

Unobtrusive methods in social research

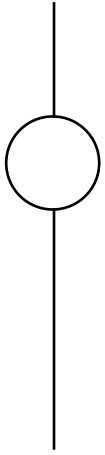
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Unobtrusive Methods in Social Research

Raymond M. Lee



Unobtrusive methods in social research

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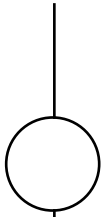
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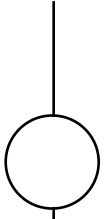
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Series editor's foreword

This Understanding Social Research series is designed to help students to understand how social research is carried out and to appreciate a variety of issues in social research methodology. It is designed to address the needs of students taking degree programmes in areas such as sociology, social policy, psychology, communication studies, cultural studies, human geography, political science, criminology and organization studies and who are required to take modules in social research methods. It is also designed to meet the needs of students who need to carry out a research project as part of their degree requirements. Postgraduate research students and novice researchers will find the books equally helpful.

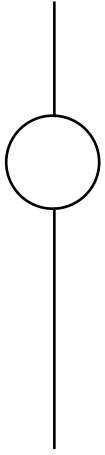
The series is concerned to help readers to 'understand' social research methods and issues. This will mean developing an appreciation of the pleasures and frustrations of social research, an understanding of how to implement certain techniques, and an awareness of key areas of debate. The relative emphasis on these different features will vary from book to book, but in each one the aim will be to see the method or issue from the position of a practising researcher and not simply to present a manual of 'how to' steps. In the process, the series will contain coverage of the major methods of social research and will address a variety of issues and debates. Each book in the series is written by a practising researcher who has experience of the technique or debates that he or she is addressing. Authors are encouraged to draw on their own experiences and inside knowledge.

One of my favourite books on social research methods has always been *Unobtrusive Measures: Nonreactive Research in the Social Sciences* by Eugene Webb and others. It was published originally in 1966 and is one of the most widely cited books on research methodology around. In part, it is a reaction to the unthinking use of research methods such as the interview and questionnaire, which, Webb and his associates suggested are limited by the research participant's knowledge that he or she is the focus of an investigation. Webb and his associates sought to loosen the reliance on such methods by suggesting a vast array, often resembling an inventory, of methods and sources of data which are not limited in this way. The style and tone of the book were irreverent, with the authors suggesting much greater use of the imagination than the simple and perhaps unthinking use of standard procedures. In fact, Webb and his associates were not opposed to commonly used methods like the interview; it was the over-reliance on such methods to which they objected. Also, they were keen to place their advocacy of unobtrusive methods in the context of recommending that measurement in the social sciences should not be dependent on one measurement process alone.

An updated and revised edition of the book by Webb and his associates was published in 1981, but since then there has been little further discussion of the topic, in spite of the immense popularity of the original publication. Raymond Lee's book on unobtrusive methods therefore plugs an important gap. His book is written very much in the spirit of the style Webb and his associates adopted. Lee discusses the very idea of unobtrusive methods in the context of wider debates and issues in research methodology. He examines the different types of unobtrusive methods, taking into account some of the discussions and distinctions that have been employed since 1966. He has updated the original work in at least two senses. First, he has included a huge number of new studies and slotted them into the various categories of unobtrusive method. Second, he has included new forms of unobtrusive method, which Webb and his associates could not have anticipated, most notably the Internet. Awareness of the Internet as a research tool is still in its infancy and I am sure that readers will find this discussion very timely and suggestive.

As Lee observes, there is still a massive reliance in the social sciences on data deriving from reactive methods of social research, such as the interview and questionnaire. This book will serve as a further reminder of the limitations of a dependence on such techniques, as well as offering a broader purview on alternative possibilities than Webb and his associates were able to provide in their seminal discussion.

Alan Bryman



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1

Introduction to unobtrusive methods

Allan Kellehear has written: 'There is today, in social science circles, a simple and persistent belief that knowledge about people is available simply by asking.' He goes on, 'We ask people about themselves, and they tell us . . . the assumption is that important "truths" about people are best gained through talk – a sometimes direct, sometimes subtle, interrogation of experience, attitude and belief' (Kellehear 1993: 1). A problem with this assumption is that what we gain 'simply by asking' is often shaped by the dynamics surrounding the interaction between researcher and researched. This is so because the act of eliciting data from respondents or informants can itself affect the character of the responses obtained. One consequence of this might be a need to accomplish the 'interrogation of experience, attitude and belief' in other, less direct, ways. Webb *et al.* (1966) coined the term '**unobtrusive measures**' to refer to data gathered by means that do not involve direct elicitation of information from research subjects. Unobtrusive measures are 'non-reactive' (Webb *et al.* 1981) in the sense that they are presumed to avoid the problems caused by the researcher's presence. Specifically, Webb *et al.* advocate that social researchers should devote more attention to sources of data such as physical **traces** (the evidence people leave behind them in various ways as they traverse their physical environment), non-participant observation, and the use of documentary sources. In other words, questions about experience, attitude and belief might be addressed just as effectively by watching what people do, looking at physical evidence

of various kinds, and drawing on the written as well as the spoken voice, as they are by interviews and questionnaires.

