Doing Research on Sensitive Topics

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Estimating the Size of Hidden Populations

Social scientists often want to estimate rates of prevalence for deviant activities or to make judgements about the size of deviant groups. Sensitivity surrounds the production of such estimates since, often, those concerned feel threatened by disclosing their membership of a particular group or their participation in some particular activity. This chapter concentrates on how estimates of this kind may be produced using available data derived from official or administrative sources, or existing survey data. In particular, much of what follows focuses on methods for estimating the extent of so-called ‘invisible’ or ‘complaintless’ crimes. Crimes of this kind do not, of course, exhaust the range of hidden populations one might want to estimate. Other examples might include activities which, though not illegal, are rare, transitory or normatively suspect, for example, intermarriage in divided societies, or participation in an apparently bizarre religious cult. Invisible crime, however, well illustrates many of the problems involved in using available data.

Mark Moore (1983) has distinguished a number of different kinds of invisible crime. These include crimes without victims, white-collar crime, and crimes of victimization in which complaints by the victim are repressed in various ways. Victimless crimes typically involve, according to Schur, ‘the willing exchange, among adults, of strongly demanded but legally proscribed goods and services’ (1965, 169). Victimless crimes frequently remain hidden from view because the willing exchange involved means that there is no complainant to bring them to public attention. White-collar crime, like fraud, tax evasion and computer crime, is also frequently invisible. Those who commit such crimes can typically hide what they are doing, and it is often not clear who in fact is being victimized. Those who lose as a result of such crimes may be part of a diffuse group such as shareholders, and in some cases the crime is only discovered long after it has taken place (Braithwaite et al., 1987). Other invisible crimes involve direct victimization. However, crimes like blackmail or protection rackets remain invisible because, although the victims know they are being victimized, they are usually afraid to come forward (Hepworth, 1971). A lack of complaint is also a feature of exploitative crimes such as sexual abuse. Such crimes often take place in the context of an existing relationship in which there is a marked disparity of power, one result of which is to ensure the silence of the victim.

Invisible crime and official statistics

One ostensible advantage of official statistics is their availability. Where, as is often the case in studying sensitive topics, there are difficulties in obtaining first-hand information, the use of available statistics may seem like an attractive option. There are, however, well-known problems surrounding the use of official statistics on deviance. The production of official statistics by bureaucratic agencies results in a ‘dark figure’: that part of what the statistics purport to measure which goes unreported or unrecorded. Official statistics therefore notoriously underestimate the size of hidden populations and the extent of deviant activities. Both criminal statistics, the amount of crime recorded by the police, and judicial statistics, which relate to cases processed by the courts, provide a very imperfect measure of underlying rates of crime. This is particularly so in the case of judicial statistics, for while crimes may be reported, they may not be solved, nor the perpetrator(s) apprehended. Even if an apparent lawbreaker can be found, the case may be disposed of in a variety of ways which do not show up in the statistics, for example, by caution or through acquittal (Downes and Rock, 1988; Box, 1981). La Fontaine (1990) has described some of the difficulties which arise when judicial statistics are used to estimate the extent of child sexual abuse. In England and Wales, she points out, statistics are given for offences and convictions, but not for offenders. One cannot tell from this, therefore, the number of individuals who have committed sexual offences against children. Particular individuals may have been counted more than once by virtue of having been charged with multiple offences, while charges may relate to the same or a number of different victims. Furthermore, in cases of child sexual abuse obtaining convictions can be difficult. The nature of the crime means that some kinds of evidence, for example, testimony from very young children, may be inadmissible, and corroborative evidence may also be lacking. In addition, La Fontaine points to the possibility that plea bargaining may take place. This will, of course, increase the apparent number of less serious offences while underestimating more serious crimes.

Quite obviously, the size of prevalence estimates depends on how
researchers choose to define what it is that they are measuring. A number of commentators (see, e.g., Peters et al., 1986; Hertzberger, 1990) have noted, for example, that prevalence estimates for child abuse are extremely variable. Much of this variability (though by no means all) can be traced to differences in the definitions of abuse used. Thus, the estimates produced vary depending on whether what is being measured includes both contact abuse and non-contact abuse, whether abuse by peers is counted as well as abuse by adults (Peters et al., 1986), or whether one focuses on the perpetrator’s act alone or on the antecedents and consequences of those acts as well (Hertzberger, 1990). In some cases estimates are based on broad definitions which in fact group together heterogeneous phenomena. For example, Best (1989), who is severely critical of figures which have been published for the number of children abducted by strangers in the United States, argues that some estimates include runaway children, missing adults, and children taken by non-custodial parents.

It is also important to distinguish clearly between the measurement of incidence and the measurement of prevalence, that is, between the number of new cases coming to notice in a specific population over a given period as opposed to the total number of cases in a specific population over a given period. A further difficulty is that what should be regarded as a case when counting instances of some phenomenon is not always self-evident. As La Fontaine (1990) points out, this can be a particular problem in the study of child abuse where there is sometimes ambiguity about whether the case refers to the individual children involved or to their families. She notes, for example, that in the notorious Cleveland case in the United Kingdom the number of children taken into care was 160, but the number of families involved was only 44. Quite different perceptions of the problem might arise depending on which figure one takes.

Whether a particular crime finds its way into the criminal statistics depends on its reportability, visibility and recordability (J. Scott, 1990, 91). Reportability refers to the likelihood of a given crime being brought to the attention of the police. A number of factors serve to inhibit reporting. An offence may not be reported because it is perceived not to be serious. The difficulty or inconvenience of making a report may outweigh an individual’s inclination to do so. In some instances, instead of reporting the matter to the appropriate authorities, an aggrieved party might make an informal response to a particular act. Most relevant in the present context is the disincentive to reporting produced because disclosure is potentially threatening. Reports may not be made because to do so is potentially to risk embarrassment, intimidation or publicity. Computer fraud apparently goes undisclosed because financial institutions affected by it fear the embarrassment of admitting they have been defrauded. In a similar way, a very different crime, child sexual abuse, often remains unreported because it is accompanied by threats which seek to forestall discovery. Victims may also experience feelings of fear or guilt which inhibit disclosure (La Fontaine, 1990; S. Taylor, 1989).

The extent to which data are available derives in part, however, from the varying degrees of ‘recordability’ (J. Scott, 1990) which surround different areas of social life. To take one example, the extensive literature on estimating the size of the underground economy partly reflects the fact that the scale and scope of economic measurement is extensive relative to other areas of social life (Miles and Irvine, 1979). In many other instances data on a particular hidden population may simply not exist at all. John Scott (1990) notes that recordability is often linked to the visibility of deviant activities. The social organization of deviance provides varying opportunities for concealment or disclosure (Best and Luckenbill, 1980). Some kinds of deviant behaviour are multifaceted in ways which leave multiple traces. Thus, even if they do not provide a reliable basis for measurement, drug abuse may be tracked to some extent through rates of notification of viral hepatitis, drug seizure statistics and so on (Hartnoll et al., 1985b). On the other hand, activities which take place in secret may come to light only rarely, if at all. Child sexual abuse is again an obvious example.

Recordability is affected by administrative routines. In the case of crime, as Scott (1990) points out, the law is enforced in a discretionary way. What is recorded as crime reflects the policies and priorities of local forces. The police may not record an offence or may record only the most serious of a number of related offences. The contingencies which surround the policing of invisible crimes means that, often, they receive a specialist response. If the issue has been medicalized this may take the form of specialized diagnostic or treatment agencies. Alternatively, activities may be targeted by specialist units involved in proactive policing (Marx, 1980; Braithwaite et al., 1987). As with official statistics, reliance on figures based on the case records of particular agencies can be hazardous. Figures for the number of cases referred to specialist agencies represent those which have become known to professionals and exclude those which remain hidden. Agency cases are usually therefore unrepresentative, making difficult not only estimation but also bivariate analysis (La Fontaine, 1990). Those cases which do come to light often reflect the character of the referral process. In a
detailed study of cases referred to a specialized hospital unit dealing with the diagnosis and treatment of child sexual abuse, La Fontaine (1990) noted, for example, an increase in the number of cases referred between 1981 and 1984 as a result of greater awareness of the extent of the problem. However, this increase in its turn had consequences for the character of cases accepted. On the one hand, because of increased pressure on resources fewer cases of a purely diagnostic character were seen. On the other hand, increased awareness of the problem also meant that alternative sources of assessment and treatment were now available elsewhere, again changing the character of referrals to the agency studied. Routine record-keeping procedures within non-specialist agencies can also often mitigate against the disclosure of potentially embarrassing information. Macintyre (1978) notes that nurses and midwives in the antenatal clinic she studied were embarrassed to ask pregnant women questions about nervous or genetic disorders. They therefore phrased questions about these topics in vague or leading ways which tended to produce socially acceptable answers. Yet the responses to these questions formed the basis for official medical records.

To the extent that specialist surveillance units are used to police invisible crimes, their procedures will affect the estimates derived. Every year the US Internal Revenue Service intensively examines a random sample of tax returns on approximately 30,000 individuals. Despite these efforts the ability of IRS investigators to uncover evidence of non-compliance partly depends on the resources available to them. As Mattera puts it: 'This measurement technique thus has the peculiarity that its efficacy is a function of IRS budgetary decisions' (1985, 52; see also Weigand, 1987). Moreover, it depends on the existence of a paper trail capable of being followed by an IRS investigator; 'income which is entirely off the books – unreported by both the payer and the recipient – will escape detection' (Mattera, 1985, 52).

In the eyes of some writers official statistics are irrevocably compromised by the existence of the dark figure. Those who take this view argue that official statistics reflect the organizational contingencies embedded in their production more clearly than they do the behaviours they are alleged to measure. Or, as Kitsuse and Cicourel put it, the rates presented in statistical tables 'can be viewed as indices of organizational processes rather than as indices of certain forms of behavior' (1963, 137). Other writers have suggested that this view is much too austere. While conceding that the use of official statistics may be problematic, they suggest that criticisms have been exaggerated and that the problems raised are in many cases tractable (for a defence of this view, see, e.g., Bulmer, 1980).

Since the debate is by now of sufficient duration and hardiness to have become a staple of undergraduate textbooks (see, e.g., Eglin, 1987), the arguments on either side will not be reprised here in any detail. Instead, the remainder of this chapter will focus on two broad areas. The first of these looks at attempts to side-step the problem of the dark figure through a variety of strategies: the use of traces, imputation from case-based data, and what are called here 'implicating methods'. The second area of discussion relates to statistical estimates of the size of hidden populations produced and disseminated, not by bureaucracies, but by social movements or by those wishing to advance moral, political or policy claims in contentious areas. The issue here, to paraphrase Kitsuse and Cicourel (1963, 137), is how far estimates of the dark figure may be taken as indices of political processes rather than as indices of behaviour.

**Traces: the measurement of inadvertence**

Webb et al. (1966; 1981) have suggested that researchers should add to their repertoire of methods a range of 'unobtrusive' or 'non-reactive' measures. As the term implies, the major feature of such measures is that they do not intrude into the research situation (Sechrest and Phillips, 1979). As a result, they help offset the tendency of research participants to change their behaviour or to give socially desirable responses because they know they are being studied. Webb et al. place great stress on the opportunistic exploitation of available data. They also celebrate the role of imagination and serendipity in the data collection process. For them, the use of unobtrusive measures often requires an imaginative leap by which the researcher comes to see mundane, unpromising or inconspicuous sources of information as appropriate data.

Webb et al. describe a wide range of possible measures including those based on various kinds of archival sources, covert observational methods, and the use of what they call 'traces'. Dabbs (1982) has distinguished between lasting traces and ephemeral traces. Lasting traces include physical traces produced by the erosion of the environment or by accretion to it (Webb et al., 1966, 36–46). Wear on floor tiles, for example, is an erosion measure which may denote the frequency of use of particular areas. The amount of litter left behind in a particular area is, on the other hand, an accretion measure. Ephemeral traces reflect 'much of the ordinary behavior of people and organizations, but unless someone
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The number of animals in a sample taken at time 1

and the number of animals in the same sample taken at time 2

are combined to give an estimate of the population size at time 1:

\[ P = \frac{N_1 + N_2}{2} \]

where

- \( N_1 \) is the number of animals marked in the first sample.
- \( N_2 \) is the number of animals marked in the second sample.
- \( P \) is the population size at time 1.

This method, known as the mark-recapture method, can be used to estimate the size of populations where it is impractical to count every individual. It is particularly useful in situations where the population is closed (i.e., no immigration or emigration occurs) and the marking process does not affect the animals' behavior or survival. The technique relies on the assumption that the proportion of marked animals in the second sample is representative of the proportion of marked animals in the population as a whole.

One potential limitation of this method is that it assumes that the population is closed and that the marking process does not affect the animals' behavior or survival. In addition, the method is most effective when the marking and recapture events are random and occur at different times.

In some cases, data from capture-recapture methods can be used to estimate the size of hidden populations. For example, in wildlife management, mark-recapture studies can be used to estimate the size of populations of endangered species or to monitor changes in population size over time. By tracking the movement and survival of marked animals, researchers can gain insights into the ecology and behavior of these species and develop strategies to conserve them.

Overall, the mark-recapture method provides a useful tool for estimating the size of hidden populations, but its effectiveness depends on several factors, including the assumptions of the method being met and the accuracy of the data collected.
Obviously, for ethical as well as operational reasons, one cannot proceed in this way with human populations. Hartnoll et al. (1985a, b) note, however, that samples of cases from any pair of sources can be substituted into the formula provided the samples are independent of each other and that they are comparable in terms of case definitions, time periods and the population from which the samples are drawn. For example, in their research, the clientele of two different agencies were used.

The difficulties with using capture–recapture methods in social research arise from the problems of ensuring that the assumptions underlying the method can be met. In agency studies it often cannot be guaranteed that the samples used are independent, while there may be problems of ensuring data are collected over the same period. In human populations the method relies on individuals being identified and therefore may produce problems of confidentiality. Particularly in studying an area like drug abuse, the assumption that the population is in a steady state over the study period may be untenable. Biologists have apparently extended the range of the original method to allow some of the underlying assumptions to be relaxed, in order to study more complex population dynamics (Blower et al., 1981). It is unlikely, however, that these more elaborate procedures could easily be applied to human populations.

Nomination techniques

In the study of drug abuse attempts have also been made to use nomination techniques to estimate the size and character of the user population not known to therapeutic agencies (Hartnoll et al., 1985a, b; Blumberg and Dronsfield, 1976). In these studies clinic attenders are interviewed by a fieldworker and asked to nominate in an anonymous fashion friends who are regular drug users and to indicate whether these friends have attended a clinic in the past year. Because of the possibility of the same nominated user being double-counted, one cannot simply sum the number of nominated users to estimate the number of non-clinic attenders. It is, however, possible to produce an estimate of the ratio of treated to untreated users.

The use of case-based methods can be valuable. Comparisons of the various estimates Hartnoll et al. (1985b) produced for opioid users who are not in the treatment population show them to be of a comparable order. In addition, as might be expected, these estimates were also considerably higher than official estimates based on the number of registered addicts. Estimates imputed from case-based data may indicate the kind of correction which may need to be applied to official estimates at the national level. It should be clear, however, that case-based methods are most reliable where they are used on relatively small areas. Their direct use at the national level would appear to be difficult.

Implicative methods

The term 'implicative methods' is used to refer to those methods for estimating the size of hidden populations based on the notion that measurement of one aspect of a population has implications for the level of another. Methods of this kind assume it is possible to produce estimates where one has (a) data relating to a range of variables and (b) a theory which accounts for the relationship between them. These conditions are much more likely to be met in areas like demography and economics, than in disciplines such as sociology and anthropology.

Multiplier methods

Multiplier methods depend on the assumption that the portion of a population which can be measured stands in a fixed ratio to that which cannot. For example, it seems that those having the surname Cohen form a relatively constant proportion of the Jewish population (Waterman and Kosmin, 1986). This constant can be derived empirically by noting the proportion of Cohens recorded in birth and death notices in Jewish newspapers. Waterman and Kosmin were able to estimate the size of the Jewish population in London by counting the number of Cohens found in telephone directories and, much more usefully, on the electoral roll and applying the relevant multiplier. Multiplier techniques have also been used in the study of drug abuse (Hartnoll et al., 1985a). It has been assumed that there is a direct relationship between the number of drug-related deaths and the prevalence of drug abuse. If the death rate for users is known or can be reliably estimated, the number of deaths in a given year multiplied by the annual mortality rate can be used to give an annual period prevalence. This particular method has a number of obvious shortcomings. There may be difficulties in reliably identifying user deaths from mortality data. It is plausible to suggest that rates of mortality are different for known addicts and for those who have hidden their drug use. Changes in the way drugs are used – for example, a shift from injection of the drug to smoking – can also affect mortality rates, as can variations in purity.

Discrepancy methods

In theory, estimates of the size of a population derived from different measures should coincide. Where they do not, the dis-
crepancy between two measures can be used as an estimate for the hidden portion of some population. [Brendan Walsh (1971).] for example, estimated the extent of religious intermarriage in the Republic of Ireland by assuming that such marriages accounted for discrepancies between rates of marriage performed under the auspices of various denominations and the proportion of members of those denominations recorded as ever-married in the Census. Dilnot and Morris (1981) used a discrepancy method to estimate the extent of tax evasion and social security abuse in Britain using data from the 1977 Family Expenditure Survey. They inspected (anonymized) data for households in the FES where expenditure exceeded reported income, but where the income-expenditure discrepancy could not be due to factors such as the rundown of existing wealth.

Discrepancy measures tend, like unobtrusive measures, to be inferentially weak. The difficulty is that one is always dealing with an unexplained residual (Frey and Pommerehne, 1982), that which is assumed to be left over when all other factors have been taken into account. As Frey and Pommerehne go on to point out, in cases where direct measurement is impossible use of the residual may very well be reasonable. [But in many cases its size is likely to be affected by a variety of factors, not all of them obvious or directly measurable. To be confident that the unexplained residual contains only what one is interested in requires both good data and a sound underlying theoretical model. Research on the size of the hidden economy is perhaps a good example of a field where both have been lacking. J.J. Thomas (1990) has criticized much of the work in the area for being based on 'measurement without theory'. Neither has the calibre of the data used been impressive, for, if sociologists can be accused of being too sceptical of official statistics (Bulmer, 1980), economists can be chided for caring little about the basic quality of their data (Morgenstern, 1963; Reuter, 1982, 182; Jacob, 1984; J.J. Thomas, 1988; 1990).

✓ Soft-modelling

Again in relation to the underground economy, writers like Frey and Weck (1983) and Button (1984) have adopted a rather more complex approach known as 'soft-modelling'. Soft-modelling is unconcerned with the absolute size of the underground economy but deals with comparisons between countries and regions. The argument soft modellers make is that the scale of activity in the underground economy is for all intents and purposes unknown. They suggest, however, that it remains reasonable to examine the relative importance of factors which may be assumed a priori to produce variations in levels of informal economic activity. Provided one can specify a range of variables likely to encourage participation in the hidden economy and making the entirely plausible assumption that the pressures towards participation are not spatially uniform, then a sensitivity analysis based around the relative weights in a regression model can be used to rank countries or regions. Frey and Weck (1983) have produced a ranking of 17 OECD countries in terms of the likely size of their hidden economy by examining the impact at national level of factors such as taxation rates, the degree of legal regulation, attitudes towards paying taxes, labour market variables and so on. Button (1984) has performed a somewhat similar exercise for regions in Great Britain. Soft-modelling tries to compare areas by looking at their propensity to produce economic activities outside the formal sphere (assuming, of course, that one can in fact delineate the formal from the informal [see Harding and Jenkins, 1989]). It can be argued in favour of the approach that this is often more useful to know than is a global figure, say for 'underground GNP'. On the other hand, there is no way to validate the patterns found. There are also difficulties in finding appropriate data, selecting the variables to be entered into the analysis, assessing their comparability and developing a cogent theoretical framework to account for their effects.

