"Case Study" in American Methodological Thought

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The term "case study" has played a variety of roles, changing over time, in American methodological discussion. This paper discusses the ways in which it has been used, and their relation to research practice. Its use has often been imprecise, carrying ideological connotations rather than analytical denonation; that does not distinguish it from other terms current in methodological discussion. The connotations cannot be fully understood without placing it in its context, which we attempt to do. We start by outlining the history of the term.

HISTORY OF THE IDEA OF CASE STUDY

Frequency of the term "case study"

A systematic search has been made of the methodological literature for references to "case study" method. The sources used include: (i) general textbooks on research methods; (ii) monographs on potentially relevant methodological topics; (iii) major journals; (iv) any other sources, such as books not mainly about method, to which references were found.[1] A broad outline of the findings on the frequency with which it has been used is given in Table 1.

There is a clustering of references in the 1920s and 1930s, when it is a near-automatic textbook topic. Some research interest continues into the 1940s, most of it connected with Burgess and his associates. By the 1950s and 1960s, the discussions are almost entirely confined to elementary textbooks; it is clear that the idea is no longer a focus of professional interest. Even in textbooks the treatment is usually

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Table 1

Reference made	Date of publication	Yes	No	N[1]
(a) Methods textbook references to case study				
	up to 1929	5	0	5
	1930–39	5	1	6
	1940–49	2	0	2
	1950–59	9	0	9
	1960–69	8	1	9
	1970–79	12	18	30
	1980–89	15	7	22
(b) Articles on or monographs treating case study				
	up to 1929			7[2]
	1930–39			16[2]
	1940–49			11[2]
	1950–59			1
	1960–69			2
	1970–79			3
	1980–89			9

[1] New editions are counted as fresh cases.
[2] Several pieces of work have been included here which described themselves as about "life histories" rather than "case studies", because the issues discussed are indistinguishable from those covered under the other head.

perfunctory, and the orthodoxy has become to treat case studies not as a distinct method but merely as an optional part of exploratory work in early stages of the complete research process. One might take it as symbolic of the complete loss of a tradition from the mainstream when Simon (1969: 267) says: "The specific method of the case study depends upon the mother wit, common sense and imagination of the person doing the case study. The investigator makes up his procedure as he goes along ... " (He then gives instances only from anthropology and market research as examples.) A term which once meant a lot has ceased to have any specific meaning, except to a few older writers or people who associate themselves strongly with "qualitative" methods. There is then a revival of interest more recently, which is discussed in detail below. Here we merely draw attention to the fact that five of the twelve textbook references in the 1970s, and three of those in the 1980s, use the term "case study" but in such a limited sense that one might question whether it should have been counted.

Changing meanings of "case study"

Methods are normally defined in part by contrasting them with alternatives, and the terms used to describe methods normally appear in sets; the conception of a single method cannot be understood in isolation. In the prewar period, "case study method" was normally contrasted with "statistical method". (Philip Hauser, a graduate student at the University of Chicago in the 1930s, tells how at that time baseball sides at the annual faculty–student picnic were chosen to represent case study versus statistical method.[2]) "Statistical method" as a term has also vanished from our conceptual repertoire, but that is certainly not because people have ceased to use statistics; it seems likely that it is because the use of statistics has become so commonplace that it is not seen as a distinguishing feature, and so diverse that it can no longer be seen as unitary. However, although these terms are no longer used there is something very familiar about the broad ideas: it is clearly a qualitative/quantitative contrast, nowadays usually referred to as such or put in terms of participant observation versus survey method.

Why has there been this shift, and what does it mean? To answer that question, we shall need to look more closely at the history of the ideas. It will not be taken for granted that general ideas, terminology and practices go together. For a given range of known strategies and techniques, conceptual boundaries may be drawn in a variety of ways, and shifts in these boundaries may have as much significance as changes in the repertoire of strategies and techniques. It is also possible for the works referred to as examples to have a rather poor correspondence to the normative definitions of the categories they are supposed to exemplify. Below we consider both the methodological literature and some key substantive works drawn on in it.

Historically, the origin of the idea of the case study seems to have had a lot to do with the social worker's "case history" or "case work". Data from social work case records were used in some of the key books of the case study tradition (e.g. Thomas and Znaniecki, 1918–20; Thomas, 1923; Cavan, 1928), and numbers of the early textbook writers take it for granted that the data available for case study use will come from social work records (Chapin, 1920; Lundberg, 1929a; Odum and Jocher, 1929; Spahr and Swenson, 1930; Elmer, 1939). In the earlier part of the period empirical research was quite as much associated with social work as with sociology; Diner (1975) argues that the social work interests of several of the earliest members of the Chicago department were important in encouraging empirical work. (See also Deegan, 1988.) The idea of sociologists collecting their own data was quite a novel one, and it seems as if the sociologists looked around for whatever was available. Issues of *Social Forces* in 1928, 1929 and 1931 contain papers from the Sociology and Social Work section at the conference of the American Sociological Society about ways in which case records may be used by sociologists. These matters were also written about from the social work side, and at least one such book by a social worker (Sheffield, 1920) was referred to respectfully by those concerned with the matter for research purposes. These origins, however, cannot account for the ways in which the idea was elaborated within sociology.

Some of the sociological sources have lengthy and sophisticated discussions, dealing explicitly with all the major issues; others mention the matter only in passing, in a naive or taken-for-granted way. It is dangerous to impute the most elaborated conceptions to other writers, and we have attempted to avoid this except where it seems necessary to understand what is being taken for granted. Bearing this in mind, some of the main lines of discussion are traced.

The idea that a distinguishing feature of the case study is the collection of data on many variables for each case, or the placing of data on individual cases in a rich context, is common throughout up to the 1960s, with no observable trend over time. The idea that a key feature is the access to personal meanings given by the method is, however, very much concentrated in the period up to the 1930s. The idea that a key feature is the intensiveness of the data available on each case, fairly common throughout, becomes particularly frequent in the 1960s, when it appears in six of the seven sources. Thus there seems to have been some movement from the idea that the method gives data of a special kind, to the idea that it simply gives a lot of it. How can this be accounted for? A closer examination of the instances emphasizing personal meanings throws more light on this.

Bogardus (1925: 50) says that the case study "penetrates the interesting personal experiences of all the individuals involved; and out of these experiences, it arrives at an understanding of the various stimuli and responses ... " and goes on to say that "After personal experiences have been fully analysed in terms of meanings, attitudes and values, then statistical methods will be of great help" (1925: 52). In his 1926 book he takes a similar position, saying "Personal experience data ... are not conclusions, but are the most important sources for interpreting all the other social research data and for the preparation of "findings" (1926: 70).

Holt, in the context of a discussion of case records in relation to religious experience, asserts that "if the commonly accepted survey which has been staged in America had been carried on in Jerusalem at the time of the early church, it would never have discovered any difference between the Scribes and the Pharisees and the goodly company of the Apostles. It would have catalogued them all as church members and let them go at that ... for our purpose the supremely important material is that which reveals the individual's attitudes and life-purposes ... " (1926: 228–229). (Note that "survey" here refers not to the modern sense of the word but to the demographic, fact finding type.)

Cooley says "We aim to see human life as an actual dramatic activity, and to participate also in those mental processes which are a part of human function and are accessible to sympathetic observation by the aid of gesture and language ... . This is what I understand by case study: a direct and all-around study of life-histories, as distinguished from the indirect, partial, and somewhat abstract information bearing upon such histories with which we often have to be content ... " (1927: 316–317). Cooley's nephew and disciple Angell, a few years later, assumes that data on "interactive behaviour" can only come from "sympathetic insight", which he sees as implying some form of the case method (1931: 204).