Webb *et al.*'s (1966) book *Unobtrusive Measures* became something of a minor classic. (A revised version under the title *Nonreactive Measures* was published in 1981.) Humorous in tone and sceptical in its orientation to the dominant methodological practices of its time, *Unobtrusive Measures* is a delightful compendium of offbeat methods and data sources. (The book, in fact, had its origins in an informal seminar at Northwestern University in which it became a game to come up with novel methods (Campbell 1981: 481).) Among the examples of unobtrusive measures Webb *et al.* proffer are the use of wear on the floor tiles surrounding a museum exhibit showing hatching chicks to measure visitor flows; the size of suits of armour as an indicator of changes in human stature over time; and (tongue in cheek) the relationship between psychologists' hair length and their methodological predilections. In all of this, of course, there is a serious purpose, a call to social researchers to think creatively about the sources and use of their data. Hopefully in the same spirit, the present book provides an introduction to and an overview of the use of unobtrusive methods in social research.

Why use unobtrusive methods?

One justification for the use of unobtrusive methods lies in the methodological weaknesses of interviews and questionnaires. As Webb *et al.* put it:

Interviews and questionnaires intrude as a foreign element into the social setting they would describe, they create as well as measure attitudes, they elicit atypical role and response, they are limited to those who are accessible and who will cooperate, and the responses obtained are produced in part by dimensions of individual differences irrelevant to the topic at hand (Webb *et al.* 1966: 1).

Interviews and questionnaires create attitudes in part because respondents commonly try to manage impressions of themselves in order to maintain their standing in the eyes of an interviewer. A fairly consistent finding on surveys, for example, is that respondents will claim to have an opinion about fictitious or obscure matters (Bishop *et al.* 1980). Looking at voting behaviour, Presser and Traugott (1992) found that some of those who claim in surveys to have voted have not done so. According to Presser and Traugott, misreporting of voting behaviour correlates with factors such as education and interest in politics. Perhaps because they regard themselves as the kind of people who should vote, the better educated and politically aware feel under pressure to report having voted even when they had not done so.

Bradburn *et al.* (1979) found a tendency for survey respondents to over-report socially desirable behaviours when interviewed using less anonymous

methods. Broadly, the more anonymous the method, that is, the less it involved face-to-face contact, the more likely respondents were to admit to socially undesirable behaviour. More recently, Tourganeau and Smith (1996) have also found evidence that more private methods of data collection yield gains in the reporting of sensitive behaviours. The characteristics of interviewers can under some circumstances affect the answers respondents give to particular kinds of question. In a study using both black and white interviewers, Schuman and Converse (1971) found little evidence that the race of the interviewer influenced responses to questions relating to racial discrimination, poor living conditions, or personal background. They did find, however, that black respondents were less likely to agree with statements expressing hostile attitudes towards whites when the interviewer was white than when the interviewer was black.

How one asks a question in an interview or on a questionnaire can have, sometimes subtle, sometimes substantial, effects on the responses received. Variations in wording, for example, can affect how people respond. Smith (1983) found across a variety of surveys that the word 'welfare' in questions about financial support for the disadvantaged produces lower levels of positive response than the word 'poor'. There is some evidence that respondents use the response categories attached to questionnaire items on surveys as cues to researchers' expectations about the range of particular behaviours (Schwarz and Hippler 1991). In one experiment, respondents, when asked if they suffered from headaches 'frequently', reported three times as many headaches a week as those asked whether they suffered headaches 'occasionally' (Clark and Schober 1982). It seems, moreover, that respondents dislike giving responses that seem extreme relative to the scale of responses presented to them. As Schuman and Presser (1981) point out, survey questions are never asked in isolation but as part of a flow of questions. Thus, the context or order in which questions appear can have consequences for the responses to them. Respondents, for example, sometimes adjust their response to a later question in order to make it consistent with an answer they have given previously. In various ways, responses to specific questions on a particular topic can be affected by responses to a general question on the same topic. Juxtaposing questions having an alternate form sometimes induces, it seems, a norm of reciprocity which can distort responses. Schuman and Presser argue that, although question order effects are not pervasive, neither are they rare.

The extent to which interviewing might intrude as a foreign and reactive element in the research situation can be seen in Veroff *et al.*'s (1992) longitudinal study of couples in first marriages. Compared with a control group, couples who were frequently interviewed showed more variability in reported levels of marital satisfaction during the second year of the study. By the fourth year, however, those who had been interviewed more frequently had generally more positive perceptions of their marriage than

those interviewed less frequently. Veroff *et al.* speculate that the interviews might have caused couples at various points to reflect on their marriage in ways that affected the quality of the relationship. Participation in a study, in other words, can change the attitudes and behaviour of those being studied. Depth interviewing might be less subject to reactive effects than survey interviewing. Yet, even here problems can arise, at least when sensitive topics are involved. The threatening character of interviews on relatively intimate matters can be gauged, for example, from reports of interviewees lapsing into embarrassed silence (George 1983) or needing to use alcohol as a prop while being interviewed (Brannen 1988). In Maureen Padfield and Ian Proctor's (1996) qualitative study of young adult women, the gender of the interviewer seemed to make little difference to what was disclosed in the interview, except in relation to one particular issue. Women volunteered information about having had an abortion to Padfield, the female interviewer, but not to Proctor. In addition, some of Proctor's respondents revealed on reinterview by Padfield that they had had an abortion. It is not clear what accounts for this pattern, but an implication might be that interviewer effects are not entirely absent in qualitative research.