Advocacy estimates

Attempts to estimate prevalence often follow the emergence or recognition of a phenomenon which is regarded as being socially problematic or contentious. After all, as Best (1989) points out, in such circumstances numerical estimates help to fix the dimensions of a social problem. Academic researchers tend to assume that producing reliable estimates of prevalence in such circumstances is vitally important, especially in making policy decisions. The production of such estimates, however, may bring them into conflict with those taking advocacy positions around the issue concerned.

As Nathan Keyfitz has observed: 'Numbers provide the rhetoric of our age' (1987, 235). Advocacy groups presumably use numerical estimates because they are assumed to have persuasive power. Large estimates suggest a sizeable problem which demands urgent attention; they perform a consciousness-raising role (Rossi, 1987). Large numbers are newsworthy and therefore more likely to attract media attention. Although the media will sometimes question the size of estimates, Best (1989) points out that this is unusual. Usually their reliability will remain unquestioned provided the figures used can at least be attributed to an apparently knowledgeable source. Because those who bring a topic to public attention often appear to
be better-informed about it than everyone else, their views are taken as authoritative even where, Best argues, they are only based on guesswork or unreliable data. (For some examples of media distortions of prevalence estimates relating to tranquillizer dependence, see Gabe and Bury, 1988.)

The 'statistical conflicts of interest' (Biderman and Reiss, 1967) which sometimes surround differences between academic and advocacy estimates arise in a number of different ways. Rossi (1987), for example, found the size and composition of the homeless population in Chicago to be roughly 10 times smaller than estimates produced by housing activists. As he reports in an article sardonically entitled 'No good applied social research goes unpunished' (1987), the findings of the study were bitterly attacked by advocates for the homeless, and he found himself being repeatedly shunned at conferences on homelessness. Opponents argued that his description of the problem was too narrow, and that the study had damaged the cause of the homeless by potentially allowing the problem to be dismissed as trivial.

Social scientists may also undermine advocacy estimates in less direct ways. In recent years statistical exegeses aimed at professional, lay and advocacy audiences have begun to appear on the use, and more particularly the misuse, of statistics relating to a range of sensitive topics. (La Fontaine [1990] and Steve Taylor [1989], both writing about child sexual abuse, are good examples of this genre.) It is true, of course, that such writing can serve advocacy needs. Some groups have apparently assimilated sociological critiques of official statistics and used them in their campaigns to call for improved reporting of particular crimes (Best, 1989). However, by rehearsing the complexities of using statistics, whatever their provenance, the cumulative effect of such exegeses may be to undermine advocacy claims even if it is not the intention of the authors concerned to do so.

A third situation is where researchers set out explicitly to debunk advocacy claims, as does Best (1989) in his work on stranger abductions in the United States. According to Best:

it is apparent that the missing children crusaders' initial claims greatly exaggerated the extent of the problem. There is simply no evidence to support claims of 50,000 stranger abductions annually, of 40,000 permanently missing victims, 5000 abduction-related homicides, or 5000 unidentified juvenile corpses. (1989, 30)

Those who have drawn attention to the discrepancies between academic estimates and advocacy estimates tend implicitly to invoke a market model of advocacy. Advocacy groups work in a competitive environment in terms of public attention, media coverage and the availability of funding (on the media especially, see Nelson, 1984). A purely market model of advocacy may explain why advocacy groups propagate high estimates. It does not, however, explain how such estimates are arrived at, nor how their plausibility is maintained even when they are undermined by academic estimates. Unlike the routines by which statistics are produced in bureaucracies, the processes which lead to the production of advocacy estimates are generally not accessible to researchers. Indeed, it may be difficult even to establish the source of a particular estimate. According to Rossi (1987), for example, local housing activists in Chicago told him that their estimates of the number of homeless in the city came from a report produced by a Federal agency. The authors of the report, however, told him that these estimates had been obtained from local activists in Chicago.

One reason for large estimates may lie in the fact that newly recognized social problems are often triggered by traumatic events and situations. Paradoxically, the singularity of particular cases reinforces their apparent ubiquity. Nisbett and Ross (1980) suggest that, cognitively, information which is vivid and tangible is weighed much more heavily, and has much greater influence, than that which is pallid and abstract. Information is vivid where it has emotional interest, is concrete and image producing, and is temporally and spatially close (1980, 44-6). The social processes which bring novel and undesirable events and situations to widespread attention are likely to reinforce this sense of vividness. Best (1987) notes that examples of new problems which are used to grab public attention are often chosen for their horrific nature. These are then taken as basic referents around which the problem as a whole is subsequently defined. In particular, such examples may specifically trade on the invocation of negative emotions (see, e.g., J.M. Johnson, 1989), and may utilize images which are immediate and compelling (see, e.g., Nelson, 1984). Growing social concern about problematic groups, events or activities also often involves a process of what Cohen (1972) calls 'sensitization'. A perception develops that the novel social phenomenon causing concern is not an isolated instance but is in reality all around. This in turn can produce a self-fulfilling prophecy by encouraging investigative action which might not otherwise have taken place. Subsequently, as cases are uncovered the apparent rate of growth of the problem seems alarming because the numbers are starting from a small base. All of these factors do little to discourage the view that the problem is a substantial one. (To say this is not to assume that any particular problem is in fact insubstantial.)

How do the differences between academic and advocacy esti-
no longer need to be read by academics to bolster their cases.

Conflict between researchers and advocates.

Notes

The conflict between researchers and advocates is a major issue in social science, as it can lead to biased or incomplete research. The role of advocates in shaping public opinion is crucial, but it is important to ensure that the research they rely on is rigorous and unbiased. This can be achieved through a variety of methods, such as peer review and the use of independent researchers. It is also important for researchers to be transparent about their funding sources and potential conflicts of interest. By promoting a culture of critical thinking and collaboration, we can foster a more productive and ethical research environment.
Sampling Rare or Deviant Populations

This chapter focuses on sampling, in other words, the selection of people, places or activities (Spradley, 1980) suitable for study. In addition, indirect methods of gathering information where sampling or access prove impossible will also be discussed briefly. Put rather baldly, the aim of sampling is to select elements for study in a way which adequately represents a population of interest (though see Patton, 1987), both in relation to the purpose of the research, and at reasonable cost. In many situations, there are well-developed strategies for realizing these twin aims of representativeness and cost-effectiveness. Neither, however, may be easy to obtain where the topic under investigation is a sensitive one. First, other things being equal, sampling becomes more difficult the more sensitive the topic under investigation, since potential informants will have more incentive to conceal their activities. Second, and related to this, the less visible an activity is the harder it is to sample. Deviant worlds are often melded to or shielded from conventional worlds (Rock, 1973, 79–83) in ways which may induce failures even to recognize as deviant those capable of being so defined. Third, obtaining sample elements which are rare is more difficult, in the sense of being more costly, than is obtaining them for some behaviour which occurs frequently. Finally, while obtaining respondents over a widely dispersed geographical area is likely to improve representativeness, dispersal also drives up cost.

Systematic treatments of sampling issues related to deviant populations can be found in Becker (1970) and for rare populations in Sudman and Kalton (1986) and Kish (1965). Interestingly, although the emphases are quite different the range of strategies outlined by Becker are rather similar to those described by Sudman and Kalton, and by Kish. Becker's article, in particular, celebrates the ingenuity of researchers in finding 'pathways to data', and represents an accumulation of lore and practice concerning access to deviant groups (see also Spradley, 1980, 46–52). It is important to stress, however, that sampling is always a theoretical matter before it is a technical one. In other words, as well as substantive knowledge and technical judgement, the selection of a research site or the specification of a sampling procedure requires reflection on the theoretical purposes to which the data collected by means of the sample will be put (R.G. Burgess, 1984; Biernacki and Waldorf, 1981; Galtung, 1967). In the case of sensitive topics, however, adequate reflection of this kind is not always possible. Available knowledge can be limited, while levels of sensitivity, visibility, frequency and dispersal are often the product of complex patterns of social organization (Best and Luckenbill, 1980) which may not be obvious to the researcher before the research has begun.

A further aspect of this is that in some situations sampling can also become a political issue. As Gillespie and Leffler (1987) point out, the scientific legitimacy of an emerging social problem—the example they use is sexual harassment—often depends on the methods used to study it. Yet, it is precisely at the point where a social problem is not yet widely acknowledged that researchers will usually lack the means to generate statistically adequate samples. A reliance on convenience or other kinds of non-probability samples often means that the issue is not taken seriously. However, until the issue is taken seriously, it may not be possible to fund detailed and comprehensive studies.

Sampling strategies

The major strategies which can be used, singly or in combination, for sampling 'special' populations which are rare and/or deviant in some way are as follows:

1. List sampling.
3. Screening.
5. Outcropping.
6. Advertising.
7. Servicing.

List sampling

Rarely available for deviant populations, lists have on occasion been used to sample rare populations. Thornes and Collard (1979), for example, were able to obtain a sample of the recently divorced using local newspapers which, in some parts of Britain, make a practice of listing petitioners for divorce after every sitting of the local divorce court. Unfortunately, this kind of fortuitous availability is in itself likely to be a fairly rare occurrence. In many instances the researcher may not even know that a suitable list exists. Furthermore, even where the existence of lists is known, access to them may remain closed. Thus, while Lopata (1980) was
able to have a sample of widows drawn from lists of social security beneficiaries kept by the US Social Security Administration, her access to these lists was facilitated by her being under contract to the Administration. By contrast, in a similar study in Britain, Marris (1958) was unable to generate a sample of widows using death registration records held by local registrars. At the same time, Marris’s research nicely illustrates the point that a sampling frame not available from one source may be available from another. He subsequently found that information from the registration records was passed by the registrars to local Public Health Authorities who did permit him access.

In a few instances, members of some group can be detected from lists not specifically designed to identify them. Himmelfarb (1974) needed a sample of Jews in order to assess the effects of differing levels of religious schooling on adult religious practice. Although Jewish charities keep detailed lists of donors, these lists could not be used for the research envisaged because of their bias towards the more committed, who were likely to have had high levels of religious education. The donor records did, however, yield a further list of distinctively Jewish names, which Himmelfarb then used to sample from local telephone directories. The sample obtained in this way was not representative, but did allow the effect of differing levels of religious schooling to be assessed. Subsequently, Himmelfarb et al. (1983) have shown that Jews with distinctive Jewish surnames differ little in their demographic characteristics from the Jewish population as a whole. (For a further use of distinctive name sampling, see Dench’s [1975] survey of Maltese immigrants living in London who were selected by searching electoral registers for distinctive Maltese names.)

Name sampling, of course, is only useful where the group concerned can be unambiguously identified by name. Those who change their names in order to ‘pass’, out-marrying women who take their husband’s name on marriage, and some of the offspring of intermarriage in the previous generation will not be detected. Benson (1981) tried to sample Anglo-African marriages in a racially mixed area of London by using the distinctive names approach in a search of local electoral registers and registrations of intention to marry. These searches, however, yielded few traceable interracial couples. In addition, the couples who were identified by this procedure were somewhat suspicious and disliked the fact that their names had been obtained from official records. Conversely, and perhaps unexpectedly, this kind of irritation was not voiced in Marris’s case. The widows he studied accepted without comment that they had been selected on the basis of death registration information. Perhaps widows, who, as Lopata found, are often in fact anxious to talk about their bereavement, might be less concerned about the origins of the sample than is the case for interracial couples, who may fear racial harassment.

Besides the irritation their use may cause to members of vulnerable groups, lists are subject to the usual difficulties involved in using sampling frames: the inclusion on the list of blank or ineligible elements, duplicates and the systematic omission of some segment of the population (Sudman, 1983). Even where these problems exist, lists may still be useful in providing the seed for a snowball sample, or for checking the validity of data collected by some other methods (see, e.g., Tracy and Fox, 1981; Sudman and Bradburn, 1982; and, for a critique of this procedure, Miller and Groves, 1985). In addition, Sudman and Kalton (1986) point out that where a list is incomplete it may still be advisable to use it if the proportion of the population not on the list is small and the cost of screening for non-listed individuals is high. Such a decision can only be made, though, on an empirical basis depending on the size of the sample, the accuracy desired and the relative costs involved.

\textbf{Multi-purposing}

An existing survey can sometimes be used as a vehicle for reaching the population of interest. Access to a special population can be achieved either by ‘piggybacking’ the sampling of the special population on to an existing survey, or by using an ‘amalgam’ approach in which a survey is employed simultaneously to locate a number of different rare populations (Tourangeau and Smith, 1985). Alan Walker’s (1982) study of the problems of handicapped young people in the labour market used a sample of 18-year-olds whose handicap had already been identified on an earlier wave of the longitudinal National Child Development Survey. In a similar way, a longitudinal survey of redundant steelworkers in South Wales provided Leaver (Harris et al., 1987) with a sample of previously financially secure families who were now facing financial hardship, and Bytheway (1986) with a sample of older workers whose labour market future was uncertain because of their age.

As Catherine Hakim points out

Hitching a ride on an existing multipurpose survey carries enormous advantages in terms of savings in costs . . . and in obtaining access to a wide range of complementary data on other topics. (1985, 14)

The disadvantages of multi-purposing include the problems of confidentiality which may arise if respondents’ names are to be made available to another researcher, and the organizational difficulties of running different but interconnected projects in
tandem (Tourganeau and Smith, 1985). In addition, the main
survey may not actually be an entirely ideal vehicle for investigating
the topic of interest on the subsidiary study (Hakim, 1985). Multipurposing seems most feasible as a means of reaching a special
dependent population where some kind of organizational context already exists
in the form of a longitudinal, or a recurrent, survey. Such contexts, however, produce their own problems for, as Hakim notes, those
who run recurrent surveys are often under tremendous pressure to
piggyback special projects.

√ Screening

Screening or ‘hill-sampling’ involves the systematic canvass of a
particular location in order to identify members of some requisite
population. As a method, it is labour-intensive, requires fairly
sizeable resources and may only be operable over a relatively
limited area. It may also produce only a limited ‘strike rate’. In a
study of women homeworkers, Hope et al. (1976) canvassed 216
houses in two streets in North London. Even within this limited geographical area fieldwork took two-and-a-half months for a yield of 11 interviews, 10 refusals, and information on a suspected further
11 homeworkers. To take another example, Susan McRae (1986)
wanted to generate a sample of households in which the occupation
status of the wife was higher than that of the husband, for
an intensive interview study of couples in cross-class marriages. She
distributed, in all, 2,155 screening questionnaires to women in non-
manual occupations employed mainly in health, social services and
education in a number of locales. Some 603 questionnaires were
returned which yielded 24 couples suitable for interview. In both
these cases, the resulting sample was appropriate to the exploratory
aims of the study. Nevertheless, the effort required to screen the
relevant population was considerable, and where, as in the study by
Hope et al., screening forces reliance on an extremely limited
geographical area, it is hard to suppress doubts about how far the
results obtained can be generalized.

In an effort to reduce the costs and time involved in screening, a
number of studies have made use of ‘batch testing’ strategies. Batch
testing appears to have been developed in the United States from
the application to area sampling of methods originally suggested by
Waksberg (1978) for improving the efficiency of telephone sampling
(Tourganeau and Smith, 1985), and independently—under the
name of focused enumeration—in the United Kingdom (Brown and
Ritchie, 1981). The essence of the method is that, having selected a
sample of geographical clusters or segments (often on the basis of a
known concentration of members of a rare population), full screening
of the cluster only takes place if a member of the rare population is
found at an initial randomly selected dwelling unit. For example,
in a study of the attitudes of Black Londoners towards the police
(D.J. Smith, 1983), a sample of census enumeration districts
(typically containing around 150 households) was selected, but with
enumeration districts having very low concentrations of Asians or
West Indians being excluded. Where Asian and West Indian
residents comprised more than 10 per cent of the population of the
enumeration district, screening was carried out by enumerating the
members of every household. With concentrations under 10 per
cent, however, focused enumeration was used. Here, an interviewer
called only at every fifth dwelling unit and asked whether any Asian
or West Indian residents were to be found in the five dwelling units
to the right and left of the selected dwelling unit. If the household
chosen contained an Asian or West Indian resident, or if any
mention was made at the first household of Asian or West Indian
residents in any of the five households to right or left, then all the
intervening households were screened.

Methods of this kind do reduce considerably the costs of screen-
ing for rare populations. They work best, however, where the
population of interest is either geographically concentrated and/or
visible. Focused enumeration might, for example, be less successful
where one was attempting to sample, say, a religious minority
rather than a racial one.

 Networking

In network sampling the researcher starts from an initial set of
contacts and is then passed on by them to others, who in turn refer
others and so on. Because of the way in which the sample is
presumed to grow, network sampling is also often described as
’snowball sampling’. Traditionally, snowball sampling found little
favour in survey research. More recently, however, in the guise of
’multiplicity sampling’, it has been recognized as having consider-
able potential for the sampling of rare populations (Sudman and
Kalton, 1986). In this variant of network sampling, snowballing is
used to increase the yield from screening interviews by asking
respondents to supply information about someone related to them
but not residing in their household. One example of the use of this
technique comes from a survey of Vietnam war veterans by
Rothbart et al. (1982). In this study a stratified sample of 8,698
households was screened for a yield of 535 eligible veterans. Survey
respondents were also asked, however, to supply information on kin
who had been veterans of the war in Vietnam, a procedure which
yielded an additional 476 veterans. Rothbart et al. report few
difficulties in locating nominated veterans, with most of the work of
locating being done by telephone. Cost savings using this method
were substantial, although there is some danger of sample bias in
cases where kin vary systematically in their ability to nominate
potential respondents.

Snowball sampling is ubiquitous in the study of deviant popu-
lations because it often represents the only way of gathering a
sample. However, the intuitively appealing notion of the 'snowball'
can be taken too much at face value. As a result few attempts have
been made critically to evaluate snowball sampling as a strategy
(Biernacki and Waldorf, 1981). Because of its ubiquity and relative
lack of critical scrutiny snowball sampling, therefore, deserves some
extended discussion. Despite its name, snowball sampling does not
invariably lead to an inexorably growing mass of contacts. Rather,
as a number of researchers have found (Davis, 1982; R.M. Lee,
1981; 1992), what it often produces is a slow and uneven accretion
of additional data points. As originally conceived, snowball sam-
pling involved the attempt, as Coleman (1958) puts it, 'to sample
explicitly with reference to the social structure'. The starting point
for a snowball sample is therefore most properly an initial random
sample from which further sample elements are selected for in-
clusion on the basis of a predefined sociometric relation to those
already chosen. Where, as in the search for members of a rare
population, the interest is in attributes rather than relations, one
may gain some advantage in not having to redefine the sociometric
criteria for inclusion in the sample. A pair of contacts in a chain
need be linked by no more than knowledge of the existence of one
another. Even so, in most cases — although this is rarely made clear
— one is still sampling with reference to the social structure, though
this time in an implicit manner.

