Similarities in the Technical Problems Associated with Quantitative and Qualitative Research

One reason for giving greater recognition to the technical aspects of decisions relating to the use of quantitative or qualitative research is that it may result in a appreciation of the common technical problems faced by practitioners working within the two traditions. The emphasis on their epistemological separateness runs the risk of failing to give due attention to these common problems. The recognition of mutual technical problems may also invite a questioning of whether the quantitative and qualitative research traditions are as far apart from each other as the epistemological argument may be taken to imply.[2]

Perhaps the most striking problem which besets both groups of practitioners is that of 'reactivity' — the reaction on the part of those being investigated to the investigator and his or her research instruments. Surveys and experiments create an awareness on the part of subjects that they are being investigated; the problem of reactivity draws attention to the possibility that this awareness creates a variety of undesirable consequences in that people's behaviour or responses may not be indicative of their normal behaviour or views (e.g. Rosenthal, 1966). In experiments subjects may be influenced by what they perceive to be the underlying aims of the investigation, or in interviews respondents may be influenced by characteristics of the interviewer, such as the latter's age, race, gender, or whatever. The recognition of such problems led Webb *et al.* (1966) to propose the greater use of 'non-reactive' methods of gleaning data, such as field experiments in which people do not know that they are under investigation. An example of the latter is Daniel's (1968) study of racial discrimination in England in which actors of different races were hired to seek employment, accommodation, car insurance, and the like in order to determine the extent of such discrimination.

Problems of reactivity confront the ethnographer as well. An obvious solution is to engage in covert observation which simultaneously deals with the problem of access. The study by Festinger, Riecken and Schachter (1956) of a religious cult is an example of this strategy, in that the investigators feigned conversion to a group which was predicting the imminent end of the world. Even here the problem of reactivity was not fully overcome, since the researchers' conversion was treated by the cult's adherents as confirmation of the validity of their beliefs and hardened their resolve to prepare for the fateful day. But the strategy of covert observation is usually frowned upon by social scientists, because it transgresses a number of ethical conventions (Bulmer, 1982), though many of the non-reactive methods suggested by Webb *et al.*

(1966) are suspect in this way as well. By and large, ethnographers prefer to be open about their participation, but frequently display a concern about the effect of their presence on the people they observe. While recognizing the potentially contaminating effects of their presence, ethnographers frequently play down this problem, suggesting that their familiarity to the people they study militates against this reactive effect. Gans's (1967) statement about the impact of this presence in a middle class American suburb is indicative of this position: 'After a while, I became a fixture in the community; people forgot I was there and went on with their business, even at private political gatherings' (p. xxiii). Likewise, Atkinson (1981, p. 129), writing about his experience of a participant observation study of clinical education in a medical school, comments:

> Although my presence on the wards had originally taken a fair amount of negotiation, once access had been granted, I was generally taken very much for granted on the wards, and by the students as they went about the hospital . . . Indeed, for some doctors I became so much a part of the normal scene that they forgot who I was.

The suggestion here is that the participant observer becomes part of the scenery and hence largely invisible. This process of absorption can be enhanced by not taking copious notes in front of subjects.

However, there is some evidence to suggest that reactive effects may occur. For example, Atkinson's assertion that he became unobtrusive jars somewhat with his admission that both clinicians and students often thought that he was engaged in an evaluation exercise (p. 125), which may have had ramifications for the behaviour of these groups in his presence. An interesting insight on the reactive effect in participant observation can be derived from an anecdote relating to research which was not conducted for the purposes of academic social science. During the 1971–2 football season, a journalist, Hunter Davies, was effectively a participant observer with Tottenham Hotspur Football Club, an experience which spawned a book, *The Glory Game* (1972). Writing in his autobiography about one particular game, the then manager of the club, Bill Nicholson (1984, p. 141), remarked:

> Hunter Davies, a Spurs fan, followed us around that season . . . He had been given permission to become virtually one of the players and was allowed in the dressing room which in my view must always be a private place. Permission was not given by me but by someone else at the club. In hindsight I should have overruled the decision.