As Webb *et al.* point out, respondents have to be accessible and to be willing to answer a researcher's questions for interview and questionnaire methods to be effective. There is, however, a fairly widespread perception that survey response rates have fallen in the United States, the United Kingdom and, perhaps, in some other countries as well (American Statistical Association 1974; Market Research Society Working Group 1976). Steeh (1981) examined non-response to two long-running US surveys associated with the Survey Research Center at the University of Michigan. In both, refusal rates rose steadily between 1952 and 1979. According to Groves (1989), a long-term increase in refusal rates has also been seen for a number of large-scale government surveys in the United States. Over a rather long time period, Goyder (1987) found declining response rates in both the United States and Canada for interview studies, though not for postal surveys. Those who do not respond to surveys are, of course, different in their social characteristics from those who do. Goyder suggests in a careful synthesis that, although many of the commonly assumed demographic correlates of non-response do not survive statistical controls for other factors, '[socioeconomic] status and age are key components of socio-demographic refusal bias in surveys' (Goyder 1987: 109).

Concern over **reactivity** extends beyond interview-based methods. In the 1930s, a series of experiments on the relationship between workplace conditions and levels of production attained classic status (Mayo 1933; Roethlisberger and Dickson 1939). During experiments at the Hawthorne works of the Western Electric Company into the effects of changing lighting levels, it was noticed that production went up whether levels of illumination were increased or decreased. The explanation suggested for this phenomenon was

that the workers had responded not to the experimental variable – the level of lighting intensity – but to being singled out for study (Mayo 1933). Dubbed the '**Hawthorne Effect**', these experiments were an early intimation that social science measurement could be *artefactual*. In other words, simply by their presence researchers could unwittingly but systematically distort their own findings.

Research on the role of **artefacts** in social science experiments crystallized in the 1960s and early 1970s. Broadly speaking, attention was directed to three areas: (a) the role of '**demand characteristics**', (b) the effects of 'experimenter expectancies' and (c) the possible over-reliance on volunteer subjects. (For a readable, if somewhat one-sided account of the field, see Rosnow and Rosenthal (1997).) Orne (1962) suggested that the experimental situation is one that motivates participants to be 'good subjects'. Research subjects therefore try to act in ways they presume will make the experiment successful. In order to do this they look for clues to the true purpose of the experiment in the experimental situation, in their own wider knowledge and, in the case of students, in campus gossip. The sum total of such clues Orne refers to as the 'demand characteristics' of the experiment. Rosenthal (1976) presented evidence that researchers who were encouraged to believe that an experiment would yield results within a particular range produced findings in line with their expectation. Subsequently, Rosenthal and Rubin (1978) reported on a **meta-analysis** of a large number of studies dealing with experimenter expectancies. The pooled results from these studies suggested that expectancy effects are to be found in a wide range of research areas. Rosenthal and Rostow (1975) have also presented evidence to suggest that a variety of background and attitudinal variables distinguish those willing to participate in social-psychological experiments from those who are not. According to Rosenthal and Rostow, volunteering is also associated with a variety of situational factors to do with the investigation itself, the characteristics of the researcher and the incentives offered for participation.

As Campbell (1981) concedes, one can argue that an assessment of this kind is overly pessimistic. Arguments that direct elicitation methods are incorrigibly reactive have been vigorously contested, and have provoked revisionist claims. In a careful study using hierarchical multilevel modelling, Hox *et al.* (1991) point out that interviewer effects found in earlier studies might not have survived appropriate statistical controls. Alternatively, they suggest, effects found in previous studies might have come about through the cumulation of small differences between interviewers. Smith (1995) has argued that the image of free-falling survey response rates is an exaggeration. Examining non-response rates for a range of different surveys over relatively long time periods, he found varying trends in response rates, although he does concede that, 'In both the United States and elsewhere increases in both overall non-response and refusals do outnumber declines . . .' (Smith 1995:

168). Even the celebrated Hawthorne Effect has not escaped critical scrutiny. According to Parsons (1974), the workers, who were paid according to output, could see records of their output, and worked harder to increase their earnings. Kruglanski (1975) suggests that experimental studies showing the presence of demand characteristics (or subject artefacts to use his preferred term) do not stand up well to critical scrutiny since researchers have not always been clear about what produces artefacts or how exactly they affect experimental variables. Kruglanski (1975) also argues that Rosenthal and Rosnow's research on volunteer subjects shows volunteering to be associated with such a heterogeneous range of factors that it is difficult to see how volunteer bias could operate in particular studies. Reviewing a large number of studies, Barber (1976) concludes that there is some evidence for the biasing effects of experimenter expectancies. However, he also argues that, in many cases, other explanations, such as failure to follow precisely experimental protocols, can account for the results found in particular experiments. Nevertheless, one can still argue that social researchers are over-reliant on direct elicitation methods. Despite the United States and Britain's very different methodological cultures, one heavily quantitative, the other qualitative, social research in both countries still relies heavily on the direct elicitation of data from respondents. Only a little more than one-quarter (29.5 per cent) of articles appearing in major sociology journals in 1995 ($n = 220$) in the United States (*American Journal of Sociology*, *American Sociological Review* and *Social Forces*) and in the United Kingdom (*British Journal of Sociology*, *Sociological Review* and *Sociology*) used data sources, mainly documents, that were not collected by questionnaire or interview.

