

on case studies in much qualitative research detracts from the investigator's ability to generalize his or her findings has been critically addressed. In drawing attention to the importance of theoretical reasoning as the crux of the issue of case study generalization, the question of the relationship between theory and research again raises its head.

Notes

- 1 The injunction to empathize and to see through the eyes of one's subjects can cause the qualitative researcher problems in certain domains. Billig (1977) has written about the difficulties associated with qualitative research on fascists; in particular, he argues that adopting a posture of disinterest or of sympathetic understanding is inappropriate in such research. Robbins, Anthony and Curtis (1973) have drawn attention to the difficulties of maintaining a stance of empathy without sympathy when studying religious groups like the 'Jesus Freaks', who define their view of the world as exclusively true.
- 2 Readers interested in the considerable attention being given by anthropologists to the tentativeness of their accounts of other cultures and to the devices they use to convey a sense of ethnographic authority should consult Clifford and Marcus (1986) and Marcus and Fischer (1986).
- 3 An example is Marcus's (1986) criticism of Willis's (1977) ethnographic research on working class lads. Marcus argues that Willis selected non-conformist boys and was uninterested in why some boys are nonconformist while others are not. Further, Marcus suggests that it is inappropriate to draw inferences about the operation of capitalism from such a sample.

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The Debate about Quantitative and Qualitative Research

In this chapter, the main contrasting features of quantitative and qualitative research will be etched out. Much of the discussion in the literature on these two research traditions has created a somewhat exaggerated picture of their differences. These discussions reflect a tendency to treat quantitative and qualitative research as though they are mutually antagonistic ideal types of the research process. This tendency can be clearly discerned in some of the programmatic statements relating to qualitative research (e.g. J. Lofland, 1971; Bogdan and Taylor, 1975). While there *are* differences between the two research traditions, as the first section of this chapter will explicate, there are also a number of points at which the differences are not as rigid as the programmatic statements often imply. Consequently, in addressing some of the contrasting features in quantitative and qualitative research, some areas of similarity will also be appraised. The discussion will then proceed to an assessment of the degree to which epistemological issues lie at the heart of the contrast, or whether it is more a matter of different styles of data collection and analysis *tout court*. This issue has implications for the extent to which quantitative and qualitative research are deemed to be capable of integration (the focus of Chapter 6). It also has implications for the question of the extent to which quantitative and qualitative research constitute divergent models of the research process, since it has been the suggestion that they represent distinct epistemologies that has played a major role in the exaggeration of their differences. Finally, the question of whether these two research traditions share some common problems is examined.

Contrasting Features in Quantitative and Qualitative Research

Some of the main contrasting dimensions of quantitative and qualitative research have been either explicitly or implicitly explored in the previous chapters. This section will draw out these dif-

Table 5.1 *Some differences between Quantitative and Qualitative Research*

	<i>Quantitative</i>	<i>Qualitative</i>
(1) Role of qualitative research	preparatory	means to exploration of actors' interpretations
(2) Relationship between researcher and subject	distant	close
(3) Researcher's stance in relation to subject	outsider	insider
(4) Relationship between theory/concepts and research	confirmation	emergent
(5) Research strategy	structured	unstructured
(6) Scope of findings	nomothetic	ideographic
(7) Image of social reality	static and external to actor	processual and socially constructed by actor
(8) Nature of data	hard, reliable	rich, deep

ferences more directly. Table 5.1 lists eight important dimensions on which the two research traditions diverge. The subsequent discussion explores these themes in some greater detail.

View of the Role of Qualitative Research

Quantitative researchers rarely totally deny the utility of qualitative research, but have tended to view it as an essentially exploratory way of conducting social investigations. Consequently, they have typically seen it as useful at the preparatory stage of a research project, a view which is clearly discernible in Blalock's (1970) attitude to participant observation which was quoted in Chapter 1. Precisely because of its exploratory and unstructured approach, qualitative research is often depicted as useful as a means of throwing up hunches and hypotheses which can be tested more rigorously by quantitative research. Such a view treats qualitative research as a somewhat second rate activity in implying that qualitative data

cannot stand in their own right because they need to be verified. The proponents of qualitative research see it as an end in itself, in particular because of its capacity to expose actors' meanings and interpretations, which is a central requirement of the approach and of its presumptive intellectual underpinnings which were discussed in Chapter 3. However, it is possible to detect a degree of unease among some qualitative researchers about the extent to which their findings can stand alone. Gans (1962, p. 350), at the end of his participant observation study of an Italian-American slum, exemplified this diffidence in proclaiming that his research 'is a *reconnaissance* – an initial exploration of a community to provide an overview', and went on to say: 'Many of the hypotheses reported here can eventually be tested against the results of more systematic social science research.' Interestingly, in the second edition of the book which derived from this research, Gans (1982, p. 414) has indicated that he would get rid of this 'apologetic conclusion' if he were able to rewrite the book. He argues that the reason for this apologetic style was that at the time social scientists were strongly influenced by a belief in the appropriateness of the scientific method. By implication, Gans seems to be suggesting that the assertiveness of qualitative researchers coupled with the growing disillusionment with quantitative research have created a different climate, whereby investigations of the kind he undertook are increasingly regarded as ends in themselves.

Relationship between Researcher and Subject

In quantitative research the researcher's contact with the people being studied is fairly fleeting or even nonexistent. While the data collection phase often extends over many months, the contact with each individual is usually brief. In longitudinal surveys or in before-and-after experiments, the investigator returns to his or her subjects, but the degree of contact is still fairly short-lived. Indeed, the use of some methods associated with quantitative research may require no contact with subjects at all, except in an indirect sense; postal questionnaire surveys, laboratory experiments in which the researcher simply observes while others conduct the experiment (e.g. Milgram, 1963), and many forms of unobtrusive, structured observation (Webb *et al.*, 1966) involve virtually no contact between researcher and subject. Even in interview surveys, the main investigator may have little or no contact with respondents since hired staff frequently carry out many (and sometimes all) of the interviews.

By contrast, qualitative research entails much more sustained contact, especially when participant observation is the central method. The degree to which there is sustained contact within a

particular study will vary a good deal; Gans (1962) had some contact with 100 to 150 West Enders but only twenty of these were intense. The need for the fostering of such relationships is a product of the qualitative researcher's need to see the world through his or her subjects' eyes, since the researcher would be unable to gain any leverage on this level of analysis from a distance. Unstructured interviewing typically entails less sustained researcher-subject relationships than participant observation, but is invariably longer than survey interviews. In any case, the wide-ranging nature of the unstructured interview invariably necessitates a fairly close relationship between researcher and subject, which re-visits (which are relatively rare in survey interviewing) may intensify.

