Beach (1939), H. E. Barnes (1948), Nisbet (1966), Coser (1971), Hawthorn (1976) and Szacki (1979) have no, or negligible, reference to any empirical matters. Works which do give

[3] One interesting example of this sort of process is the recent strong growth of interest among some European sociologists in the interwar Chicago school; this is associated with active support for qualitative methods of empirical research not previously well known in those countries.

some serious attention to empirical work have included those of House (1936), H. E. Barnes and H. P. Becker (1938), Bernard and Bernard (1943), Odum (1951), Hinkle and Hinkle (1954) and Mitchell (1968). These are almost all, however, in one way or another in the genre of the textbook, and so give little researched detail. There are also a number of books specifically about empirical sociology; these include Madge (1963), Oberschall (1972), Easthope (1974) and Ackroyd and J. A. Hughes (1981). Only Oberschall's rests on original research, and its period finishes in 1930. Other serious historical studies of broad scope, such as Furner (1975) and Dorothy Ross (1991), stop at the same period or earlier. The interwar period has in general been much neglected in recent writing, except in the large number of works on the Chicago department – Faris (1967), Bulmer (1984a), Kurtz (1984), Harvey (1987b), D. Smith (1988); Hinkle (1994), though focusing on theory, is a very welcome exception to this. Another shining exception is S. P. Turner and J. H. Turner (1990), which describes itself as 'an institutional analysis', and is centrally concerned with empirical work from before the First World War until the present day.[4] Another source of material is the 'sociology of sociology' literature, such as Friedrichs (1970) and Gouldner (1971), though this has often been at least as much concerned with drawing morals for the present as with understanding the past. There have, of course, been many relevant publications of narrower scope, often articles rather than books, but still with a marked shortage of serious historical work on the period since 1930 or on empirical research and its methods.

This book does not attempt to fill the gaps with a complete narrative history, but draws on narrative materials in relation to key thematic issues. (This means that some of the same material is referred to more than once, in different connections.) Many of the themes pursued are interpretations put forward by other authors, whether or not those were grounded in detailed research; this is part of a continuing discussion. The uneven spread over the field of work by others is taken into account. Jean Converse's really excellent history of the social survey (1987) makes much work unnecessary which would otherwise have been called for; that and other existing work are freely drawn on. To complement what has

[4] It is unfortunate, for historical purposes, that legal difficulties with the use of archival material (S. P. Turner 1994: 64) made it necessary for many of its sources not to be cited.

been done already, a special effort has been made to provide here more extended documentation on some groups and topics relatively neglected by other writers; these include the interwar 'case study method', the intellectual circle of George Lundberg, and foundations other than Rockefeller ones. This may have risked creating an impression skewed in an unconventional direction, but that seems a risk worth taking.

This risk has been taken not only to fill out the descriptive picture, but because the pattern of existing historical work – some done as a minor part of other enterprises – is itself part of the phenomena we are concerned to explain. Why, for instance, do the 'Chicago School', and Paul Lazarsfeld's version of the survey, loom quite so large in customary accounts? This and similar questions do not apply only to formally historical writing, but also to the amateur history of unresearched introductory comments, taken-for-granted textbook versions and orally transmitted understandings. There are things all sociologists know which are probably misleading, and there has been a shortage of systematic historical work to examine them.

One obvious possible answer to the question of why some parts of the history have received more study is that most attention has naturally been given to the best work. I certainly have my own evaluative judgments of methods and methodological writing, and have not always concealed them in this book. But, while I would not entirely subscribe to the 'strong programme' in the sociology of science,[5] I accept as a methodological imperative in writing on the history of knowledge its injunction to regard every outcome as requiring explanation, independently of its intellectual merit. The merit or demerit may constitute part of the explanation, but it cannot be the whole of it. It cannot be taken for granted that only the best has survived – and if it had, it would still be of interest to document how the best came about, and the processes by which that

[5] The 'strong programme' in the sociology of science, especially associated with the Edinburgh group of sociologists of natural science, is defined by Bloor as consisting of these tenets: it is causal, i.e. concerned with the conditions which bring about belief or knowledge; it is impartial with respect to truth or falsity, success or failure, rationality or irrationality, in that it sees both sides as requiring explanation; it is symmetrical, in that the same type of cause is assumed to explain both true and false beliefs etc; it is reflexive, in that its patterns of explanation are also seen as applicable to itself (Bloor 1976: 4–5).

outcome is ensured. It is evident that evaluative criteria have changed over time and differ between subgroups. Moreover, the ephemeral and second-rate is, by whatever evaluative criteria, a high proportion of the total work done; the historian, especially the sociological one, should be interested in this work too. To focus only on what is seen as important is more appropriate to the seeker for exemplary ancestors, or others with normative agendas. Normative agendas are rightly common in writing on research methods, but this book does not aim to meet the needs to which they respond.

