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2

THE HISTORY OF THE INTERVIEW

◆ Jennifer Platt

The "interview" has existed, and changed over time, both as a practice and as a methodological term in current use. However, the practice has not always been theorized or distinguished from other modes of acquiring information; there have been some cases of practices that we would today describe as interviewing, although contemporaries did not. Interviewing has sometimes been treated as a distinct method, but more often it has been located within some broader methodological category, such as "survey," "case study," or "life story."

At each stage, more fully institutionalized practices have been less likely to be written about in detail, except for trainees; we must therefore exercise caution in generalizing from the prescriptive literature to current practice. In principle, my aim in this chapter is to look at both the theorization and the practice of the interview, without assuming that there has always been a close correspondence between the two. But interview *practice* has been very unevenly described. It is most common for interview practice to be described when some aspect of that practice becomes salient because what has been done is seen as novel, or unconventional. Even then, what is described is commonly a policy or strategy rather than the actual practice, which in reality may not always conform to the stated policy. This creates a problem of data, so for this historical account I must draw largely on prescriptions for practice as it should be.

I have decided to concentrate here on the book literature, although many articles have appeared on aspects of interviewing. It is my assumption that the main points in the journal literature are soon taken up in books if they are practically influential, so an emphasis on the book literature should be adequate for a broad overview of the pattern of development. It is with regret that I have also decided, given the limitations of space, to focus entirely on the U.S. experience. For the pre-World War II period, especially its earlier part, this is quite misleading, as other national sociologies

Table 2.1   GENRES OF BOOKS RELATED TO INTERVIEWING

Genre	Examples
Practitioner textbooks	Garrett, *Interviewing: Its Principles and Method* (1942)
Polling and market research practice	Gallup, *A Guide to Public Opinion Polls* (1944); American Marketing Association, *The Technique of Marketing Research* (1937)
Social science methods textbooks	Goode and Hatt, *Methods in Social Research* (1952)
Instructions to survey interviewers	University of Michigan, Survey Research Center, *Manual for Interviewers* (1954)
Critiques of method, general or particular	Christie and Jahoda, *Studies in the Scope and Method of "The Authoritarian Personality"* (1954); Cicourel, *Method and Measurement in Sociology* (1964)
Empirical work discussing its methods	Kinsey, Pomeroy, and Martin, *Sexual Behavior in the Human Male* (1948)
Handbooks	Denzin and Lincoln, *Handbook of Qualitative Research* (2000)
Monographs on special groups, novel approaches	Dexter, *Elite and Specialized Interviewing* (1970); Douglas, *Creative Interviewing* (1985)
Philosophical/theoretical discussion	Sjoberg and Nett, *A Methodology for Social Research* (1968)
Reports of methodological research	Hyman, *Interviewing in Social Research* (1954)

had some of their own distinct traditions and discussion. From about 1945 to 1960, U.S. social science and the survey became so hegemonic elsewhere that the U.S. literature can perhaps be treated as representing the whole; after the high period of hegemony, that becomes less reasonable.[1] Because I am a sociologist, this chapter is unavoidably written from a sociologist's perspective; the most likely bias is one toward work that sociologists have used and treated as important, whether or not the authors were sociologists. The choices of work to review might well differ somewhat if I were equally familiar with anthropology, political science, and psychology; scholars from other backgrounds are invited to supplement my examples with their own.

The U.S. book literature on interviewing can be broken down into a number of cate-gories, of which some illustrative examples are listed in Table 2.1. (Where possible, these are chosen from works not extensively discussed below, to indicate more of the range of material drawn on.) There are a number of relatively distinct intellectual and practical traditions, despite overlaps and some strong influences across traditions, and that needs to be taken into account in any discussion of the stances and concerns of single texts.

It is not always easy to decide what in the literature should be treated as a part of interviewing as such; for instance, some discussions of questions to be put in an interview are only about the construction of schedules, without reference to how those are presented to the respondent, and many discussions of the interviewer's role include sections about sampling decisions that may fall to the interviewer. For the purposes of this chapter, my focus is on what happens while the interviewer is in contact with the respondent.

I concentrate here on social scientific interviewing, but that has not always been distinguished from the interviewing techniques of psychiatrists, social caseworkers, or personnel managers. When distinctions have been made in the literature, social scientists still have often drawn upon work in such fields. But the character of the literature has changed historically. The earliest relevant work was not specifically social scientific. As new practices and bodies (such as polling and survey organizations) emerged, they generated writing that expressed their concerns, and their professional commitment to work in the same area led to methodological research concerning issues in which they were interested.