This contrast between the two research traditions can be illustrated through the work of Hirschi (1969) and Adler (1985). In the former case, self-administered questionnaires were the chief source of data. The questionnaires were administered to the children by their schools, so that Hirschi's contact with his subjects was minimal. By contrast, Adler had contact over a period of six years with some of the drug dealers she was investigating.

The Researcher's Stance in Relation to the Subject

The quantitative researcher adopts the posture of an outsider looking in on the social world. He or she applies a pre-ordained framework on the subjects being investigated and is involved as little as possible in that world. This posture is the analogue of the detached scientific observer. Hirschi was chiefly concerned to test a theory of delinquency, and adopted a stance towards his subjects which entailed limited contact with them. They were merely fodder for the examination of his concerns, and not people with their own views and perspectives in relation to delinquent behaviour, school, and the other elements of Hirschi's research.

Among qualitative researchers there is a strong urge to 'get close' to the subjects being investigated — to be an insider. For qualitative researchers, it is only by getting close to their subjects and becoming an insider that they can view the world as a participant in that setting. Thus, Hirschi's outsider stance can be contrasted with Adler's view: 'the only way I could get close enough to [upper-level drug dealers and smugglers] to discover what they were doing and to understand the world from their perspectives (Blumer, 1969) was to take a membership role in the setting' (Adler, 1985, p. 11). The insider standpoint may have its costs, the most frequently mentioned of which is the problem of 'going native', whereby the researcher loses his or her awareness of being a researcher and is seduced by the participants' perspective. Oakley (1984, p. 128), drawing on her research on becoming a mother, describes the experience of going native as follows:

at three forty-five after two hours of a busy antenatal clinic I too would sigh with the doctors as we jointly peeped into the corridor and saw, still waiting, another row of abdomens . . . Or at two in the morning I wanted someone to get in there quickly and do a forceps delivery so I could (like them) go home to bed.

The experience of going native was not entirely negative in that it enabled her to understand the pressures obstetricians are under. In any event, qualitative researchers are likely to see such drawbacks (if indeed they acknowledge them as such) as unavoidable consequences of a standpoint which is needed to gain access to their subjects' views. It is also apparent that the possibility of going native, with its implication of a loss of detachment, is to a significant degree incongruent with the image of impartial scientist which many quantitative researchers espouse.

Relationship between Theory/Concepts and Research

The model of quantitative research presented in Figure 2.1 implies that theories and concepts are the starting point for investigations carried out within its framework. Thus Hirschi in the Preface to *Causes of Delinquency* wrote: 'In this book I attempt to state and test a theory of delinquency.' By contrast, qualitative researchers often reject the idea of using theory as a precursor to an investigation (except perhaps as a means of providing an initial orientation to the situation as in 'grounded theory') since it may not reflect subjects' views about what is going on and what is important. Consequently, as one advocate of qualitative research has put it, 'It is marked by a concern with the discovery of theory rather than the verification of theory' (Filstead, 1979, p. 38). Thus Adler's (1985) chief theoretical contribution — the notion of upper-level drug dealing as a component of a hedonistic life-style rather than an occupation — was an outcome of her research rather than a precursor to it.

In fact, the extent to which quantitative research is explicitly guided by theory has been questioned of many commentators. Instead, theoretical reasoning often occurs towards the end of the research process (Cicourel, 1982, pp. 19–20). Indeed, quantitative research is often much more exploratory and unpredictable in outcome than its description by the advocates of qualitative research seems to imply. An example of the misleading nature of the view that quantitative research is devoid of surprise is the study of the International Typographical Union in the USA by Lipset, Trow, and Coleman (1956), which involved a mixture of qualitative research and survey data. The latter were compiled in order to examine *inter alia* the relationship between union shop size and members' political involvement. However,

This analysis did not merely test hypotheses already held before the survey was conducted. Rather, the earlier hypotheses pointed to a fruitful line of enquiry, but many of the ideas and insights regarding the bearing of shop size on union politics emerged only in the course of the analysis of the survey data. (Lipset, 1964, pp. 116–17)

Lipset (1964, pp. 111–12) also provides a number of other examples of the way in which his survey data were a source of surprise. Similarly, Pugh (1988), writing about the Aston research which was discussed in Chapter 2, has commented on his disappointment that the effect of organization size on structure was so pervasive, an observation which can be interpreted as indicative of a certain element of surprise.

Quantitative research is often depicted as a routine practice whereby theories and their integral concepts are simply operationalized with a view to verifying their validity (see, for example, Filstead's remark on this issue in the passage cited on p. 97). Ironically, some qualitative research is showing an explicit concern with theory, not solely as something which emerges from the data, but also as a phase in the research process which is formulated at the outset (Woods, 1986, pp. 156–61). Some of the school ethnographies cited in Chapters 3 and 4 show signs of a movement in this direction. Consequently, the contrast between quantitative and qualitative research in terms of verification of theory against preferring theory to emerge from the data is not as clear-cut as is sometimes implied.

Research Strategy

Quantitative research tends to adopt a structured approach to the study of society. To a large extent, this tendency is a product of the methods with which it is associated; both surveys and experiments require that the issues to be focused upon be decided at the outset. In the previous section, the point was made that there is the possibility of an element of surprise in survey research which is frequently underestimated. However, it is evident that such investigations require that the variables be mapped out and introduced into the survey instruments. Survey research is structured in the sense that sampling and questionnaire construction are conducted prior to the start of data collection and then imposed on the sample members. Similarly, in experimental designs, independent and dependent variables, experimental and control groups are all part of the preparatory stage. In both cases, once the research has been designed the broad shape of the findings can be discerned, that is, before one person has been interviewed or one experimental subject has received a treatment. An examination of Hirschi's (1969) questionnaire reveals that he was fully aware of the material that needed

to be collected in order to test the theories of delinquency which were to be examined.

By contrast, qualitative research tends to be more open. Many ethnographers advocate that the delineation of a research focus be deferred as long as possible (e.g. Cohen, 1978). Consequently, many qualitative researchers refer to a sensation of being overwhelmed during their early days in the field, since everything they observe is potentially 'data'. Whyte (1984), for example, sees ethnographic research as deriving much of its strength from its flexibility, which allows new leads to be followed up or additional data to be gathered in response to changes in ideas. But he also notes a limitation of such flexibility since 'you may find so many interesting things to study that you are at a loss to delimit the scope of your project and focus on specific problems' (Whyte, 1984, p. 225). Barrett (1976) has shown how his use of a prior theoretical framework (and a structured research strategy which derived from this framework), which was used to guide his anthropological research on the factors associated with the economic success of a village in Nigeria, caused him to misconstrue his data. Initially, his theoretical focus had led him to believe that his data implied that the communal economic organization of the village, rather than religion, was the main contributor to its success. Almost two years after the completion of his thesis which was based on this fieldwork, Barrett felt impelled to accord religion a stronger role, following some critical comments he had received on his explanation. However, Barrett has since revised his explanation yet again to provide what he believes to be the most accurate explanation of the village's success. This explanation differs radically from its predecessors in that it goes far beyond the simple juxtaposition of religion and communal organization and emphasizes the emergence of development as a paramount goal within the village. But Barrett's main point is that he was able to arrive at this more accurate account only when he had cast off the shackles of his prior theoretical framework. Such an experience underscores the strength of the qualitative researcher's preference for postponing theoretical reflection, albeit at the possible cost (noted by Whyte) of being overwhelmed by data. It also suggests that it may be disadvantageous to ethnographers to structure their strategies in advance.