I know Davies' work was highly acclaimed, but after this particular match I was forced to keep quiet when I wanted to say a lot of things straight away.

It is not apparent whether this was the only incident whereby Davies's presence influenced Nicholson, but the suggestion that he would have overruled the decision indicates that this is at least a possibility. Nor do we know how representative Nicholson's experience is – but then the subjects of participant observation do not normally write books in which they can refer to their experience.

There is, then, at least a possibility that the participant observer's presence may have an effect on what is observed. It is surprising that the widespread acceptance of interviewing in qualitative research has not been given greater critical attention as regards the problem of reactivity. Even unstructured interviewing is an obtrusive method and would seem to share some of the limitations of the familiar survey interview in this regard. Researchers using unstructured interviews, even though such instruments are longer and much more likely to produce repeat visits than those of the survey kind, are unlikely to have the participant observer's capacity for becoming part of the scenery. The potential for reactive effects in the unstructured interview, though arguably less than in survey interviews, would still seem to be more likely than in participant observation.

Indeed, the extensive use of interviews in qualitative research points to another area in which the two research traditions share a common technical problem. The pervasive acceptance of the unstructured interview as a legitimate tool of qualitative research – either in conjunction with participant observation or more especially on its own – is occasionally surprising on at least five further accounts. First, writers like Cicourel (1982, p. 15) have criticized interviews for their lack of 'ecological validity': 'Do our instruments capture the daily life, conditions, opinions, values, attitudes, and knowledge base of those we study as expressed in their natural habitat?' Cicourel points to a lack of ecological validity in interviews in a number of ways; for example, they are very sensitive to slight changes in wording, and the availability of the necessary knowledge to answer a question on the part of the respondent is rarely addressed. Although survey research is certainly more culpable in these respects than unstructured interviews, it is possible that they do not totally escape this criticism. The respondent in an unstructured interview study is questioned in a much more probing manner than in the conventional interview, but the issue of whether this means that ecological validity is obtained tends to be

unexplored. This issue again suggests a common thread to the technical problems faced by quantitative and qualitative researchers.

Secondly, it is not always clear how well the unstructured interview fits with the suggestion that qualitative research exhibits a concern for process. Participant observers would seem to be in a much stronger position to impute the interconnections of events over time than researchers who rely exclusively on unstructured interviews. The relative absence of a sense of process in research which relies wholly on unstructured interviews can be mitigated to a certain degree by building in a longitudinal element. In their study of individuals in arrears with their mortgages, Took and Ford (1987) conducted unstructured interviews on three occasions with many of their respondents in order to chart changes in the experience and perception of debt. Thirdly, survey interviews have long been criticized for their tendency to rely on attitudes and people's reports of their behaviour, both of which may bear little relation to actual behaviour (LaPiere, 1934; Deutscher, 1966); participant observation displays a technical advantage in this respect by virtue of the researcher's ability to observe behaviour directly (as recognized in the Warwick and Lininger quotation above). There is little reason to believe that unstructured interviews are substantially superior to survey interviews in this connection which also indicates another similarity in technical problems faced. Fourthly, the qualitative researcher may experience some difficulty in establishing the appropriate climate for an unstructured interview on some occasions. For example, in her qualitative research on grandparenthood, Cunningham-Burley (1985) found that some of her unstructured interviews with grandparents took a more formal, ordered character and departed very little from her schedule. Such interviews seem to have had an adverse effect on the qualitative depth of the data. Finally, the unstructured interview is not obviously consistent with the commitment of much qualitative research to naturalism. The interview is an obtrusive interruption of the natural flow of events, so that it is slightly surprising to find writers like Blumer (1969), who are committed to the naturalistic viewpoint, suggesting that it is a legitimate tool for such research. Cunningham-Burley (1985), for example, found that even her most informal interviews conformed to a question–answer format. Together, these features point to a possible need to question the extent to which unstructured interviewing is entirely consistent with the qualitative approach and to more areas in which the two approaches exhibit similar technical problems.