A justification for the use of unobtrusive methods sees them less as alternatives to direct elicitation methods, and rather more as complementary to those methods. In fact, having stressed the reactivity of interviews and questionnaires Webb *et al.* reserve their greatest scorn for studies using only a single method of data collection. For them, the principal objection to traditional data collection methods is typically 'that they are used alone' (Webb *et al.* 1966: 1). Webb *et al.* argue that data collection methods used singly are inferior to the use of multiple methods. They use a metaphor from geology, the idea of an 'outcropping', to point to how theory might guide the selection of data points (1966: 28). Any given theory has innumerable implications and makes innumerable predictions. The testing of theory can be done only at available outcroppings, those points where theoretical predictions and available instrumentation meet. Any one such outcropping is equivocal, but the more remote or independent such checks, the more confirmatory their agreement. The idea that data and theory meet at available outcroppings is clearly a justification for the use of multiple sources of data. The argument is that the results one gets from one's research are always to some degree an artefact of the method used to collect the data. In other

words, any finding is potentially subject to a 'plausible rival hypothesis' (Webb *et al.* 1966: 5) that the findings reflect fallibilities inherent in the data collection method. However, these fallibilities are not the same across methods. In particular, the problems of reactivity that afflict direct elicitation methods are absent when data are collected unobtrusively. From this point of view, configuring different methods, each of which is fallible in a different way, gives greater purchase on the problem to hand than an over-reliance on a single method.

Another justification for the use of unobtrusive methods is related to their adaptability. My own interest in unobtrusive measures derives from an early field research experience where obtaining elicited data was extremely difficult (Lee 1995a). Studying a relatively rare and, in the context of the religious conflict there, deviant population, interreligiously married couples in Northern Ireland, a group who in addition often take care to hide themselves from the attention of the wider community, the need to supplement interview data with information from other sources soon became apparent. For example, record-based data on the geographical patterning of marital choice had to be substituted for a survey. An added attraction of unobtrusive methods over directly elicited data in conflict situations is that they reduce the potential physical hazards associated with having to venture into dangerous locales in order to carry out interviews (Lee 1995a). Another way to see this is to recognize that researching sensitive or dangerous topics encourages researchers to innovate in order to find pathways around the obstacles certain topics put in their way. Methodological innovation has its drawbacks, notably in an absorption with the technical fix, but it also requires an imaginative cast of mind. As C. Wright Mills (1959) argued, imagination forms a counter to trained incapacity. Technical competence in research skills, even of the highest quality, is leavened by imagination.

Kellehear (1993) points out that Webb *et al.*'s work predates the advent of 'post-structuralist' approaches in the social sciences. Kellehear uses the term loosely to refer to a variety of contemporary theoretical stances such as deconstructionism, discourse analysis, narrative analysis, phenomenology and **semiotics**, many of which can be collected together; Alasuutari (1995) suggests, under the rubric of 'cultural studies'. As Atkinson observes, these approaches have 'roots in diverse philosophical, theoretical and methodological inspirations' (Atkinson 1992: 38). They share, however, a hostility to the notion that the task of social science is measurement, whether unobtrusive or not, as opposed to interpretive or critical understanding of social reality. The somewhat surprising intersection of those approaches with that of Webb *et al.* lies in a rather sideways glance at traditional sources of data in social research. In the post-structuralist vision, everything in and of the world is irredeemably cultural, and therefore open to study, no matter how seemingly peripheral, insignificant or taken for granted. In addition, shaped by literary or humanistic study, such approaches have usually been less

wedded to elicited data than traditional social science disciplines. Instead, they focus on the cultural meaning of products, artefacts and objects, in a sense revalorizing sources marginalized by dominant social science. In particular, textual materials, from the very grand to the very humble, products of popular culture and material culture find themselves falling under the post-structuralist gaze.

Where do unobtrusive measures come from?