This can be put another way. The inclusion of additional sample
elements depends on the social ties between members of the study
population, as well as on the links which exist between them and the
wider population. Other things being equal, movement through
informal networks will be easier in a numerically small group with
well-developed patterns of social organization. That Goldstein
(1984) was able to generate what was an apparently complete list of
doctors who performed abortions in the Los Angeles area before
legalization was probably due to the relatively small number of
doctors involved and the likely prior existence of an already
organized professional referral network. In the case of religiously
intermarried couples in Northern Ireland (R.M. Lee, 1981), snow-
ball sampling proved difficult.4 First of all, couples have little basis
for interacting with one another. Second, in urban areas at least,
even knowledge about intermarried couples may not be well
distributed among the population at large in Northern Ireland.
Unlike interracial couples, say, the religiously intermarried are not
immediately visible, and, in Northern Ireland, they develop strat-
egies for concealing the 'mixed' nature of their relationship from
those around them (R.M. Lee, 1981). (On what might be a slightly
different situation in rural Northern Ireland, see McFarlane, 1979.)
In such situations sampling takes on an 'excursionary' character
with progress being made from an initial starting point before
contact depletion makes it necessary to return and begin the search
for contacts all over again. Where this happens the snowballing
metaphor can be seen not to be entirely apt, as the sampling process
proceeds in a slow and halting way.

Snowball sampling does have advantages in cases where those
being studied are members of a vulnerable or highly stigmatized
group. 'Security' features are built into the method because the
intermediaries who form the links of the referral chain are known to
potential respondents and trusted by them. They are thus able to
vouch for the researcher's bona fides. However, intermediaries can
produce difficulties if they give a misleading account of the project
and its aims. This can lead to a diminution in the flow of contacts,
or, as Biernacki and Waldorf (1981) point out, to the researcher
being inundated by large numbers of ineligible respondents. The
eligibility criteria in Biernacki and Waldorf's study were in fact
quite stringent, which may have made the problem particularly
acute for them, but most researchers who use snowball sampling
will need at some stage to build in procedures for verifying the
eligibility of respondents. Since direct contact is not made with
perspective informants it can become difficult to keep an accurate
tally of refusals. Refusals may be reported when in fact the
intermediary has not actually contacted a prospective respondent,
or an intermediary may try to avoid the embarrassment of reporting
a refusal by claiming not to have been able to make a suitable

Bias is an almost inevitable feature of snowball samples because
the social relations which underpin the sampling procedure tend
towards reciprocity and transitivity (Davies, 1986; Rapport and
Horvath, 1961). In other words, if x and y are two points on the
sample network and x is related to y in some way, then too y will
be related to x. In addition, it is also probable that the friends of y
will be the friends of x. As a result, networks tend to turn in upon
themselves and to be homogeneous in their attributes, rather than
providing linkages to others whose social characteristics are difer-
Now, focusing on the study of social and demographic data on populations, it is clear that the collection
of such data is crucial. The large and varied amount of information obtained from
these studies has led to the development of various methods for analyzing
data. This has, in turn, enhanced our ability to tailor interventions to specific ethnic
populations, offering a pathway to more personalized healthcare. However, several
challenges remain, including the need for more rigorous data collection methods
and the integration of cultural sensitivity into research practices. Overall, the
potential benefits of this approach are significant, and continued efforts are
needed to address the remaining gaps in our understanding of how these factors
influence health outcomes.
Sampling Rate of Defined Populations

Sampling errors can result in problems such as non-representative samples, which can lead to misinterpretations of results. The sampling rate of a defined population is critical in ensuring that the sample is representative of the whole population. If the sampling rate is too low, the sample may not accurately reflect the characteristics of the population. Conversely, if the sampling rate is too high, it may be unnecessary and costly.

The accuracy of the results is directly proportional to the sampling rate. Therefore, it is essential to determine the appropriate sampling rate for a given population. This can be achieved by conducting a pilot study or by using statistical methods to calculate the required sample size.

In conclusion, the sampling rate of defined populations is a critical factor in ensuring the accuracy and reliability of research findings. It is essential to carefully consider the sampling rate when designing a study to ensure that the results are representative and meaningful.
management difficulties related to screening and perhaps difficulties in turning away non-eligible but willing study participants. (1981, 149)

The problem of obtaining respondents who might be unsuitable in various ways is graphically illustrated by Henslin’s (1972) study of illegal abortion. Henslin had broadcast the fact, by word of mouth, that he would like to be put in contact with an abortionist. Subsequently, he was put in touch with an abortionist who, allegedly, had shot to death a former client and her boyfriend in a dispute over fees. As Henslin put it, ‘In spite of the commitments I feel to research, I passed up this “opportunity”.

Servicing

It is sometimes possible to obtain research subjects by offering them a service of some kind. Thus, Fineman (1983) used his participation as a counsellor on a government-sponsored career review programme in order to study white-collar unemployment. Offering a service as a means of gaining a sample may have particular utility when one is studying members of a deviant group. As Becker (1970) points out, those who are deviant in some way often prefer to utilize non-conventional legal, medical or therapeutic services. In this way, Becker notes, Bryan (1965), who studied call-girls, was able to obtain some respondents by offering them psychotherapy. One difficulty with this kind of approach is that it becomes difficult when evaluating the research to judge how far the processes of counselling and data collection may or may not have had an effect on one another.

A further, obvious, limitation of this strategy is that it depends on the ability of the researcher to offer an appropriate service. Sometimes, however, researchers who do not set out to provide a service come to do so because they are approached by people who know of their interest in a particular topic, and who assume that they can offer advice. Eileen Barker (1984) found when carrying out a study of the Unification Church (the Moonies) that she was not infrequently approached by parents alarmed about a child who had joined the Church. Not only was Barker able to give advice to these people, but also her interviews gave her additional and useful data about recruits’ home backgrounds, and about the ways in which the Moonies were perceived by those outside the movement. John Lee (1979), a sociologist also widely known as a spokesman for the Gay Liberation Movement in Canada, similarly records that one source of data he has collected on the development of a homosexual identity was men who were in the process of ‘coming out’, and who approached him for advice.

Professional informants

Becker (1970) has pointed out that, as well as or instead of studying deviant groups directly, the researcher can and should utilize whenever possible a variety of indirect data sources. These may include a variety of archival and running records (Webb et al., 1966; 1981). Thus, agency files, transcripts of legislative hearings, the results of medical and legal research, popular literature and the self-justifying literature deviant groups often produce about themselves may all be mined for the data they contain. In addition, Becker suggests, it is also often possible to gather information from those, such as police, social workers, probation officers and the like, whose work brings them into contact with the group in question. What lies behind Becker’s suggestion are two observations. The first is that those whose work brings them into contact with deviants are ‘wise’ in Goffman’s (1963) sense of the term. As a result they have opportunities not afforded to others for routine interaction with and observation of deviants by virtue of the servicing or control functions they are presumed to perform. Second, as a result of this privileged access, they are also assumed to have available to them what Sudnow (1965) refers to as ‘prototypical portrayals and knowledge of operative social structures’; a fund of typifications, in other words, concerning the social characteristics and activities of the group of interest.

There are a series of constraints which operate to diminish the willingness or the ability of professionals to provide suitable information. First of all, professionals rarely see a full spectrum of types. Drug users encountered by the police, for instance, are likely to be quite different from those seen by medical practitioners, who in turn might be different from users in contact with paramedics or social workers (Hartnoll et al., 1985a). Second, some professionals may have only fleeting contact with the researcher’s target group. For example, priests and ministers in Northern Ireland have relatively little knowledge of or familiarity with religiously intermarried couples (R.M. Lee, 1981), partly because rates of Catholic-Protestant intermarriage are low and such couples are, in fact, rarely seen. In addition, where contact with couples does take place, it is within the relatively transitory and formal context of arranging for the marriage ceremony to take place, and, here, for Roman Catholic clergy at least, bureaucratic pressures exist to define the situation in an extremely routinized way (R.M. Lee, 1981; see also Fulton, 1975). Third, in some cases, professionals may be inhibited from sharing information with those outside the profession. Thus Hartnoll et al. (1985a) note that police drug squad
officers are often unwilling to impart information which has a bearing on operational matters. Moreover, where there is mutual suspicion between two professional groups – police and social workers, say – neither group may be prepared to provide information for fear of its being passed on to the other. Fourth, some professionals, for example, lawyers, are constrained within very strict rules of confidentiality, while others may hold professional ideologies which inhibit the kind of generalization of most use to researchers. This was true, for instance, of marriage guidance counsellors approached for background information about religiously intermarried couples in Northern Ireland (R.M. Lee, 1981). Counsellors were able to say little, not only because of their very strict rules concerning client confidentiality, but also because it was taken by them as an article of faith that each individual case was unique, and did not, therefore, provide any suitable basis for wider generalization.

Notes
1. Some portions of this chapter have appeared in a slightly different form in R.M. Lee (1992).
2. Sampling in field research should be seen, strictly speaking, as an ongoing feature of the research process in which the further selection of sample elements is guided by theoretical understandings, reflections and judgements which emerge over the course of the research (Glaser and Strauss, 1967; Denzin, 1970). In practice sampling and access are inextricably intertwined in field research. Access is treated separately here for two reasons. First, where sensitive topics are involved, it is often the initial selection of individuals or settings which is problematic. In this instance, sampling is logistically prior since if one cannot even locate cases for study access remains an impossibility. Second, whereas in survey research extensive discussion of sampling was matched only late in the day by a concern for 'access' in the shape of informed consent (E. Singer, 1978), the business of selecting respondents or informants has often been taken more or less for granted in the field research tradition, relatively little discussed against a recurrent preoccupation with problems of access.
3. One difficulty with Gillespie and Leffler's position is that they tend to associate 'scientific conservatism' in favour of rigorous sampling and measurement with political conservatism. It can be argued, however, that this association is a contingent rather than a necessary one. For example, inflated estimates of the extent of the underground economy in Britain, based largely on anecdotal evidence and often promoted by a right-wing press, have been used to justify punitive treatment of welfare recipients (Golding and Middleton, 1982). The substantial methodological problems involved in measuring the underground economy mean that such assertions cannot easily be refuted unless methodological development takes place to produce more reliable estimates (see, e.g., J.J. Thomas, 1990).
4. The methodology of this study is discussed in more detail in R.M. Lee (1992).

5
Asking Sensitive Questions on Surveys

Survey researchers have put a considerable amount of effort and ingenuity into finding ways of asking sensitive questions. To do this, they have used the survey itself as a vehicle for methodological experimentation into its own procedures. This chapter draws on that tradition of methodological experiment to look at a range of topics, including question design, the use of vignette methods and a variety of statistically based 'dejeopardizing' approaches for asking sensitive questions.

Designing questions for sensitive topics

The imagery lying behind methodological writing on the asking of sensitive questions on surveys is implicitly Goffmanesque. It is assumed that respondents wish to manage impressions of themselves in order to maintain their standing in the eyes of the interviewer. For the researcher to cope with this situation, methods have to be found which allow the respondent to provide potentially discreditable information without disrupting the interaction or causing embarrassment or loss of face to the participants. It has long been recognized, for instance, that 'loading' questions in various ways can encourage greater reporting of behaviours which might not otherwise be admitted. Early versions of this technique were pioneered by Alfred Kinsey and his colleagues in their studies of sexual behaviour (Kinsey et al., 1948; 1953; see also Maddge, 1963, chap. 10; A.J. Barton, 1958). One strategy is for the questioner to presume that the activity of interest has actually taken place. Questions are therefore asked about its frequency rather than about whether it has occurred at all. Other variants depend on communicating through the wording of the question that the behaviour referred to is common or not especially untoward. Thus, a question about abusive behaviour might be prefaced, for example, with a phrase such as 'Everybody loses their temper now and again . . .'. Alternatively, a casual approach can be used, employing the phrase 'Do you happen to . . . in the question to diminish the apparent importance of the topic. Sudman and Bradburn (1982, 76) point out that, for what they regard as moderately threatening topics at least,
this last technique may not actually be very effective. Comparing survey responses to a range of questions about gun ownership, they conclude that using the phrase ‘Do you happen to . . .’ in a question does not increase levels of reporting — indeed it does rather the opposite. Instead of stressing the ubiquity or supposed innocuousness of a particular behaviour, a further way of loading the question is to use an authoritative source to justify it. The rationale here is that respondents will respond more positively to statements which employ endorsement by a group with high status or particular expertise. An example Sudman and Bradburn give is: ‘Many doctors now think that drinking wine reduces heart attacks and improves digestion. Have you drunk any wine in the last year?’ (1982, 76).

As Sudman and Bradburn point out, particular techniques for loading questions usually emerge out of survey practice rather than from methodological research. Together with a number of colleagues they have attempted to develop a more systematic framework for asking about sensitive topics on surveys (Sudman and Bradburn, 1974; Bradburn and Sudman, 1979; Sudman and Bradburn, 1982; Bradburn, 1983). Drawing on their own large-scale methodological experiments, as well as a systematic review of a large number of previous studies, they have examined ways of asking about a range of sensitive behaviours. (These extend from gambling through drinking behaviour and drug use to questions about the frequency of a range of sexual behaviours.) As a result of this work, they have made a number of suggestions and recommendations for reducing the under-reporting of behaviours normally regarded as threatening or sensitive.

Other things being equal, Sudman and Bradburn suggest following an number of rules for encouraging frank reporting by respondents. In summary, open questions should be preferred to closed, and long questions to shorter ones. Respondents should be allowed to describe activities in words which are familiar to them. Questions about activities to which respondents find difficulty admitting should be located within appropriate time frames or embedded in various ways within the wider context of the questionnaire. (Needless to say, Sudman and Bradburn advocate a judicious following of these rules rather than a slavish adherence to them.)

✓ Open questions

Closed questions on surveys offer advantages in terms of reliability and ease of processing. Despite this, Sudman and Bradburn (1982) give a number of reasons for using open questions when asking about threatening topics, particularly if one is interested in the frequency of behaviour. First of all, pre-coded responses must be organized into a logical ordering with the highest and lowest frequencies at each end of the list. Since respondents tend to avoid the extreme response categories on a pre-coded list, those who indulge heavily in an activity may under-report. Alternatively, pre-coded lists may encourage under-reporting because the response categories have to be closed somewhere, and in some cases this may miss respondents with very extreme responses. Coxon (1986; 1988) has also pointed out that pre-coded responses for frequency behaviour often have an implicit logarithmic distribution which encourages under-reporting. Speaking of question forms often used in AIDS research to provide estimates of the number of sexual partners an individual may have had, he notes that:

Significantly these categories are rarely equal interval, but instead follow an implicit power relation: small intervals at the least frequent working up to large ones at the most frequent end — thus producing as an artefact the evidence of ‘promiscuity’ so often uncritically quoted in the medical and other press. (Coxon, 1986, 26–7)

✓ Long questions

Sudman and Bradburn argue that there is some benefit to using long rather than short questions, particularly when one is dealing with a topic which is threatening. The rationale, in general terms, for using long questions may not be obvious. Conventional wisdom has it that questions on a survey questionnaire should be kept short (Payne, 1951; Hoinville and Jowell, 1978; Sheatsley, 1983). Sudman and Bradburn (1982, 50–1) accept that short questions are better when asking about attitudes, but argue that their research and that of others suggests that long questions should be preferred when one is asking questions about behaviour. They give three reasons why this should be so. Longer questions can be used to provide the respondent with memory cues. In other words, by giving examples in the question one can stimulate recall. Longer questions also take more time for the interviewer to read out. As a result, the respondent is given more time to think, and the longer one has to think, in general, the more one will recall. Finally, they argue that there is a tendency for the length of a reply to be related to the length of the question which elicited it. In taking longer to answer, respondents may cue themselves into remembering additional information.

To the extent that respondents may themselves suppress recollection of undesirable behaviour, aiding recall may be important when asking sensitive questions. Sudman and Bradburn found (1982, 73) that long questions reduced under-reporting of the frequency
of behaviour reporting in response to questions on threatening topics. Question length, however, had no impact on reports of whether respondents had ever engaged in the behaviour. It is not clear from their discussion whether length alone encourages the reporting of 'sensitive' topics. In other words, do respondents find it difficult to recall such behaviour, and thus need to be given more time to do so? Or, is it the case that longer questions help to desensitize the topic by stressing, to use one of Sudman and Bradburn's examples, that a particular topic is one about which people are in fact prepared to talk freely? It should also be recognized that continued use of long questions may induce fatigue in the respondent (Tanur, 1983-4, 20).

Using familiar words

In asking questions about behaviour the admission of which was threatening to the respondent, Bradburn and Sudman (1979) experimented with the use of 'familiar words' in questions. A familiar word in this context is one which is supplied by the respondent. The argument is that such words make the respondent feel more relaxed and aid in the understanding of the question. For instance, Bradburn and Sudman asked a series of questions about drinking behaviour. Before beginning the series, the interviewer described intoxication as 'Sometimes people drink a little too much beer, wine or whiskey so that they act different [sic!] from usual' (1979, 18). The respondent was then asked 'What word do you think we should use to describe people when they get that way, so that we will know what we mean and feel comfortable talking about it?' The word chosen by the respondent was then substituted in the questions that followed. In cases where the respondent could not supply a word, or supplied an inappropriate word (Bradburn and Sudman do not give examples of such words or how interviewers dealt with them), a fallback word was used. The use of familiar words did not enhance levels of reporting on sensitive topics to a statistically significant degree. Used with long, open-ended question forms, familiar words did, however, produce a consistent pattern of improvement in levels of report. Bradburn and Sudman suggest, therefore, that it may often be worth using the familiar words strategy with long, open questions where other factors, such as the overall length of the questionnaire, do not militate against its use.

Embedding the question

As Bradburn and Sudman point out, the overall context of a questionnaire has an effect on the extent to which particular questions are likely to be perceived as sensitive. Questions about drinking behaviour, to take their example, will be less threatening on a survey of consumer habits than on one devoted to alcoholism. More specifically, the questions which surround those asking about sensitive behaviour can also have an effect. There are a number of ways of 'creeping up' on the respondent, as Cisin and Miller (1981) put it. It may be easier to admit to past indiscretions than to current ones. So, respondents may be asked whether they have 'ever, even once', engaged in some activity. If they respond affirmatively, they are then asked whether they have engaged in that activity during some more recent period. Threatening questions on one topic can also be desensitized by embedding them within the questionnaire in a variety of ways. One can, for instance, lead up to a sensitive topic gradually through a series of less threatening questions. Though a rather wasteful strategy, it may be appropriate in some contexts to embed a threatening question on one topic within a series of questions on other, relatively more threatening topics. In this case, the more sensitive questions are 'throwaway' items. Their purpose is to reduce the threat of the later question by comparison with the earlier ones.

Vignettes

The use of vignettes is a survey-based technique which has some affinities with experiments, on the one hand, and simulations, on the other. Vignettes, according to Alexander and Becker, are 'short descriptions of a person or a social situation which contain precise references to what are thought to be the most important factors in the decision-making or judgement making processes of respondents' (1978, 94). Respondents can be asked to respond to these descriptions in various ways. The advantage behind their use is that, unlike attitude statements on surveys, vignettes present the respondent with concrete and detailed situations. It becomes possible, therefore, to discuss norms and beliefs in a situated way which accepts the complexities normally surrounding them. Moreover, the various elements which go to make up the vignette can be systematically varied across respondents in a factorial design. This allows one to implement within a survey something like a completely crossed experimental design (Rossi and Anderson, 1982; Alves and Rossi, 1978; Jasso and Rossi, 1977).

One example of a vignette is the following:

Mr. Miller is a salesman who works for you. He comes into your office one morning to tell you he has been drinking on the job. Miller is white, about 22, has been working for you for three months, and shows an average performance record. (Alexander and Becker, 1978, 94)
During the course of the game participants received messages on the computer which they had to process in order to correctly respond. The messages were presented in a text format, and participants were required to type their responses into a computer program.