Burgess in 1927 says that "The prestige of statistics as the one scientific method has naturally often led in sociology ... to a naive and uncritical application of quantitative measurement ... " (1927: 107), and goes on to argue that statistics show correlation rather than causation and deal with crude external aspects rather than the inner life; the case study does not have these disadvantages, and someone using it who has a sense for the dramatic and broad sympathies can get beneath the surface. In another article, discussing what sociologists would like to have in social case records and arguing that social workers should want the same, he emphasizes the importance of recording the interview in the subject's own words and says "To enter the interview in the words of the person signifies a revolutionary change. It is a change from the interview conceived in legal terms to the interview as an opportunity to participate in the life history of the person, in his memories, in his hopes, in his attitudes, in his own plans, in his philosophy of life ... " (1928: 527).

Finally, Thomas and Thomas, while advocating the use of statistics where possible, criticize the "premature quantification" of unsuitable data (1928: 567) and argue that "even the highly subjective record has a value for behaviour study ... if men define situations as real, they are real in their consequences" (1928: 572).

These examples make it clear that the emphasis on the value of the case study for eliciting personal meanings is normally part of an antithesis between this method and more quantitative methods which are seen as dry, abstract, narrow, and only suitable to elicit a limited range of "external" data. To the modern eye, it is striking how far the virtues imputed to some sort of semi-structured interviewing resemble those now more commonly imputed to participant observation as opposed to interviewing; this suggests that those to whom these virtues are most important now call "participant observation" what in older terms would be a "case study". Howard Becker provides an interesting example here. He is in the direct line of Chicago succession, and continued to write about "case study" when others ceased to, but what he writes more or less equates the case study with participant observation (Becker, 1970: 76), and he is generally regarded as the great apostle of participant observation.

But the "case study" was still written about after the 1930s, and "participant observation" in the modern sense did not become institutionalized as a term until the 1950s (Platt, 1983). Why did the emphasis on personal meanings die away? I suggest that this can be understood by the antithesis which implies that quantitative methods could not deal adequately with meanings. The period was one in which there were enormous developments in techniques of attitude measurement and of systematic interviewing. By the time that the basic modern techniques of coding the answers to open-ended survey questions had been elaborated, a stark antithesis between quantitative methods and personal meanings could no longer so easily be drawn. The antithesis was also eroded from another direction, by developments within case study method.

It became evident to the proponents of case study method that it raised problems of analysis, and work was done on strategies for dealing with them. The work done by specialist methodologists (Stouffer and Lazarsfeld, 1937; Lazarsfeld and Robinson, 1940) does not seem to have been applied, perhaps because it was overtaken by other developments, but that done by those with stronger substantive commitments (Angell, 1931, 1936; Burgess, 1942) was at least applied by themselves. They recognized two central problems: how to describe the contents of case studies in a sufficiently objective way for the results to be replicable and comparable with those of other case studies, and how to generalize from case studies to a wider population.

The answers given to the first problem led inexorably in the direction of a convergence with "statistical method". An article by Ross, clearly the heartfelt plea of an experienced consultant social statistician, reveals that he had often been called in to help deal with large masses of data on "cases", collected in such an unstandardized form that "when careful tabulation is made, large numbers of these are found to be too incomplete to permit inclusion", and "The very fact of careful distinction between cases makes necessary intricate subdivisions of the system of classification ... " so that where information is missing cases cannot be adequately classified (1931: 33). Ross's approach, presumably acceptable to those he advised, is to look for ways of classifying cases along a variety of dimensions. This is also the strategy used by Burgess (1941), since he got judges to score interview material on a set of predetermined factors. As Vold in the same journal issue points out, "the logical next step would be to make the standardized interview schedule the basis of the interview and to treat the resulting information quantitatively and statistically" (1941: 374). Lazarsfeld and Robinson take as their problem the classification of whole case studies, but propose that this should be done by giving numerical values to indicators of the main continuum and combining them to make a final score; the only concession to the supposed distinctive features of the case study is that it is not seen as necessary for the number and type of indicators used for different cases to be the same.

No author succeeded in describing a non-quantitative model of analysis sufficiently precisely to provide instructions which could be followed. This suggests that it was normal to do it more or less impressionistically, although even strongly antiquantitative versions such as Bogardus' tend to introduce quasi-quantitative ideas – "Certain undertones come out repeatedly as in an orchestra – and thus we obtain understanding ... " (1926: 199) – when they come to the point on reaching conclusions. Sarbin's work, at the end of the period, provided a devastating exposé of the underlying processes likely to have been at work when he showed that the data used in making predictions from case studies were essentially the same basic general features as those used in a regression equation for actuarial prediction, the difference being that in practice those making the case study predictions gave the factors empirically inappropriate weights (Sarbin, 1943: 596). (Sarbin was a sociologically minded social psychologist whose work was cited by sociologists.)

The problem of generalization

The use of case studies obviously raises the issue of their generalizability. On this there are two strands in the literature, which will be treated separately. The first is the more purely academic, where the issue of generalization could be treated as relatively distant and hypothetical; the second is the strong tradition of research related to social work and social problems, where there was a highly practical interest generally formulated in terms of the possibility of prediction. We look first at the more academic work.

Bogardus here, as elsewhere, provides the most extreme statement, saying that in the case study method based on personal experience "one case is proportionately as vital as a million – to the extent that it brings something new before the mind that may be related to what is already known, and hence may be understood" (1926: 192). However, two pages earlier he wrote of the need to reduce experience to types. He did not elaborate, so it is not clear what he meant, but in raising the general idea of the classification of cases into types as an appropriate intellectual strategy he was representative of a recurring theme in the literature. In the 1920s this tended to be connected with ideas about the identification of causal factors to be incorporated into general theories. The emphasis on individual differences is not taken to imply that there are no underlying laws, but rather that these can only be identified by looking at personal meanings. By some writers (Burgess, 1927; Palmer, 1928; Waller, 1934) the case study method is even seen as specifically related to a version of natural-scientific modes of proceeding: "each case may be assumed tentatively to display the common qualities of the species and may be treated as a specimen ... the beginner in social research ... can conduct his investigation of a group ... much as a medical student dissects his cadaver to discover the universal, fundamental functions of different parts of the human body" (Palmer, 1928: 21). This statement, it must be recalled, is made in the context of a textbook emphasizing the method as a mode of teaching. Both Palmer, and Burgess writing on the method more generally, go on to say that the results of different case studies must be compared and special attention given to negative and marginal cases, which are "especially valuable inasmuch as they point the way either towards new generalizations or toward more adequate descriptions of previous ones." A negative case "creates a new problem which must be solved by further research, and usually results in a more accurate definition of a concept or a statement of some scientific law", while a marginal case "accentuates the identifying marks of the previous cases and leads to a refinement of class definitions" (Palmer, 1928: 22). The emphasis is clearly on classification as the crucial intellectual activity and product, with the implication that when cases have been correctly classified laws covering all members of a class will, more or less ipso facto, be established. Cooley's less explicit suggestion that "If we can have enough of it and of sufficiently varied types to be representative of the social process, it will go far to enable us to understand that process, and perhaps to foresee its course ... " (1927: 317) can probably be placed under the same heading. The connection with the ideas of analytical induction (on which see below), not yet formulated as a general position but probably already appearing in Znaniecki's ideas, is obvious. (Znaniecki spent part of his time at that period at the University of Chicago, where Burgess and Palmer also were.) Waller puts the issue in the context of a wider discussion of scientific method by attacking Pearson's probabilistic conception of causation, and asserting that "if one perceives a single instance correctly, he can generalize from that instance, when an instance in which a causal relation has been observed is followed by another instance in which this relation is not present, one needs to refine his observation and to restate the conditions under which his generalisation is valid" (1934: 287). Cooley's ideas on "sympathetic insight" are heavily drawn on. The question of validation of insight, however, is dealt with perfunctorily in a footnote, the criteria suggested seeming to amount to goodness of fit between the insight and relatively complex data (1934: 297).