There has been some re-appraisal of the role of interviews in qualitative research, which may have been motivated slightly by

such considerations. Paget's (1983) discussion of interviewing artists in New York is indicative of such a re-orientation. She views the in-depth interview as a scientific means of developing systematic knowledge about subjective experience. She sees the in-depth interview as a medium through which the interviewer and interviewee jointly create this knowledge; the former is fully implicated in the process of gaining knowledge about the interviewee's subjective experience. Paget implicated herself in this process by making clear her own interest in the art world and allowed this personal concern to be reflected in the questions she formulated and her own responses to the interviewees' replies. Her interviews, transcripts from which are quoted at length, include numerous switches of both content and direction which are fully followed up in the course of the sessions. The content of replies is fully explored to discern their meaning and significance. Thus a simple question about the nature of an artist's paintings (e.g. whether they were fun to do) rapidly turns into a statement by the interviewee about her commitments to art and some of the financial problems she encountered during the early years (Paget, 1983, pp. 70–1). These points are then followed up and addressed during the interview. Further, the meaning of replies is embedded in the context of the interview situation itself; thus a particular reply is examined by the interviewer in the context of the interviewee's other replies and is possibly returned to when, for example, later exchanges produce information which is puzzling, when viewed in relation to the earlier reply. It is the contextual understanding of respondents' replies that sharply distinguishes this emerging conception of the role of the unstructured interview from the survey interview (Mishler, 1986). The unstructured interview is viewed by writers like Paget as a dynamic process whereby the researcher seeks to gain knowledge – by which she means 'illuminating human experience' (p. 88) – about what art work or whatever entails. This perspective on interviewing seems to harmonize somewhat better with the qualitative research tradition than much unstructured interviewing, by virtue of its explicit concern with respondents' subjective experiences. Further, the tendency to invite interviewees to speak at length possibly renders such interviewing less obtrusive than much unstructured interviewing. The problem of ecological validity may also be reduced since the focus is very explicitly upon what is important to the interviewee rather than to the researcher.

A further technical problem shared by both research traditions relates to the selection of people who are the focus of the research. Quantitative research is concerned to establish that respondents are representative of a wider population, so that the generalizability of

findings can be enhanced. In fact, much research departs significantly from this ideal in a number of ways. However, there is a case for suggesting that the issue of representativeness confronts the qualitative researcher too. The avowal to see through the eyes of one's subjects can be interpreted to imply that the ethnographer needs to attend to the question of the typicality of the eyes through which he or she is seeing. This kind of concern may be regarded as indicative of the application of an inappropriate criterion (that is, one deriving from the framework of quantitative research) to the ethnographer's mode of research. For example, in their exposition of grounded theory, Glaser and Strauss (1967) advocate that the qualitative researcher should give less attention to the need to meet statistical sampling criteria in assessing the adequacy of a 'sample'; rather, the researcher should be much more concerned with the issue of whether the sample conforms to the investigator's emerging theoretical framework. For example, in an ethnographic study of power in a medical school in the USA, Bucher (1970, p. 5) proposed that, following a preliminary analysis of her initial observations, 'data are being sought from areas of the organization which should provide test cases, so to speak, for emerging propositions'. According to the principles of 'theoretical sampling' (the term coined by Glaser and Strauss), the researcher observes only as many activities, or interviews as many people, as are needed in order to 'saturate' the categories being developed, and then turns to the next theoretical issue and carries out the same process over again. Thus the question of the adequacy of a sample is determined by the degree to which it permits the qualitative reseacher to develop and confirm one or more categories; as soon as the researcher feels satisfied that the theoretical point has been established, he or she can move on to the next issue. This procedure allows the constant interplay between theory and research that Glaser and Strauss are keen to develop.