Once an orthodoxy was established, there was room for critiques of it and declarations of independence from it. Those working on special groups developed special ways of dealing with them; then, with an understandable lag, theorists began to take an interest in more philosophical aspects of interviewing. Textbooks regularly strove to keep up with the main developments, whereas authors of empirical studies wrote about the special experiences and needs of their particular topics. In later times, as quantitative and qualitative worlds became increasingly separate, discussions of interviewing diverged correspondingly. The quantitativists carried forward an established tradition with increasing sophistication, from time to time taking on technical innovations such as telephone interviewing, while qualitative workers blossomed out into focus groups, life histories, and own-brand novelties. However, an interesting recent link has been reestablished between the qualitative and quantitative camps in the use by surveyors of conversation-analytic techniques to analyze what is happening in their questions and answers.

In the rest of this chapter, I sketch the trajectory of the field of interviewing by using selected examples of such writings, starting with the prescriptive methodological literature and going on to empirical work that has been treated as methodologically important. I then review some key analytic themes in the literature. I consider the literature of research on interviewing as much for what the issues reflected there show us about the researchers' focuses of interest as for what the findings have been, although research has surely influenced practice. I briefly explore the interlinked issues of changing interest in and thinking about validity, the conceptions held of appropriate social relations between interviewer and respondent, and the types of data sought by those working in different styles; I make a particular effort to draw out points of potential interest to researchers whose concern is less with the history as such than it is with informing their own practice. Finally, I draw the strands of the discussion together to present a synthetic account of the ways in which interviewing and thinking about it have changed over time.

◆ The Trajectory of Change in Methodological Writing

To give a sense of the broad trajectory of change in methodological writing about interviewing, I present below, in order of historical appearance, descriptions of some arguably representative accounts of interviewing, its forms and purposes. I outline key points of content and assumptions, and briefly place each in its context.

HOWARD W. ODUM AND KATHARINE JOCHER

An Introduction to Social Research (1929)

Odum and Jocher's volume is one of the first general social science methods text-

books. In it, in addition to *interview*, the terms *schedule* (an instrument to be used by an enumerator) and *questionnaire* (an instrument to be answered unaided) are mentioned; for these, there is discussion of questions and presentation, but nothing on interviewing as such. (During this period the conduct of structured interviews was not treated as at all problematic, and so was hardly discussed.) Odum and Jocher state:

> An interview is made for the purpose of securing information ... about the informant himself, or about other persons or undertakings that he knows or is interested in. The purpose may be to secure a life history, to corroborate evidence got from other sources, to secure ... data which the informant possesses. [It] ... may also be the means of enlisting the informant's cooperation ... in the investigation, or ... advice ... in the procedure to be followed. ... If the student is not acquainted with the informant, some method of introduction through a mutual acquaintance should be secured. (Pp. 366-67)

They note also that the interviewer should request permission to take notes.

In the 1920s and 1930s, an "interview" was often, as here, assumed to be with a key informant or gatekeeper, rather than with a respondent who is merely one member of a sample (see Bingham and Moore 1931; Fry 1934). The implicit model of the old, fact-finding "survey" in the tradition of Charles Booth was still in the background, and Booth's data in the family in the street was provided by middle-class visitors (Bales 1991). The interviewee was thus seen as an informant about the situation studied, as much as or more than as being part of what was studied. Thus the respondent might be of status superior to the interviewer, another reason for an unstructured approach. This does not mean that no questionnaires were being used with mass samples, although these were not common yet in academic social science; rather, the use of questionnaires was seen as a distinct method. (The lack of development of theories of sampling meant that successful contact with previously identified mass respondents was not yet felt as a need.) It was often recommended that notes should not be taken during the interview, or only to a minimal extent; rather, recording should be done as soon as possible afterward. Questions might not be revealed, or might be written on the back of an envelope to appear informal and spontaneous (see Converse 1987:51). Clearly the role of respondent was not felt to be sufficiently institutionalized for no concealment of the mechanics to be necessary.

PAULINE V. YOUNG
Scientific Social Surveys and Research (1939)

In Young's very successful general methods textbook, *interview* is again distinguished from *schedule* and *questionnaire*, which are dealt with separately. Young distinguishes between respondents who are adequate sources on factual matters and those who are of interest as subjects individually or in relation to the larger situation. A personal introduction to the subject is still seen as desirable: "The interview proper does not begin until a considerable degree of rapport has been established. ... The most important touchstone is probably the mutual discovery of common experiences" (p. 189).

What does Young see as the value of the interview?

> The personal interview is penetrating; it goes to the "living source." "Through it the student ... is able to go behind mere outward behavior and phenomena. He can secure accounts of events and processes as they are reflected in personal experiences, in social attitudes. He can check inferences and external observations by a vital account of the persons who are being observed. . . .