Some other writers of the same period take the "statistical" approach even to case studies, arguing that they should as far as possible be quantified, and here there are clear signs of convergence with "the statistical method". W.I. Thomas is generally strongly associated with the case method, but by 1928 his ideas had been influenced by his second wife, Dorothy Swaine Thomas, with whom he wrote *The Child in America*. Here experimental method is seen as the ideal, but impossible to apply in the social sciences because of the difficulty of holding factors constant; statistical methods of holding factors constant are therefore necessary. The best kind of data is that contained in case studies based on life histories, and as far as possible these should be quantified; however, some things cannot satisfactorily be quantified, and the use of technically inappropriate statistical manipulations is absurd. Statistical methods of verification are important (1928: 565–571). In this conception case studies are nearer to being a type of data than a complete method, and statistical methods are seen as desirable wherever they can properly be applied. Odum and Jocher, similarly, argue that "The research student must, whenever possible, convert his purely descriptive subjective terminology into objective quantitative measures, and then apply statistical analysis ... " (1929). The distinctive characteristic of case method is that each factor "is analysed in its relationship to every other factor in the group" (1929: 231); it is especially appropriate for the study of attitudes, which cannot be dealt with satisfactorily by schedules and questionnaires (1929: 237). Statistical method is a technique of analysis applicable whatever the earlier approach or method (1929: 285). Here again there are strong elements of the idea that case methods are for collecting data and statistical methods for analysing them, with clear scope for the partial supersession of case methods by technical improvements in more statistical modes of attitude study. Whitley (who had experience under Thrasher), while recognizing that others disagree with him, argues that one can only generalize from a case if it has characteristics common to a statistically large proportion of the population from which it is drawn, and that it is best to gather data in a standardized form with a schedule (1932: 570–571). The main features distinguishing this from a modern survey approach are the emphasis on individual cases taken as wholes, and the absence of developed ideas on sampling. Finally Lundberg, always an opponent of the case method, argues that the value of a sympathetic picture of an individual case depends upon its typicality, which can only be established by statistical method: "The most objective description of typicality which we have developed ... is represented by an average, with its measure of dispersion and probable error ... It is not a question of abandoning literary description ... It is a question of developing a technique of testing the generalisations suggested by such documents ... " (1929b: 412–413).

Whatever the details, however, there was a fair level of consensus in the 1920s and 1930s that individuals should be understood to be representative of large classes, that separate case studies should be compared with each other, that deviant and marginal cases should be distinguished from typical ones, that cases should be classified into types and/or ideal types constructed, and that the numerical prevalence of different types should be investigated. At this point, obviously, the distinction between case study and statistical method becomes a little blurred.

The literature of the later 1930s and 1940s, with Angell in 1931 as an early forerunner, addresses itself primarily to the question of how to make the complexity of case data accessible to statistical manipulation without undue loss of information. The obvious strategy is some form of typological reduction, but by now the type is conceived of more as a point of intersection of values of separate descriptive variables than as an integrated theoretical entity analogous to a biological species.

Angell argues that techniques for the comparative treatment of large numbers of case studies must be developed if valid generalizations are to be made, and that such techniques must not be as subjective as the "mental winnowing" of *The Polish Peasant*, but must, unlike the statistical techniques of parole prediction, give real explanations, not just correlations. His suggestion is that, first of all, the amount of variation to be coped with should be limited by focussing on the impact of one new factor on a homogeneous class of entities (1931: 204–205). The entities should then be classified into "adjustment classes", and these should be cross-tabulated with prior factors likely to be relevant to the adjustment made, taking into acount up to four variables simultaneously. In this way key factors could be isolated, and one could then go back to the cases to discover why those factors should have produced those results and analyse any deviant cases (1931: 206–208).

This proposal retains the idea of keeping the case as a unitary whole and the emphasis on understanding and accounting for all the cases, while suggesting a statistical mode of analysis; it has been grouped with some later work because of its particular concern with numbers of variables and with explicit procedures for dealing with them. The later work is by leading methodologists, and so shows that the issues involved were still regarded as worthwhile by those in the forefront of methodological developments. Stouffer and Lazarsfeld, in a discussion which lays considerable emphasis on the value of case studies in suggesting causes, argue that if they are to be treated rigorously they must be analysed statistically, but for this to be practical the number of variables must be reduced, which can be done typologically. Each case should be classified on a number of variables, locating it in an n–dimensional attribute space; the attribute space can then be divided up, on a functional, numerical or pragmatic basis, and all cases falling into the same division regarded as instances of the same type (1937: 195–196). They point out that the principle of division will be arbitrary, thus drawing attention to the contrast between this approach and that which assumes the existence of natural types. (Cf. also Stouffer, 1933, which is not specifically about case studies as such.) Lastly, the Lazarsfeld and Robinson article discussed above proposes a system of scoring cases which enables a variety of factors to be summarized in one score, thus making for more homogeneous and manipulable categories; here there is more emphasis on quantification and less on retaining information. Obviously the application of all such methods requires standardized information on relatively large numbers of cases, and it is only as a matter of empirical luck that one will be able to make generalizations about them without loss of information.

From the 1950s until much more recent times there are no articles except Becker's which discuss these issues. Such textbook references as there are almost all suggest that the only roles for case studies in relation to generalizations are to suggest hypotheses for more systematic investigation, to provide illustrations, or to refute or set limits to general propositions. (Hints of older themes are confined to the earliest 1950s and to subsequent editions of books first published earlier.) Thus we can trace a rough sequence of stages: (i) prime concern with classification, with the representativeness of specimens of their class not seen as problematic; (ii) movement in the direction of quantitative analysis of case study data, without any very clear or distinctive techniques; (iii) elaboration of procedures of typological reduction; (iv) loss of interest in the issue, case studies no longer seen as providing any basis for generalization. However, it would be misleading to treat this simply as the historical development of ideas about generalizing from "case studies", since it is clear that those taking different positions had different aspects in mind, even if they used the same words to refer to them. Those grouped under (i) thought of case study method as a complete method, with a logic, approximating to that of analytical induction, which separated possibilities of generalization from numbers or formal sampling of cases studied. Those grouped under (ii) thought of case studies as a method for the collection of data, especially about attitudes, which could in principle be analysed in a variety of ways, limits being set mainly by the stage of technical development of methods; statistical conceptions of generalizability were seen as the appropriate ones. Those grouped under (iii) saw case studies primarily as large bundles of unstandardized variables, where the problem in generalization was to apply statistical ideas with the loss of as little information as possible. For most of those grouped under (iv) case studies were simply unrepresentative cases from which it was clear that one could not generalize.

The prediction issue

In the literature of prediction, some slightly different themes are raised. Although some of the same writers, in particular Burgess, were involved in this field, its primary concern is with prediction for the purposes of treatment or administrative decision-making in policy and problem areas; the classic substantive topics are juvenile delinquency, parole success and social adjustment in the family.