Also, the role of luck may be more apparent in such research, where being in the right place at the right time may significantly affect the direction of the research (Bryman, 1988), or alternatively may give access to a potential research site (Buchanan, Boddy, and McCalman, 1988). For example, Bresnen (1988) refers to a lucky encounter in the pub soon after he had started his research on a

construction project. Most of the site management team were present, but two senior managers had gone home. It became apparent that there was a considerable gap in attitudes between senior and junior management, which prompted Bresnen to develop a new line of questioning following on from this unexpected lead. One of the undoubted strengths which qualitative research affords the practitioner, by virtue of its unstructured nature, is precisely this capacity to encounter the unexpected and possibly to change direction.

Scope of Findings

It is common to conceive of the quantitative/qualitative dichotomy in terms of respective commitments to *nomothetic* and *ideographic* modes of reasoning (Halfpenny, 1979). This distinction effectively refers to the scope of the findings which derive from a piece of research. A nomothetic approach seeks to establish general law-like findings which can be deemed to hold irrespective of time and place; an ideographic approach locates its findings in specific time-periods and locales. The former mode is taken to be indicative of the scientific approach, whereas ideographic reasoning is often more closely associated with the historian's method. By taking random, and hence representative, samples, survey research is taken to exhibit a nomothetic approach because of the investigator's ability to infer findings to larger populations. Thus, Hirschi (1969) took great pains to ensure that the data on the children he studied would be representative of the wider population of school children through a stratified random sampling procedure which took account of such population characteristics as race, sex, and school attended.

By contrast, the qualitative researcher frequently conducts research in a specific milieu (a case study) whose representativeness is unknown and probably unknowable, so that the generalizability of such findings is also unknown. Adler's (1985) subjects were acquired in an apparently much less rigorous manner than Hirschi's children. Her initial contacts were accidental and were her source of further contacts. Moreover, these subjects were mainly located in a limited geographical area, so that their broader representativeness may be questioned. Qualitative researchers often exhibit some unease over this point. Liebow (1967) conducted participant observation in relation to 'two dozen Negro men who share a corner in Washington's Second Precinct' (p. 11) and goes on to note:

To what extent this descriptive and interpretive material is applicable to Negro streetcorner men elsewhere in the city or in other

cities, or to lower-class men generally in this or any other society, is a matter for further and later study. (Liebow, 1967, p. 14)

The discussion about generalization in quantitative research in Chapter 2 suggests that the extent to which investigations within this tradition are nomothetic is often exaggerated. Surveys are often not based on random samples and, even when they are, they refer to highly restricted populations. For example, writing about the field of organization studies, Freeman (1986, p. 300) has observed: 'They rarely work with samples that are representative of even the restricted types of organizations they choose to study.' The fact that Hirschi's sample derives from a geographically restricted area — a county in the San Francisco-Oakland metropolitan area — is given much less attention in his book than his attempts to select a random sample of that region's population of school children. Further, the consistency of findings over time is rarely given much attention. Experimental research also suffers from a number of deficiencies in regard to the generalizability of findings stemming from such designs. Moreover, as the discussion in Chapter 4 on case study research implies, qualitative researchers are building up strategies for enhancing the generalizability of their research. Consequently, caution is necessary in treating the two research traditions as being strictly associated with nomothetic and ideographic findings.

Image of Social Reality

Quantitative research conveys a view of social reality which is static in that it tends to neglect the impact and role of change in social life. Surveys examine co-variation among variables at a particular juncture; experimental research usually entails the exploration of a restricted range of variables within a restricted time period. While both styles of research examine connections between variables, the proponents of qualitative research argue that quantitative research rarely examines the processes which link them (e.g. Blumer, 1956). They also charge that the 'independent' and 'dependent' variables fail to take into account the flow of events in which these variables are located. Quantitative researchers might argue that they do take such factors into account. For example, the notion of an 'intervening' variable, which is both a product of the independent variable and an influence on the dependent variable, might be interpreted as a device which examines intervening processes (Rosenberg, 1968). However, the suggestion is still open to the accusation that intervening processes are ignored (e.g. between the independent and intervening variables) and that the nexus of factors within which such chains of causality are grounded is rarely examined. For example, the causal chain in Hirschi (1969), quoted

in the Introduction, suggests that academic incompetence is causally related to delinquency via a sequential series of intervening variables (poor school performance, dislike of school and rejection of school's authority). It might legitimately be argued that the processes which account for the intermediate connections (e.g. rejection of school's authority and delinquent acts) are unexplored.

The qualitative researcher is in a better position to view the linkages between events and activities and to explore people's interpretations of the factors which produce such connections. This stance affords the qualitative researcher a much greater opportunity to study processes in social life. Adler's (1985) ethnographic research was concerned to demonstrate the nature of the 'career progressions' of the dealers and smugglers with whom she was in contact. She shows how dealers enter and climb to the top of these 'occupations' and how they and their experiences change with their ascendancy into upper-level activities. Similarly, Adler and Adler (1985) used participant observation and unstructured interviewing to study basketball players at an American university in order to examine the relationship between athletic participation and academic performance among college athletes. They note that the bulk of the literature implies that participation in college sports is associated with poor academic performance, although some studies are not consistent with this finding. Adler and Adler confirmed the negative relationship between athletic participation and school performance but show that most athletes come to college with a commitment to doing well in their academic studies. However, they encounter a number of experiences which conspire to deflate their academic motivation: athletic experiences (e.g. the time spent in training, playing and recovering), social experiences (e.g. the domination of their lives by interaction with other athletes) and classroom experiences (e.g. adverse attitudes towards them indicated by their professors) have a deleterious effect on their interest in academic work. Both of these studies inject a sense of process and transformation in social life which quantitative research can rarely address.