> the field worker ... needs to know in a general way why he is interviewing this particular person or group and what he intends asking. Too rigid definition is, of course, fatal to any scientific pursuit; the mind of the interviewer needs to be open to unforeseen developments. ... Before the interview proceeds very far the interviewer should aim to learn the interviewees' point of view, their habitual reactions to the social situations under consideration, their opportunities for and degree of familiarity with these situations, their ability to give an accurate and unbiased account. (Pp. 175, 179)

She advises that the interviewer ask as few questions as possible:

> When people are least interrupted, when they can tell their stories in their own way ... they can react naturally and freely and express themselves fully. ... [Interruptions and leading questions are likely to have the effect that] ... the adventure into the unknown, into uncharted and hitherto undisclosed spheres, has been destroyed. (P. 190)

Young notes that it is rarely advisable to complete an interview at one sitting (p. 195). She also asserts that it is better for the interviewer not to take notes, except maybe a few key words, and states that there is some controversy as to whether the interviewer should record the interview in first or third person and whether a verbatim account is to be preferred to a summary by the interviewer (pp. 196, 200).

Young was at the University of Southern California, which was oriented toward the training of practitioners; her *Interviewing in Social Work* (1935) was widely cited in sociology when there were few other such sources available. Its perceived relevance owed something to the widespread use by sociologists of case histories collected by social workers, especially at the University of Chicago, where Young was trained; this connects with the idea of the case study and of the significance of life history data, which are clearly the contexts she has in mind in the passages quoted above (Platt 1996:46). One may also perhaps detect formative traces of the participant observation she used in her doctoral work. George A. Lundberg's (1942) important—and intellectually far superior—textbook takes a similar approach, although with a slight twist in the direction of the more modern concern with personality and psychoanalytic interests. Lundberg's own tastes were strongly scientistic, but it is interesting that he still offered advice on ways of gaining the confidence of the informant (see below) of a kind that would soon be regarded as thoroughly unacceptable.

In the 1949 edition of her text, Young mentions the modern survey, although she is still far from treating it as the paradigm:

> A specialized form of the interview is useful in the collection of personal data for quantitative purposes. This type of interview aims to accumulate a variety of uniform responses to a wide scope of predetermined specific questions. (Generally these questions appear on a printed form.) (P. 244)

This distanced account was in effect one of the last traces of an older conception of the interview.

CHARLES F. CANNELL AND ROBERT L. KAHN
"The Collection of Data By Interviewing" (1953)

Cannell and Kahn produced this chapter for what became one of the standard general-methods textbooks, written by a group from the Institute for Social Research (ISR) at the University of Michigan. Cannell and Kahn, a clinical and a social psychologist, were members of a team that started in the Department of Agriculture before the war, became the wartime Division of Program

Surveys (DPS), and after the war transformed itself into the ISR. In this chapter they attempt to go beyond current rules of thumb and to draw on work in counseling and communication theory to understand the psychology of the interview. (Their later book *The Dynamics of Interviewing* carries this forward, coming to the formulation of objectives and questions only after three chapters on the interviewing relationship; Kahn and Cannell 1957.)

Note, in the following quotation, the relatively qualitative orientation, which nonetheless goes with a strong commitment to scientific procedure; one may detect some tension between the two:

> Even when the research objectives call for information which is beyond the individual's power to provide directly, the interview is often an effective means of obtaining the desired data [e.g., Adorno et al.'s rating of anti-Semitism or personality features]. . . . Bias and lack of training make it impossible for an individual to provide such intimate information about himself, even if he is motivated to the utmost frankness. But only he can provide the data about his attitudes towards his parents, colleagues, and members of minority groups from which some of his deeperlying characteristics can be inferred. . . . Considering . . . the interviewing process as a scientific technique implies that we are able, through the application of a specific instrument in a specific manner, to achieve identical results in given situations . . . [but] the interviewer cannot apply unvaryingly a specified set of techniques, because he is dealing with a varying situation. . . . [Given that] we cannot tailor the question for each respondent, the best approximation to a standard stimulus is to word the question at a level which is understandable to all respondents and then to ask the question of each respondent in identical fashion. . . . The only instance in which the interviewer is permitted to vary this procedure is when

> an individual is unable to understand the question as worded. . . . the interviewer's role with respect to the questionnaire is to treat it as a scientific instrument designed to administer a constant stimulus to a population of respondents. This technique is necessary when quantifiable data are desired. (Pp. 332, 358)