Although the term is Bouchard's (1976), Webb *et al.* might easily have described *Unobtrusive Measures* as an inventory. Indeed, the core of the book, with only some injustice, can be read simply as an extended list of instances held together by little more than the non-reactivity they have in common. Formal sociologists such as Simmel and Goffman were fond of 'saturative instantiation', the piling on of examples, as a way of demonstrating the ubiquity, significance and interpenetration of social forms. (Mention of Goffman in this context is, of course, ironic. His largely undescribed methodology, whatever else it may have involved, made full use of unobtrusive methods, from disguised observation to documentary analysis (Burns 1992).) Saturating the reader with relevant instances is persuasive. The many and varied examples Webb and colleagues deploy urge the reader to recognize that the use of unobtrusive measures is both possible and desirable. Inventories have been used as a method for collecting data. Although the term 'inventory' is still sometimes used to refer to the set of items making up a psychometric scale, methods of this kind are rare today. If they survive at all, it is in the form of time budgets. Used to collect data, inventories are unwieldy and often reflect a rather atheoretical tendency towards factual accumulation. The more common use of the inventory is to list in a readily retrievable form resources of various kinds. Survey researchers, for example, can turn to published inventories (for example, Miller 1991) to locate well-validated attitude scales for inclusion in their questionnaires. The advent of the Internet, for example, produced a blizzard of inventories, in print and on line, of listservs, **newsgroups** and websites (see, for example, the resources at <http://www.sosig.ac.uk>).

Creating an inventory of measures provides a database of retrievable examples. Since inventories are inclusivist in character, an inventory makes visible the range, scope and relevance of the measures contained within it. As a result, it becomes easy to identify gaps or miscellaneous elements (Bouchard 1976) within the overall framework. Inclusion also ensures that the reader is very likely to find something that catches the interest. Inventorial organization encourages browsing. Browsing in its turn encourages the 'discovery of accidents' (Glaser and Strauss 1967: 174–5). This is because the salience of each browsed item, and the basis on which it is

deemed salient, tend to be assessed independently for each item. To put this another way, relevance, rather than being imposed, emerges out of browsing. The problem is that although browsing is useful, it is also quite limited. As Dabbs points out, there can be 'too many measures, some of which, quite frankly, are worthless. If one included a measure of every sign or effect of a phenomenon, the number would be endless. There are more measures than phenomena, and one must carve out a domain of measures likely to be useful and to the point' (Dabbs 1982: 34). Just as inventories are unwieldy as data collection tools without some means of data reduction, resource inventories are similarly unwieldy unless the user has a mechanism or a methodology that allows retrieval of relevant items in the database. In the original incarnation of *Unobtrusive Measures* such a methodology was conspicuous by its absence. In other words, there is little explicitly to guide a researcher seeking unelicited data relevant to a particular research problem.

Two different approaches to dealing with this problem have been taken in subsequent writing. On the one hand, some writers have adopted what might be called an *orientational* approach. This stresses the importance of the stance the researcher brings to possible and actual sources of data. The orientational approach can be contrasted with a *taxonomic* strategy, the basic aim of which is to identify particular properties of measures. From these properties, it is argued, one might be able to produce a generative taxonomy that could be used to produce on demand unobtrusive measures fitted to specific research purposes (Sechrest and Phillips 1979; Webb *et al.* 1981: 287–306).

Although implicit in much of Webb *et al.*'s writing, the most clear-cut expression of the orientational approach can be found in Webb and Weick's (1983) discussion of the principles governing the use of unobtrusive measures. The first principle is that investigators should 'construct and impose multiple indices that converge' (Webb and Weick 1983: 211–12). The preference here is for capturing the complexities of social situations through the use of multiple theories, which provide potentially competing explanations needing adjudication, multiple methods of research and multiple sources of data. Second, enthusiasts for unobtrusive methods 'assume noise is rare' (Webb and Weick 1983: 212). In other words, much of what we see around us can be considered as a potential source of data. Such data should not be discarded out of hand even when at first sight they seem to be trivial, perplexing or out of the ordinary. A third principle is that 'investigators believe in amortization' (Webb and Weick 1983: 213). The assumption is that data, no matter how they are generated, can have more than one purpose and can be put to more than one use. Data obtained opportunistically should not be seen as inherently inferior to data designed for a particular purpose. Fourth, those committed to the use of unobtrusive methods 'find foolishness functional' (Webb and Weick 1983: 213). What Webb and Weick have in mind here is the importance of being able to reflect on

research problems in an imaginative, playful, even fanciful, way. Such reflection serves, as they put it, to 'generate novel inputs and permit people to recognize and break the singular focus toward a problem in which they had persisted' (1983: 213). Fifth, unobtrusive measures require the researcher to 'ponder the variance rather than the mean' (1983: 214). If one measures some phenomenon, a focus on how it is distributed can be as useful as an inspection of a typical case. Webb and Weick add that the absence of variance can in some cases be theoretically significant. For example, power in an organization can be indexed by the degree to which some within it have less discretion than others. Finally, 'investigators use expectancy as a control', that is 'sophisticated and successful use of unobtrusive measures requires that investigators lay out in advance what they expect to find so that the surprise when they don't find it is visible and documented' (1983: 214–15).