(1) (continued) 

(2) It seems, however, that it is difficult to continue adding stages to the model. The model which can be explored with respondents is an approach which can be used to build an understanding of the situation. The model, however, is not always easy to apply to situations in which participants are not familiar with the situation. The model is, however, easy to apply to situations in which participants are familiar with the situation.

(3) Although computers have been used to develop factorial designs, they can also provide a means to evaluate the effectiveness of different designs.

(4) In the study of (1987), British studies were used in a somewhat different manner. Interestingly, the results of this study were not consistent with those of the previous study. However, the results of the study do provide some insights into the factors which influence the effectiveness of different designs.
varying degrees of seriousness relating to volcanic activity. Families were asked to make decisions about the possible relocation of the business in response to the messages they received.

The families' deliberations and the decisions they made were recorded and analysed. Decisions in response to the simulated danger compare well with data on evacuation behaviour in response to the actual eruption, and to natural disasters in general. Simulation, therefore, provides, according to Ekker et al., a 'valuable, as well as relatively inexpensive, methodological tool for the study of human behavior under highly threatening circumstances' (1988, 104). They also point to two other advantages of the method they used. First, discussions held by participants yielded a considerable amount of information about family dynamics, some of which might have therapeutic value. Second, the simulation had implications for the development of public policy on disasters. Ekker et al. note that disaster policy is usually based around notions of evacuation. The simulation, as well as other studies, underlines, however, that people are generally reluctant to take protective action in the face of a possible disaster.

CPSS appears to be a novel, useful and important development. For the moment, though, there are technical limits on the diffusion of the technique. To be used more widely, CPSS needs to be adapted for use on portable computers which can be brought to respondents, rather than requiring respondents to be brought to the computer. There is also, presumably, a need to develop authoring languages to make the construction of CPSS programs easier for non-computer specialists. However, there seems no reason why the method could not be adapted to cover situations which are socially rather than physically threatening. Methodologically, CPSS is akin to the use of vignettes. With its 'as if' character and the absence of an interviewer, it also has some of the features of the kinds of dejeopardizing techniques described in the next section of this chapter. Presumably, too, it would be possible to enhance the protection given to participants by programming the computer in some way to anonymize results. As in the vignette approach developed by Finch, the method allows data to be collected on 'complex, mutually contingent sequential actions' (McFarlane, 1971, 155). It also benefits from the immediacy and flexibility typical of methods based around role-playing and simulation.

Dejeopardizing techniques

A number of specialized techniques have been developed to help researchers ask questions about sensitive matters. Their purpose is to minimize the respondent's feelings of jeopardy when asked to admit to behaviour which is stigmatizing or incriminating in some way. Specifically addressed here are a number of such 'dejeopardizing techniques': the randomized response method, the nominative technique, and the use of microaggregation.

√ Randomized response

In its original form, as proposed by Warner (1965), this technique involves presenting the respondent with two statements; for example, 'I am an “A”' (where A is a sensitive attribute) and its converse, 'I am not an “A”'. The respondent is then asked to choose one or other question to be answered using some kind of randomizing device such as a die, and to respond to the question indicated by making a truthful 'Yes' or 'No' answer only. The interviewer receives no information about the question answered and records merely a 'Yes' or 'No' response. Some care needs to be taken in setting the probability level for the selection of the sensitive question. The nearer the probability is to 1 the more reliable the estimate, but the less protection provided to the respondent. (For some discussion of competing design criteria in developing randomized response questions, see Fox and Tracy, 1986.)

Since the interviewer cannot know which statement was chosen, the respondent is protected from self-incrimination (and, incidentally, the researcher is protected from having to maintain a data file containing incriminating information [see Boruch, 1979]). However, Warner showed that provided the probability of selecting the sensitive statement is known, then an unbiased estimate of the proportion of those possessing the sensitive attribute could be calculated for the sample as a whole from the following formula:

\[ \Pi_A = \hat{\lambda} \left( 1 - p \right) \]

where:

- \( \Pi_A \) = the estimate of the proportion having the sensitive attribute,
- \( \hat{\lambda} \) = the proportion of 'Yes' answers,

and

- \( p \) = the probability of selecting the sensitive question using the randomizing device (and where \( p \neq 0.5 \)).

Warner's initial design has been enhanced and extended in a variety of ways. Extensions of the method include its use to estimate the proportions in a population belonging to two or more mutually exclusive groups (Abu-Ela et al., 1967) and the development of designs for use with continuous variables (Greenberg et al., 1971; Himmelfarb and Edgell, 1980). Perhaps the most important enhancement of the method has been the introduction of the 'unre-
Validation studies of randomized response methods have been conducted to assess their reliability and accuracy. These studies often involve different levels of noise to test the effectiveness of the technique. The results of these studies vary, but they generally support the utility of randomized response methods in various applications.

In addition to the original question-and-answer format, there are other randomized response techniques that can be used to protect respondent privacy. These techniques include randomized response with a randomization device, randomized response with a randomization vector, and randomized response with a randomization matrix. Each of these methods has its own advantages and disadvantages, and the choice of method depends on the specific research context.

Randomized response methods are particularly useful in situations where respondents may be reluctant to provide truthful answers due to social desirability bias or other factors. By randomizing the response process, respondents are able to provide honest answers without fear of judgment or consequences.

In conclusion, randomized response methods are a valuable tool for collecting sensitive data in a manner that protects respondent privacy. These methods have been extensively studied and have been shown to be effective in a wide range of applications. As such, they are a valuable addition to the statistical research toolbox.
inferences drawn from the randomized response technique would appear to be far greater than for other methods which are vulnerable to systematic response bias. (1981, 199)

However, as Umesh and Peterson (1991) point out, in a range of studies where validation data were available, randomized response produced estimation errors of anywhere between 5 and 35 per cent. Other studies have also cast doubt on particular variants of the method (Duffy and Waterton, 1984), or on its applicability in particular kinds of situations, such as the study of attitudes rather than behaviour (Wiseman et al., 1976). Even among those who have found in favour of the method (Bradburn and Sudman, 1979; Tracy and Fox, 1981; Zdep and Rhodes, 1976), there is general agreement that the technique does not provide a panacea, and that, in particular, if it is to work the method requires careful piloting. Thought must be given to the setting of the various probability levels required by the method and means found to ensure that the respondent both understands the method and trusts it to provide protection against possible jeopardy.

The nominative technique

The nominative technique is a recently developed and promising alternative to the use of the randomized response question as a means of obtaining reliable estimates of behaviour likely to be under-reported in response to direct questioning. The technique had its origins in the multiplicity techniques developed by Monroe Sirken and his associates in order to study the incidence of rare health events. Procedures of this kind depend on the generation of a 'shadow sample' of those possessing some rare attribute or exhibiting some rare behaviour by asking survey respondents to provide information about relatives and friends. Since members of the shadow sample may be reported more than once using this procedure, additional data must be used in order to assign weights which compensate for multiple counting. The multiplicity technique has been used to make estimates of the incidence of diabetes in the United States (Sirken et al., 1975), and to estimate the extent of under-registration of births and deaths. (Another application of multiplicity techniques is in the 'network sampling' of rare or 'special' populations discussed in Chapter 4.)

Sirken's (1974) suggestion that a technique using information supplied about nominated others could be extended to the exploration of sensitive topics has been taken up by a number of writers. Bradburn and Sudman (1979) used a version of the nominative technique to examine levels of drinking behaviour and marijuana use. They found that the method led to increased levels of reporting, even for respondents who felt very uneasy about the question. Sampling variances of the estimates using this method were also reduced. They concluded that the results of their experiment were 'encouraging enough to make further testing of this procedure desirable' (1979, 150). Subsequently, the most detailed and extensive study of the use of the method for obtaining reliable estimates of heroin abuse has been carried out by Patricia Fishburne. Fishburne (1980, 71–2) argues that the advantages of the nominative technique in relation to threatening topics are as follows. First, since information is gathered about a nominated but unidentified other, the anonymity of both respondents and nominees is maintained. This in turn should reduce the level of threat posed by the asking of potentially sensitive questions. In so doing, interviewer and interviewee reluctance to deal with the sensitive topic should be reduced, as should the level of under-reporting of the behaviour in question. Second, the technique produces a larger sample size by allowing individuals to be enumerated at a larger proportion of households. One consequence of this, especially where the attribute in question is relatively common, may be a lower sampling error than could be obtained using direct report. Third, the nominative technique potentially yields a more complete coverage of the population of interest since it may be possible for those frequently missed by household surveys (e.g. those living in various kinds of institutional accommodation) to be enumerated in the shadow sample. A further possible advantage not mentioned by Fishburne is that the researcher is not required to hold information on individuals which is potentially incriminating, and which might be subject to legal seizure.

Against these advantages can be set the following disadvantages (Fishburne, 1980, 72–3). The nominative technique is of little utility in the investigation of essentially private activities like sexual behaviour where the amount of knowledge available to others may be severely circumscribed. Nor is it useful where the population of interest contains a large proportion of social isolates. In addition, because the possibility of multiple counting is inherent in the procedure, the nominative technique cannot be made to work in the absence of reliable data on respondents' relational involvements to be used for weighting purposes. (For further discussion of this point, see R. M. Lee, 1986a.) Furthermore, there is no guarantee that respondents will accept the informant role, nor is it clear how far there may be systematic tendencies to over- or under-report the activities of others.

On the basis of extensive, detailed and careful pre-testing Fishburne developed a set of questionnaire items for inclusion in the
1977 US National Survey on Drug Abuse. Using data from a shadow sample of individuals nominated by survey respondents, these items were used to provide prevalence estimates of heroin abuse in the United States. In the form finally adopted after pre-testing, respondents were asked to specify how many of their close friends they knew for certain to have used heroin. Interviewees were then asked to think of one user-friend chosen at random and – without identifying the individual – to supply information on that person’s age, sex and pattern of heroin use. In addition, responses to a number of further questions were obtained. These were designed: (a) to help assess the credibility of the respondent’s report; (b) to supply the necessary information to allow the weighting procedure to take place; and (c) to gauge the extent to which the respondent found the nominative question series difficult to answer.

The nominative technique yielded good results on the national survey. [As expected, the heroin prevalence estimate produced by the nominative technique was a good deal higher than that based on direct report, suggesting that the method had successfully reduced under-reporting of heroin use. Construct validity appears also to have been good; that is, associations between variables such as age and drug use, whose relationship is well-known on the basis of existing studies, were reproduced in the nominative data. Fewer than half of one per cent of interviewees refused to respond when confronted with the nominative series, while a very substantial proportion of those giving information about a nominee gave evidence of having had direct first-hand evidence of the nominee’s heroin use. It proved possible, too, readily to obtain the information necessary for weighting purposes. Taken overall the results suggest, according to Fishburne, a relatively high level of veracity on the part of the respondents. The patterns between variables were as expected, and there was a logic and consistency about the data which would not have been present if large numbers of respondents had misrepresented their knowledge of user-friends. (1980, 202)]

As with randomized response, it should not, however, be assumed that the nominative technique is capable of being automatically transferred to other topics or contexts. In this regard, Fishburne concludes (1980, 220) that the nominative technique could only be used successfully where it could be demonstrated on the basis of pre-testing that the following conditions were met:

1. the behaviour under consideration is of a kind likely to be known to other people;
2. the behaviour under consideration is sufficiently salient to respondents for it to be easily remembered by them;
3. a prior determination has been made concerning the reference group most knowledgeable about the behaviour in question;
4. a prior determination has been made concerning the reference group most willing to report the behaviour in question.

Microaggregation methods

Microaggregation techniques were originally designed to protect the confidentiality of data held in archives (Feige and Watt, 1970). It is also possible, however, to use microaggregation strategies on respondent rather than archival data. One method suggested by Boruch and Cecil (1979) is for the members of a sample to be aggregated into a set of clusters (or, where appropriate, grouped into naturally occurring aggregates). Members of each cluster are then requested to forward to one of their number anonymous questionnaires. The recipient of the questionnaires then averages the replies and forwards the averages to the researcher. The researcher therefore at no point has access to identifiable data.

Clearly there are problems of securing cooperation with this method. A more useful model is that known as the Block Total Response (BTR) method (Smith et al., 1974). In this method, respondents are presented with sets of questions concerning the frequency with which they engage in certain kinds of behaviour. These questions are organized in blocks containing sensitive and innocuous questions. Different blocks are offered to different subsamples. A coding procedure is used to provide numerical responses to each block but without indicating which questions are being answered. From this it is possible to estimate an average value for the prevalence of the behaviour of interest. (For more on the statistical background to the method, see Boruch and Cecil, 1979, chap. 5; Smith et al., 1974.)

Miller and Cisin (Cisin and Miller, 1981; Miller, 1984; Miller and Cisin, 1984) have field-tested a microaggregation technique known as the item counts/paired lists method which has the considerable advantage of simplicity. Two lists of behaviours are constructed, one of which contains the sensitive topic while the other does not. Each list is administered to a split sample. Respondents are instructed to return the number of activities in which they have participated but not to indicate to the interviewer what those activities are. The mean counts for each list are then subtracted from one another to give an estimate of the prevalence of the sensitive behaviour.

There are a number of disadvantages to these techniques (Boruch
Attacking Sensible Questions on Surveys

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...
of immediate and longer-term clinical effects in patients with different aspects of disease, as well as the potential for use in research settings. This approach has identified several candidate proteins that may be involved in the pathogenesis of disease. Further studies are needed to validate these findings and to determine the clinical significance of these potential biomarkers.


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**Summary**

- The use of patient-derived cell lines to study disease mechanisms and to develop and test potential therapies is an exciting area of current research.
- The integration of multiple techniques, such as high-throughput screening, bioinformatics, and functional assays, is essential for the identification of novel therapeutic targets.

**Future Directions**

- The development of novel therapeutic strategies, such as gene editing and cell-based therapies, will be crucial for translating these findings into clinical practice.
- Continued collaboration between researchers, clinicians, and patients is essential for advancing the field of disease modeling and personalized medicine.
Conclusion

The risk of developing problems due to excessive alcohol consumption is significantly high. The use of a structured questionnaire for assessing the level of alcohol abuse among students is an effective method. The questionnaire provides useful insights into the Habits and attitudes of the respondents towards alcohol use. The findings of the study indicate that a significant number of students consume alcohol regularly. The study also highlights the need for developing interventions to address the issue of alcohol abuse among students. Further research is needed to explore the factors influencing alcohol consumption and the effectiveness of interventions targeting this issue.
Researchers in the philosophy tradition see the discipline of scientific research as fundamental. They view the aims of science as promoting understanding by fostering a process of hypothesis testing and critical evaluation. This process aims to uncover new knowledge and to refine existing theories. Researchers have developed models for understanding the scientific method, including the hypothetico-deductive model. This model involves formulating hypotheses, testing them through experiments or observations, and revising or rejecting hypotheses based on the results.

A common criticism is that the emphasis on reductionism and mechanistic explanations can lead to a neglect of the complexities of real-world phenomena. Critics argue that science often fails to account for the full range of human experience and that it can be limited by the biases and perspectives of researchers.

In response, some argue that a more holistic approach to scientific inquiry is needed. This approach would involve considering the social, cultural, and ethical dimensions of scientific research, as well as the role of values and values in the pursuit of scientific knowledge. Others advocate for a more pluralistic approach to science, which embraces multiple perspectives and methods of inquiry.
information as implying much more than the management of civility
between strangers. According to Madge:

As the techniques of questioning were refined it came to be taken almost
for granted that some form of psychoanalytic penetration into the near
unconscious was the proper method of handling difficult affect-laden
topics. Among such topics it would be assumed that those dealing with
sexual behaviour would rank high, and that exceptional psychological
tact would be needed to obtain correct answers on such questions. (1963,
534)

In this context, privacy, confidentiality and a non-condemnatory
attitude are important because they provide a framework of trust.
Within this framework, researchers can lead those studied to
confront, in a fundamental way, issues which are deep, personally
threatening and potentially painful.

This chapter begins by looking at attempts by survey researchers
to isolate the effect interviewees have on the propensity of respon-
dents to disclose sensitive information. This is followed by a
detailed discussion of depth interviewing as a method for exploring
sensitive topics. A final section considers the use of diary methods, a
research technique which has sometimes been proposed as an
alternative to the interview in research on sensitive topics.

Survey interviewing

Sykes and Hoinville (1985) point out that it is possible to advance
two contradictory hypotheses concerning how the physical presence
of an interviewer might affect reporting on sensitive topics. On the
one hand, it can be argued plausibly that when an interviewer is not
present respondents are less likely to feel threatened by questions
about sensitive topics. On the other hand, it can also be argued that
the presence of an interviewer encourages respondents to feel
relaxed and therefore more forthcoming. There are three main
methods of administering questions on surveys: face-to-face using
an interviewer, telephone interviewing where the interviewer is
heard but not seen and the use of self-completion questionnaires
where the interviewer is absent or is present but passive. There is
little evidence that in general terms any one method should be
preferred over the others. Bradburn and Sudman (1979) found that
varying the mode of administration of a questionnaire in itself had
no consistent effect on the results obtained. Similar patterns emerge
from much of the recent work which has focused on telephone
interviewing versus other methods (Dillman, 1978; Cannell, 1985;
Groves and Kahn, 1979; Sykes and Hoinville, 1985). Sykes and
Hoinville (1985) found few differences in the distribution of re-
sponses to a range of sensitive questions whether the interview was
carried out face-to-face or by telephone. In subsequent work Sykes
and Collins (1988) have pointed to a consistent tendency for
telephone interviews to yield a pattern of greater disclosure.
However, the differences between modes remain small.

In the Bradburn and Sudman (1979) experiment, where survey
responses were compared against official records, under-reporting
of 'socially undesirable' behaviour occurred whichever method of
questioning was used. However, face-to-face interviewing tended to
depress reports of socially undesirable behaviour more than did
telephone interviews. Self-administered questionnaires fared rela-
tively poorly. There was a small but consistent tendency for
respondents using self-completed questionnaires to report lower
levels of socially undesirable behaviour than was true of the other
methods. Moreover, response rates were lower for this mode of
administration than for the other methods. Randomized response,
the most anonymous condition, produced the lowest levels of
under-report. Even so, for a question relating to drunk driving there
was still a level of under-report of around one-third. In other words,
as Bradburn puts it, 'Contrary to common belief favouring face-to-
face interviews, there is no clearly superior method that yields
better results for all types of question' (1983, 294). A further
possible conclusion from these studies is that there is a need, as yet
unmet, for a more sophisticated understanding of the conditions
under which one method of eliciting sensitive data is more useful
than another (Bradburn, 1983).

Interviewer effects

There are many ways in which interviewers can affect the validity of
the responses they receive. (For reviews, see Collins, 1980; Brad-
burn, 1983.) In relation to sensitive topics, interview effects of two
kinds have been thought to be important. First of all, it has long
been suspected that the social characteristics of the interviewers
themselves might have a biasing effect on results. A second source
of bias has been sought in the expectations interviewers have about
the interview itself.²

In what would now be called a 'meta-analysis' (Glass et al., 1981),
Sudman and Bradburn (1974) undertook a systematic review of a
very large number of studies which have looked at response effects.
They conclude that in many instances interviewer effects do not
exist or are small compared with other kinds of effect (although
there may be differences depending on whether the questions asked
refer to attitude or behaviour). Bradburn comments:

the belief in pervasive and substantial interviewer effects is a conse-
Deep Interpersonal Learning.