In addition to their respective tendencies to convey static and processual views of social life, quantitative and qualitative research differ in their view of the mutual relationship between the individual and social reality. There is a tendency for quantitative researchers to view social reality as external to actors and as a constraint on them, which can be attributed to the preference for treating the social order as though it were the same as the objects of the natural scientist. By contrast, the influence of perspectives like phenomenology, symbolic interactionism, and naturalism led qualitative researchers to suggest that 'we cannot take for granted,

as the natural scientist does, the availability of a preconstituted world of phenomena for investigation' but must 'examine the processes by which the social world is constructed' (Walsh, 1972, p. 19). Thus, whereas quantitative research tends to invoke a perspective which implies that social reality is static and beyond the actor, the image deriving from qualitative research gives a sense of that same reality in processual terms and as socially constructed. This point can be illustrated by reference to the study of organization structure. Quantitative research on this topic, like the Aston Studies (Pugh and Hickson, 1976; Pugh, 1988), depicted organization structure as something which is determined by forces such as an organization's size or its technology. In turn, organization structure was seen as affecting the behaviour and orientations of its members (Pugh and Payne, 1977; Pugh, 1988). This approach seems to view organization structure as external and as a constraint on the actor, and differs from the qualitative research on a psychiatric hospital by Strauss *et al.* (1963) which suggests that the organization's structure was a 'negotiated order'. This latter study suggests that the behaviour of the hospital's members was largely unaffected by a formal structure of rules and role prescriptions; instead, the various groupings within the hospital produced their own structure, which they negotiated and which was in a constant state of renegotiation.

Nature of the Data

The data emanating from quantitative studies are often depicted as hard, rigorous, and reliable. These adjectives suggest that such data exhibit considerable precision, have been collected by systematic procedures and may be readily checked by another investigator. These positive attributes are often taken to mean that quantitative data are more persuasive and hence more likely to gain the support of policy-makers. Okely (1987), for example, has described how she was under great pressure from her employers at a research centre, in which she was to conduct research on gypsies, to use survey methods, because they believed that such research provided the only means of influencing policy-makers. She writes: 'At the outset the declared ideal was to be a report "with a statistical table on every page"' (p. 62). Such a view is indicative of the very considerable power of quantitative data, possibly because of their association with 'science', to impress by virtue of their apparent rigour.

Qualitative researchers routinely describe the data deriving from ethnographic work as 'rich' and 'deep', often drawing a contrast with quantitative data, which tend to be depicted as superficial. The denotation 'rich' is generally indicative of the attention to often intricate detail which many qualitative researchers provide. Their sustained contact with the people they study permits a

penetrating account, which can explore incidents in great detail and can illuminate the full extent of their subjects' accounts of a variety of phenomena. Further, the predilection of ethnographers for conveying social life in the language and style of their subjects adds to this sense of richness. In terms of conventional sampling, Liebow's (1967) street-corner men constitute an unacceptably small, non-random sample of unknown representativeness. But they provide, as in much qualitative research, the route to a vivid, detailed portrayal of a small sector of social life. Further, the potential of the attention to rich detail in qualitative research to policy-making and other 'applied' contexts is gaining increasing recognition (Finch, 1986). An interesting anecdote in this respect has been supplied by Okely (1987, p. 58):

in the 1983 general election [in the UK], the Conservative party geared its campaign to the daily reactions of the floating voter in marginal seats, mainly in southern England. These potential supporters were the subject of in-depth qualitative interviews several times a week. Feedback from these data was used within days to adjust the emphasis in campaign issues.

To many qualitative researchers, quantitative research produces superficial data. They tend to view survey research, for example, as a source of surface information which relates to the social scientist's abstract categories. By contrast, the quantitative researcher may be suspicious of the limited generality of a study of two dozen men in one area of one city (Liebow, 1967) from which data were collected that may have been heavily influenced by the particular emphases and predispositions of the researcher.

A Question of Epistemology or Technique?

What *are* quantitative and qualitative research, as outlined in the preceding section? In the book thus far, there has been a strong suggestion that epistemological issues underpin the divide between them. By an 'epistemological issue' is meant a matter which has to do with the question of what is to pass as warrantable, and hence acceptable, knowledge. In suggesting that quantitative researchers are committed to a positivist approach to the study of society (Filmer *et al.*, 1972), the view is being taken that they subscribe to a distinctive epistemological position, since the implication is that only research which conforms to the canons of scientific method can be treated as contributing to the stock of knowledge. Similarly, by subscribing to positions, such as phenomenology, *verstehen*, and naturalism, which reject the imitation of the natural scientist's procedures and which advocate that greater attention be paid to

actors' interpretations, qualitative research can also be depicted as being underpinned by an epistemological standpoint.¹ The tendency among some writers (e.g. Filstead, 1979) to refer to quantitative and qualitative research as 'paradigms' (following T. S. Kuhn, 1970) underscores the fact that they are frequently conceived of as different epistemological positions. The tendency to view the two research traditions as reflecting different epistemological positions, and hence divergent paradigms, has led to an exaggeration of the differences between them. As a consequence of such thinking, quantitative and qualitative research are frequently depicted as mutually exclusive models of the research process.

The following is a representative version of the view that quantitative and qualitative research reflect different epistemological positions:

Quantitative and qualitative methods are more than just differences between research strategies and data collection procedures. These approaches represent fundamentally different epistemological frameworks for conceptualizing the nature of knowing, social reality, and procedures for comprehending these phenomena. (Filstead, 1979, p. 45)

Similarly, Rist (1977, p. 62) suggests that each of the two research traditions rests on 'an interrelated set of assumptions about the social world'. The view that quantitative and qualitative research constitute different epistemological positions would seem to imply that researchers formulate their views about the proper foundation for the study of social reality and choose their methods of investigation in the light of that decision. This would imply that a researcher's personal commitment to the view that the natural sciences provide the only acceptable basis for generating knowledge would mean that his or her approach to conducting an investigation, as well as the methods of data collection, will be chosen in this light. Likewise, a view that the scientific method provides a poor basis for the study of people, coupled with a commensurate endorsement of a position like phenomenology, will propel an investigator in the direction of a qualitative approach. Alternatively, it might be suggested that a researcher who chooses to carry out a survey, for example, has to recognize that his or her decision to use that method carries with it a train of epistemological implications which need to be recognized at the outset, in case the selection does not fit with the researcher's broader intellectual proclivities.