While Glaser and Strauss's view of sampling serves well to exemplify the disinclination of qualitative researchers to accept the quantitative researcher's preoccupation with representativeness, like other aspects of grounded theory, their particular view of the sampling process is probably cited far more frequently than it is used. Of course, qualitative researchers (especially ethnographers) may not simply sample people, but also activities, time periods, and locations (Burgess, 1984). None the less, if the aim of the exercise is to see as one's subjects see, there is still a problem of the representativeness of the eyes. For example, qualitative researchers sometimes display a concern that the people with whom they come into contact may be marginal to the social setting. Blau (1964) found that during the early period of his field-work in a federal

agency, his early contacts and suppliers of information were marginal officials who were keen to voice their criticisms of the agency and their colleagues. As Blau acknowledged, had he not recognized this problem, a distorted picture of the agency would have been generated. Interestingly, Blau (1964, p. 30) argues that the field researcher's own marginality may render him attractive to those who are marginal to the settings in which research is carried out. Similarly, Ball (1984, p. 81), drawing on his ethnographic study of a comprehensive school, has remarked on the untypicality and, in the case of two teachers, marginality of his main informants. Other writers (e.g. Hammersley, 1984, pp. 51–3) have drawn attention to biases which stem from the problem of establishing a picture drawn from a representative spread of contacts and informants.

The purpose of drawing attention to some common technical problems faced by quantitative and qualitative researchers has been to highlight the possibility that discussions of the differences between the two traditions purely in terms of epistemological issues run the risk of exaggerating their distinctiveness. This is not to imply that the differences between quantitative and qualitative research are insignificant, but that they may not be as far apart as is sometimes implied by the epistemological version of the debate about the two research traditions.

The Link between Epistemology and Technique

Quantitative and qualitative research are each associated with distinctive methods of data collection and research strategy, although their differences are not as clear cut as some of the more programmatic statements imply. In particular, it has been suggested in this chapter that the differences between the two research traditions are less precise than writers who emphasize epistemological issues suggest. According to the epistemological version of the debate about the two research traditions, the choice of research method flows from an allegiance to a distinctive position in relation to how social reality ought to be studied. This view suggests that, for example, it would be wholly inappropriate to use a survey in order to conduct research which is grounded in the cluster of intellectual predilections associated with qualitative research. It is not simply that the survey is likely to generate quantitative data; rather, it is seen as better suited to a natural science conception of how social reality ought to be studied, while a technique like participant observation is better attuned to the epistemological commitments of qualitative research.

In other words, the epistemological version of the debate assumes a correspondence between epistemological position and

research method. Is this an acceptable view of the way social research is conducted? This question can be tackled in a number of ways, but in the rest of this section some problems with assuming a clear correspondence between epistemology and research technique will be examined.

Ethnography and Positivism
Not all commentators on the nature of social research have accepted the bond between ethnographic research and a non-positivist approach to the study of social research. In particular, the view has been expressed that ethnographic investigations may also engender a form of empiricism. Willer and Willer (1973) argue that, although participant observation differs from what they call 'systematic empiricism' (as exemplified by the experiment and the social survey), it is none the less empiricist in that it establishes the connections between observed categories. According to this view, the empiricism of participant observation resides in the researcher's distrust of categories which are not directly amenable to the senses. This point is highly congruent with Rock's (1973) view that much research in the sociology of deviance (a great deal of which stems from ethnographic investigations) is phenomenalist by virtue of a disinclination among many researchers to dig themselves out of the data on the social world in which they bury themselves. This phenomenalism can be discerned in the following passage from a text on field research by a proponent of qualitative research in the sociology of deviance:

> We begin with direct experience and all else builds on that . . . [W]e begin with and continually return to direct experience as the most reliable form of knowledge about the social world. (Douglas, 1976, p. 7)

In like fashion, Willis (1980), the author of a celebrated ethnographic study of working class youth (1977), has referred to a 'covert positivism' in participant observation. By this phrase, Willis means that the researcher sees the subject of his or her research as an object who can provide data; further, he argues, the preference of participant observers to postpone the generation of theory in relation to their data enhances a positivist hue by virtue of deferring to what is directly observable. Finally, Delamont and Atkinson (1980) have argued that the tendency towards atheoretical investigations in much school ethnography is conspiring to produce a form of empiricism.

In each of these comments is a view that participant observation does not depart radically from a positivist epistemology. In the