Cannell was a research student of Carl Rogers, recruited by Rensis Likert to the DPS to draw on what he had learned with Rogers about nondirective styles of questioning. It is assumed in the book of which Cannell and Kahn's chapter is a part that an interview schedule is used, but this heritage was shown in the team's long-term commitment to more open-ended questions than those favored by other teams and explains some of the assumptions made here about interviewing. At an early stage there was controversy between the proponents of closed- and open-ended questions, contrasted by one participant within the DPS as the "neat reliables" and the "sloppy valids." This was reflected in a classic paper by Paul F. Lazarsfeld (1944) in which he aimed to resolve the conflict between two wartime research outfits with divergent styles. Converse (1987:195-202) shows that the dispute was as much about the costs of more open-ended work, and whether the gains were worth it, as it was about validity. It became evident even to those committed in principle to the open style that it not only created coding problems, it was impossible to sustain when less educated interviewers were used, and interviewers were based all across the country, so that training and supervision were difficult.

CLAIRE SELLTIZ, MARIE JAHODA, MORTON DEUTSCH, AND STUART W. COOK

Research Methods in Social Relations (1965)

This classic textbook, written by psychologists, has passed through many edi-

tions. Selltiz et al. still distinguish between *interview* and *questionnaire*, seeing the interview, which may be structured or unstructured, as practically advantageous because it does not require literacy, has a better response rate than postal questionnaires, and is the more flexible and "the more appropriate technique for revealing information about complex, emotionally laden subjects, or for probing the sentiments that may underlie an expressed opinion" (p. 242). However, much of the discussion concerns question wording, with no distinction made between interview and questionnaire, and clearly a standard survey interview, by now well established, is what the authors have in mind. They note that the interviewer should put the respondent at ease and create a friendly atmosphere, but "must keep the direction of the interview in his own hands, discouraging irrelevant conversation and endeavouring to keep the respondent to the point" (p. 576); the interviewer must ask the questions exactly as worded and not give impromptu explanations. Complete verbatim recording is needed for free-answer questions, "aside from obvious irrelevancies and repetitions" (p. 580).

This shows development well beyond the approach of George Gallup (1944) in early work conducting the simple political poll designed for newspaper rather than academic publication. The interview there was unequivocally designed for quantification of the responses made to fixed questions by members of the general public. The need for accuracy and precision was emphasized, but uniformity of stimulus was not given the importance that it later acquired; reliability was seen primarily in terms of getting the public predictions right. Many of those involved in the early development of polling and market research into the survey were psychologists, and for them the experiment was usually the model, so they laid great emphasis, as here, on the importance of applying a uniform stimulus.

GIDEON SJOBERG AND ROGER NETT

A Methodology for Social Research (1968)

This book represents quite a new genre of work, reflecting wider movements in sociology. Sjoberg and Nett were not closely involved with survey units and were writing not a conventional methods text but a textbook/monograph with a standpoint: "The scientist who employs . . . [structured interviews] is usually intent upon testing an existing set of hypotheses; he is less concerned with discovery per se. And, of course, standardization greatly enhances reliability" (p. 193). Standardization also saves time and money. However, it has the drawback of imposing the investigator's categories on informants:

> The unstructured type is most useful for studying the normative structure of organizations, for establishing classes, and for discovering the existence of possible social patterns (rather than the formal testing of propositions concerning the existence of given patterns). (P. 195)

Sjoberg and Nett describe four types of unstructured interviews: the free-association method interview, the focused interview, the objectifying interview, and the group interview. Of these, they prefer the objectifying interview:

> The researcher informs the interviewee from the start . . . concerning the kinds of information he is seeking and why. The informant is apprised of his role in the scientific process and is encouraged to develop his skills in observation (and even in interpretation). . . . Besides examining his own actions, the interviewee is encouraged to observe and interpret the behavior of his associates in his social group. Ideally, he becomes a peer with whom the scientist can objec-

tively discuss the ongoing system, to the extent that he is encouraged to criticize the scientist's observations and interpretations. (P. 214)

Throughout the discussion, Sjoberg and Nett stress the social assumptions built into different choices of questions. They discuss status effects in the interview situation and the consequences of varying cultural backgrounds, especially for work in the Third World.

These authors approach the matter from a theoretical and—in a turn characteristic of the period—sociopolitical perspective; they propose to involve the respondent as an equal, not so much for instrumental reasons of technical efficacy as because they see a nonhierarchical, nonexploitative relationship as intrinsically right. It is also notable that this is a sociologists' version; there is no orientation to psychologists' usual concerns, and the topics envisaged are sociological ones. Although Johan Galtung (1967) and Norman K. Denzin (1970) wrote books that are more like conventional methods texts, theirs have key features in common with Sjoberg and Nett's: more theoretical and philosophical interests, a more distanced approach to surveys and their mundane practicalities, and a clearly sociological frame of reference. Interviewing of various kinds had by this period become a standard practice to which even those with theoretical interests related their ideas.