Two basic alternative approaches are set out in the June 1929 issue of *Social Forces*, reporting the proceedings of the American Sociological Society's section on sociology and social work. Burgess describes a study in which he used statistical analysis of parole records to construct "an expectancy rate, that is, a statistical statement of the probabilities of a certain type of behaviour which would apply to a group of persons rather than to any specific individual ... " (1929: 534). Lewis E. Lawes, a social worker, criticizes Burgess' strategy on the ground that "The tendency of modern penology ... is along case treatment of individualities ... And yet here he is apparently utilizing modern methods of investigation ... in order to treat these types or groups in the aggregate rather than individually. Would that not be a reversion to the old and much deprecated method of class penalization ... ?" (1929: 546). Lawes' comment, though put in social work terms, brings out the important point that in this sphere the interest was in the outcome of known individual cases; the social worker has to deal with the particular individuals in his case load, and will not in the ordinary course of duty have access to data on a representative sample. (This fact goes some way to explain the ideas developed in academic sociology at a stage when the case data available was assumed to come from social work records.) Stouffer's 1930 PhD thesis, although not directly on prediction, was clearly related to it. He demonstrated that a simple Thurstone scale gave essentially the same results as a judgment based on case histories; one moral is that the simpler and more statistical method provides an adequate basis for prediction. The Committee Foreword to an SSRC-sponsored volume on prediction published in 1941 (Horst) reviews the controversy between case study and actuarial methods of prediction, and argues judiciously that both techniques have their strengths. The proponents of the case study are seen as arguing that they can make certain rather than probabilistic predictions, while the statisticians argue that this can only be done on an implicitly actuarial basis (Horst, 1941: 27–29). Wallin, one of Burgess' collaborators, argues in his chapter that "The possibility of comprehending all the relevant factors and of getting all the data for understanding the manner of their operation in the subject, theoretically at least, should make for greater accuracy in case study than in actuarial prediction. In practice, however, the accuracy of case study prediction is limited by the skill of the investigator and by the inadequacy of present scientific knowledge of human behaviour" (Horst, 1941: 183–184). He goes on to quote Sarbin's evidence for the case study's lack of superiority in practice, and to suggest that perhaps the best method might be to make actuarially based predictions and then modify them for the individual in the light of case data. (Many of his references are to psychology rather than sociology, and the discussion is clearly linked with that reviewed by Meehl on the role of clinical judgment in psychiatry.) As he points out, some modes of prediction from case studies are logically equivalent to those using tests or questionnaires (p. 210); those which are not cannot be expressed in a formula, and are likely to involve non-analytic modes of understanding such as empathy.

Burgess, making an early report on the study on prediction of success in marriage, lists possible procedures for prediction: (i) intuitive judgment, (ii) analysis emphasizing the individuality of the case, assuming fixed characteristics of the person from which extrapolations can be made; (iii) typology, observing the extent of deviation of the case from an ideal or empirical type; (iv) analysis in terms of factors. The last procedure is the one he uses in this study. He notes the classic disadvantage of this method, that "Statistical prediction upon any given case is essentially, in its present form, the application of the average weight of a given item or group of items derived from the entire sample to each individual case", with the implicit assumption that the items are not affected by context and have the same weight in each case (Burgess, 1941: 330–332). The research he reports made some attempt to meet this by allowing raters to give special weights to factors that seemed to have special importance in particular cases; this seems to be one of the two points in which the ethos of the case study as distinctive is clearly retained. (The rating procedure could be seen as distinguishable only in degree from any coding of open-ended data. However, it involves an element of judgment without clear operational rules, since raters were asked to take general "factors" such as "temperamental compatibility" and rate couples for the factor's contribution to likely marital success on a scale of disruptiveness/bindingness from –2 to +2.)

Cottrell, who had also cooperated with Burgess, writing in the same issue of *Sociometry* on "The case study method in prediction", argues that there are two kinds of case study. One, whose prime function is the development of insights and hypotheses, is essentially empathetic; it "... involves the use of the observer's personality as an instrument of observation ... involves the conscious and skilful use of the incorporative or role-taking processes which go on most fully in the more intimate interpersonal relationships" (1941: 365–366). The other, aimed at isolating typical patterns which correlate with behavioural outcomes, is informally a statistical procedure and could without loss in future become formally such; the more empathetic mode is a prerequisite for this stage. Any modern reader would assume that the quotation referred to participant observation; the bifurcation of the tradition into informal exploratory work or participant observation, and various modes of quantification of qualitative data, is clearly foreshadowed here. Lundberg, again in the same issue, makes his usual point, with his usual verve and aggression, about the implicitly statistical nature of case study procedure, and the impossibility of prediction without quantitative knowledge of other cases.

The concern with individual prediction, if it continues at all, is now outside mainstream sociology. Actuarial modes of prediction are, of course, of the utmost importance in demography, and are also used in other areas of administrative interest. The probabilistic approach has taken over, and sociologists no longer feel that their generalizations are threatened if there are observed exceptions to them. (Whether this is progress may be questioned!)

WHY DID THE TERM "CASE STUDY" DISAPPEAR?

It has been shown above how thinking about the idea of case study method changed over time. It is clear that there were always some who saw very little role for case studies, and attacked case study method as a valid sociological approach. Some active supporters of the idea attempted to develop more sophisticated and systematic ways of using it, and some interesting work was done which was at the forefront methodologically at the time. Only a few years later, however, after the lull in publication caused by World War II, the idea was fading rapidly into insignificance, becoming first historical and then forgotten.

This rapid change is epitomized in the fate of a methodological research project directed by Burgess. The project was funded in the early 1940s by the Social Science Research Council's Committee on the Appraisal of Research, whose previous projects had been published and received a lot of publicity, and involved several leading sociologists (including Robert Merton). A lot of work was done, a draft report was written, but nothing was ever published and it has effectively vanished. What happened? The aim of the project was to test the reliability and validity of case study method by replicating Angell's *The Family Encounters the Depression*; various scholars reanalysed his cases in different ways, and Angell reclassified his own cases. The reliabilities found were moderate, and Burgess may have had difficulties in making sense of the material, but those who saw what had been written, especially Merton's contribution, thought it very interesting. The statistical techniques used, however, eliminated some of the traditionally distinctive features of case study method, pushing it in the direction of schedules and quantification – on which great advances had, meanwhile, been made elsewhere. It seems likely that by 1945 the initial problem no longer looked of so much interest or relevance, and so it did not seem worth the effort of producing a final report. (For a more detailed account of this episode, see Platt, 1987). What were the advances elsewhere, and how did they affect the issue?

Internal explanations

Unstructured interviews had always been seen as a valuable tool of case study method, even intrinsically associated with it, but the technique was developed and rationalized in other contexts. Social workers and psychologists worked on interviewing technique for their own purposes, and some of this fed back into sociology. The Hawthorne Studies (Roethlisberger and Dickson, 1939) combined interviewing in the tradition of clinical psychology with an experimental style, but in a "real-world" setting evidently relevant to sociologists. Likert's research unit at the US Department of Agriculture, which as the Division of Program Surveys came to play an important part in wartime research on the civilian population, drew on the work of Carl Rogers on non-directive therapy for its interviewing technique (Campbell, 1946; Skott, 1943). Market research increasingly developed interviewing techniques for the understanding of motivation (see Lazarsfeld and Rosenberg 1955, Section V) and of response to mass communications, and these were rationalized and presented to sociologists in such works as Merton, Fiske and Kendall (1956). Closely linked to this is the emergence of techniques for the precoding of attitude questions and the coding of answers to open-ended questions in surveys (Converse, 1987). In the 1920s and 1930s, "interview" and "questionnaire" were sharply contrasted; "interview" was associated with the unstructured and intensive study of a small number of cases to explore attitudes, and "questionnaire" with simple closed questions to find out facts. As schedules to measure attitudes evolved, the distinction became obsolete (cf. Homans, 1951). Another angle of approach to similar issues was provided by what became known as "content analysis", which flourished and expanded enormously during the war as analysis of enemy propaganda and other documents; this provided further ways of quantifying unstructured qualitative data. Finally, there are two developments in method specifically associated with Lazarsfeld: panel study, and the analysis of deviant cases in surveys. The panel, first described by Lazarsfeld and Fiske (1938), could be seen as an alternative to the life history for the provision of historical depth and the temporal location of causal factors. (A contribution to this was also made by Lazarsfeldian techniques for establishing the time order of variables in surveys, on which see Hymans, 1954). The analysis of deviant cases went some way to meet the traditional objection to the statistical method, which was that in its satisfaction with merely probabilistic statements it ignored disconfirming cases. (The transition is nicely shown by the appearance in the section on deviant case analysis in *The Language of Social Research* [Lazarsfeld and Rosenberg, 1955] of a piece by Horst, who had worked with Burgess, on the role of case study method; the newer selections, by colleagues of Lazarsfeld, see such analysis as only an intervening stage in the fitting of deviant cases into modified general rules.)