There is in this a strand of what Gouldner (1973) calls 'methodological romanticism'. It is impossible here to do justice to Gouldner's complex argument about the multifaceted influence of Romanticism on the social sciences. For present purposes, what is important are his observations on how Romanticism affected the way in which social scientists think about data. First, Romanticism encouraged an 'open' conception of data. Faced with the disappearance of peasant culture under the impact of industrialization, writers in the German Romantic tradition undertook direct observation of rural life. In doing so, they extended the scope of social research to encompass those previously thought of as 'low, indecorously deviant, and worthy only either of contempt or neglect' (Gouldner 1973: 351). The idea that nothing social escapes the purview of the social scientist was fostered in this way. There are, of course, other less Romantic influences that have also encouraged an open conception of data; the application of random sampling methods to the selection of respondents is an obvious example (Marsh 1985). Romanticism, however, did not simply widen the scope of what might be studied. In true Romantic manner it encouraged a particular *sensibility* towards data. Gouldner identifies three aspects of this sensibility. First, the Romantics blessed 'the ordinary, everyday world with the pathos of the extraordinary'. Ordinary, mundane things, Gouldner goes on, 'were to be rescued by viewing them from a perspective that endowed them with new and enhanced value, rather than being routinized, ignored or thingified' (Gouldner 1973: 331). Webb and Weick's assumption that 'noise is rare' invokes a concept – signal-to-noise ratio – from modern electronics. Nevertheless, the contention that data can be found anywhere and everywhere, even in dustbins, has a Romantic cast to it. A second aspect of methodological Romanticism is a preference for solutions to research problems which rely on imagination, insight or vision rather than standardized procedures. The slogan 'foolishness is functional' trades on just such a preference. Foolishness helps, for example, to spark and maintain interest in a research topic. More significantly, it aids the development of novel analytic

perspectives and counters what Webb and Weick regard as a 'preoccupation with rational models' on the part of researchers (1983: 213). Notice how the slogan 'foolishness is functional' is premised on an incongruity. A third strand of methodological Romanticism is a fondness for the grotesque. We tend today to associate ideas of the grotesque with monstrosity. (James (1994: 80), for example, points to how popular tradition surrounding the monster in Mary Shelley's *Frankenstein* increasingly focused on his size and physicality.) More properly, the term refers to the juxtaposition of *incongruous* elements. As Gouldner notes, imagination in the social sciences is often taken to arise out of incongruity, from the analyst's ability to shift from one perspective to another or to juxtapose in a meaningful way seemingly incompatible elements. Many of Webb *et al.*'s examples provide an ingenious bridging of the commonplace and the significant. On the one hand, the populations and settings that figure in these examples are immediately recognizable. (Even those of us who have not bought jade or played baseball can identify the situations described as instances of more prototypical encounters.) On the other hand, although the measurements and instruments used have a rather humble quality, they are heavily consequential for the analytic purposes of the investigator. Moreover, to the extent that they encourage the reader to respond with 'Why didn't I think of that?', the various examples celebrate the ingenuity and imagination of those investigators.

It would be a mistake to overemphasize the Romantic elements in Webb and Weick's account. There seems to be some contradiction between an opportunistic, data are where you find them, stance and the principle of using expectancy as control, setting out what you expect so that your surprise registers when you don't find it. Moreover, although Webb and Weick stress that expectancy comes from eclecticism, they tend to restrict their examples to variability in distributions. Might not a more systematic, taxonomic strategy be a better approach to generating unobtrusive measures? The generative taxonomy proposed by Webb *et al.* (1981) takes the form of a large (7×13) matrix. Specific unobtrusive measures are located in relation to the two dimensions of the matrix. One of these dimensions is the characteristic the researcher wishes to investigate. Webb *et al.* suggest the following categorization. In relation to a particular characteristic a researcher is likely to want to know (i) the *probability* of its occurrence; (ii) its *capacity* to perform some task or role; (iii) its social *value*; (iv) the *sentiment* attached to it; (v) the *affective states* surrounding it; (vi) its degree of *category membership*; and (vii) its standing on some *trait* or dispositional dimension. The second dimension of the taxonomy refers to features of unobtrusive measures that provide a basis for making inferences about the characteristic of interest. Webb *et al.* suggest that 13 features of a characteristic can be measured: (i) *frequency*, how often something occurs; (ii) its *magnitude* or size; (iii) the degree of *latency*, or time taken for some subsequent action, event or consequence to take place; (iv) the

degree of *resistance to change* associated with the characteristic; (v) the degree of *functional smoothness* or ability to perform skilfully associated with the characteristic; (vi) the level of *association* those who possess the characteristic have with socially similar or dissimilar others; (vii) the *acquisition* of things of value (a measure of interest); (viii) the *consumption* of valued goods (a measure of liking); (ix) the willingness to expend *effort* (a measure of interest); (x) the *inappropriateness* of responses (a measure of hidden characteristics); (xi) *bias* or the degree of divergence from expected standards; (xii) *cognitive articulation*, the ability to make more or less fine discriminations between different objects; and (xiii) the *revelatory*, the extent to which traces divulge information about characteristics.

There are some immediately obvious problems here. Although Webb *et al.* stress the importance of multiple methods, most of the categories they describe seem to involve only single measures. Webb *et al.* acknowledge that the generative taxonomy is hardly elegant; some of the categories overlap, and the dimensions they present do not have a clear logical structure. In addition, it is not clear what relationship there is, if any, between the generative taxonomy and the typology of data sources Webb *et al.* originally set forward. The characteristics Webb *et al.* (1981) list are primarily concerned with the analysis of performances, dispositions, affective states and (marginally) social relationships. It is not obvious whether these categories are relevant only to certain areas of social psychology or are meant to have general applicability. Presumably those with other disciplinary, or even subdisciplinary interests, might produce a different set of features. Sociologists, for example, might prefer to focus on acts, activities, meanings, participation, relationships and settings (Lofland 1971: 14–15). It would also seem appropriate to distinguish characteristics in relation to some level of scale. Is the characteristic to be assessed, in other words, at the individual, group, organizational, societal or some other level?