STEVEN J. TAYLOR
AND ROBERT BOGDAN

Introduction to Qualitative
Research Methods (1984)

Taylor and Bogdan produced a specialized methods textbook, again with a strong standpoint:

In stark contrast to structured interviewing qualitative interviewing is flexible and dynamic. . . . By in-depth qualitative interviewing we mean repeated face-to-face encounters between the researcher and informants directed toward understanding informants' perspectives on their lives, experiences, or situations as expressed in their own words. The in-depth interview is modeled after a conversation between equals, rather than a formal question-and-answer exchange. Far from being a robotlike data collector, the interviewer, not an interview schedule or protocol, is the research tool. The role entails not merely obtaining answers, but learning what questions to ask and how to ask them. (P. 77)

Taylor and Bogdan note that without direct observation to give context to what people say in an interview, their responses may not be adequately understood, and there may be problems of deception and distortion; it is important, therefore, to interview in depth (see Johnson, Chapter 5, this volume),

getting to know people well enough to understand what they mean and creating an atmosphere in which they are likely to talk freely. . . . it is only by designing the interview along the lines of natural interaction that the interviewer can tap into what is important to people. In fact, the interviewer has many parallels in everyday life: "the good listener" "the shoulder to cry on," "the confidante." . . . there has to be some exchange in terms of what interviewers say about themselves. . . . The best advice is to be discreet in the interview, but to talk about yourself in other situations. You should be willing to relate to informants in terms other than interviewer/informant. Interviewers can serve as errand-runners, drivers, babysitters, advocates. (Pp. 83, 101)

This reaction against "robotlike" standard survey interviewing is part of the growth of a separate "qualitative" stream that recommends many practices that have

previously been anathema to surveyors. It will be noted that the rhetoric is very distant from that of science. These authors often refer to the "Chicago school" as a model, drawing on a widely current image of it—if one more useful for ideological than for historical purposes (Platt 1996: 265-69). The ideal is clearly participant observation or ethnography, and this type of interviewing again blurs the boundary with that. It could not be adapted to large representative samples and makes implicit assumptions about what kinds of topic are of interest, which, one somehow infers, exclude (for instance) the demographic or economic. Other representatives of this broad tendency are Jack D. Douglas (1985) and James A. Holstein and Jaber F. Gubrium (1995).

Many feminists have practiced and argued in favor of similar styles on feminist grounds. Shulamit Reinharz (1992) suggests that interviewing appeals to feminists because it

offers researchers access to people's ideas, thoughts and memories in their own words rather than in the words of the researcher. This asset is particularly important for the study of women because [this] . . . is an antidote to centuries of ignoring women's ideas altogether or having men speak for women. (P. 19)

Reinharz points out, however, that declared feminists have also done positivistic research, and concludes by suggesting that close relations with every subject are not practicable, and that too much emphasis on rapport may limit the range of topics covered unduly (see Reinharz and Chase, Chapter 11, this volume). It is notable that the work she cites in the chapter from which I quote above is almost all on such topics as rape and hysterectomy. Others have pointed out that many of the arguments used by feminists as though they were specific to the study of women can be seen as equally applicable to men.

One might speculate on how much of this tendency rests on the increased availability of good-quality portable tape recorders; the assumptions made about what it is practical to record have not been much examined, and research on the consequences for practice of changing techniques and technologies for the recording of free answers is strikingly absent.

◆ Empirical Work
and Its Influence

Important contributions to the discussion of interviewing have also been made by authors whose primary concerns were with their substantive topics; these do not necessarily relate directly to the professional methodological discussion and cannot be explained by their location within that. Below, I review some of these. It is probably not by chance that the empirical exemplars that come to mind, as well as much methodological research, are mainly from work done in the period 1935-55. This was the time when the modern survey was emerging, and so the problems that its practice raised were live ones being confronted and disputed for the first time, while its high profile and popularity also encouraged those with criticisms, or alternatives suited to less usual topics, to write about them. None of the exemplars employs a conventional, standardized survey because, where there is a structured schedule, the tradition has been to provide a copy of it without describing the interviewing process; what took place is implicitly assumed (not always rightly) to have been determined, and sufficiently described, by the schedule.