All these developments eroded the boundaries of the case study method, either by hiving off parts of what had been regarded as its distinctive characteristics and elaborating them as separate methods in their own right, or by developing other methods in ways that either converged with the ideals of the case study tradition, or met them better while having as much in common with "statistical method". At the same time, there was clear technical progress in statistical methods and their application to sociology. Hagood (1941) provided the first complete textbook on statistics for sociologists. Despite the pioneering work of Bowley and others, the use of formal sampling on social data was slow to spread, partly because of the resources it required; various governmental programmes in the depression and then the war were important in the development of techniques (Stephan, 1948; Stephan and McCarthy, 1947). As soon as ideas about sampling and its criteria for representativeness were widely diffused within the sociological community, they provided an obvious ground for criticism of the utility of case studies.

It is, thus, easy and plausible to argue that case study method faded away for internal reasons: always subject to criticism, changed from within by its proponents in ways which weakened its distinctiveness, its boundaries eroded from without, and so replaced by other versions of "qualitative" method where it was not superseded. This could make a simple old-fashioned story of intellectual progress, a broadly Kuhnian account in which a new paradigm arises which solves some of the intellectual problems of the old, or maybe a Lakatosian account of the rightful fate of a degenerating research programme (Kuhn, 1962; Lakatos, 1970). However, this would ignore the distinction between the term "case study" and the practices to which it refers. It is much less clear that the practices vanished from research than it is that the term vanished from methodological discussion.

So far we have considered only the possible internalist account of the decline of the case study idea. Before drawing a conclusion, we need to look also at the "external", or social, factors which were relevant. In the absence of clear agreement that the idea had been refuted intellectually, the very sharp observed discontinuity invites explanation in terms of historical events.

External explanations

First, case study method had been especially associated with the Chicago department. By 1951 most of the key senior figures who were interested in it had died (Park, Thomas) retired (Burgess) or left (Blumer). New young members were recruited, most of whom came from different intellectual traditions. In addition, over the postwar period Chicago became numerically less dominant than it had been among graduate schools (Platt, 1991). These factors in themselves are not sufficient to account for the discontinuity, however, since they do not show why those trained by the older generation did not carry the tradition forward.

Here the war is crucial: it emptied the graduate schools of young men, and placed many of the most promising in the wartime research effort (Cartwright, 1947: 334). This involved many leading figures from academic, commercial and governmental research, and great strides were made, with far larger resources available than could be had for social research under normal circumstances (Schneider and Allport, 1944: 171–172). The interdisciplinary nature of wartime work was important (Lyons, 1969: 81), and the opportunity to do repeated studies on large samples – seldom available in civilian life – gave good reason to mistrust more impressionistic methods (Stouffer et al., 1949: 38–40, 46) as well as allowing methodological experimentation. There was a high level of communication and cooperation, which helped to diffuse the best current practice and to create intellectual stimulus to improvement. For those who took part it performed some of the functions of an intensive summer school or centre for advanced studies, combined with the excitement and social cohesion brought about by doing work of practical importance as well as novelty, and sometimes under dangerous conditions (Clausen, 1984). Several of the cohesive teams created stayed together after the war and moved out to become university research groups, thus creating new institutions which would carry forward the new styles of work. What this intensive school taught was mainly survey method. As Stouffer explains in the first chapter of *The American Soldier*, the practical situation in which the Research Branch of the Army found itself led to the main emphasis in their work falling on the techniques of public opinion research. In addition, and more especially in relation to the civilian work, the idea of democracy gave an ideological rationale for opinion research which should not be seen purely in a cynical light. Likert, writing about his research section in the Department of Agriculture, calls his article "Democracy in agriculture – why and how?", and gives it a long introduction about the institutional and psychological conditions for effective democracy (Likert, 1940). The cultural assumptions which made "democracy" such an important theme also helped to create the situation where "Many necessary measures for the war effort lacked legal sanctions from the beginning of the war ... we leaned over backwards to make full participation on the home front a voluntary matter for our citizens" (Katz, 1946: 241), and so social research took on the function of finding out how to encourage citizens to play their part and how to make policies such as rationing effective. This follows on from the prewar developments described by Skott, which responded to what he saw as the Department of Agriculture's special need to know the reasons for farmers' opinions (Skott, 1943: 288). Campbell (1946: 276) adds that "Answers to 'why' questions are especially important in cases where the public's reaction to a program has been uncooperative or hostile". The conception of democracy inevitably implied the counting of heads and thus quantitative methods, while the concern with motives implied study of attitudes. To the extent that, in wartime, emphasis shifted to a manipulative rather than a responsive use of the results of research, the mode of manipulation suited to a voluntaristic war effort was through advertising and propaganda; this gave special importance to the contribution of those with experience in market research and mass media studies.

Another significant historical factor is Hitler's contribution to American social research: the intellectual exiles, a number of whom played key roles, as individuals or as representatives of the traditions they bore. "Lazarsfeld ex machina" is a popular line of explanation, and does indeed carry some conviction. His intellectual interest in the reconciliation of quantitative and qualitative concerns was peculiarly apt to blur the boundaries of case study method, his commitment to codification ensured that new methods were made explicit and were diffused, his creation of the form of the research institute was a vital piece of institution-building (Glock, 1979), and his personal charisma gave him many disciples. However, the great man theory of history only works if the great man finds his social context as Lazarsfeld did. Other exiles were also important. Lewin and other members of the Gestalt school did much to undermine behaviourism in psychology, and so to make room for concern with attitudes; several of his graduate students took part in opinion research during the war, and continued to play a leading role in it afterwards (Mandler and Mandler, 1969). The Frankfurt Institute of Social Research had already in Germany done a questionnaire study in which answers were recorded verbatim and then analysed: "the way a psychoanalyst listens to the associations of a patient" for clues to the reality underlying the manifest content; their hostility to empiricism led to the style of work shown in *The Authoritarian Personality* (Adorno et al., 1950) which used sophisticated indirect questions to elicit underlying attitudes (Jay, 1973: 117, 240–241). The technique of indirect questioning is also shown in Komarovsky's *The Unemployed Man and His Family* (1940), which followed on from the European studies of authority. Open-ended interviews were conducted, with very careful instructions to the interviewers, and the results analysed typologically. Lazarsfeld supervised the work, and says in his introduction that the study "endeavoured to contribute a more careful analysis of those non-quantitative procedures which very often are left to the haziness of common sense" (Komarovsky, 1940: ix). It was in this context that the technique of "discerning", with its careful concern for the elucidation of causal processes and the checking of hypotheses, was worked out, and fed into what became the tradition of survey analysis.

Thus the war and political events associated with it both led to new movements in social research method, and created the conditions which led to a sharp break in the smooth transmission of traditions. When the graduate schools filled up again after the war, they did so under circumstances which gave hegemony to the new survey tradition that became dominant. We could, therefore, see the decline in the use of the category of "case study method" as over-determined, in the sense that either internal or external events alone might have been sufficient to bring it about. Arguably, though, many of the factors described might be regarded as necessary but not separately sufficient conditions to produce the end result. It was not inevitable, either intellectually or socially, but a complex combination of circumstances brought it about.