While it is relatively easy to fit examples from Webb *et al.*'s work into the taxonomy, it is not clear how much *independent* generative power the taxonomy has. In fact, the idea of a generative taxonomy sits uneasily with other aspects of Webb *et al.*'s work. It is difficult, for example, to reconcile a taxonomic approach with Webb *et al.*'s emphasis on the imaginative and opportunistic use of data. Indeed, it is instructive in this context to look at the list of exemplars of unobtrusive measures Webb *et al.* provide at the beginning of both editions of their book. Traced back to their original source, there is often little information about how the measure was generated. Some of the examples (including the famous hatching chicks) derive from anecdotal reports and, as is common in published work (Merton 1957), authors usually dwell more on their findings than on how the data were generated. In other cases, however, it is hard to avoid the conclusion that often the measures used simply reflect expedience and/or what was available to hand. It seems, then, that suitable sources of data cannot simply

be 'read off' from theoretical concerns; they have to be found by means of a directed search. Another way to put this is to say that the generation of unobtrusive measures involves the use of heuristics. Heuristics are rules of thumb that help to increase the probability of solving a problem, typically by providing methods for 'eliminating unfavourable solutions, narrowing down the search space, [and] breaking complex problems into subproblems' (Lamb 1991: 102–16). A close reading of Webb *et al.*'s work suggests that underpinning their discussion of various data sources are a set of implicit heuristic strategies for finding data sources relevant to a particular research problem. This is perhaps seen more clearly if their (passive) typology of data sources is recast into a (more active) classification of modes of data acquisition. This means that, instead of focusing on physical traces, observation and documents, one should ask for a given social context what data can be *captured*, what can be *found* and what can be *retrieved*.

According to Dabbs, proponents of unobtrusive measures tend to assume that 'entities move through time and space and through social and physical encounters, all the while shedding signs that, taken cumulatively, reveal their true nature' (Dabbs 1982: 34). One important set of signs are 'ephemeral traces', as Dabbs calls them. Such traces 'make up much of the ordinary behavior of people and organizations'. However, 'unless someone or something is there to make a record, the ephemeral trace is lost' (Dabbs 1982: 34). Ephemeral traces need to be captured, in other words, if they are to be of use to the social scientist. (Used in this sense, the term 'capture' need not imply a pre-existing, external 'objective' reality; merely that if some things are not 'frozen', they disappear.) Identifying ephemeral traces relevant to a particular research problem is highly dependent on theoretical and cultural knowledge. In many cases researchers will draw directly on this knowledge. In other cases, the overwhelming 'naturalness' of such knowledge is precisely the problem. Human beings have a well-developed cultural sense. They also have a perceptual apparatus highly sensitive to interactional cues. As a result they often apprehend and understand the activity of others in ways they find difficult to articulate. The basic heuristic is therefore to ask: What features of some setting or situation can be made perceptually, normatively or culturally problematic and how? In the social sciences three strategies have generally been used to problematize social situations. These might conveniently be labelled 'perceptual shift', 'decentring' and 'disruption'. Perceptual shift involves altering the normal way in which we perceive human activity, perhaps by changing the time base for observation or the depth of focus through which some setting is viewed. This strategy often depends on shifting the capabilities of normal human perception. Decentring involves *explicitly* attending to those interactional or communicative features of settings or situations that are normally only *implicitly* apprehended. A conscious shift from focusing on the content of speech to paralinguistic features, such as pauses, pitch, intonation and the like, is an

obvious example. Disruption involves altering a social situation in some way so that its underlying features are revealed.

Ephemeral traces, which need to be captured, can be distinguished from physical traces that are normally found *in situ*. The basic question in considering the generation of physical traces is as follows: 'How are the physical properties of objects inadvertently implicated in their social use?' In part, the answer Webb *et al.* give to this question is a quasi-economic one. They tend implicitly to see research problems where the use of physical traces is appropriate in terms of production, consumption, demand and supply. More specifically, a basic heuristic they employ is to ask in effect, 'What production is implicated in consumption?' It is this that triggers their interest in garbage, litter and so on. They also ask the converse question, 'What consumption is implicated in production?' However, in practice they resolve this by asking the somewhat different question of how demand is naturally calibrated. Doing so leads them to consider the frequencies to which car radios are tuned, the size of suits of armour, the abrasion of surfaces and so on. Related to all of this is a somewhat different heuristic: 'What performative opportunities do objects offer?' This allows one to consider graffiti, inscriptions and the like. Webb *et al.* distinguish between **running records**, on the one hand, and what they refer to as episodic and private records on the other. A heuristic for identifying data from running records relevant to a given research question is to ask: 'At what points and in what ways in society is information logged about social behaviour?' The same heuristic can be applied to identifying at least some kinds of **episodic records**, given their largely bureaucratic character. More generally, however, although Webb *et al.* (1981: 193–4) recommend that greater effort be put into identifying and locating potential caches of data, to use Glaser and Strauss's (1967) term, they do not suggest ways of doing so. Relevant heuristics, in other words, must be found elsewhere. In information science the growth of the World Wide Web in particular has encouraged renewed interest in how researchers 'forage' for sources of documentary data (Cronin and Hert 1995). If one takes this metaphor seriously, an appropriate heuristic might be to treat repositories as field sites, and to adapt to documentary work the sampling strategies typical of field research (Glassner and Corzine 1982; Helmericks *et al.* 1991). Drawing on procedures advocated by Helmericks *et al.*, documentary sources might be regarded as research 'sites', and their producers as 'informants'. Using personal knowledge, knowledgeable others and published guides, procedures analogous to snowball sampling can then be put in train. When no more likely sites are generated, sampling stops.