F. J. Roethlisberger and William J. Dickson ([1939] 1964) made an early contribution to unstructured interviewing technique —although the intellectual responsibility for this arguably lies more with Elton Mayo, who led the work. Mayo's ideas on method were influenced both by his inter-

est in Jungian psychoanalysis and by his friendship with the anthropologist and fieldwork pioneer Bronislaw Malinowski, whom he met in Australia.[2] Roethlisberger and Dickson began their interviewing program to collect employees' views about their work (for use in improving supervisor training) but found that the workers often wanted to talk about "irrelevant" material, so in 1929 the decision was made to adopt an "indirect approach," following the workers' leads without changing the subject and asking only noncommittal questions. Interviews were now recorded as far as possible verbatim, rather than under target headings, and the data were seen as information not so much on real problems as on the meanings that the workers gave to the realities. "Rules of Performance" were set up, such as "Listen in a patient, friendly but intelligently critical manner" and "Do not display any kind of authority," but these rules were to be treated as flexible: "If the interviewer understands what he is doing and is in active touch with the actual situation, he has extreme latitude in what he can do" (Roethlisberger and Dickson [1939] 1964:286-87). Years of training were necessary for such interviewing. The interviewing program was not initially intended for social scientific purposes, but it came to be used for social science.

W. Lloyd Warner and Paul S. Lunt (1941) said that in their work they used techniques suggested by Roethlisberger and Dickson, although their research, an intensive community study, was of a very different character. However, Warner was an anthropologist by training, and the anthropological fieldwork tradition seems more relevant to its general style. Many of Warner and Lunt's "interviews" were done without the subjects' awareness of being interviewed, and interviewing shaded over into observation: "The activity of the investigator has been classed as observation when the emphasis fell on the observer's seeing behavior of an individual; as interviewing, when emphasis fell on listening to what was said" (p. 46). These authors expressed great

skepticism about the utility of questionnaires, which they saw as liable to take items out of their social context and useful only when one is already familiar with the general situation from interviews (pp. 55-56). Although they described their main method as interviewing, this should probably be regarded primarily as part of the history of what we now call participant observation.

The next example, Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E. Martin's *Sexual Behavior in the Human Male* (1948), is more idiosyncratic. Kinsey was a professor of zoology and devised techniques to suit his special topic. There was a list of items to be covered in the interview, but no fixed order or standardized wording for them. Additional questions were designed for subjects with uncommon ranges of experience. The questions placed the burden of denial of sexual practices on the subject and were asked very rapidly to increase the spontaneity of answers (pp. 50-54). Interviewer neutrality was not valued:

> Something more than cold objectivity is needed in dealing with human subjects. . . . The interviewer who senses what these things can mean . . . is more effective, though he may not be altogether neutral. The sympathetic interviewer records his reactions in ways that may not involve spoken words but which are, nonetheless, readily comprehended by most people. . . . These are the things that . . . can never be done through a written questionnaire, or even through a directed interview in which the questions are formalized and the confines of the investigation strictly limited. (P. 42)

The researchers' aims were not at all concealed from respondents, and if a respondent appeared not to be answering truthfully, the interview was broken off. Very lengthy training was again seen as nec-

essary for the interviewers, who were also required, in the interests of confidentiality, to memorize a large number of codes to record respondents' answers. Any use of this method by others has not been identified in the mainstream sociological literature; Kinsey et al.'s reasoning suggests that it would be applicable only in areas posing the same problems as research into sexual behavior.

Radically different, almost equally famous, and more influential in social science method was Theodor W. Adorno, Else Frenkel-Brunswik, Daniel J. Levinson, and R. Nevitt Sanford's *The Authoritarian Personality* (1950). Here again there was a schedule, but interviewers were not expected to stick closely to its questions or order. The model followed was that of the psychotherapeutic encounter, and the instructions distinguished "underlying" and "manifest" questions. It was taken that "the subject's view of his own life . . . may be assumed to contain real information together with wishful—and fearful—distortions," and consequently methods were needed "to differentiate the more genuine, basic feelings, attitudes, and strivings from those of a more compensatory character behind which are hidden tendencies, frequently unknown to the subject himself, which are contrary to those manifested or verbalized on a surface level" (p. 293). Kinsey, too, distrusted overt statements of attitudes, but his solution was to ask only about behavior and (unless untruths were suspected) to accept what was offered at face value.

Perhaps surprisingly, given the lack of social scientific precedent for Kinsey's approach, Adorno and his associates were treated more harshly in published critiques. Kinsey et al. were criticized, but critics concluded that empirical evidence to show that their results were less valid than those of researchers who used alternative approaches was not available (Cochran, Mosteller, and Tukey 1954:78-79). Adorno and his colleagues, however, were accused of inconsistency and speculative overinterpretation of data not appropriate for their uses (Christie and Jahoda 1954:97, 100).