One might question whether research is conducted in these ways, but the suggestion that the two research traditions are rooted in divergent epistemological implications seems to carry with it con-

notations of these kinds. However, the view that quantitative and qualitative research represent different epistemological implications is not held by all writers, even though they view the two approaches as distinctive. The alternative standpoint is to suggest that quantitative and qualitative research are each appropriate to different kinds of research problem, implying that the research issue determines (or should determine) which style of research is employed. For example, Walker (1985, p. 16) has proposed: 'Certain questions cannot be answered by quantitative methods, while others cannot be answered by qualitative ones.' This view implies that the decision over whether to use a quantitative or qualitative approach should be based on 'technical' issues regarding the suitability of a particular method in relation to a particular research problem. Accordingly, the different characteristics of quantitative and qualitative research which were summarized in Table 5.1 can be interpreted as pointing to the respective strengths and weaknesses of these two research traditions. Consider the following rationale for the procedures employed in a study of entrepreneurs in Britain:

As with all social research, the methods adopted in this enquiry were largely dictated by the nature of the research problem. We set out to study the *dynamics* of small-scale capital accumulation and the *social processes* which account for the reproduction of the entrepreneurial middle class. In addition, it was our intention to define more precisely the *nature* and *interrelationships* of the constituent groupings within this class. The complexity of these issues did not favour quantitative investigation; in our view a qualitative approach was more appropriate. [The authors provide three considerations which determined this view] . . . Consequently, we undertook an intensive study of a limited number of proprietors using semi-structured interviews which were, to a considerable extent, shaped by the personal experiences of the respondents. (Scase and Goffee, 1982, p. 198)

In this account, there is a recognition of the strengths and weaknesses of quantitative and qualitative research, coupled with a technical decision that the latter will suit their needs better; epistemological issues are not in evidence. By inference, writers who perceive the distinction between the two styles of research in terms of their relative suitability for a particular research topic are effectively suggesting that the differences between them boil down to little more than 'differences between research strategies and data collection procedures' (see quotation from Filstead, 1979, cited on p. 105). This position is not new; it can be discerned in a celebrated

exchange between Becker and Geer (1957) and Trow (1957) in which the former argued that participant observation provides 'the most complete form of sociological datum' (p. 28). In reply, it was suggested that 'the problem under investigation properly dictates the methods of investigation' (Trow, 1957, p. 33).

There seem, then, to be two fairly distinct versions of the nature of the differences between quantitative and qualitative research which might usefully be referred to as the 'epistemological' and 'technical' accounts. However, there is a tendency for many writers to oscillate between these two versions. This is particularly evident in some of the discussions about whether it is possible to integrate quantitative and qualitative research within a single study. The ways in which they might be combined constitute the focus of the next chapter; in the meantime, the broader question of whether in principle they might be combined is addressed here. The technical version of the differences between quantitative and qualitative research seems to provide few impediments to the possibility of a research strategy that integrates them. While a researcher may prefer to use one to the relative exclusion of the other (as with Scase and Goffee, 1982), if the research problem invites a combined approach there is little to prevent such a strategy, other than the usual reasons of time, money, and possibly inclination. The researcher may choose to conduct a predominantly ethnographic study, but decide to add some survey evidence relating to people who are not accessible through the focal method. Woods (1979) buttressed his ethnographic research on a school with a survey of parents for precisely this reason.

The epistemological account would seem to pose more problems in regard to the possibility of combining the two approaches. If quantitative and qualitative research are taken to represent divergent epistemological packages (or paradigms), they are likely to exhibit incompatible views about the way in which social reality ought to be studied, and hence what should be regarded as proper knowledge thereof. This incongruence is particularly evident in the implicit critique of the application of the scientific method to the study of society which phenomenology contains. It is not obvious how a marriage of such divergent epistemological positions as positivism and phenomenology (even in the metaphorical use of the term) can be entertained. Guba (1985) has argued vehemently against the suggestion that the two research traditions might be reconciled. In his view, attempts to combine the two approaches fail to recognize the distinction between a paradigm and a method. He argues that the idea that quantitative and qualitative research can be dovetailed rests on a view that they represent only different methods of investigation; instead, 'we *are* dealing with an either-or

proposition, in which one must pledge allegiance to one paradigm or the other' (Guba, 1985, p. 80). Thus in the same way that Kuhn regards paradigms as incommensurable, Guba is suggesting that the collection of assumptions and beliefs about the study of the social order that underpin quantitative and qualitative research should be treated in the same manner.

Not all writers subscribe to this view. Reichardt and Cook (1977) suggest that the tendency to view the two styles of research as paradigms stands in the way of their joint use within a single project, and prefer to see them as 'method-types'. What is somewhat more surprising is that some writers who subscribe to the epistemological view of the differences between quantitative and qualitative research simultaneously suggest that they might be integrated. Filstead (1979), who was quoted above for his view that they represent 'different epistemological frameworks', suggests that 'great advantages can be obtained by creatively combining qualitative and quantitative methods' (p. 42). Bogdan and Biklen (1982), after a discussion of the intellectual foundations of qualitative research (in phenomenology, etc.), ask whether it can be used in tandem with a quantitative approach. They acknowledge that it can, but display a lack of enthusiasm for the idea, not because of any kind of epistemological incompatibility, but for practical reasons. They suggest that research which combines the two approaches 'is likely to produce a big headache' (p. 39), because of the practical problems of producing both a good quantitative and a good qualitative design. Delamont and Hamilton (1984) oscillate in the other direction. In contrasting structured and ethnographic observation in classrooms, they argue against the view that these two styles of observation are 'the equivalent of self-contained epistemological and theoretical paradigms', but then go on to argue that the two methods reflect 'the tension between positivism and interactionism' which 'cannot be done away with by calling for interdisciplinary *rapprochements*' (pp. 5, 6).

There seems, then, to be a tendency for many writers to shuttle uneasily back and forth between epistemological and technical levels of discourse. While much of the exposition of the epistemological debts of qualitative research helped to afford it some credibility, a great many decisions about whether and when to use qualitative methods seems to have little, if any, recourse to these broader intellectual issues.

The Strengths and Weaknesses of Quantitative and Qualitative Research

Underlying much of the preceding discussion is the suggestion that the distinction between quantitative and qualitative research is

really a technical matter whereby the choice between them is to do with their suitability in answering particular research questions. Such a view draws attention to the different strengths and weaknesses of the methods of data collection with which the two research traditions are typically associated. It is not uncommon for textbooks on research methods to draw attention to such issues:

The sample survey is an appropriate and useful means of gathering information under three conditions: when the goals of the research call for quantitative data, when the information sought is reasonably specific and familiar to the respondents, and when the researcher himself has considerable prior knowledge of particular problems and the range of responses likely to emerge . . .

Participant observation is usually more appropriate when the study requires an examination of complex social relationships or intricate patterns of interaction; . . . when the investigator desires first-hand behavioural information on certain social processes . . . ; when a major goal of the study is to construct a qualitative contextual picture of a certain situation or flow of events; and when it is necessary to infer latent value patterns or belief systems from such behaviour as ceremonial postures, gestures, dances, facial expressions or subtle inflections of the voice. (Warwick and Lininger, 1975, pp. 9-10)

As they then go on to say, 'Each is useful for some purposes and useless for others' (p. 12).