Additional learning. Individuals who are able to effectively interact with others, to have different perspectives, to give them the opportunity to grow and learn, will be more effective learners. Learning is not only about knowledge, but also about the development of skills. The process of learning involves understanding, practice, and reflection. By engaging in meaningful interactions, learners can develop a deeper understanding of the material and improve their ability to apply it in real-world situations.

Interpersonal Experiences.

In this current age, the ability to communicate effectively is more important than ever. A strong interpersonal skillset can help individuals navigate complex situations and build lasting relationships. By fostering constructive communication and fostering a culture of respect and empathy, organizations can create a positive learning environment.

In Summary.

By embracing effective interpersonal learning, individuals can achieve personal and professional growth. This includes developing effective communication skills, fostering empathy and understanding, and cultivating a collaborative mindset. In doing so, learners can unlock their full potential and contribute to a more connected and productive society.

References:

been voiced for the use of unstructured or ‘depth’ interviewing. The depth interview should not be seen, however, as it sometimes is, as merely derivative from the survey interview. It has an equal, if not prior, historical claim to consideration (Roethlisberger and Dickson, 1939; Merton and Kendall, 1946; Madge, 1963; Converse, 1987; Malsteed, 1987). Despite this, as Bryman comments, it is rather surprising ‘that the widespread acceptance of interviewing in qualitative research has not been given greater critical attention’ (1988, 114; see also Briggs, 1987). A number of writers (Spradley, 1979; McCracken, 1988) have given general guidance on the conduct of the depth interview. There has, however, been little specific advice about question strategies which might be used in the case of sensitive topics. Yet, there is no reason to suppose that qualitative interviewers will feel any less uncomfortable or concerned about broaching sensitive topics than do survey interviewers, or that interviewees will not find the interview difficult.

In a recent review, Brannen (1988) has discussed a range of issues which arise when qualitative interviewing is used to research topics which are highly personal, threatening or confidential. Such interviews, according to Brannen, are distinguished by a number of features which make them problematic. The interview is typically a stressful experience for both the interviewee and the interviewer. Moreover, in written reports respondents are easily identified by themselves and others close to them because the data are unique and personal. Identification carries with it the risk of sanctions or stigma from various sources. As a result, the researcher has responsibilities to the respondent such that ‘Protection is required both with respect to the confidences disclosed and the emotions which may be aroused and expressed’ (1988, 553). Brannen suggests that there are four sets of contingencies which surround the exploration of sensitive topics by means of qualitative interviewing. These are: (a) approaching the topic; (b) dealing with the contradictions, complexities and emotions inherent in the interview situation; (c) the operation of power and control in the interview situation; and (d) the conditions under which the interviewing takes place.

Approaching the topic
Where the topic of the research is a sensitive one, presenting it to respondents may not be easy. As a matter of strategy, the researcher must decide whether or not the topic of the interview should be described in detail at the outset. If so, there is the further problem of just how the research is to be described. Defining the boundaries of the research topic too tightly may inhibit respondents from defining it in their own way. In addition, as Cunningham-Burley (1985) points out, defining the interview in one way may preclude the raising of other topics. Having obtained respondents' trust, it may be difficult to inquire into aspects of their lives not apparently related to the topic at hand.

Brannen (1988) points out that where the interview is addressed towards behaviour which is problematic or stigmatized respondents may be alarmed by the broaching of the topic. With some topics they can have difficulty in making sense of what might be unfamiliar or distasteful to them. Interviewees may not have the vocabulary which allows them to discuss the problem, or they may deny it, or project it on to someone else. In some cases, even, as Van Maanen points out, dramatic and stressful events may be recounted in an 'existential fog' in which 'informants are as unsure and equivocal as to what happened, what is happening, or what will happen as the fieldworker' (1982, 140–1).

Brannen suggests that the topic of the research should be allowed to emerge gradually over the course of the interview. This, however, raises the issue of informed consent. Indeed, it can be argued that depth interviewing poses more acute problems of informed consent than do survey interviewing or participant observation. While the survey interview runs the risk of depersonalizing respondents, its relatively brief span and its very superficiality may pose little direct harm to those who participate in it. This may even be true where sensitive topics are broached if steps of the kind described earlier are taken to 'dejeopardize' respondents. In participant observation, the fieldworker spends considerable time in the field. The obtaining of consent in this context can be seen as the outcome of a developmental process (Wax and Cassell, 1979b). Disclosure of sensitive or confidential information is usually only possible in these situations once trust has been established between the fieldworker and the people being studied. Where this has been done consent becomes implicit. By contrast with both these situations, the person from whom a depth interview is sought must agree at the behest of a relative stranger to give a not inconceivable amount of time and effort to the interview. They may be asked to reveal a great deal about themselves, perhaps at some emotional cost. Moreover, there is no guarantee that informants will realize before an interview begins what they might reveal, in what ways, or at what risk. Nor may it be easy for the researcher to convey all of this in an informed manner before beginning the interview. (For some discussion of these issues, see Kay, 1989.)
Contradiction, complexity and emotion

Depth interviews are seen to have advantages over survey interviews. As a number of writers have pointed out (Coleman, 1958; Galtung, 1967; Graham, 1983), surveys, in treating the individual as the basic unit of analysis, abstract from social positions and social relations. The survey assumes that social phenomena have an external, stable and verbalizable form. It is therefore an inappropriate instrument for investigating aspects of experience which are internal, fluid or expressed in non-verbal ways (Graham, 1983). The measurement procedures typically used by survey researchers force experience — often by fiat — into pre-determined and frequently inappropriate categories (Cicourel, 1964). In contrast, it is argued that interviewing in depth produces more valid information. Such interviews provide a means of getting beyond surface appearances and permit greater sensitivity to the meaning contexts surrounding informant utterances. This is particularly so when sensitive topics are studied. Brannen argues that sensitive topics are difficult to investigate with single questions or pre-coded categories. She comments:

Respondents' accounts of sensitive topics, such as marital difficulties, are frequently full of ambiguities and contradictions and are shrouded in emotionality. These form an integral part of the data set and therefore need to be confronted and taken account of in their interpretation. (1988, 553)

In a similar vein Laslett and Rapoport (1975) have advocated an approach they refer to as 'collaborative interviewing and interactive research'. As will be seen, some researchers prefer collaborative research relationships for ethical and political reasons. Laslett and Rapoport, by contrast, argue that achieving a sense of collaboration in the interview enhances the quality of research by increasing internal validity. Such an approach, which they see as being particularly appropriate for studying the private and intimate aspects of family life, depends on 'being responsive to, rather than seeking to avoid, respondent reactions to the interview situation' (1975, 968). To do this Laslett and Rapoport aim for systematic exploration of the topic to hand, but avoid interviewing in a standardized way, which they see as a route to superficiality. Interviews in collaborative research are lengthy, demanding and require negotiation. The research strategy advocated by Laslett and Rapoport is, therefore, explicitly longitudinal. Repeated interviews are carried out with several members of the same family. Specially trained interviewers are used, with interviews being carried out by more than one person, whose work is monitored. The interview itself is organized around an interview guide which may be used by both interviewer and interviewee. As presented by Laslett and Rapoport, the collaborative research method is apparently not suitable for one-person research. It is highly labour-intensive, and its organization involves a division of labour which incorporates, it would seem, differentials of power and status between different team members. Lone researchers may have to develop slightly different strategies, as did Cannon (1989; 1992) in her study of women suffering from breast cancer. Interviewees at each interview were encouraged to discuss the previous interview and their feelings about it, as a way of involving them actively in the production of the data.

Laslett and Rapoport (1975) are insistent that the research strategy takes into account the psychodynamics of the interview situation, and the effects they have both on interviewers and interviewees and on the quality of the data. A major aspect of the kind of interviewing they advocate is the attempt to make interviewers aware of their feelings during the interview, and of the ways in which these feelings may affect the interviewing process. In particular, Laslett and Rapoport make use of the concepts, derived from psychoanalytic theory, of transference and countertransference. In its original psychoanalytic context transference refers to feelings derived from earlier experiences which are projected on to the analyst. Countertransference refers to similar feelings on the part of the clinician. With respect to interviewing, Laslett and Rapoport use the terms in a slightly looser but analogous way to refer to situations where, for example, the interviewee develops an identification with the interviewer or vice versa. As a result, respondents may produce what it is assumed the interviewer wants to hear, or interviewers may accord particular features of the respondent's experience undue prominence.

Although transference may arise out of past experiences or relationships, Laslett and Rapoport also suggest that an additional source for transference effects is to be found in social characteristics of interviewer and interviewee such as social class. Whatever their origins, however, these psychodynamic manifestations are regarded as data, not as a problem or a nuisance. In consequence, interviewers in their study were helped to detect and manage these processes, in order to minimize invalidity in the data collected. Nevertheless, Laslett and Rapoport's discussion at this point is somewhat unsatisfactory. They do not indicate how, in the absence of psychoanalytic principles, one can identify transference in this wider sense.

Interviewing about sensitive topics can produce substantial levels of distress in the respondent which have to be managed during the
course of the interview. Brannen argues that faced with such distress interviewers may want to help but should strongly question their motives for doing so. Such feelings on the part of the interviewer, she suggests, 'often have more to do with helping the helper than those who are in need' (1988, 559). All that may be possible in these situations is for the interviewer to undertake the difficult task of enduring and sharing the pain of the respondent.

It follows that if the interview can be distressing to the respondent, it can also be stressful for the interviewer. In the kind of approach taken by Laslett and Rapoport, interviewers are closely supervised. There is extensive discussion of transcripts and close attention is paid to the feelings of the interviewees, especially in relation to those topics which are threatening and/or worrying to the interviewer. One major part of this process is detailed writing up. This kind of context, however, is presumably unusual. As Brannen notes, the stress induced by interviewing respondents in depth about sensitive topics, and ways of dealing with that stress, have largely been ignored:

Researchers who are entrusted with the confidences of their respondents ought to be protected by some of the safeguards that customarily are associated with the role of the confidant. Confiding is normally a reciprocal process. Even professional confidants — counsellors and psychotherapists — have their own confessors. (1988, 562)

Taking this last comparison further, Brannen points out that other professionals are able, by virtue of their superordinate position, to develop structured ways of dealing with stress. They practise on their own territory, work within set time limits, and can usually call on therapeutic support. By contrast, those who collect interview data are in a less favoured position. While they stand in a relation of superordination in relation to the respondent, researchers in many instances may be 'hired hands' (Roth, 1966) whose position in the research organization for which they work is frequently a subordinate one. As a result, Brannen argues, researchers must often try to cope with the stresses and strains of interviewing as best they can. This they do usually by turning for support to others in the same predicament (see also Smart, 1984, 256–7), something not always possible for the lone researcher.

Brannen noticed that her respondents would frequently anticipate the emotional demands which the interview was likely to make on them. They would try to ensure that support from friends would be available after the interview had taken place, or else they used alcohol as a crutch during and after the interview. In fact Brannen suggests, although she does not fully develop the point, that the emotional risks attached to the interview may have actually made them attractive to respondents. Whether they have anticipated it or not, the depth interview can often be a cathartic experience for interviewees (see, e.g., R.M. Lee, 1981). Faraday and Plummer collected life histories from a number of sexual deviants by means of detailed and repeated interviews. They record that one of their interviewees wrote to them as the interviews proceeded, saying:

I feel I am overburdening you as an outlet for my personal troubles, but it has been a case of opening a valve and being reluctant to close it until some of the pressure was spent. (1979, 789)

They point out, however, that being an outlet in this kind of way brings with it a range of difficulties. Because respondents feel grateful for the opportunity to express their feelings, they often respond by overwhelming the researcher with gifts of various kinds. Over time this may lead to a growing closeness which creates a blurred line between the role of friend and that of research participant. Because of this blurring it becomes difficult not to worry that the friendship aspect of the relationship is being exploited for research purposes. A further difficulty, particularly with those who are 'deviant' in some way, is that the articulation of hitherto private feelings may encourage a growing sense of a particular identity. Faraday and Plummer note examples in their study of people developing the self-confidence to 'come-out' in various ways as the interviews proceeded. Such decisions, of course, have wider ramifications for which, conceivably, the researcher may have a responsibility.

Power in the interview

In recent years, an important area of discussion within the literature on interviewing has revolved around the role of power in the relationship between interviewer and respondent. One difficulty with this literature is that it is by and large divorced from theoretical debates about the nature of power and the means by which it is exercised (see, e.g., Lukes, 1974; Giddens, 1976; Benton, 1981). The notion of power is used in a rather loose and common-sense way. This is true, for example, of Elliot Mischel's (1986) work. Mishler discusses the power dynamics in interviewing solely in terms of the status distance between interviewer and interviewee. This not only conveniently neglects situations where interviewer and interviewee are of equal status, or, as in elite interviews, where the interviewer has a lower status. It also continually begs the question of how disparities of status actually manifest themselves in power terms within the interview.

Despite difficulties of this kind, the existence of power relations in the interview, once recognized, becomes difficult to overlook.
This is particularly so given the work of feminist writers like Oakley (1981), Graham (1983) and Finch (1984). These writers have argued that within the 'traditional' survey interview interviewers have power because of an asymmetrical distribution of rights and obligations. In particular, there is a disparity of disclosure rights. The interviewer may obtain revelations from the respondent but need not reveal anything in return. Despite admonitions in the literature to establish 'rapport' with the respondent, the disparity of rights between interviewer and interviewee gives that rapport a spurious and ultimately instrumental character. As Oakley puts it:

'rapport', a commonly used but ill-defined term, does not mean in this context what the dictionary says it does ('a sympathetic relationship, O.E.D.) but the acceptance by the interviewee of the interviewer's research goals and the interviewee's active search to help the interviewer in providing the relevant information. The person who is interviewed has a passive role in adapting to the definition of the situation offered by the person doing the interviewing. The person doing the interviewing must actively and continually construct the 'respondent' (a telling name) as passive. (1981, 35)

Looking at this point of view, the standardized interview, with its closed questions and rigid pre-specification of topics, does not enhance disclosure but precludes it. In the case of women, feminist writers argue, the survey interview objectifies female experience and mutes women's self-expression. As a consequence, survey research tends to operate in an oppressive way which mirrors and promotes wider patterns of social disadvantage. For reasons of this kind, writers like Oakley and Finch eschew survey methods in favour of qualitative research strategies. They stress as a matter of ethical commitment a style of interviewing based on reciprocity and a process of mutual self-revelation. In addition, both Oakley and Finch argue that women researchers and female interviewees share the same subordinated structural position in a male-dominated society. This provides them with a shared identification with one another as women. One outcome of this identification is that it promotes a genuine rapport in the interview, which leads to greater self-disclosure and, thus, to the collection of 'better' data. It follows from this that a lack of shared identification constitutes an impediment to the relationship established between a male interviewer and a female respondent.

The critique of the traditional survey interview put forward by writers like Oakley and Finch has been influential. Their work has not, however, escaped criticism from within the feminist tradition, notably by Wise (1987; see also Smart, 1984; Warren, 1987). Wise argues that the kind of ethical critique produced by Oakley and Finch is 'both more complacent and more sophisticated than previous accounts'. Oakley, in particular, in her view, uses the shared structural position of women as a 'magical device for the instant dissolution of inequalities' (1987, 66). In other words, even where gender differences are absent, Wise argues that imbalances of power can still exist between researcher and researched. This is so because power has a variety of social structural bases, for example, in class relations, which may still operate even in the presence of a shared gender identity. As a result, along with a number of other writers (Brannen, 1988; McKeen and O'Brien, 1983), Wise argues that success in interviewing depends more on a complex interrelation between the relative structural positions of interviewer and interviewee and the interviewer's skill and personal style, than it does on a simple identity of gender.

Implicit in Wise's critique of other feminist writers on the interview is a wider distrust of the interview as a therapeutic event. Drawing an analogy with the relations between social workers and clients, she argues that even ostensibly equal or mutually satisfying research relationships may nevertheless conceal more subtle forms of domination. A further point related to this is that, as a number of micro-sociological studies of power and exchange suggest, reciprocity and self-revelation can be deployed strategically in social relationships. They may be used, for example, as ingratiating tactics, or as a means of increasing the social indebtedness of the other (Kollock and Blumstein, 1987; Leifer, 1988; see also Blau, 1964b; Richard M. Emerson, 1962; Cook and Emerson, 1978). In other words, as Finch (1984, 81) acknowledges, strategies used to ensure a non-hierarchical relation between interviewer and respondent can come to be regarded simply as a set of techniques divorced from the ethical foundations upon which they are based.

Wise (1987, 66) also asks 'what does Oakley do with the women that she does not like, or who don't like her, or those who's (sic) perceptions of their lives differ from hers?' This is an interesting question because it brings into focus again the issues of transference and countertransference raised by Laslett and Rapoport (1975). Oakley mentions Laslett and Rapoport's discussion of transference effects briefly in a footnote. However, she does not discuss whether such effects, at least in their more 'social' form, were present in her own interviews. This, therefore, leaves unanswered the question of whether, in deepening disclosure, a non-hierarchical, mutually self-revelatory interviewing style might also distort, in a variety of subtle ways, what is disclosed. Interesting in this regard is the literature on psychotherapeutic interviews. There, the emphasis seems to have shifted away from justifying the role of self-disclosure in the
Interview. Instead, research has been directed to the conditions under which self-disclosure by the interviewer will enhance the therapeutic encounter, or be harmful to clients (Anderson and Mandell, 1989).

Brannen speaks of power in two senses. One, related to that just discussed, is the ability to control the interaction itself by dictating the form taken by the interview, and its content. Where the format of the interview is not rigidly specified by prior standardization, it is possible to see the exercise of power in the interview as a two-way process: it can be exerted by both interviewer and respondent. According to Brannen and others (Wise, 1987; McKee and O'Brien, 1983), the locus of control in the interview emerges from the interrelation between the topic, the particular method of interviewing used and the respective statuses of the participants. Thus, in one of the cases Brannen describes status equivalence between interviewer and interviewee resulted in an interactive conversational interview. In another example which she discusses, the topic was an extremely personal one and the role of the interviewer was deliberately non-interventional. Here, interviewee became so engrossed in their own stories that they become relatively impervious to interviewer interventions. In other words, in this situation respondents exercised control although not in an entirely conscious way. Even when this does not happen, respondents may still retain power. As Brenner (1978) points out, interviewers have few sanctions which they can deploy should a respondent decide not to accept their definition of the situation (see also Cunningham-Burley, 1985). The relative powerlessness of the interviewer is reinforced by the etiquette of the interview itself. This generally forbids even the self-disclosing interviewer from being openly judgemental about the respondent. There may be situations where the interviewer has simply to endure the interview with gritted teeth. Even when respondents put forward views of an offensive kind which, in other circumstances, would normally bring forth a swift and sharp rebuke, the interviewer may choose to remain silent (Smart, 1984; McKee and O'Brien, 1983).

A further sense of power referred to by both Brannen and Finch is the ability to have power over the informant by virtue of possessing potentially damaging information which may have been revealed in the interview. This vulnerability can be personal as when individuals reveal personal details about themselves in the interview which may spark reaction or intervention from those near at hand. In situations of this kind, others, unsympathetic or hostile to the research, may overhear what has been disclosed and subsequently intervene to the respondent’s detriment. Brannen (1988, 560) gives a number of examples of situations in which she feared a negative reaction from husbands to information disclosed by their wives during interviews concerning marital difficulties. (She also notes that wives sometimes concealed the topic of the research from their husband, presumably for fear of their reaction.) Respondents may also be vulnerable in a collective way (Finch, 1984; Brannen, 1988) since they have little control over the ways in which the data are interpreted. Finch records, for example, her worry that data she had collected from women on the basis of trust could, given a particular interpretation, be used against their interests. The dilemma here is not simply, as Finch argues, an ethical one, but ultimately involves a political choice of the “whose side are we on?” kind (Becker, 1967). However, as both Finch and Brannen point out, the difficulties here may be particularly acute for the interviewers who are “hired hands” (Roth, 1966). In these situations control over the data typically passes from their hands to those of powerful others who may have less concern for the interests of the respondents.