What might be seen as a more social version of such an approach, used to generate large ideas about historical change in American society, is shown in other work from the same period by David Riesman and associates. They carried out many interviews, but certainly did not take them at face value: "Everything conspired to lead to an emphasis not on the interview itself but on its interpretation . . . such a method . . . requires repeated reading of the interview record . . . in search of those small verbal nuances and occasional Freudian slips that might be clues to character" (Riesman and Glazer 1952:14-15). Of course, character as a topic hardly lends itself to direct questions of a factual nature, but the extent of "interpretation" here goes strikingly beyond the literal data. It is interesting that there are two books from the project: the main interpretive one, Riesman, Glazer, and Denney's *The Lonely Crowd* (1950), which contains almost no direct interview data, and Riesman and Glazer's *Faces in the Crowd* (1952), consisting mainly of raw interview data without analysis. The issue of how well the data support the interpretation is thus avoided.[3]

The genre of publication of raw interview data is one with a history—sometimes, like the work of Studs Terkel, a history not within academic social science, even if social scientists refer to it. However, material that looks raw may be at least lightly cooked. Terkel describes his own procedure: "*The* most important part of the work, is the editing of the transcripts . . . the cutting and shaping of it into a readable result. The way I look at it is I suppose something like the way a sculptor looks at a block of stone: inside it there's a shape which he'll find" (quoted in Parker 1997:169). Thus to treat the published version as showing just what took place in the interview would be quite misleading. Whole "life stories" have been published in sociology, although sometimes written by their subjects rather than elicited through interviewing;[4] the genre was treated as of central importance in the interwar period, and much more recently has been revived.

Some recent work on life stories takes a similar approach, on the one hand putting a very high value on the subject's own version of events, but on the other hand permitting the interviewer a considerable editorial role (e.g., Atkinson 1998; see also Atkinson, Chapter 6, this volume). Note that this, interestingly, shifts the stage intended as active researcher intervention from data elicitation, as with a questionnaire or interview guide, to data presentation. The version presented is, however, nearer to raw data than are the figures and tables of the quantitative tradition.

Topics of research have their own traditions and intrinsic needs (Platt 1996:129-30), and so some methodological ideas arise from the substance of the work being done: Kinsey et al.'s conceptions of interviewing technique followed directly from what they saw as the needs of work on sexual behavior. (On the other hand, Adorno et al.'s ideas followed as much from their general intellectual backgrounds as from the substantive topic.) One might expect the influence of such work to follow the same paths, although whether or not it has cannot be explored here. It is clear that the choices of method did not simply follow from the current state of methodological discussion, although the results fed into that, if only by evoking criticism. The level of attention paid to the methods of such work has depended on the extent to which it has departed from the survey paradigm as well as on the general interest in its substantive content.

◆ Some Analytic Themes

Discussions of empirical work take us a little nearer to what has happened in practice. Research on interviewing gives us one of the other windows through which we may see something of the actual conduct of the interview, as distinct from the prescriptions for it. Practice has often been indeed distinct. Interviewers are repeatedly shown to use their own ways of dealing with problems in eliciting the data wanted. Julius Roth (1966) long ago documented a few cases where research employees had, for their own reasons, departed from the investigator's plan in ways that damaged it. He argued that this was only to be expected when interviewers were employed as "hired hands," with no personal commitment to the research goal or control over content and methods.

More recent authors have also identified interviewer cheating. Jean Peneff (1988) observed some of the most experienced and valued interviewers working for a French governmental survey organization, all highly motivated, and found that they regularly adapted their behavior and language to the social context: "They intuitively improvised a blend of survey norms and fieldwork practices" (p. 533). He offers a less pessimistic perspective, querying whether departure from specifications should be regarded as "cheating"—although it tended to make what was intended as standard survey work more "qualitative." It sounds as though there was an implicit bargain between interviewers and their supervisors, in which good-quality work was exchanged for lack of close inquiry into the way in which the quality was achieved. (The great underresearched and undertheorized area of interviewing is that of the social relations between employed interviewers and their supervisors, and the consequences of those relations.) We do not know how far such patterns as those found by Peneff have held more widely, but we ought not to be surprised if sometimes they do.