This passage illustrates well the suggestion that the decision about which method to employ is essentially a technical issue. As such, the decision about whether to employ quantitative or qualitative research stands cheek by jowl with other familiar technical issues on which students of social research methods are reared, such as when it is appropriate to use a postal questionnaire, or to construct a stratified random sample. Warwick and Lininger's list of respective strengths and weaknesses can usefully be expanded. Social surveys are likely to be particularly appropriate where larger scale issues are concerned. The study of social mobility is a case in point (e.g. Goldthorpe, 1980). Of course, it might be argued that much of the recent ethnographic research on schools is concerned with social mobility too (e.g. Lacey, 1970; Woods, 1979; Ball, 1981). However, such research is typically concerned with the processes through which social class is perpetuated by the structure of the school, teachers' practices, and the class based sub-cultures within the school. Studies of social mobility like Goldthorpe's are essentially concerned with the *extent* of social mobility and changes in patterns. Thus, the technical version of the debate would imply

that the critical issue about whether a method fits a research problem is not a matter of the area of social life being investigated, but the nature of the issues being raised in relation to it. Similarly, juvenile delinquency may be studied by both survey methods (e.g. Hirschi, 1969) or participant observation (e.g. Patrick, 1973), but the nature of the questions being asked about juvenile delinquency differ. Hirschi was concerned to test the validity of three theories of the causes of delinquency. This preoccupation with the causes of variation in delinquency led him to carry out a survey of over 5,000 school children in California in order to collect sufficient data to separate out the variables which impinged on his dependent variable, delinquency. By contrast, Patrick joined a particularly violent gang in Glasgow as a covert participant observer 'to comprehend and to illuminate their view and to interpret the world as it appears to them' (Matza, 1969, p. 25, cited in Patrick, 1973, p. 9).

Social surveys are also likely to be preferred when, as in the case of Hirschi's study, there is a concern to establish cause-and-effect relationships. Experiments are even stronger in this department, but are likely to be appropriate only to situations in which the independent variable is capable of manipulation and in which random assignment (or at least some form of matching) is feasible. Qualitative researchers are not uninterested in causes, in that they are frequently concerned to establish how flows of events connect and mesh with each other in the social contexts they investigate, or how their subjects perceive the connections between facets of their environment. However, survey and experimental researchers tend to be much more concerned with the precise delineation of a causal factor, relative to other potential causes.

As Warwick and Lininger suggest in the extended quotation above, participant observation has its own strengths. The absence of a highly structured research design means that the investigator can change direction if he or she is lucky enough to hit upon an unexpected but interesting facet of the social setting. The participant observer is better able than the survey researcher to understand social processes.

A further, and neglected, strength of ethnographic studies is their capacity to reveal covert, hidden, even illegal activities. Studies of informal groups in large organizations (Dalton, 1959), output regulation in industrial work groups (Roy, 1960), and pilferage at work (Ditton, 1977) all demonstrate the capacity of ethnographers to look behind the scenes and bring to the centre of the stage aspects of these milieux which would otherwise either be inaccessible or possibly not even uncovered in the first place. The same point can be made about the study of deviant behaviour, a

more probing study of which requires sensitivity and a capacity to provide reassurance to the subjects that a survey researcher is unlikely to be able to inculcate. It is difficult to see how Adler's (1985) research on drug dealers could have been conducted with a more formal approach. Indeed, the capacity of ethnographers to gain access to hidden arenas can occasionally cause them difficulties. Serber (1981) sought to conduct an ethnographic study of a government bureaucracy responsible for the state regulation of the insurance industry in California. He rapidly discerned a range of informal practices (e.g. off-the-record meetings) which he chose to make the focus of his investigations, but found his access to people and documents sharply curtailed by senior managers as his awareness of the significance of such undercover processes grew.

The purpose of this discussion was to provide a flavour of the sorts of considerations that are relevant to the technical version of the distinction between quantitative and qualitative research. It is also the case that the choice between the two may derive from reasons other than the epistemological and technical reasons which have been encountered thus far. For example, many women social scientists have drawn attention to the affinity between qualitative research and a feminist perspective (Stanley and Wise, 1983). Oakley (1981) has argued that the typical survey interview, in which the researcher appropriates information from a respondent for the former's use, is incompatible with feminism. A feminist researcher conducting research on women would be setting up an asymmetrical relationship which exploits the already exploited interviewee. In order to mitigate this perpetuation of exploitation she advocates an approach to feminist research whereby the research situation is treated much more as an exchange in which the feminist researcher gives something back — of her own views, experiences, and the like — to those being interviewed. Such an approach implies a much more unstructured interview (and hence one closely associated with the qualitative approach) than that associated with the survey. It is striking that research on women's subordination in the workplace, as well as the interface between the workplace and the home, has tended to be the product of qualitative investigations in which participant observation and unstructured interviews figure strongly (Pollert, 1981; Cavendish, 1982; Griffin, 1985a). The underlying issues to the discussion about the appropriateness of particular methods to feminist research imply that considerations other than those which have figured thus far in this book may impinge on choices of method, e.g. ethical, political, ideological considerations.

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 an 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 researcher 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 phenomenism 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

view of these authors, the empiricism in much ethnographic research is exaggerated by the widespread tendency to postpone theoretical reflection, if indeed theory comes into the reckoning at all. In Glaser and Strauss's (1967) grounded theory, the view of theory as an emergent product of an investigation is systematized. However, the positioning of theory at the outset of an investigation can also be regarded as retaining positivist elements, by virtue of 'theory' being envisioned as something which needs to be tested by recourse to an examination of the real world. The problem here is that, irrespective of whether theory is seen as something which precedes or succeeds the collection of ethnographic data, a basically positivist precept is being adhered to, since the world of the senses is the ultimate arbiter of whether a theory is acceptable or not. Thus the quest for a more explicit grounding of qualitative research in theory (which some writers have expressed – see Chapter 4) supplants the more obvious empiricism of waiting for the theory to emerge, with the positivist preference for being 'entitled to record only that which is actually manifested in experience' (Kolakowski, 1972, p. 11). It is the manner in which theory is conceptualized in relation to the collection of data that points to an affinity with positivism, and not simply whether theory comes before, during, or after the data collection phase.

Qualitative research may also allow the investigator to impute causal processes which bear a strong resemblance to the kinds of causal statements that are the hallmark of quantitative research (although without the precise delineation of cause and effect which quantitative researchers seek to generate). McCleary (1977) conducted participant observation and interviews with parole officers in a division of a state parole agency in Chicago. He notes that officers should report parole violations known to have been committed by their parolees, but frequently they do not. Through his research, he was able to identify five factors which result in officers' disinclination to report their parolees: full reporting cuts into the officer's time; it may reflect badly on the officer and result in a negative evaluation by his or her superior; the officer's options may be restricted as a result of reporting a violation; and so on. Thus, McCleary was able to identify causes of failure to report parolees, whilst retaining fidelity to the perspectives of parole officers themselves.

Quantitative Research and Meaning

The recurring theme within qualitative research of viewing attributes of the social world through the eyes of the people being studied has led to a convention that only methods like participant observation and intensive interviewing are acceptable in this light.