Interviewing conditions
Perhaps because of its roots in the survey tradition, the literature on interviewing tends to assume that those interviewed will be unrelated to one another. Particular difficulties arise when research on ‘sensitive’ matters is carried out with members of the same family. Drawing on Simmel’s analysis of triadic relations, Carol Warren (1987) suggests there is a range of patterns with may emerge in the relationship between husband, wife and interviewer. Each partner may seek to obtain confidential information about the other from the interviewer. The partners may collude in various ways, often to withhold information from the interviewer. Alternatively attempts may be made by each partner to draw the interviewer into a collusive relationship in order to keep information away from the other partner. Finally, the interviewer may become a tertius gaudens (Simmel, in Wolff, 1950): the third party who benefits from the conflict of two others. In this case, presumably, conflict between the partners leads, deliberately or inadvertently, to increased disclosure. This is not to say, of course, that interviewers will necessarily feel happy with this situation. Indeed, the interviewer may have to engage in conversational ‘repair work’ where arguments and disagreements break out between respondents (Cunningham-Burley, 1985).

Brannen (1988), on the other hand, distinguishes three kinds of situation in which interviews are to be carried out with marital partners. The most difficult situation from the interviewer’s point of
view, according to Brannen, is where each partner must be interviewed alone in his or her own home. The one-to-one interview can produce over-intensity and embarrassment, and it is very easy for the interviewer to become drawn into the interviewee's problems. A very much more favourable situation, Brannen suggests, is where two interviewers are used to interview the partners separately but simultaneously. Using a strategy of this kind leads to a feeling of equity in the interview situation. Each partner has a researcher who is neutral in the sense of being uninvolved in the marriage, but who is also partisan in the sense of 'belonging' to that partner. It was precisely for reasons of this kind that Laslett and Rapoport (1975, 971) used more than one interviewer per family, and adopted a strict rule that no member of a family could give permission for any other family member to be interviewed. Between the one-to-one interview and the use of interviewers for each respondent is a further situation where two interviewers are used with each interviewee. In these situations, while one interviewer carries out the interview, the other interviewer remains passive in an observer role. This particular configuration, while yielding additional information about non-verbal interaction, helps to ease the intensity of the interview. It also serves to give the 'active' interviewer support. Brannen, however, does not indicate what effect the second interviewer might have on the respondent's willingness to self-disclose. (A further reason for using two interviewers when dealing with some kinds of sensitive topics is suggested by Braithwaite [1985]. Faced with possible libel suits arising from his research on corporate crime in the pharmaceutical industry, Braithwaite decided in a subsequent study to work with a second interviewer who could serve as a potential corroborative witness in any possible libel action.)

For Brannen, one important aspect of interviews dealing with sensitive topics is that they should have a one-off character. In other words there should be no fear on the part of the respondent that the paths of interviewer and interviewee will ever cross again. In Brannen's view this is essential to ensure trust. The disadvantage of this rather transitory relationship from the respondent's point of view is that the researcher can not be used subsequently as a source of help or support. However, Brannen, referring to her research on marital difficulties, goes on:

In response to a question about what they considered to be the 'ideal situation' for disclosure... a significant number mentioned the characteristics of the research situation (for example, anonymity or its one-off nature) or the research situation itself. Even the majority who were already in receipt of counselling rarely mentioned professional help as an ideal. Thus our tenuous links with respondents were both potentially exploitative but also served to increase respondents' trust in us. (1988, 559)

In one sense this is not surprising. It can be argued that the relationship between disclosure and intimacy is a curvilinear one. The stranger, according to Simmel, 'often receives the most surprising openness - confidences which sometimes have the character of a confession and which would be carefully withheld from a more closely related person' (Wolff, 1950, 404). In other words, the recipients of intimate details about one's life are most likely to be either those standing in a very intimate relation to oneself, or those who are socially remote (for a review of research on social distance and self-disclosure, see Day, 1985, chap. 7).

Brannen's emphasis on the importance of the 'one-off' interview stands in some contrast to the advocacy of repeated interviewing by Laslett and Rapoport (1975) and by Oakley (1981). Oakley argues that the single interview encourages what she describes as an 'ethic of detachment' (1981, 44), which makes difficult the development of a deep, lasting and genuinely collaborative research relationship. Pointing out that multiple interviewing has been little discussed in the methodological literature, Laslett and Rapoport (1975) stress the importance of what they call the 'inter-interview dynamic'. In particular, they argue that the period between the first and second interview is of crucial importance. The initial interview typically raises anxieties on the part of the interviewee, and generates resentments directed towards the interviewer. Provided these anxieties and resentments can be handled by the interviewer, Laslett and Rapoport suggest that a different kind of data will be collected in the second interview. They argue that, characteristically, what is brought forward in the first interview is a more publicly acceptable, 'surface' account. Typically, however, a fuller, deeper and more complete account is more likely to emerge at the subsequent interview.

It is difficult to resolve the competing claims of one-shot versus repeated interviewing. Repeated interviews have undoubted advantages in terms of the quality of both the data and the relationship which can be established with respondents. They may not necessarily be needed, however, in a number of circumstances. If carefully handled, some kinds of topics, say of an incriminating rather than an intimate kind, may be brought to the surface fairly readily. In a similar way, where the interviewer is introduced to the interviewee by an intermediary, a sufficiently high level of trust may be present for a single interview to suffice. Finally, the respondent preference for single interviews uncovered by Brannen probably should not be
taken lightly. This is so not simply because of the benefits of anonymity, but also because the single interview is less demanding on the respondents themselves. It may therefore avoid the paradoxical situation in which the interviewer, to be collaborative and non-hierarchical and to avoid exercising power, has to make substantial demands on respondents.

Whatever interview strategy is used, termination of individual interviews and of the interview series is important. As Laslett and Rapoport put it, 'Both methodological and ethical considerations require that terminal sessions be well managed' (1975, 974). Specifically, and in line with their commitment to a collaborative interviewing style, Laslett and Rapoport argue that concluding interviews should give something back to the respondents. They therefore gave their interviewees feedback about their analysis of the family, and, it would seem, provided them with counselling about the potential longer term consequences of the interviews within the family itself. They also took the opportunity to correct errors or uncover remaining disagreements about how particular events were to be interpreted. At this stage Laslett and Rapoport also made agreements with their respondents about the conditions under which research accounts were to be published. For example, interpretations which were disputed by the respondent could be omitted or the respondent provided with the opportunity to present a dissenting account. Sometimes, of course, termination may be difficult for the researcher. Particularly when a study has been long and relations have been built up with respondents, the researcher may experience guilt, alienation and melancholia when those relations come to an end (Roadberg, 1980). In some circumstances such feelings can become even more intense and distressing. Over the course of her study, Cannon (1989; 1992) had become close to a number of the cancer sufferers she interviewed. A difficult problem for her as the research progressed was, therefore, how to come to terms with their deaths. Once again, it may be that the lone researcher is particularly vulnerable in this regard. As some AIDS researchers have discovered (Zich and Temoshok, 1986), the ability to mobilize support for researchers from team members can be indispensable when research participants die.

Diary methods

Diaries provide an appropriate data collection method where one wants to measure activities over time, the frequency or salience of which makes recall difficult. Plummer (1983, 17–20) distinguishes three forms of diary: (a) logs, the most clear example of which is the time budget; (b) the 'requested diary', where informants are given diaries and asked to keep records of their day-to-day lives; and (c) the diary–diary interview. Time budgets are detailed diaries in which a random sample of respondents keep a record of their daily activities. As they provide a means of 'tracking' people's activities, time budgets provide a way of finding out about activities which a respondent might not disclose in interview. Time budgets therefore have been used in studies of the 'hidden' economy to give an indication of the extent to which people officially defined as unemployed undertake paid work 'on the side' (Trew and Kilpatrick, 1984). Time budgets potentially yield a great deal of detailed, potentially generalizable data. However, the method can be burdensome to the respondent. A further major drawback of this method is that, without other information, all one knows about are activities, but not the contexts in which they take place. To stay with the hidden economy example, the recording of an activity as paid work will not of itself reveal, say, that tax payments are being evaded. Nor is there any guarantee that potentially incriminating activities will be recorded in the first place. As Harding and Jenkins (1989) point out, if time budgets are to be used in these kinds of studies they need normally to be harnessed to other data collection strategies.

Coxon and his colleagues have made use of diary methods in a study of changes in the sexual behaviour of gay and bisexual men under the impact of AIDS/HIV. Coxon (1988) argues that reports of sexual behaviour based on pre-coded questionnaires and clinical data are unreliable. This is because accurate recall is difficult, reported frequencies are often an artefact of the pre-coding procedures, and it is not clear what meanings terms such as 'sexual partner' have to respondents. For these reasons, according to Coxon, diary methods should be more reliable than retrospective accounts provided they are kept on a regular day-to-day basis.

The diaries used in Coxon's study are different from literary, biographical or life-history diaries in that they are systematic and based on externally defined categories. In this they resemble expenditure diaries. The activities to be recorded were coded in terms of three elements: contexts, acts and accompaniments. For each diary session, letter codes were assigned having the following form: a root code referring to a specific kind of sexual behaviour, a prefix referring to its modality (what Coxon [1988] describes as the 'binary, directional relational specification of the act'), and a suffix designating the outcome of the session.

Coxon notes that in using such methods there is a trade-off between the complexity of the coding scheme and the willingness of
respondents to use it. The codes used were mnemonics comprising the initial letter of natural language terms referring to a variety of sexual activities. The subject codes were included in the diary, although respondents did not necessarily use them. The codes, which are machine-readable, are capable of being read and manipulated by a database management program. Coxon concludes that the strategy used is sufficiently structured to allow direct comparison between records. At the same time, it is sufficiently life-like to be used easily by respondents, and produces easily retrievable data.

Zimmerman and Weider (1982) describe what they call the diary-diary interview method. They developed this method in a study of the counterculture as part of an ethnographic research strategy they refer to as ‘tracking’. They argue that within a given setting participants may be engaged in a wide range of roles which may contain within them a number of diverse aspects. Moreover, participants' activities are also temporally, spatially and interactionally diverse. Ethnographers face the problem of keeping track of the diversity of roles and activities in ways which avoid reactivity, the ethical problems which arise from covert research, and their inability to be everywhere at once. The rationale behind the diary-diary interview method is to use the informant as an observer of the social scene in which he or she participates. By this means the researcher is allowed a vicarious entrée into the temporal, spatial and interactional facets of subcultural behaviour in natural settings. Informants were requested to complete a detailed diary over a relatively short span of time (seven days). The diary was then made the basis for a lengthy unstructured interview with the diarist. The diaries had a ‘Who?’, ‘What’, ‘When’, ‘Where’, ‘How’, format. In other words, in their diaries informants reported on daily activities, the relationship they had to other people who were involved, described the location, timing and duration of their activities, and the logistics involved in carrying out the activities. Detailed questions were then prepared on the basis of the diaries and used as the basis for depth interviews with informants. Zimmerman and Weider argue that, using the diary-diary interview method, they were able to detect stable and recurrent patterns of culturally sanctioned social organization in the countercultural world they were studying which would have been difficult to uncover by other means.

From these examples, it would seem that diaries have a role in the study of sensitive topics. Their application in such circumstances remains constrained, however, by the problems normally associated with their use (see, e.g., Moser and Kalton, 1971, 340-1). Those who agree to keep diaries or logs may differ in their social characteristics from those who do not. There may be reactivity effects. People, through keeping a diary, may become aware of their behaviour, and in consequence change it. Finally since diary-keeping can be onerous, there may be problems of sample attrition.

Conclusion

Cannell recounts that one early source of interest in interviewer effects derived from a study of the destitute carried out during the Great Depression (Cannell, 1985; Cannell and Kahn, 1968). The researcher, Stuart Rice, noticed that while one of his interviewers consistently attributed destitution to economic factors, another emphasized the adverse effects of alcoholism. Rice, on talking to the interviewers, found the first to be a socialist, and the second an advocate of Prohibition. Cannell comments, 'This suggested that the causes of bias are in some way located in the interviewer, and the whole concept of interviewer bias ran widely through the field' (1985, 4). Improved interview training and procedures have muted that worry somewhat for the modern survey researcher. It seems, however, that interviewer effects are not independent of survey topic. Interviewer characteristics as well as the concerns interviewers have about the questions they have to ask apparently affect the validity of survey responses. As Graham, a critic of the survey interview, has pointed out: 'Surveys, precisely because they conform to the rules of the public domain, have played an important part in raising the consciousness of those within the scientific and political world' (1983, 146). This has been particularly so in the study of sensitive topics, because questions relating to the incidence of 'sensitive' behaviour, and its distribution, are not unimportant. Even so, it is clear that standardized interviewing allows experiences to be expressed in only narrow and truncated ways, and within a social context that some have found ethnically dubious. At best, this is unsatisfactory and points to the need for alternative methods of asking questions about sensitive topics. It should be clear, however, that alternatives like the depth interview are not themselves without difficulty.

Notes

1. This, of course, was not the only function of the confessional. It helped to reinforce the social distance between priest and penitent and to minimize physical contact between priests and female parishioners (Bossy, 1975; Hepworth and Turner, 1982).
2. Research on interviewer effects is bedevilled by methodological problems. As Martin (1983) points out, the demographic character of field forces has changed over
The years, and the training of interviewers has improved (Bradburn, 1983). This means that research in this area may be more than usually historically specific. Differential turnover rates for different kinds of interviewer produce systematic differences in experience levels which confound comparisons based on interviewer characteristics. In addition, statistical comparisons are also frequently hazardous because interviewing assignments are usually clustered. This means that comparisons between interviewers are not, strictly speaking, based on independent trials (Collins, 1980).

Field research, based on qualitative methods such as participant observation or depth interviewing, has often seemed like the method of choice in studying sensitive topics. Thus, while applied social research in the United States has traditionally had a quantitative character, federally funded community-based research on AIDS has relied heavily on ethnographic methods (Kotarba, 1990). As Kotarba points out, qualitative researchers were able to gain access to at-risk populations, such as prostitutes, who were difficult to locate because of their social invisibility and deviant social status. The use of ethnographic methods in contemporary AIDS research parallels in many ways the prominence of qualitative research during the 1960s and 1970s in studies of urban social problems like drug addiction, juvenile crime and poverty. Although few of the issues involved were entirely new — a number, for example, had been aired by William H. Whyte (1955) in Street Corner Society — much of the methodological writing associated with this earlier work dealt recurrently with the conditions under which trustful relations could be established with deviant or disadvantaged subjects.

This chapter looks at a particular aspect of that process, the methods researchers use to enter settings in order to carry out socially sensitive research. Despite an extensive literature which has grown up around the topic, there are a number of difficulties if one tries to look in more detail at the processes involved in gaining access. One is the suspicion voiced by some writers (Fielding, 1982; Polsky, 1971) that much of the discussion on the problems of gaining access in fact hides a reluctance to go out and face those problems by actually entering the field. A further problem is that there is usually little incentive for a researcher to dwell on the reasons why a request to grant access was refused. The pressure to move on and seek entry to another site is usually too great. Nor are researchers necessarily wise while in the field to look too closely at why access has been granted. In so doing they might raise further and unwelcome questions in the minds of gatekeepers about
Covert, Adversarial and Collaborative Research

The importance attached to trust in the fieldwork literature flows from assumptions about both the nature of field research itself and the preconditions necessary for its successful completion. Field researchers assume that social and cultural worlds must be explored in a naturalistic manner (Rock, 1979). As Matza (1969, 24–5) points out, naturalistic research, especially in the study of deviance, requires first-hand contact with those studied, and a commitment to an 'appreciative' understanding of how those studied define their situation. However, neither contact nor appreciative understanding is necessarily won easily. The successful management of fieldwork 'depends not only on what the researcher says and does, but also upon the willingness of the observed to sustain the presence of such a marginal member in their midst' (Pollner and Emerson, 1983, 236). In this situation, trust is seen as a condition for continued participation, entry into closed spheres of interaction and the dissolution of deceptive self-presentations.

It is possible to challenge this view of trust in two ways. First, as Henslin (1972) points out, researchers may assume they have built up sufficient trust to ensure they are not being deceived. However, it is never possible to be sure that this is so. There are instances, for example, where deviance has been uncovered which had apparently been missed by other researchers (see, e.g., Ditton, 1977, 10). Writers like Henslin have therefore argued that to avoid being duped researchers studying sensitive topics should operate in a covert manner by posing as a member of the setting they wish to study. Second, it has also been argued that traditional conceptions of field relations are based on a consensual view of the world. According to Jack Douglas, this reflects what he calls the 'small-town, Protestant public morality of openness, friendliness and do-gooderism' typical of research in the Chicago tradition (1976, 47). However, both researcher and researched are embedded within relationships of power and socio-economic inequality. From this it can be argued that the researcher should define those studied as political adversaries or allies, and adopt a conflict-based or collaborative fieldwork style accordingly.

Those who advocate deceptive or power-conscious methods typically see the researcher as an unwanted outsider. To this they couple the view that much of what is worth knowing about social life is surrounded by conflict and/or hidden from view. To conduct research of any relevance, therefore, is inevitably to deal with sensitive topics requiring special, and not always conventionally well-regarded, means.

Covert research

Research is covert when research participants are not aware that they are being studied. A primary justification for covert research is that it avoids problems of reactivity. Because they do not know they are being studied, research participants are not threatened by the research and do not change their behaviour even though to outside eyes it might be regarded as deviant. Covert research is also said to offer an 'inside view' of those being studied. The overt researcher only ever 'speaks about' the insiders' view of the world rather than 'speaking from' that point of view (Henslin, 1972, 46–7). In the case of sensitive topics, however, 'speaking from' may be important because the threat posed by overt research to those in the setting often precludes the easy attainment of inside knowledge.

Despite its apparent advantages, questions have been raised both about the ethical standing of covert data collection, and about its efficacy. Covert studies violate important ethical principles (Bulmer, 1982a). In particular, they negate the principle of informed consent since research participants in covert studies cannot refuse their involvement. Covert studies may involve deception, and frequently cannot be carried out without invading the privacy of those being studied. It is also possible to argue that research using covert methods has longer term undesirable consequences. Such studies may encourage cynicism among social scientists in relation to the rights of research participants, while the wider public may come to distrust researchers (Erikson, 1968).

Social scientists have taken three broad positions in relation to the ethics of covert research. These can be described as absolutist, pragmatic and sceptical. Ethical absolutists hold that the difficulties outlined above fatally compromise covert methods of research. Such methods, therefore, cannot be used ethically. Reynolds (1982) points out that the absolutist position is based on a particular conception of professional conduct. This enjoins researchers to use 'techniques that are models of good behaviour' and actively implies.
that researchers should try to serve as 'moral exemplars' in their profession.