The discussion starts with an overview of the types of methodological writing done over the period, and the circumstances under which it was produced; it shows that the sets of concepts used have changed over time, so that what are in one sense the same practices can form part of different 'methods', the boundaries between which are drawn in shifting ways. Writing followed practice at least as much as it led it, but not every practice has figured equally in methodological writing; what gets written about has been to a surprising extent dependent on the enthusiasms of strategically placed individuals. The question of theory and practice is considered further by an examination of the scientism often seen as dominant for much of this time. It is shown that even its keenest proponents in principle, George Lundberg and his circle, did not agree among themselves on what it consisted of, and did not necessarily exemplify what they preached in their practice; it was a slogan and a dream as much as a clear message. A more general discussion of theory and practice shows that some theoretical positions commonly seen as underlying particular methods cannot be causally responsible for them, and that the balance of quantitative and qualitative empirical work probably does not correspond to the balance in methodological writing. Methods in practice are caused by, and chosen for, a whole range of reasons, many of which have little to do with theory of any kind. One explanation offered for the overall trajectory of methodological change has been the pressure of funding bodies for quantification. The pattern of research funding is reviewed. The plausibility of accounts which rest on the interests of capitalists and the capitalist state is questioned, and it is argued that foundation behaviour can be better understood in terms of the immediate situation of foundation officers – who were not very clearly distinct from those they

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funded. It is possible to interpret the course of events as revealing manipulation of funders by social scientists as much as vice versa. It is shown that there is reason to believe that methods would have become more quantitative even without their funding, though not in quite the same way. Some other social structures relevant to academic life are sketched, suggesting that these form part of the relevant background; real social groupings are more consequential than the factitious 'schools' of thought sometimes proposed as the relevant units. Finally, it is argued that collective memories, reputations and the choice of exemplars are socially structured so that they are much more likely to preserve some parts of the whole past than others; our unresearched shared knowledge of the history of the discipline is the product of such processes, creating stories and stereotypes, and selecting ancestors suitable to provide the origin myths we need to legitimate current stances.

METHODS USED

The data in this book come from diverse sources. Published work has of course been examined. The study started, some years ago, as one of methods textbooks, and there are few enough of those for it to have been possible to examine all of them, as well as all the monographs on method. Empirical studies have also been examined and occasionally, when the topic and the practical possibilities justified it, aspects of their methods have been analysed quantitatively. Archival sources have been extensively drawn on and, for materials not often systematically archived – such as teaching documentation – the unofficial archives of departments have been used wherever possible, although what has been kept may be largely accidental. In addition, a considerable number of interviews have been conducted with sociologists (and occasionally also others involved in related activities) professionally active before 1960.

These interviews have been unstructured in form, though I have come to each with an agenda; inevitably, that agenda became more sophisticated as I learned more. In general, however, each respondent was asked to tell me the story of their life from a methodological point of view, and I would then pick up particular points for further enquiry or ask about matters not spontaneously mentioned. Topics normally covered included their initial methodological training and the publications emphasised in its setting, the methods used in their subsequent research and the reasons for that, their evaluations of well-known works and writers and their influence, and how it came about (where applicable) that they had written on methodological matters. I prepared for each interview by checking publicly available sources about that person's career, and asked in some detail about aspects of particular methodological relevance in it. I also made a special effort to elicit comments relevant to my emerging ideas on particular points. People were used as informants about the social settings they had experienced, as well as about their own careers. (Some colleagues, usually emeritus, occupy unofficial positions as custodians of departmental history, and they were particularly valued informants.) In a few cases, as when the interview was an opportunistic one snatched at a conference, the focus was more narrowly on the special reasons why I had chosen that person to talk to; very brief or casual encounters are described in the bibliography (where only those directly cited in the text appear) as 'conversation' rather than 'interview'.