But quantitative researchers also make frequent claims to addressing issues relating to the meaning of aspects of the social world to the people being studied. Social science research on work provides a number of examples of such investigations. The classic study of a sample of adults in the USA by Morse and Weiss (1955) used a survey to discern the range of reasons why people work and what meaning work has for them. The authors found that work does not simply mean the ability to earn money, but has a number of other meanings for people. Goldthorpe *et al.* (1968) conducted a survey in Luton to examine industrial attitudes and behaviour. One of the study's central notions – the idea of 'orientation to work' – draws attention to the variety of meanings which work may have for industrial workers. Finally, in their monograph on social stratification which reports a large scale survey of white-collar employees, Stewart, Prandy and Blackburn (1980) draw attention to the tendency to treat clerks as an undifferentiated category in many discussions of their position in the class structure. By contrast, the purpose of their research was to show that 'the meaning of clerical work will not be the same for all engaged in it' (Stewart, Prandy and Blackburn, 1980, p. 112 – emphasis added).

Marsh (1982) has also drawn attention to the capacity of social surveys to provide insights into questions of meaning. For example, the widespread tendency among social researchers to solicit their respondents' reasons for their actions, views, and the like, provides the researcher with people's interpretations of a variety of phenomena. She also points to the research by Brown and Harris (1978), which examined the connection between critical life events (e.g. loss of job, death of husband, childbirth) and depression. Marsh observes that the researchers went to great pains to establish the meaning of each life event to each respondent. For example: 'Childbirth was not normally rated severe unless it happened in the context of bad housing and shortage of money' (Marsh, 1982, p. 117).

The field of cognitive social psychology provides a contrasting example of a subject which is explicitly concerned with meaning but which relies heavily upon quantitative experimental research as a prominent data gathering procedure. Cognitive social psychologists are concerned with 'how people make sense of other people and themselves' and 'people's everyday understanding both as the phenomenon of interest and as a basis for theory about people's everyday understanding' (Fiske and Taylor, 1984, p. 17). For example, in the field of leadership research, a prominent interest has been leaders' perceptions of the causes of their subordinates' success or failure (Bryman, 1986). This level of analysis is concerned with everyday understandings of the meanings of success and

failure. Such research has proceeded by experimentally manipulating subordinate behaviour and then gauging leaders' perceptions of the causes of particular levels of that behaviour. Investigators have been particularly concerned to establish the circumstances in which good or poor subordinate performance is deemed by leaders to be a consequence of internal factors (e.g. subordinates' levels of ability or effort), or of external factors (e.g. task difficulty or luck). Thus such research is concerned with the meanings people ascribe to events and to others' behaviour.

It seems, then, that quantitative researchers also make the claim that their methods can gain access to people's interpretations and to the ways in which they view the world.

Participant Observation and Theory Testing

Quantitative research tends to be depicted as well suited to the task of testing explicitly formulated theories, whereas qualitative research is typically associated with the generation of theories. However, there is nothing intrinsic to participant observation, for example, that renders it inappropriate for the testing of pre-formulated theories. Becker (1958) provided a framework which would facilitate the examination of previously formulated theories by participant observation. He anticipated that his proposed approach would allow qualitative research to assume a more scientific character than that with which it is most closely associated. Other writers, like McCall (1969) and Campbell (1979), have argued along similar lines that the association of qualitative research solely with theory-creation does less than justice to its potential.

Indeed, one of the most celebrated studies using participant observation – Festinger, Riecken and Schachter's (1956) investigation of a religious cult – was designed to test a theory about how people are likely to respond to the disconfirmation of a belief to which they are fervently wedded. The authors suggested that a number of conditions can be envisaged which would allow the belief to be held with greater zeal even when it has been proved to be wrong. Festinger *et al.* learned of a millenarian group that was prophesying the imminent end of the world and felt that it would provide an ideal case for the examination of their theoretical concerns. As mentioned on p. 112, along with some hired observers, they joined the group as participants and 'gathered data about the conviction, commitment and proselytizing activity' (p. 31) of its adherents. More recently, as observed above, some writers have argued for a more explicit approach to the testing of theory by qualitative researchers (e.g. Hammersley, 1985; Hammersley, Scarth and Webb, 1985). Further, the view that qualitative research is compatible with a theory testing approach is implicit in the more

recent treatments of the issue of case study generalization which were mentioned in Chapter 4. It will be recalled that Mitchell (1983) and Yin (1984) have both suggested that the question of the generalizability of case studies (and thereby much qualitative research) misses the central point of such investigations, in that the critical issue is 'the cogency of the theoretical reasoning' (Mitchell, 1983, p. 207). The Festinger, Riecken and Schachter study is a case in point: the representativeness of the cult is not particularly important; it is its relevance to the theoretical framework which constitutes the most important criterion for assessing the study. Accordingly, the view of qualitative research which plays down its role in relation to the testing of theory may be missing an important strength that qualitative investigations possess. In other words, there is nothing intrinsic to the techniques of data collection with which qualitative research is connected that renders them unacceptable as a means of testing theory.

Conclusion

It has been suggested that there are a number of ways in which the posited connection between epistemology and data collection can be questioned: participant observation (and indeed unstructured interviewing) is not without positivist leanings; survey researchers frequently claim to be looking at the social world from their respondents' perspectives; and participant observation can be deployed within a theory testing framework with which the epistemological basis of quantitative research is conventionally associated. None the less, a recurring theme of this book thus far is that a prominent view of the debate about quantitative and qualitative research is that they are competing epistemological positions, each of which is associated with particular approaches to data collection and research strategy. How should we understand the apparent clash between the suggestion presented here that the link between epistemology and method is not clear-cut and the epistemological account of the debate about quantitative and qualitative research?

One of the most unsatisfactory aspects of the epistemological version of the debate is that it is unclear whether its proponents are arguing that there *is* a link between epistemology and method of data collection or whether there *ought* to be such a bond. If the argument is that there *is* such a link, the epistemological argument runs into difficulties. In addition to the points made in the previous section, which suggest that the bond between epistemology and method may be exaggerated, it is also clear that methods like participant observation and unstructured interviewing have been used

by various practitioners who have not had an epistemological axe to grind. Writers like Lupton (1963), Gans (1962), and Skolnick (1966), all of whom have written much admired monographs deriving from the use of such methods, seem to have exhibited few, if any, philosophical pretensions in their justifications for the use of qualitative research. These researchers were able to produce highly regarded ethnographic studies without recourse to the programmatic statements surrounding qualitative research. This very fact invites a questioning of the role of programmatic statements in relation to the pursuit of good social research. Rather, these researchers were concerned to get close to the people they were studying, to allow for the possibility of novel findings, and to elucidate their findings from the perspective of the people they studied. While the last of these three concerns is invariably taken as a keynote of the epistemological substructure of qualitative research, a preoccupation with meaning and subjects' perspectives is not exclusive to the qualitative tradition. Further, the methods with which qualitative research is associated have often been chosen on technical grounds rather than epistemological grounds.