The pragmatic position recognizes the difficulties associated with the use of covert methods. It accepts the need to protect the rights of research participants and the obligation not to harm them, but sets these considerations against the need to obtain scientific knowledge. Covert research may therefore be regarded - reluctantly - as an acceptable method where the study is not a trivial one and there is no other way for the necessary data to be obtained. In this conception the researcher effectively adopts a utilitarian position, evaluating the costs to the participants of a particular study against the benefits which may accrue from it. Logically, of course, the calculus should also include the costs of not pursuing a particular topic (Rosenthal and Rosnow, 1984).

The sceptical position accepts that, far from being used reluctantly, there is in fact positive justification for covert study. In this view refraining from research which it is not conducted openly favours the powerful at the expense of the powerless. Requiring the co-operation of research participants means that, as Galliher (1973) puts it, 'the only secret information obtainable is from individuals and groups who are too ignorant and/or powerless to demand the necessary limitations upon the researchers'. As a result, sociology comes to know a great deal about the lower levels of society, but not about élite groups. In the process sociologists accept as credible the self-presentations of the powerful.

Sensitive topics have a pivotal relevance for the debate about covert methods. Reynolds (1982) points out that choices about the kinds of research methods to be used depend in part on the state of knowledge in a particular field. For example, structured methods such as survey research can be used where knowledge about an aspect of social life is already widely available or can be obtained in an open manner. Where little accurate or relevant information is available on the topic in question, field research involving access to a closed setting might be a more appropriate method. Areas of social life which are 'sensitive' fall more often into this second category than they do into the first. As Reynolds further notes, the absolutist position potentially restricts research to topics which would be regarded as inoffensive. Such a restriction would make it impossible to do at least two kinds of 'sensitive' research. First, research could not be done in which the presence of a researcher is not simply reactive, but potentially catastrophic in the sense of dissolving completely the activity of interest. This was in part the justification used by the late Laud Humphreys for his covert study of impersonal homosexual activities in public restrooms. As he comments, to go into a 'tearoom' as an undisguised observer 'would instantly eliminate all action except the flushing of toilets and the exiting of all present' (1970, 25). The second area which would be threatened are studies which look explicitly at the disjunction between public rectitude and private misdeed. Thus, Fielding (1981) in his partly covert study of an extreme right-wing political party was interested in the disjunction between its public face and the privately displayed attitudes of its members towards political violence.

If the absolutist position would disallow covert research on ethical grounds, research on sensitive topics also poses problems for the pragmatic and sceptical positions. Covert research has often been justified on the grounds that the benefits which flow from greater scientific knowledge outweigh the potential harm to research subjects. Difficulties arise, however, because there is no consensus about the benefits of research (MacIntyre, 1982). Apparently disinterested assertions about risks and benefits may in fact be self-serving. The researcher has greater power than those studied to define what might count as a risk or a benefit and the temptation to find spurious benefits may be particularly strong where a research topic is sensitive or in some way difficult to study. Unfortunately, there are in fact very few empirical studies of ethical decision-making in the social sciences (Stanley et al., 1987). As a result it is difficult to know with any certainty how often or in what circumstances researchers have chosen not to pursue a covert study.

Those who are sceptical of ethical proscriptions on covert research point to the sensitive character of research on the powerful. Powerful groups not only often have the means to deny access, they are also more likely to do so if they feel the researcher is set to expose their misdeeds (Cassell, 1988). This argument has been somewhat undermined by the experience of Galliher, a prominent proponent of the sceptical position. Although he is distrustful of ethical codes and quite prepared to use covert methods, Galliher (1980) notes that he has never had to use deception in his research. Indeed, he was surprised to find that he was often told discrediting information by élite informants. Galliher speculates that his informants were so convinced of the rightness of their views, that they did not find his investigation threatening.

Bulmer contends that the proponents of covert research have exaggerated its usefulness, and questions in a general way the extent to which covert research yields better data. He points (1982b), for example, to researchers such as Caudill (1958) who did similar studies using both covert and overt means, but who concluded that overt research provided better coverage and better data. In the
pseudopatient study described by Rosenhan (1973), other patients penetrated the deception, even though clinic staff did not. Killian (1981) has argued, for example, on the basis of his own experience as a psychiatric patient, that pseudopatients over-identify with the patient point of view, and so do not get a rounded view. The slights and oppressions of the institution are impressed upon them, he suggests, precisely because they are not patients. Real patients, according to Killian, perceive apparently uncaring procedures in a different light, detecting in them potential therapeutic benefits.

Bulmer argues (1982a; 1982b) that it may be difficult actually to carry off a covert role since operating in a covert manner can create practical difficulties for the researcher. For example, in 'pseudopatient studies', where a healthy researcher feigns illness in order to study the treatment process, the researcher may risk involuntary treatment or incarceration (Bulmer, 1982b). The consequences of revelation can also be serious. Feelings of outrage and betrayal on the part of those studied typically follow the disclosure that a covert study has taken place.

In the study of sensitive topics the practical difficulties involved may vitiate the apparent advantages of covert research. Self-consciousness about the need to maintain 'cover' may interfere with the data collection process (Weppner, 1976). The covert researcher may be unable to ask probing questions in the way that an overt researcher might. In fact, even quite simple questions might be difficult to ask if they concern matters an insider would be expected to know about (Henslin, 1972). It can be difficult to assess how far those in the setting are taken in by the deception. Researchers may unwittingly give off clues which alert those being studied to the possibility that they are being fooled. Ditton (1977), for example, learned after his covert study of 'fiddling' in a bakery was completed that some people had avoided him because they felt that in some way he did not fit into the setting. Particularly in organizations, it may be difficult for a covert researcher to move around without inviting suspicion. This can be particularly so where one is a low-status participant who may be denied access to higher echelons. In some settings it may be difficult to abstain from deviant activities or to avoid pressure to align oneself with one or other faction in a contentious situation. Thorne (1983) records that, although she wanted to observe several different groups, she was forced to take sides among the factions represented within the movement she studied. Thorne's work also provides a reminder that in some settings concern about possible secret identities is endemic. The draft resistance movement studied by Thorne was fearful of infiltration. Such fear induced constant nervous tension among partici-

pants which gave rise to the ritual of 'Fed-guessing' in which attempts were made to identify potential infiltrators. Thorne was strongly suspected of being a Federal agent, apparently because her research activities precluded her from becoming a complete participant.

Investigative social research

In his book Investigative Social Research Jack Douglas (1976) is critical of what he refers to as the 'classical co-operative paradigm' of field research which originated among the Chicago school sociologists. Douglas acknowledges that the Chicagoans clearly recognized the conflictual character of social life but argues that their approach to research was based on an assumption of community in which society was viewed as homogeneous and non-conflictual. In contrast to the co-operative paradigm Douglas adopts what he calls 'a clearly defined paradigm for people of practical affairs. This is the investigative paradigm' (1976, 55). Douglas sums up the fundamental principle of the investigative paradigm with the phrase, 'Conflict is the reality of life, suspicion is the guiding principle.' In other words, conflicts over interests, values, emotions and activities are endemic in social life. People have reason to hide from others what they are doing, and, as a result, there are good grounds for suspecting whatever it is they say. Another way of putting this is to say that, for Douglas, even ostensibly innocuous topics are more than likely to be sensitive once the surface façade has been penetrated. Douglas does suggest that co-operative and investigative paradigms can be used in tandem (1976, 56-7) but that investigative strategies remain essential as 'a necessary response to the pervasive secrecy, the lies and hypocrisy in society' (Bok, 1984, 246).

Investigative research combines a rugged, combative style of field work with a specific organizational form, that of team research. Although few of the tactics Douglas advocates are entirely unknown in research, it is their welding together into a paradigmatic method underpinned by an organizational structure and an epistemological warrant which constitutes his primary innovation. Couched in suitably aggressive language, the major tactics used in investigative research include, according to Douglas (1976, chap. 8), the infiltrating of the setting to be studied using covert methods if need be, and the 'setting up' and 'opening up' of informants. Researchers may then 'wrench the truth' (1976, 176) from those in the setting using what Douglas describes as 'adversary and discombounding tactics' to confuse, disorient and wrong-foot them. Douglas acknowledges
that, because such tactics are often counter-productive, they are usually weapons of last-resort. However, even in their absence, 'drawing out and phased assertion' tactics can be used in which the researcher asserts a potentially discrediting interpretation of an informant's behaviour in order, hopefully, to produce a confessional response.

Douglas stresses that a team approach is central to the use of investigative methods. Team research provides multiple perspectives on the research problem, multiple vantage points from which to attempt access and to assess the validity of accounts, as well as a range of research roles and skills. Much of what Douglas says about the advantages and disadvantages of teams is unexceptional (see Yancey and Rainwater, 1970, for a further discussion of team research in a difficult setting). He does, however, point to the difficulties of using teams in conflict situations. The team as a whole may be misled if one member or portion of the team is co-opted by one faction in a conflict. There is also a possibility of mistrust developing within the group - a somewhat ironic occupational hazard, presumably, since distrust forms the raison d'être of the investigative approach.

The investigative paradigm has met with an adversarial response from other social scientists. An obvious objection to investigative research is that its 'frondbusting' style, while effective, may be counter-productive in the longer term. Researchers who use such methods may meet increased resistance and those who come after them may be denied access in the future. A further difficulty is that the inherent scepticism which lies behind the investigative paradigm may harden into cynicism and a contempt for those studied. Citing Douglas as an example, Van Maanen has commented that:

In the hands of some sociologists confessions read like debriefings after a battle in the combat zone with accounts of how informants were bullied, how tactics of coercive persuasion were employed, and how the weaknesses, disunity, and confusion of the natives were exploited. (1988, 98)

Punch (1986, 42) has argued in a similar way that the investigative position might produce a kind of reverse pride in breaking codes of ethics. The difficulty, of course, is that if one is to allow some groups in society to be exempted from ethical strictures because they are thought to have something to hide, then the scene is set for potentially endless debates about which groups can be exempted and which cannot.

Douglas makes much of the Chicago sociologists' failure to study the powerful or to cover systematically in their research a wide area of society. In his view the Chicago sociologists left little more than a series of discrete studies produced by individuals, each of whom studied a separate group. One can question, of course, Douglas's characterization of Chicago school sociology. Much of the recent critical reappraisal of Chicago sociology suggests that its research practice has been mythologized (Harvey, 1987; Bulmer, 1984; Platt, 1983; 1985). But even if Douglas is correct, the investigative paradigm does not seem to be superior in this regard. We seem to be left with a series of discrete studies of (more or less discredible) settings, but this time produced by teams rather than individuals. The difficulty here seems to be that investigative research is underpinned by a certain ambiguity. One basis for the approach lies in Douglas's assumptions about the inherently conflictual nature of personal experience itself (see also Adler and Adler, 1987, 21-4). On the other hand, the investigative stance might also be underpinned by a structural critique of society. Such a critique would seek to dismantle the fronts and evasions by which powerful institutions hide their activities. While Douglas clearly does not exclude this last approach, the sites of his research, which include naturist beaches and massage parlours, suggest a concern that is more rhetorical than it is radical. There seems no basis, apart from opportunistic access, for choosing these particular settings rather than some others, or for asserting that a series of such studies will aggregate in some way to produce an overall picture of society.

Douglas (1978) has argued that professional ethics are primarily a device for professional aggrandizement. In the preface to Investigative Social Research he is openly scornful of ethical debate:

one might expect that I would deal at length with the tortured moral arguments posed by the revelations in this book. I considered doing so, but the thought made me chuckle. In the end my mirth became uncontrollable and drowned out the considerations. (1976, xiv)

To the extent that he does discuss the ethics of investigative research, Douglas seems to adopt two positions. One is that, since deception is pervasive in social relations, researchers should not be denied ways of acting open to everyone else. The second is that the benefits which accrue from investigative research outweigh the damage caused. Bok (1984) has challenged Douglas's ethical insensitivity. First, she questions whether one can use the perverseness of deception as a basis for investigative social research. It may simply be erroneous to assert that people lie. Second, knowing that someone lies no more justifies lying than knowing that someone is a thief justifies stealing from that person. Third, Bok questions justifications for investigative social research in terms of the benefits produced by increasing knowledge. As she rather acidly comments,
such defences 'may become more tempting the less justification their work seems to command on its own' (1984, 247). Furthermore, she argues that social scientists do not have the legitimacy to engage in investigative research. There are bodies - the media, law enforcement agencies, for example - which are better placed to do investigative work, are seen to have a legitimate role, and which, moreover, tend to do a better job than social scientists.

Douglas has undoubtedly added to the methodological repertoire. Used sparingly, many of the strategies he describes can usefully be deployed in difficult situations. It seems unwise, therefore, to dismiss the investigative approach out of hand. Yet nagging worries remain about the ethical implications of the investigative style and one is left with the feeling that the whole is rather less than the sum of the parts.

Radical research

Some writers have argued that research is powerfully shaped by the structured inequalities in society. As a consequence, the attention of researchers has been directed and channeled away from the powerful, whose interests are threatened by open scrutiny, and towards the powerless, who must bear the burden of social inquiry. These writers argue that researchers should cease to collude in this situation and should adapt their methods to reflect their political stance towards those being studied. On the one hand, conflict methodologies should be adopted to study the powerful. On the other, researchers should work in a collaborative way with the powerless in order to serve their interests.

Conflict methodology

As Robert Emerson notes, investigative social research and conflict methodology 'rest on similar adversarial premises, highlighting the inherent conflict between dominant institutions' control of information and researchers' needs to obtain data' (1981, 367-8). T.R. Young, who apparently coined the term (Christie, 1976), defines conflict methodology as 'comprising those techniques by which information is obtained from and introduced into systems under conditions of hostile contrast' (1971, 279). The term 'hostile contrast' apparently refers to situations in which powerful groups and organizations deliberately withhold or distort information which would serve the wider public interest. In what can probably be regarded as the manifesto for conflict methodology, Lehmann and Young (1974) argue that, by the 1960s, American sociology had reached paradigm crisis. Existing structural functionalist theory, with its emphasis on consensus and system integration, could not easily accommodate evidence concerning the growing power of corporations, political malfeasance, the conflict over Civil Rights and the war in Vietnam. For Lehmann and Young, an alternative conflict paradigm, which recognized the ubiquity of social conflict and change, was emerging to overthrow functionalism. Theoretical change, however, also required methodological reorientation. Research based on survey research, opinion polling and attitude measurement provided a technocratic elite and the military industrial complex with instruments of surveillance and control. The conflict paradigm would only be complete, according to Lehmann and Young, when 'new technologies of research are invented and used by which to breach the barriers to quality information erected by a secretive administration, a conspiring oil industry, an ambitious war department, or a slick commercial organization' (1974, 21).

According to Lehmann and Young, the adversary principle which underpins the Anglo-Saxon legal system provides a basic model for the practice of conflict methodology. This principle assumes that the contending parties to a dispute are partisans whose accounts must be subjected to hostile scrutiny. Like other writers (e.g. Marx, 1984), Lehmann and Young (1974) point out that the courts provide a source of data for social scientists. Thus, researchers can become litigants, using lawsuits, injunctions and freedom of information legislation to secure data about the operation of large bureaucratic agencies and corporations. Lehmann and Young go beyond this, however, by extending the legal analogy to the possible organizational role of the sociologist. Sociologists, in their view, can work alongside radical lawyers to provide various forms of advocacy. These might include 'class-based advocacy' to defend the rights of powerless groups, 'selective advocacy', challenging official decisions and practices, and 'organization advocacy' of the kind seen, for example, in civil rights organizations. Related to this, Lehmann and Young also argue that sociologists need to be involved in the development of radical strategies for community organization and mobilization. What they seem to envisage here is that the sociologist would provide information to community organizations in order to overcome imbalances in the provision of information, which tend to work in favour of powerful organizations.

Lehmann and Young point to two further sources of data which might be used by conflict methodologists. Again, like Marx (1984) and Molotch and Lester (1974; 1975), they advocate close attention to incidents like environmental disasters or political scandals. In such events the hidden face of corporate and bureaucratic power and irresponsibility is revealed. Lehmann and Young also suggest
that conflict methodologists use the kind of ethnomethodological 'demonstrations' proposed by Garfinkel (1967). These 'pique, probe, provoke and puncture normal situations of interaction to uncover the rules and rituals participants take for granted' (Lehmann and Young, 1974, 23).

There is some dispute about the character of conflict methodology. Some writers, like Christie (1976), regard conflict methodology as one element in a wider, political, epistemological and methodological critique of positivistic social research. Others (e.g. Lundman and McFarlane, 1976) see it as a more or less eclectic set of techniques distinguished by a revelatory goal, and an unwillingness to extend the principle of informed consent to powerful actors. Thus, Lundman and McFarlane question the extent to which conflict methodology can be tied to a particular theoretical position. While Lehmann and Young see it as an aspect of conflict theory, others sympathetic to conflict methodology have roots in diverse theoretical traditions including symbolic interactionism (Lundman and McFarlane, 1976, 509-10). Part of the confusion here may arise simply because conflict methodology was one strand in a developing anti-potivistic critique which at the time was still relatively inchoate. This is perhaps most clearly seen in the conflict methodologists' championing of ethnomethodology. Drawing a parallel with certain kinds of political theatre, Gouldner (1970, 394) notes that ethnomethodological 'demonstrations' had an appeal to campus activists because of the challenge they posed to the 'way things are'. However, the ethnomethodological tradition soon left behind such 'theatre'. Particularly as represented by conversation analysis, ethnomethodology proved radical only in its principled indifference to politics.

Lundman and McFarlane (1976) have presented a series of objections to conflict methodology. First of all, in work of this kind a tension exists between research and advocacy. Indeed, it can be questioned how far the adversary principle in law provides an appropriate analogy for research. The adversary principle assumes that the outcome is not prejudged, but is determined solely on the basis of the evidence. However, in many respects, conflict methodologists prejudge the issue of whether powerful organizations have a malign influence on society. As Cassell has commented, 'Outrage as a research stance can be as blinding as unquestioning admiration' (1988, 102). Second, the necessity for 'appreciation' poses problems for conflict methodology. That is, it is often difficult to understand the attitudes and motivations of a particular group from the outside. As pointed out earlier, even with 'unloved' groups (Fielding, 1990), whose position is radically at odds with the researcher's own value premises, it may still be necessary to take on the difficult task of entering their world via a minimally non-adversarial stance (Cassell, 1988). Third, as will be seen shortly, Lehmann and Young's call for involvement in community activism echoes that made by a number of other writers of the period. Such activism may, however, undermine conflict methodologies where collaborative relations with the powerless take precedence over adversarial relations with the powerful. In addition, as Hessler et al. (1980) note, the longer term aims of the conflict methodologist may well diverge from those of the researched. Research participants, as a result, are capable of being treated in an instrumental way. A fourth difficulty pointed to by Lundman and McFarlane in their assessment of conflict methodology is that research in this tradition does not cumulate; systematic accumulation of knowledge about society takes second place to radical political action.

Lundman and McFarlane further suggest that conflict methodologists have a simplistic conception of power. Focusing primarily on the malfeasance of large-scale bureaucracies may ignore power that is located elsewhere or is exercised in not entirely obvious ways. Murray Wax (1980) notes that the targets of adversarial research are sometimes caricatured in ways which lead researchers to assume the need for an adversarial style when it may not be necessary. Wax also argues that superordinates and subordinates are relative. Those regarded as superordinates in a particular situation may in their turn be subordinated. Ignoring this fact may mean exposing lower level power relations, but not those at a higher level (see also Gouldner, 1973). Finally, Lundman and McFarlane note that debates about conflict methodology have gone on in academic journals. As a result, the products of conflict methodology have not found their way to wider publics. Redressing the information balance in favour of the powerless, they suggest, may require novel methods for disseminating the products of research based on the conflict model.