RESEARCH PRACTICE

It is suggested above that the history of research practice did not follow that of methodological discussion; we turn now to consider practice. To compare practice with principle, we would need criteria for identifying instances of "case study". The diversity of the themes which have been associated with the term, and the vagueness of some of the discussion, causes some difficulty here. In practice, "case study method" in its heyday seems to have meant some permutation of the following components: life history data collected by any means, personal documents, unstructured interview data of any kind, the close study of one or a small number of cases whether or not any attempt was made to generalize from them, any attempt at holistic study, and non-quantitative data analysis. These components have neither a necessary logical nor a regular empirical connection with each other; nonetheless, the ideas that linked them had an important social reality in the US sociological community in the interwar years. We could perhaps do them more justice by offering a constructed type based upon the methodological literature:

> It is of the essence of case study method that it entails the collection of intensive data about all aspects of the individual case, including those which may be unique, and that it treats the case holistically rather than isolating variables.
>
> It aims to make behaviour intelligible by providing data about personal experiences and their meaning, and taking into account the history and social context of the case. It follows that special importance is attached to the individual's own version of events, which means that the researcher's preconceptions should not be imposed and that as far as possible data should be collected in the subject's own words. No particular mode of analysis is implied, but it should be non-quantitative; generalisation may be of interest, and if it is a typological strategy is appropriate.

This gives a reasonable representation of the central tendencies of that literature. It does not, however, give an accurate picture of the empirical studies cited as exemplars in the literature. We have chosen some instances to illustrate this point, taking ones which as a group show the range that the term can cover: Thomas, *The Unadjusted Girl* (1923); Shaw, *The Jack Roller* (1930); Angell, *The Family Encounters the Depression* (1936b); Steiner, *The American Community in Action* (1928). The first three are very well known; the last is not known these days, but was well thought of at the time. All are treated as exemplars of the method by some authors. *The Jack Roller* is the life history of a single person; *The American Community* is a collection of studies of communities; *The Unadjusted Girl* refers to a large number of cases, many of them mentioned only briefly and hardly any treated holistically. Most of *The Jack Roller* was written by its subject, though in response to questioning. The data of *The Family Encounters the Depression* are accounts written by students, responding to an open-ended questionnaire, about their families. The cases in *The Unadjusted Girl* which cover the most ground are court and social work records, while some others are letters to a newspaper advice column; many of the "documents" used are brought in to make a point about only one of the "four wishes" which Thomas posits. *The Jack Roller* focuses on its subject's criminal career and the reasons for it; *The Unadjusted Girl* is concerned with the causes of promiscuity; *The Family Encounters the Depression* is about reactions to the Depression, as shown by what the family was like before and after a significant loss of income; *The American Community...* has no explicit overall theme, but the general approach is very much one of "community work", with a normative stance on the extent to which the communities are modern or progressive. Thus the extent to which these books have intensive data about all aspects of their cases, treat them holistically, provide data on personal meanings and collect them in the subjects' own words, varies very considerably; only *The Jack Roller* comes really near to our constructed type, and it is also the only one which unequivocally meets the criterion of providing full "life history" material. It seems clear that the function of the term "case study method" is not simply one of empirical description, and this is further confirmed when one notes that there are a few studies which, it could plausibly be argued, exemplify the idea better (e.g. Waller, 1930; Abel, 1938) but are not used as exemplars (Platt, 1984).

This line of approach, thus, does not seem very promising. If even at the peak of the methodological discussion cases used as exemplars do not exemplify the abstract principles well, it seems unlikely that there is a clear pattern which relates practice to them; they need to be treated as ideals rather than as literal descriptions. In this connection we may also note that, while there was a conventional contrast between case study and statistical method, even at the height of the controversy many writers argued that both were useful and acceptable; Carey (1975: 186) reports that what the former Chicago students whom he interviewed said suggested that "the conflict was overdrawn", and this picture is confirmed by the use in most of the classic Chicago monographs of a variety of types of data. We shall, therefore, not look for instances of the pure type of case study method.

Even with a clear type in mind, it would be impossible to review the whole history of US empirical sociology systematically to see how far it was followed. We are compelled to fall back on a more impressionistic account which draws on general knowledge, or on work done by others for other purposes. What, then, do these suggest? McCartney (1970) defines a case study report as one which is descriptive and analyses a social unit as a whole in qualitative terms. Using that definition he shows a long-term decline from 1895 in the proportion of the empirical articles in three leading US journals which use only case studies – but that still leaves 17.8% in 1955–64. Brown and Gilmartin (1969) study the research papers in the two leading journals in 1940–1 and 1965–6 and show that even over that period the number of cases used in the typical article has increased, but that still in 1965–6 leaves 13.9% with only one case and 11.8% with two to ten cases; 8.5% drew their data from participant observation, and 15.4% were "descriptive" rather than "quantitative". Bennett's important book (1981) traces the history of publishing oral histories of delinquents in their own words, and shows that when circumstances were propitious these continued to be done. Recently there has been a revival of interest in the life history reflected in the collection edited by Bertaux (1981), although only two contributors were American. Elder's work reported in that volume uses a cohort study which, instead of collecting retrospective accounts, returned to the same individuals for repeated data-collection. This does not necessarily give accounts in their own words, or which reflect their own perspectives on the past, but it provides material of great historical depth and potential richness. Even as survey method established its hegemony there was at least a steady trickle of work collecting rich qualitative data by "fieldwork" or "participant observation" or – more recently – "ethnography", and numbers of these studies became disciplinary classics, for example, Whyte, *Streetcorner Society* (1943); Becker, *Outsiders* (1963); Becker et al. *Boys in White* (1961); Liebow, *Tally's Corner* (1967). Some writers have seen a "Second Chicago School" as playing a key role in this, and there were indeed some important studies of this kind done at Chicago in the postwar period, though there is some reason to question whether this either carried forward the interwar Chicago tradition or fully characterized the postwar department. Perhaps more important in practice is that an idea of a Chicago tradition evolved and was invoked to legitimate contemporary practices by those committed to ethnographic styles of work from the 1970s onwards (Platt, 1991).

Less obviously, a very large number of studies were done of particular communities, organizations or small groups. Many of these, however, were treated mainly as studies done in these social units, not as studies of them. Often the choice of where to do the study was dictated by purely practical considerations, and a national sample might well have been preferred if it could have been afforded. Such research has been criticized for the inadequacy of its sampling strategy, and with good reason to the extent that the author intends to use the data with the logic of a representative sample. However, the same data could often have been used in a different way; *Management and the Worker* (Roethlisberger and Dickson, 1939) might have been *General Electric in the Depression*, or *Personal Influence* (Katz and Lazarsfeld, 1955) might have been *Opinion Leadership among Women in Decatur*. The sample of convenience which almost could be counted as a case study is, from the rather different points of view of either the researcher or the sampling critic, not logically different from the innumerable studies done on students because students were available rather than because there was any interest in students as such. Those may look much less like potential case studies – but even there, remembering Newcomb's intensive study of Bennington College (Newcomb, 1943), one might see possibilities which the researcher did not take up. What makes such studies unlike case studies is not just that the results are reported as from "a manufacturing company" or "a high school", or that the data collected may be rather superficial and standardized and are counted, but that the focus of interest is on variables rather than historical individuals. The variables are assumed to be in some sense autonomous from the individuals, even if individuals may be the point of intersection of some unusual values on them. The goal of generalization is taken for granted, and the lack of distinction between individuals which follows is not felt as a loss (cf. Ragin, 1987).