The lack of a definitive link between broad epistemological positions and methods has also been suggested by Snizek (1976), who analysed 1,434 articles in sociological journals covering the period 1950 to 1970. Snizek was concerned to find out whether there was a connection between the three paradigms which Ritzer (1975) had suggested underpin sociology and the methods with which they are associated. If these three basic approaches to sociology really are paradigms (T. S. Kuhn, 1970), one would anticipate a link between the endorsement of the epistemology with which each is associated and the methods of research used. Two of the paradigms — the 'social factist' and 'social definitionist' orientations — correspond to quantitative and qualitative research respectively to a fair degree. However, Snizek was unable to discern a clear pattern which linked the general orientation of each paradigm with the methods of investigation employed.

The alternative position is to suggest that there *ought* to be a connection between epistemological positions and methods of data collection. This view would imply that researchers should be much more sensitive to the wider epistemological context of methods of data collection and that they are not neutral technical devices to be deployed under a variety of auspices. Choosing to conduct a survey or an ethnographic study would mean accepting a package of views about social reality and how it ought to be studied. Accordingly, it might be argued that researchers who claim to study subjects' views of the world with a survey (e.g. Goldthorpe *et al.*, 1968) are misguided, since they should have chosen a method more suited to

this perspective, like unstructured interviewing. It is not at all clear from the various writings on the debate about quantitative and qualitative research that the view exists that there ought to be a recognition of a mutual interdependence of epistemology and method (as against a view that there *is* such a connection). The problem with the 'ought' view is that it fails to recognize that a whole cluster of considerations are likely to impinge on decisions about methods of data collection. In particular, the investigator's judgements regarding the technical viability of a method in relation to a particular problem will be important, as the technical version of the debate about the two research traditions implies.

Methods are probably much more autonomous than many commentators (particularly those who espouse the epistemological versions of the debate) acknowledge. They can be used in a variety of contexts and with an assortment of purposes in mind. Indeed, the very fact that many qualitative researchers instil an element of quantitative data collection into their investigations underlines this point to a certain degree. Similar points can be made in relation to the connection between broad theories and methods. As pointed out in Chapter 2, Platt (1986) has strongly questioned the supposed link between functionalism and the survey. Similarly, while some writers have found an affinity between a Marxist perspective and qualitative research (e.g. Sharp and Green, 1975; Willis, 1977), others have preferred the methods of quantitative research for the empirical elucidation of concepts associated with this theoretical perspective (e.g. Wright and Perrone, 1977). Symbolic interactionism, while typically associated with participant observation (Rock, 1979), is not universally identified with qualitative research and an anti-positivist epistemology: M. H. Kuhn (1964) used the techniques and research strategies of quantitative research in his attempt to use symbolic interactionist notions; many studies conducted within this theoretical tradition make substantial use of standard survey techniques alongside participant observation; and there is even a questioning of whether G. H. Mead's (1934) writings lead in a direction which is antithetical to the application of the methods of the natural sciences (McPhail and Rexroat, 1979). The tendency to associate particular methods with particular epistemological positions is little more than a convention (which took root in the 1960s), but which has little to recommend it, either as a description of the research process or as a prescriptive view of how research ought to be done.

In comparison with the rather stark contrasts between quantitative and qualitative research which have permeated the pages thus far, the next chapter examines some of the ways in which the two traditions may be used in tandem. The epistemological version

of the debate does not readily admit a blending of quantitative and qualitative research since the two traditions are deemed to represent highly contrasting views about how social reality should be studied. The technical version of the debate much more readily accommodates a marriage of the two since it acknowledges the respective strengths and weaknesses of the two approaches as methods of data collection. Many writers shuttle uneasily back and forth between the two ways of thinking about the two traditions. It is little wonder that confusion ensues when there is a lack of clarity about what quantitative and qualitative research are. In this context, the view of a leading writer on the ethnography of schooling is instructive:

It is not surprising that some work called 'ethnography' is marked by obscurity of purpose, lax relationships between concepts and observation, indifferent or absent conceptual structure and theory, weak implementation of research method, confusion about whether there should be hypotheses and, if so, how they should be tested, confusion over whether quantitative methods can be relevant . . . and so forth. (Spindler, 1982, p. 2)

Precisely because many qualitative researchers have failed to sort out whether the style of research to which they adhere is an epistemological or a technical position, it is possible for such confusion to reign. However, when quantitative and qualitative research are jointly pursued, much more complete accounts of social reality can ensue, as many of the examples cited in the next chapter imply.

Notes

- 1 Symbolic interactionism, by contrast, is a theoretical position developed largely within the social sciences, but which has its roots in an epistemological position, namely, pragmatism.
- 2 Frequently, discussion in the literature of such issues takes the form of evaluating qualitative research in terms of its validity and reliability (e.g. LeCompte and Goetz, 1982). I have resisted such an approach in this book, because I feel that it imposes a cluster of standards upon qualitative research which to a large extent are more relevant to the quantitative tradition, within which such terms were originally developed.

6

Combining Quantitative and Qualitative Research

The rather partisan, either/or tenor of the debate about quantitative and qualitative research may appear somewhat bizarre to an outsider, for whom the obvious way forward is likely to be a fusion of the two approaches so that their respective strengths might be reaped. The technical version of the debate more readily allows this solution to be accommodated because it is much less wedded than the epistemological version to a view that the two traditions reflect antagonistic views about how the social sciences ought to be conducted. In this chapter, the focal concern will be the ways in which the methods associated with quantitative and qualitative research can be, and have been, combined. As noted in Chapter 3, there are examples of investigations carried out by investigators who locate their work largely within the tradition of qualitative research, but who have used survey procedures in tandem with participant observation (e.g. Woods, 1979; Ball, 1981). Such research will be employed as an example of the combination of quantitative and qualitative research, because the chief concern of the present chapter is with the *methods* with which each is associated.

The focus on methods of investigation should not lose sight of the significance of a distinction between quantitative and qualitative *data*. For example, some of the findings associated with an ethnographic study may be presented in a quantified form. In their research on the de-skilling of clerical work, Crompton and Jones (1988) collected much detailed qualitative information, in the form of verbatim reports, on the work of their respondents. In spite of considerable reservations about coding these data, they aggregated people's accounts of their work in terms of the amounts of control they were able to exercise in their work. Even among qualitative researchers who prefer to resist such temptations, the use of quasi-quantitative terms like 'many', 'frequently', 'some', and the like, is common (e.g. Gans, 1982, p. 408). Further, survey researchers provide the occasional verbatim quotation from an interview, or one or two case examples of respondents who exemplify a particular pattern. Sometimes, the reporting of