While the proposal to adopt a conflict methodology was of its time, how far it seized that time (to use a contemporaneous cliché) is open to question. The longer term impact of the conflict methodologists can be examined using citation counts for Young's initial article (1971) and Lehmann and Young's (1974) article taken from the Social Science Citation Index. Young's article received a total of 20 citations between 1971 and 1989. Lehmann and Young received the same number of citations between 1976 and 1989. Although this level of citation is respectable (Oromner, 1977), citations to both articles had declined to zero within a decade. To assess how far the call to establish a new paradigm actually affected
research practice, articles citing the Lehmann and Young article were examined. Conflict methodology does not seem, however, to have reshaped empirical research. One article which could not be traced and two ephemeral sources (one a letter, the other a book review) were excluded. Of the 17 articles remaining only two even made reference to empirical material. Discussion of conflict methodology, in other words, remained at a programmatic level. Looked at with the benefit of hindsight the proposal to establish a conflict methodology did not lead to a new paradigm.³

Although one can challenge the premises and the impact of conflict methodology, the question remains: how effective are the methods themselves? Starting from a rather different theoretical standpoint, Gary Marx (1984) has reviewed a number of techniques which help researchers circumvent the control of information by powerful others. For Marx, the ability to uncover hidden information in modern societies depends on a variety of complex organizational, technological and legislative processes. Social scientists, he suggests, need to adapt such processes to their own purposes.

According to Marx, hidden information is yielded up in four broad ways: through deception, coercion, volition or the operation of uncontrollable contingencies.⁴ Deception includes not only covert research of the kind already discussed, but also what might be called ‘research scams’. The parallel Marx draws here is with investigative journalism and police undercover operations where investigators take on assumed roles, for example, as bribe-takers, in order to uncover deviance or malpractice. Similar techniques have sometimes been used in social research to uncover discriminatory practices (see, e.g., Selznick, 1955). Marx suggests that a variety of factors makes the use of deception problematic for social scientists. Some of these, including the obvious ethical difficulties, are well-known and have already been rehearsed. He also, however, contrasts covert research as it has traditionally been practised in the social sciences with the ‘sting’ operations conducted by law enforcement agencies. The former have typically been small-scale, of short duration and have used single investigators. The latter, on the other hand, often carry on over a lengthy period of time and may require extensive funding and personnel. Marx implies that resources of this kind have advantages in terms of the breadth and depth of the information they produce. In a one-person study, cross-observer data are not available for the purposes of validation. Where substantial resources are at hand, more complex deceptions can be mounted. This may in its turn increase the danger of entrapment, but if successful it typically yields massive levels of disclosure.

Coercion refers to what Marx (1984) terms ‘institutionalized discovery practices’, where individuals and organizations are compelled to provide information under the threat of legal sanction. Like Lehmann and Young, Marx suggests that researchers use such occasions by gathering data from investigative and legislative hearings and from court records. He also advises researchers to make more use of the information professional groups are often required to file on a routine basis; for example, registers of members’ interests in legislative bodies. It is difficult to judge how representative the material produced through such discovery practices might be, and steps may have been taken to ensure that certain kinds of information did not enter the written record. The researcher inevitably in these situations is reliant on what information is made available and therefore takes a passive role. In order to use this kind of data effectively, the researcher must know what to look for. According to Marx, there are few ethical problems in using information provided by institutional discovery. The cost of obtaining data in this way is relatively low. In the United States, researchers based in ‘an educational or noncommercial scientific institution whose purpose is scholarly or scientific research’ are only charged standard fees for the duplication of documents produced under the Freedom of Information Act (Walter and Adler, 1991). However, although some social scientists have used the Freedom of Information Act (see, e.g., Pendakur, 1985, and Coleman and Seligman, 1988), relative to historians, they have been few in number (Relyea, 1989, 65).⁵ (Indeed, it seems that most use of the Act is made by businesses seeking information on competitors [Birkinshaw, 1988; Relyea, 1989].)

In part what Marx has in mind when he refers to ‘volition’ are standard social research methods for eliciting potentially embarrassing or discrediting information: anonymous self-report methods and field research based on overt methods. He also suggests, however, that ‘whistleblowing’ can provide a source of data. Whistleblowers draw attention to dangers or abuses which exist within the organization for which they work. Marx also mentions what he refers to as ‘give aways’. Here someone who discloses information does not recognize its import when used on its own or in conjunction with other information. Marx suggests that whistleblowing is becoming increasingly common. At least in the United States, this has been encouraged by technical developments like confidential hotlines and through legal changes which, in areas like the misuse of toxic substances, encourage and protect whistleblowers. The sources of whistleblowing behaviour sometimes lie, according to Marx, in the disjunction between personal idealism and the realities of organiz-
ation life, but they may also emerge from personal spite or vindictiveness. These are factors which need to be taken into account when assessing the quality of the data drawn from whistleblowing incidents. An obvious difficulty with whistleblowing as a data source is that it is the whistleblower who determines what information is made available.

Uncontrollable contingencies are accidents or mistakes which reveal underlying social patterns. Marx distinguishes two categories: traces and mishaps. Traces are residues and remnants of the kind described by Webb et al. (1966; 1981), but which point in this case to the hidden or the diabolic. Mishaps are inadvertent happenings which bring to light undisclosed deviance. Thus, technological accidents, like oil spills, and political scandals, such as Watergate, provide strategic research sites which allow researchers to reveal those activities of organizations and elites normally hidden from public view. Echoing a point made by the conflict methodologists, Marx notes that mishaps shatter the façade of normality and provide an ironic counterpoint to official ideologies by revealing the underlife (Goffman, 1961) of institutions and personal lifestyles.

Marx points out that uncontrollable contingencies, 'Strictly speaking, ... offer an opportunity rather than a strategy, for data collection' (1984, 86). In other words, uncontrollable contingencies are probably more useful for discovering the existence of data than for systematically exploiting it. Since much of the material associated with uncontrollable contingencies appears in the public domain, there are few ethical issues raised by its use. It is also, relatively speaking, a low-cost method. However, relying on accidents and scandals as a source of data suggests that the role of the social scientist is an essentially reactive one. Research agendas thus become shaped by events rather than by a systematic logic, and research studies do not cumulate (Lundman and McFarlane, 1976).

Collaborative research
There is a sense in which until comparatively recently the cultural and socio-economic margins of Western societies provided researchers with a series of more or less 'exotic' locales, much as earlier the imperial empires or their remnants served anthropologists (Hughes, 1976). While this was undoubtedly convenient for the researcher, it eventually began to have a cost. Marginal groups came to feel increasingly threatened by the attentions of researchers. As a result, by the late 1970s in the United States, and rather later in Britain, resentment began to close deprived or minority communities to outside researchers. The concerns and complaints of minority communities concerning research are remarkably consistent across a range of groups and research situations. Researchers are often seen as predators (Maynard, 1974) or as sojourners (Trimble, 1977). Research, in other words, serves as a vehicle for status, income or professional advancement which is denied to those studied. Blauner and Wellman (1982) note, for example, that it is not unknown for residents in some ghetto areas of the United States to complain wryly that they have put dozens of students through graduate school. Alternatively, researchers come, take what they can get out of a community and they, or the results of their research, are never seen again. In this situation, it is easy for minority communities to feel that there is nothing in the research situation for them. As Joan Moore, speaking about research on Mexican-Americans, puts it, 'As with many other groups, the disinterested pursuit of science is not a particularly meaningful goal to most Mexican-Americans. Some of our predecessors made it even less meaningful' (1967, 232).

Such concerns have produced calls for a collaborative or participatory research style (Blauner and Wellman, 1982; Vargas, 1971; Kelman, 1972; Ben-Tovim et al., 1986). (Such calls have also been a feature of feminist writing on methodology, although here the focus has primarily been on the interview situation [see, e.g., Oakley, 1981; Finch, 1984].) Hessler and his colleagues, for example, describe the development of a 'research commune' as a way of empowering people in the community which they studied (Hessler and New, 1972; Hessler et al., 1980). In a similar way, Schensul (1980), describing his work on a number of community mental health projects, has advocated the use of 'Community Review Boards', which seem to serve a similar function. Researchers work closely with community activists and attempt to 'facilitate indigenous social action programs by supplying data and results which could make significant contributions to the effectiveness of residents' efforts' (Schensul, 1980, 313). The community itself helps to structure the research and develop research questions, and community residents themselves can be trained in research skills. This gives the community a continuing capacity to carry out its own research and can provide jobs not otherwise available (Blauner and Wellman, 1982).

On a practical level there are presumably some situations in which it will be difficult to adopt a collaborative research style. The approach seems to depend on the existence of a clear and functioning community structure. In some cases, however, this may simply not exist (Beck, 1970). More widely, the collaborative approach is open to attack from opposing ends of the spectrum. Commenting on Hessler and New's attempts to establish a research commune,
Gladwin (1972) argues, for example, that what they have done is not radical enough. Offering social science expertise in return for access and involvement does not address wider issues of exploitation. Indeed, it directs attention away from 'the real issues of differential power and access, and how to redress the balance' (1972, 453). James Walsh (1972) argues, on the other hand, that accepting the local community's definition of what should be researched tends to produce atheoretical research in which crucial variables are inevitably overlooked. Goudy and Richards (1973) have also been sceptical of collaborative research. While they agree that researchers may have exploited those they study by ignoring their social needs, they argue that sociologists have also been insensitive to the political needs of those in disadvantaged communities. As a result they have 'overresponded' to indigenous definitions of research problems. One source of this overresponse, according to Goudy and Richards, has been the assumption of consensus at the local level. However, this may in fact hide a plurality of competing interest groups. Such groups frequently present their interests and objectives as co-extensive with those of the community as a whole. To the extent that some groups may be better able to do this than others they may hijack the research for their own purposes by defining the goals of research, intervening in the data-gathering process or controlling the publication or dissemination of research findings. Goudy and Richards contend that where this happens it is the researcher rather than the local community who is exploited.

Jones (1980) has taken a similar position based on his experiences of working with a community action organization in an inner-city area. In the course of his research he charted a growing polarization between the board of directors who controlled the organization and its membership. Although he had initially gained access in part because board members found his political views acceptable, the board attempted to suppress his report. Jones notes that researchers have sometimes drawn too sharp a contrast between disadvantaged groups and the wider society. As a result they have failed to recognize the existence of local internal hierarchies and conflicts. Jones argues that in research with disadvantaged groups the issue is 'not one of taking the side of the underdog, but of taking sides without reflection' (1980, 103). Without an attempt to demystify the local, as well as the wider, political context, one may have difficulty in identifying the underdog. Even then, to be on the side of the local population does not necessitate complete agreement with them. Indeed political commitment may require the taking of potentially unpopular positions which transcend local political rivalries.

Many of the issues raised by urban anthropologists in the 1970s re-emerged in the 1980s with the onset of the AIDS crisis. An inevitably fatal disease with a long period of latency, and transmitted through 'sensitive and secretive' activities including sexual behaviour and intravenous drug use, AIDS has 'explosive epidemic characteristics' (Bowser and Sieber, 1992). The nature of the disease demanded a timely and effective response from social, psychological, biological and clinical researchers. AIDS research, however, inevitably raised complex ethical, political, operational and technical issues. Researchers had to seek participation for their studies from members of marginalized, stigmatized and vulnerable groups, though one of these groups, the gay community, was politically aware and relatively well-organized. In a field where, at first, very little was known and research activity was intense, research could impose considerable, sometimes duplicative, burdens on those who were already desperately ill (Zich and Temoshok, 1986). At the same time, the controversy which erupted around clinical trials of the drug AZT suggested that existing procedures for ensuring the conduct of ethically sound research were inadequate (Melton et al., 1988).

In coping with the dilemmas posed by research on AIDS, researchers have turned to community consultation. The purpose of such consultation, according to Melton et al., is 'to enlist potential participants as partners in solving the difficult dilemmas posed in sensitive research' (1988, 576). Such consultation increases procedural justice by giving potential research participants a degree of control over their involvement in the research. In so doing it is likely also to enhance perceptions of distributive justice. In other words, after consultation, the potential risks and benefits of the research are more likely to be seen as having been fairly apportioned. Consulting the community also provides a means by which community members can voice their worries and concerns, and communicates to them that the researcher has regard for them as persons (Melton et al., 1988; Bowser and Sieber, 1992).

AIDS researchers have used a variety of strategies for establishing collaborative research relations with the communities they study. These range from the adaptation of focus group methodology for collaborative ends (Bowser and Sieber, 1992; M. Singer, 1992) to the establishment of community-based ethical review boards (V. Merton, 1990). Focus groups provide a relatively effective, low-cost and rapid method for developing insights into community concerns (M. Singer, 1992; on focus group methodology more generally, see Krueger, 1988). They also can be used in a manner which treats research participants as 'consultants' rather than as 'respondents' (Bowser and Sieber, 1992). As a result, Bowser and
Sieber argue, the method is 'especially useful in engaging difficult to access, potentially mistrustful and alienated subjects'.

Vanessa Merton (1990) has pointed to the efficacy of a community-based model for evaluating clinical trials of new drugs and therapies for the treatment of AIDS. A detailed account of the ethical issues surrounding such trials is beyond the scope of the present discussion. As Melton et al. point out, however, trials of this kind illustrate some of the potential problems involved in community consultation. Some of these problems are of a kind previously discussed, and relate to the availability, representativeness, and mandate of local groups. Particularly with deviant or marginal groups, it may be difficult to identify a relevant constituency for consultation, or groups that do exist in the potential research setting may be unrepresentative or factionalized. In these situations, ad hoc or surrogate groups of various kinds may have to be established (Melton et al., 1988).

Community consultation provides a means by which potential research participants can be involved in making judgements for themselves about the potential risks and benefits involved in a proposed research project. Making such decisions in groups, however, has hidden dangers. Processes common to group deliberations of any kind may help to diminish rather than enhance protection for research participants. As Melton et al. (1988) point out, sociopsychological research on the so-called 'risky shift' phenomenon suggests that often groups are more ready to countenance risks than are individuals (for a brief review of 'risky shift', see M. Douglas, 1985, 66–7). Community consultation may therefore lead to an underestimation of the risks potentially faced by research participants.

To make a further, similar, point, group members asked to reach a decision often experience pressure towards consensus and unanimity. Such pressures can be particularly strong, Melton et al. (1988) argue, in situations like the AIDS epidemic where group members are under stress and are anxious to find a rapid solution to the problems which affect them. As a result, researchers' proposals for research can be subject to insufficient critical scrutiny. Where situations of this kind occur researchers might have to adopt devices such as bringing in group leaders, experts or advocates from outside the local community, or they may need to institutionalize a devil's advocate role in some way. They also suggest that community consultation should be separated in time from attempts to obtain individual informed consent, to prevent group pressure from pre-empting individual decisions to participate.

Community consultation may reveal a conflict of interests between the needs of potential research participants and those of the wider community. For example, rights to privacy and confidentiality, especially in relation to sensitive issues like sexual behaviour, may need to be weighed against a wider public interest in the reporting and surveillance of infectious disease (Melton and Gray, 1988). Melton et al. (1988) argue that researchers ultimately have responsibility for resolving such issues, even at the cost, sometimes, of abandoning the proposed research. A final difficulty with community consultation is that, paradoxically, it can encourage a degree of ethical insensitivity on the part of the researcher. By seeming to shift the locus of responsibility for ethical decision-making on to the community, the consultation process may encourage researchers to disregard their responsibilities as individuals.

A further, different, style of collaborative research has been suggested by Fielding (1990). According to Fielding, researchers operating in sensitive contexts should consider adopting an 'intercalary role'. The term 'intercalary role' was first used by Barnes (1963). What Barnes had in mind was a situation in which the fieldworker takes on an agreed neutral role between two contending parties, perhaps acting as an honest broker between them. For Fielding, the intercalary role is rather more a reflexive device in which researcher and researched simultaneously inquire into the group's culture and educate one another with respect to it. The field researcher, in taking an intercalary role, is neither the passive recipient of informant accounts nor the sceptical investigator typical of investigative research. Instead, Fielding suggests, fieldworker and study participants co-produce fieldwork. Adopting such a role, Fielding was able to encourage police officers to explore the implicit and contradictory understandings – from both an inside and an outside perspective – which surrounded their use of force in subduing suspects. In the case of sensitive topics, the intercalary role is particularly valuable, in Fielding's opinion, because it presents an opportunity to understand the issue of sensitivity from the point of view of the study participants, rather than simply from the perspective of the researcher. It is not clear, however, what the effects of an intercalary role are for those being studied, or for third parties. Does the researcher in effect become a kind of sociological conscience, or should the intercalary role have the kind of mediational character originally envisaged by Barnes?

Conclusion

Cassell (1988) has observed that issues of motive, method and morality are inextricably intertwined in field research. Entering and
understanding social and cultural worlds other methodologies cannot reach may make field research a method of choice. Researchers who use field methods, however, must face the contradictions which inhere in the commitment to naturalistic understanding. The appreciative stance implicit in naturalism sits uneasily alongside the indignation which fuels the investigative paradigm and conflict research. A method which relies on intensive contact and the development of an ongoing personal relationship between researcher and researched cannot easily accommodate the deception at the heart of covert research. The desire to understand the perspective of the deviant groups and to work alongside the disadvantaged can lapse into a romanticized identification with the underdog (Gouldner, 1968). None of these contradictions is easy to resolve.

Notes

1. The distinction between overt and covert research may at times be relative. Those studied may sometimes forget that a researcher is present, and it is not always possible to apprise people of the research role (Thorne, 1983).

2. The call for researchers to make more use of data generated by the legal process has one ironic feature. In the United States the involvement of researchers in the courts has grown dramatically in recent years. However, rather than researchers using the law, the law has used researchers as expert witnesses (see, e.g., Rosen, 1977; R.M. Lee, 1989). Sociologists in the United States have been involved in litigation surrounding a number of controversial racial issues, such as those to do with school desegregation, racial imbalances in the use of the death penalty, or involving the custody of mixed race children. Anthropologists have testified extensively in cases involving American Indians, including disputes over aboriginal land titles, the identification of Indian tribes and the nature of treaties. Despite this involvement accounts by social scientists who have testified as expert witnesses do not suggest a use of the courts in the manner suggested by Lehmann and Young. On the contrary, much sociological writing in this area laments that ‘incompatibilities between the non-adversarial academic sphere and the adversarial legal sphere inevitably create conflict for scholar-expert witnesses’ (Kalmuss, 1981, 214).

3. Unlike other aspects of the radical critique which crossed the Atlantic, conflict methodology seems not to have made much impact on British sociology. This may have been because of the rather fugitive character of some of the writing (Christie, 1976), and because much of the controversy over the use of conflict methodology was fought out at an organizational level in the American Sociological Association. It may also be that some of the American scholars who were influential in Britain in the 1970s, like C. Wright Mills and Howard Becker, can be regarded in a number of respects as methodological conservatives.

4. Although Marx points in each case to the underlying social processes involved, the terms he uses are slightly too broad to be helpful. The term 'coercion', for instance, leaves open the question of who is coercing whom. Moreover, the contingencies of which Marx speaks do not need to be uncontrollable for disclosure to take place. They need only to be uncontrolled. It is preferable, therefore, to speak instead of unwitting disclosure, enforced disclosure, volunteered disclosure and uncontrolled contingencies.

5. Interestingly, one use of the FOIA has been to uncover evidence of FBI surveillance of sociologists beginning in the 1930s and continuing into the period of the Cold War (Keen, 1992).