Very few works in the case study tradition really want to distinguish each historical individual from every other, but they do retain the whole social unit (person or group) as the unit of analysis; the typologies group together units of the same kind. Although the name "analytical induction" (Znaniecki, 1934) was only invented, and its strategy formalized, when the case study idea was well established, it can be argued that the degree of intellectual fit between them is sufficient to treat it as the implicit logic of the method generally. In particular, analytical induction requires that all cases should be accounted for theoretically, focusing on theoretical explanation of the whole range of variation rather than proportional representation of the numbers in the population.

RECENT WRITING

Glaser and Strauss's *The Discovery of Grounded Theory* (1967) takes up related ideas and develops them, though its emphasis is on generating theory rather than testing it, and its success shows that it met a felt need. However, even those committed to qualitative work have not emphasized the retention of uniqueness in recent years; Glaser and Strauss aim to develop generalizing theory, even if they approach it by an inductive route and stress accuracy of fit to particular cases. The renewed interest in historical macro-sociology, and the influence of Campbell's analysis of the logic of experimental design, have nonetheless probably led to an increase in comparative strategies. It is not coincidental that some of those who have most recently written on case studies have worked on organizations or in policy settings (van Maanen, 1979; Yin, 1984, 1989) where those who fund research are likely to want results which apply to their own organizations. Kennedy's very interesting paper (1979) raises this theme explicitly, pointing out that practitioners will often want to generalize to one case and that a single-case study may be more useful for this than one that only provides data on groups. She proposes some rules, comparable to those used in generalizing from statistical data, for making plausible inferences from single-case studies. Ragin's (1987) contrast between case-oriented and variable-oriented approaches to comparative studies, while it uses nations rather than schools or children as its typical units, has some strong logical analogies because of the constraints thus created by the existence of only a limited number of unique cases. Lieberson (1985), writing critically about the assumptions embedded in standard uses of quantitative methods, also makes some valuable points relevant to these issues. In particular, he argues that "accounting for" the highest possible proportion of the variance in studies of large samples is not always a desirable goal, and that a simple case study with high-quality data could be more useful (1985: 105). He consistently distinguishes between studies concerned with accounting for particular phenomena (whether few or many) and those concerned with evaluating the theories meant to explain them.

Since the later 1960s there has been a marked revival in writing about qualitative methods generally, even if this has remained a minority tendency, and it has had some of the characteristics of a social movement. Howard Becker suggests that he and some of his contemporaries wrote on participant observation and related topics in the 1950s and 1960s because they felt a need to justify what they did because of its minority status. These writings were taken up by a slightly later cohort who reacted strongly against the dominant quantitative ethos (Platt, 1991). It is only in the later 1980s, though, that the term "case study" has made a noticeable return to serious discussion. Let us trace some of its more recent history in the methodological literature.

We look first at the textbooks, where there has not yet been much trace of a real revival. Part of what looks like it in Table I is purely terminological: there are a number of brief references, in discussions of research design, to a "one-shot case study" design, in which a single group is studied only once, after the occurrence of a factor assumed to be causally relevant. These references are made in a context which takes for granted the logic of experimental design as discussed by Campbell (e.g. Campbell and Stanley, 1966) as the basic model, and follows his conclusion that this is not a satisfactory scientific method because it does not allow the secure imputation of causes. It is interesting, and instructive about the processes by which textbooks are produced, that these authors do not seem to have caught up with Campbell's more recent thought (e.g. Campbell, 1975; Cook and Campbell, 1979) in which he suggests a different way of describing case studies which emphasizes the richness of their data and sees logical strengths in this.

Another set of textbook authors define "case study" as the study of a single case, and go on to suggest that this may sometimes be useful in exploratory research or to deal with special circumstances, which are sometimes defined in quite idiosyncratic ways. Sommer and Sommer (1986: 170–174), for instance, build a whole short chapter round the idea that case studies are appropriate when an unexpected and unusual event such as a tornado occurs and can only be studied retrospectively. These authors seldom give the issue more than a few lines, so that it is hard to analyse what they mean. Babbie (1989: 261) treats "case study" as simply one of the things he includes under the head of "field research", and some other authors feel obliged to say that case studies cannot be equated with participant observation; another set of ideas is, thus, current in which the term is seen as associated with a method of data collection rather than a research design. Lofland (1971: 1), whose book we have treated as a monograph rather than a textbook because it confines itself to qualitative methods, in his first sentence mentions the "case study approach" only to say that this is one name for what the book is about, and "participant observation" or "qualitative observation" are others.) Kidder and Judd (1987), finally, use the words only in the context of a conventional reference to one-shot case study design, but then go on in the next chapter to a long, sophisticated and favourable discussion of fieldwork and participant observation and the logic by which more general conclusions can be reached from "studying particular people and places" – without using the words "case study". Many of the less sophisticated textbooks have similar chapters about participant observation/fieldwork, but with less attention to design, focusing on issues such as access and ethics. These chapters commonly stand a little to one side of the main line of the book's structure where there are, for instance, general chapters on sampling and on data analysis which are plainly irrelevant to that kind of work. It would probably be true, if a little unkind, to say that the typical methods textbook discusses design in terms of experiments, and sampling and analysis in terms of surveys, while other means of collecting data make a serious appearance only in the chapters on data collection. These different discussions do not join up to make a coherent whole.

To a considerable extent this textbook pattern reflects the areas where systematic methodological work has been done and given accessible general formulation. It is only quite recently that writers other than Campbell (Rosenblatt, 1981; Yin, 1984/1989; Platt, 1988; Stoecker, 1991) have turned their attention again to work on case study methods in sociology – at least under that name. (At the time of writing it is known that two relevant US books are in preparation: Feagin et al., 1991; Ragin and Becker, 1992.) A brief review of scattered treatments across the social sciences before these (Platt, 1988: 3–5) shows that what recent writers mean by "case study" depends on what they think of as the alternatives to it. Thus Lijphart (1971) in the context of a discussion of comparative method in politics, takes it for granted that the case is a whole polity, and a case study can only have one case since if more were used the study would become "comparative". Runyan (1982), a psychologist, implicitly focuses on individual persons considered as objects of treatment, and treats single-case experimental designs as the alternative. Mitchell (1983), an anthropologist, defines a case study as a detailed examination of an event which exemplifies a general principle; because he takes for granted the anthropological paradigm in which single societies are studied intensively and taken as the focus of interest, the use of a "case study" as a distinctive procedure cannot imply depth study of the whole society, though it must refer back to that by exemplifying something general about that society as a unique entity. Writers in the field of education may have backgrounds in more than one disciplinary tradition, and so both survey and experiment appear as the perceived alternative to case study, defined as intensive study of a single-bounded system. Much educational research is aimed at an audience of practitioners, so less emphasis on theorizing and more on "understanding" and "tacit knowledge" is likely to speak to their concerns.

The book edited by Feagin et al. (1991) is perhaps most interesting for our purposes as an indication of growing enthusiasm for something called "case study", whatever that may be. The editors offer a definition of case study as "an in-depth, multifaceted investigation, using qualitative research methods, of a single social phenomenon. The study is conducted in great detail and often relies on the use of several data sources ... The social phenomenon studied ... can be an organization; it can be a role or role-occupants; it can be a city; it can even be an entire group of people. The case study is usually seen as an instance of a broader phenomenon, as part of a larger set of parallel instances." They add that some case studies may also use quantitative methods. The definition, thus, is not very precise, in which it has much in common with the historical tradition. Most chapters give an account by their authors of studies they have done, which are very different from each other and do not clearly reflect a shared methodological position. Many interesting comments are made, and examples provided, but the work as a whole does little to advance our analytical understanding – and, indeed, does not seem to have that as a primary aim. The emphasis of the book as a whole, though not of all its authors, is on a political or evangelistic advocacy of qualitatively rich, real-life sociology as against standard contemporary American "journal sociology". The book edited by Ragin and Becker promises to be considerably more analytical; its focus is on the idea of "case" rather than of "case study", though the cases considered could often also qualify as case studies.