Conclusion

Although the defects of self-report methods can be exaggerated, a case can be made that social scientists rely too heavily on direct elicitation of information

from research subjects. The presence of the researcher potentially has consequences for the quality of responses, typically shaping them in socially patterned ways. In addition, research based on self-report is vulnerable to the social factors affecting both the availability of research participants and their willingness to respond to researchers' questions.

Unobtrusive methods commend themselves as ways of producing data complementary to direct elicitation methods, but with different weaknesses and strengths. In their own right unobtrusive methods can also provide an alternative where direct elicitation is, for various reasons, difficult or dangerous. A problem is, however, that existing approaches to the generation of unobtrusive measures, whether orientational or taxonomic in character, are unsatisfactory. It might be more appropriate to develop heuristic strategies for identifying ways of generating non-reactive data for specific research situations.

In the preface to the 1966 edition Webb *et al.* record that *Unobtrusive Measures* emerged only late on as a title for the book. While in preparation it had a variety of working titles including *Oddball Research*; for a time it was even cryptically called *The Bullfighter's Beard* (Webb *et al.* 1966: v). (It seems that the length of a bullfighter's stubble is an unobtrusive measure of anxiety, since his trembling hand on the morning of the bullfight ensures that he doesn't shave too closely.) Although there is no necessary inconsistency between humour and academic writing, humorous writing in the social sciences is sparse. Where it exists, such writing has tended towards the ironic, sometimes with a sarcastic or satirical edge, or, perhaps less commonly, has taken the form of parody (Jones 1980; Fine and Martin 1990; Fine 1994). Such humour as *Unobtrusive Measures* possesses derives from its irreverent tone. But this is irreverence with a purpose. There is in *Unobtrusive Measures* a rather mild echo of the critique of 'abstracted empiricism' offered by C. Wright Mills (1959). Mills challenged what he saw as the growing dominance of a new kind of research practitioner, the survey technician, who was completely at home with the technical requirements of the research process but who was devoid of imagination and creativity. Webb *et al.* scarcely go very far in this direction. Nevertheless, there is a clear underlying tone in *Unobtrusive Measures* that implies that much existing work is pedestrian, routine and lacking in imagination.

It can be argued that unobtrusive methods are valuable in themselves because they encourage playful and creative approaches to data, undermining in the process the tendency to rely on particular research methods because they are familiar or routine rather than appropriate to the problem in hand. What this further implies is an eclectic, not to say ecumenical, stance towards data collection. Social scientists can find useful data sources by drawing on other methodological traditions. Physical trace data require one to think like a forensic scientist. Data capture methods reflect the sensibilities of the ethologist. Documentary research draws on the insights of

historiography and cultural studies. All of these things, Webb *et al.* contend, should be part of the normal methodological repertoire of the social scientist.

Recommended reading

- Webb, E. J., Campbell, D. T., Schwartz, R. D. and Sechrest, L. (1966) *Unobtrusive Measures: Nonreactive Research in the Social Sciences*. Chicago, IL: Rand McNally. (The original, now classic, treatment of unobtrusive methods in social research. Engagingly written, and full of unusual examples.)
- Webb, E. J., Campbell, D. T., Schwartz, R. D., Sechrest, L. and Grove, J. B. (1981) *Nonreactive Measures in the Social Sciences*. Dallas, TX: Houghton Mifflin. (*Unobtrusive Measures* revised, updated and retitled.)
- Sechrest, L. (1979) *Unobtrusive Measurement Today*. San Francisco, CA: Jossey-Bass. (A collection of theoretically oriented articles on the generation and use of unobtrusive methods in a variety of fields and disciplines.)
- Webb, E. and Weick, K. E. (1983) Unobtrusive measures in organizational theory: a reminder, in J. Van Maanen (ed.) *Qualitative Methodology*. Beverly Hills, CA: Sage. (A succinct statement of the 'orientational' approach to the generation of unobtrusive measures, with a particular focus on organizational research.)
- Kellehear, A. (1993) *The Unobtrusive Researcher: A Guide to Methods*. St Leonards, NSW: Allen & Unwin. (Short, student-friendly introduction to unobtrusive methods. Good on practicalities but rather weak on analytic topics.)