Some respondents spoke freely and at length, others were more restrained. The answers were recorded as near as possible to verbatim, by hand. Several testimonials to my ability to get the material down have been received, and one respondent's tape recorder confirmed my general accuracy. That was not usually the final version, though, as my normal practice was to send a transcript with a request for any necessary corrections, afterthoughts, or answers to questions which occurred to me after the event, so that the eventual version would be a full and agreed one. Many responded to this invitation, and in doing so some chose to 'improve' their oral English and to remove the original indiscretions, with which I have parted with some regret.

How were respondents chosen? Several criteria intersected, and the eventual sample cannot be seen as a representative one from a defined population. (It has not been used in ways which make that assumption.) The first criterion was to include as many people as possible who were especially associated with research methods: writers of textbooks, articles or monographs on methodological topics, authors whose substantive work has been seen as methodologically important, people who had worked in units or on projects significant in the history of methods. The second criterion was to represent graduate schools, or other groupings such as research units, firstly those seen as methodologically significant, and secondly others from the same period which might have been different. On the latter principle, some more 'ordinary' people were included to insure against the possible bias of picking only those well known for particular stances. The third and fourth criteria were practical ones, geographical and demographic. The USA is a large country, and my research funds were restricted; I therefore made a special effort to plan trips so that the maximum number of respondents could be found for one fare. Thus otherwise equally relevant respondents had a greater chance of inclusion if they were in the same area as an archive or someone else I wanted to see, though efforts were made to counterbalance the consequent skew to those located at the time of interviewing in large departments or major metropolitan areas; I visited Missoula, Montana, as well as Boston. Conferences were very helpful in this respect, since they gather people from all over the country in one place, and advantage was taken of this. I also developed a useful conference strategy of offering papers likely to interest different constituencies – participant observation one year, Stouffer and Lazarsfeld's contributions to the survey another – as a way of making contacts and establishing credentials, or eliciting corrective comments to tentative interpretations. My response rate was excellent, with few refusals, though in one or two cases a mutually convenient time could not be found or a home turned out to be too inaccessible. However, some people I would have liked as respondents were dead, or too old and ill. (At one stage I wondered if those left alive were not a skewed sample, biased against more quantitative styles. When I mentioned this to a respondent who had seen the anti-positivist light, his face lit up and he said yes, of course, qualitative methods are life-enhancing! I have not attempted a systematic check of this interesting hypothesis.) The names of all those interviewed are given in the appendix, with some basic background information about their affiliations. In the text, their names have generally been used, with their permission; who they are is often significant to the meaning of what they said, and it would do them little credit to treat them as interchangeable anonymous subjects.

This book strives to provide well-grounded descriptive material, and that has often implied sticking rather close to details, and exercising restraint in either supporting or rejecting large, macroscopic interpretations. I hope that, when a choice had to be made, restraint has been chosen, even if that may have been done at a cost. But that is not the book's only aim for its own method, and it is not unconcerned with larger theses. Historical work is seldom framed as testing hypotheses, and it is often not appropriate that it should be, but even for historical topics the kind of intellectual discipline which that strategy implies can be valuable. To make an interpretation convincing, or justify an explanation, it is not sufficient to show that it fits a reasonably large body of relevant data; other explanations might fit them as well or better, and further data might create a different impression. Too many of the 'explanations' in this field have started from a known outcome and worked backwards to earlier factors which can be connected with it, without considering whether other outcomes would have been more likely if the general interpretation implied were correct, or the same outcome equally predictable from another theory. Insufficient attention has been given to the counterfactual conditional – what would have happened if things had been otherwise. In some cases, too, the connection between cause and effect has been taken as almost self-evident, rather than requiring investigation. One version of this is where apparent similarity of ideas between two thinkers is assumed to show that the first influenced the second. Another version is that where normative links are made: theory must have led to method, because that is the way it should be; capitalism and quantification must go together, because they are both bad things. We attempt to identify and to avoid such questionable structures of argument, and to take seriously the search for negative cases and alternative possible explanations. If that sometimes leads to more negative arguments than positive ones, so be it. In writing a book, there is a strong temptation to make everything fit a neat template and lead inexorably to the foreseen, overdetermined conclusion. There is also a place for untidy antithesis to follow tidy thesis. The reader who reads on will see that this set of decisions fits into an established historical pattern.