The best-known modern work is, however, a specialist textbook by Yin; he is a psychologist by training, though writing in a social-research series at least equally aimed at sociologists; it is as relevant that he has made a career in consultancy, specializing in case studies of organizational processes. His approach is, thus, eclectic, but tends to assume that policy conclusions will be drawn. (His first edition was published in 1984, but we use the revised edition of 1989.) His technical definition of "case study", intended to isolate the features which distinguish it from other strategies, is a rather puzzling one, given his declared intention to focus on design and analysis issues more than the commoner ones of data-collection (Yin, 1989: 11):

A case study is an empirical inquiry that:
* investigates a contemporary phenomenon within its real-life context; when
* the boundaries between phenomenon and context are not clearly evident; and in which
* multiple sources of evidence are used. (Yin, 1989: 23)

This definition seems to work better negatively than positively, since it provides features which Yin uses to distinguish what he has in mind from history, experiment or survey, although it does not overtly specify anything about design logic or analysis; it is as much a restatement of some of the points he made a few pages earlier about the circumstances to which a case study strategy is appropriate as it is of what the case study might consist of. We need to look elsewhere in the book to see what he really had in mind. It then becomes very clear that he is concerned with something significantly different from the classic interwar "case study method", even if some features are shared. Shared features are the study of one or a small number of cases, the collection of data by any – and probably multiple – means, and a logic of generalization different from that of sampling. What distinguishes his version is the manner in which he suggests this be done. Crucially, he does not conceive of case study method as inductive: one should start with theoretical reasons for thinking a case study appropriate, choose the case(s) on theoretical grounds, and plan the data-collection to answer theoretical questions. The data collected may be quantitative or qualitative; that distinction is not important. Nor is the number of cases important, although he does prefer multiple to single cases. Single-case designs are seen as appropriate when the case provides a critical test of theory, is a rare or unique event, or serves a revelatory purpose. Multiple-case designs should follow the logic of replication rather than of sampling, with each case carefully chosen because either similar or contrary results are predicted. The number of cases needed would depend on the circumstances: if, for instance, rival theories are very different two or three direct replications may be sufficient, while if they have subtler differences or a higher degree of certainty is wanted five or more could be needed; where differences might be anticipated the kinds of variation theoretically expected to be relevant will need to be represented, using the same logic as that of stratified sampling. (His approach is a rather practical one, so he does not attempt to address the philosophical issues raised.) Less importantly, he is not especially concerned with time-span and historical depth, with richness of data, or with access to personal meanings, and shows no interest in emphasizing data in people's own words. This is probably linked with the fact that, although he briefly mentions the possibility of single individuals being the cases, it is obvious that his prototype is the study of organization(s) or policy innovation(s) and he thinks of larger groups; within those, he does emphasize the need to take diverse perspectives into account.

What Yin has done, thus, is to redefine case study method as a logic of design, seeing it as a strategy to be preferred when circumstances and research problems are appropriate rather than an ideological commitment to be followed whatever the circumstances. The logic he uses is, moreover, one generally accepted among contemporary methodologists rather than an alternative one; he has brought (his conception of) case study method into the mainstream intellectually, even if this does not yet show in the general textbooks. Whether those with "qualitative" commitments will take over his account remains to be seen; it could provide a basis for legitimation and reconciliation with what has been seen as the enemy, but at the cost of giving up some of the traditional claims and strategies.

It is noticeable that, although Yin refers to many of the classic postwar texts of the "qualitative" tradition, both substantive and methodological, he uses them within his own somewhat different frame of reference, and also draws on a literature of consultancy and policy research not usually mentioned at all in the sociological discussions. It seems likely that this means a little more than the obvious fact that writers have different experiences, and draw on their experience for examples and ideas. In practice Yin could be seen as writing especially for the needs of those in the policy/consultancy world. These people are typically interested in formal organizations and deliberate policies, rather than informal groups. They are interested in policies over the general range of their concern, which may be narrow and local or wider and over many sites, but have responsibility for outcomes in particular instances and can afford only an instrumental interest in general theories which cover more than their responsibilities. Finally, they can afford relatively elaborate research efforts and do not naturally confine themselves to what a solo researcher with limited resources can achieve.

CONCLUSION

The last point above draws attention to the fact, important in making comparisons, that Yin has entirely dissociated the idea of case study from that of fieldwork or participant observation. That is a useful step towards conceptual clarity, since it has been an oddity in the tradition of writing in this field that so much of it has used a term whose overt reference is to a design feature to mean many other things. In this respect Yin is in line with the general movement in the kinds of distinction made among different methods in sociological discussion. The postwar shift from case study versus statistical method to participant observation versus survey reflected a need, from the "qualitative" side, to emphasize an aspect of what was being done which was clearly different from the changed practice of the "quantitative" side, and made the basis of distinction between methods rest on the manner in which the data were collected. However, it is obvious that a term such as "survey" is used to mean both a way of collecting data and a type of design (usually a "snapshot" representative sample). It is probably the detailed elaboration of the logic of experimental design, for which Campbell is mainly responsible, that has increasingly pushed textbook authors to think about how "survey" could be distinguished in design terms from such alternatives as "experiment". These efforts are important, though in my judgment they could hardly be regarded as successful. (Yin too approaches this issue in his introduction but, as with his "definition" of the case study, is really writing about the situations in which different strategies are to be preferred rather than about what distinguishes them.) It might be suggested that one prime need of the textbook writer is for a convenient way to divide the material up into chapters, and another prime need is to be able to make clear and simple statements which show the right way to do things; the first need can be met in many ways, and the second militates against dwelling on hard distinctions or cross-cutting categories. To the extent that textbook categorizations influence the way professionals think, that is unfortunate, because it tends to institutionalize unhelpful distinctions and discourage analytical thought.

However, textbooks generally depend heavily, though often with a time-lag, on the work done in methodological articles and monographs, and in reflections on research experience. Research practice does not always reflect current methodological discussion; probably this often happens because substantive specialists and methodologists move in different intellectual worlds. (The loss this can cause to research is obvious, but there are also gains: practical researchers can extend the range of alternatives and raise new issues as they grapple, sometimes successfully, with new problems. Methodology can move forward through the analysis of practice.)

It has been argued that case study practice has often had a poor correspondence to case study theorizing, whether that has been fashionable or unfashionable, and so it has been necessary to consider both. Much case study theorizing has been conceptually confused, because too many different themes have been packed into the idea "case study". The current resurgence of interest in it is very promising, because some recent work shows genuine analytical advances, not least in its distinctions between the functions which different types of study may perform. These also seem likely further to undermine the unhelpful distinction between "qualitative" and "quantitative", which is all to the good.

NOTES

1. It can be said with some confidence that the list is probably complete up to the end of the 1960s, at least for the first three types of source; no textbooks or monographs were identified which could not be found, and all existing US sociological journals were scanned. After that the number of publications had expanded so much that some might have been missed, and it was not possible to inspect copies of all the textbooks identified, though it seems reasonable to assume that those easier to find were more popular and influential. Articles were sought by scanning appropriate headings from the American Sociological Association Cumulative Index of Sociology Journals 1971–85. Work published outside the US, or in journals specifically associated with disciplines other than sociology, has been excluded from the count, although some of it is referred to in the text. A list of the textbooks used is available from the author.

2. This anecdote was heard as a student in one of his classes.