But Roth's and Peneff's work is unusual. Research on interviewing has come overwhelmingly from those active in specialist survey units. (A list of main book sources presenting research on interviewing is given in Table 2.2.) It is not surprising that it should be those with continuing reason for professional concern with the matter who do such work, but this does mean that the research has been skewed toward their distinctive preoccupations. What was

Table 2.2	KEY WORKS PRESENTING RESEARCH AND ANALYSIS ON INTERVIEWING
1947	Hadley Cantril, *Gauging Public Opinion*
1954	Herbert H. Hyman, *Interviewing in Social Research*
1965	Stephen A. Richardson, Barbara Snell Dohrenwend, and David Klein, *Interviewing: Its Forms and Functions*
1969	Raymond L. Gorden, *Interviewing: Strategy, Techniques, and Tactics*
1974	Jean M. Converse and Howard Schuman, *Conversations at Random: Survey Research as Interviewers See It*
1979	Norman M. Bradburn and Seymour Sudman, *Improving Interview Method and Questionnaire Design*
1981	Charles F. Cannell, Peter Miller, and Lois Oksenberg, "Research on Interviewing Techniques"
1984	Charles Turner and Elizabeth Martin, eds., *Surveying Subjective Phenomena*
1990	Lucy Suchman and Brigitte Jordan, "Interactional Troubles in Face-to-Face Survey Interviews"
1991	Paul P. Biemer et al., eds., *Measurement Errors in Surveys*

problematic about interviewing for them can be seen from the topics researched, and it is from that point of view that some of their themes are considered.

A major preoccupation over the years has concerned the variation in answers elicited by different interviewers. This is commonly taken as the measure of "error," implying that validity is defined as arriving at the correct overall figures rather than as fully grasping individuals' meanings or correctly identifying their real opinions. Cantril (1947) suggested, for instance, that researchers could deal with the problem of interviewer biases by selecting interviewers with canceling biases. Other writers have seen careful selection of interviewers for their personal characteristics, whether of race or of personality, as valuable—although they often faced the fact that the real labor market made this difficult. Fowler (1991:260) points out that the conventional definition of "error" that he uses makes standardization across interviewers tautologically necessary to reduce error; this approach inevitably ignores the possibility that some nonstandardized interviewers might be better than others. In the earlier work, there was a strong tendency to blame the interviewers for problems, and to see greater control over interviewers as the answer to those problems.

An extreme of this definition of the situation is suggested by Norman Bradburn and Seymour Sudman's (1979) chapter on interviewer variations in asking questions, where the nonprogrammed interviewer behavior studied through tape recordings included such minutiae as stuttering, coughing, false starts, and corrected substitutions.[5] Converse and Howard Schuman (1974), in contrast, studied the interviewers' point of view and were not concerned primarily with their errors and how to control their behavior—which may owe something to the fact that the interviewers in question were graduate students, members of "us" rather than "them." Consequently,

Converse and Schuman emphasize the tensions their interviewers experienced between conflicting roles and expectations.

Later work, however, more often recognizes respondents' contributions and takes the interview as interaction more seriously. For Cannell, Peter V. Miller, and Lois Oksenberg (1981), the aim was to decrease error in reporting due to the respondent rather than to the interviewer. Because their study was on topics appearing in medical records—which could, unlike attitudes, be checked—they were able to identify some clear factual errors made by respondents. They found that interviewers were giving positive feedback for poor respondent performance, in the supposed interests of rapport, so that correction of this, and clearer guidance to respondents on what was expected of them, improved performance.

More recent writing about "cognitive" interviewing has revived the issue of accuracy in ways that deal seriously with the issue of validity, if only in relation to "factual" questions. Lucy Suchman and Brigitte Jordan (1990), anthropologists using a conversation-analytic perspective, stress the extent to which "the survey interview suppresses those interactional resources that routinely mediate uncertainties of relevance and interpretation" (p. 232), so that reliability is bought at the cost of validity. They recommend encouraging interviewers to play a more normal conversational role, so that respondents may correctly grasp the concepts used in the questions asked. Suchman and Jordan's article, which appeared in the *Journal of the American Statistical Association,* raised considerable discussion; perhaps its ideas would not have seemed so novel to the readership of a more social-scientific journal.

Nora Cate Schaeffer (1991) balances such considerations against the need for some uniformity if the answers are to be added to give a total. She points out that "artificiality" in the interview situation does not necessarily mean that the answers given are less valid, but that to elicit them as intended, the researcher needs to bear in mind the rules of interaction that the respondent brings to the situation. Michael Schober and Frederick Conrad (1997) have shown that less standardized and more conversational interviewing can markedly increase the accuracy of the responses given —by, for instance, allowing interviewers to help respondents fit their relatively complicated circumstances into the categories of answers provided. Schober and Conrad illustrate the self-defeating extremes to which the pursuit of the uniform stimulus has gone, with researchers forbidding interviewers even to provide guidance that would ensure that the meanings the researchers sought were indeed conveyed in the answers chosen.

Presumably, training for practice will follow the latest findings. It is notable, however, that most of the examples used in these recent discussions have been drawn from large-scale national surveys, often carried out for governmental purposes and with fact-finding as a key aim. This reflects the increasing tendency for academics to use data of high quality that they have not gathered for their own purposes, which has led discussion in the directions suitable to the character of such work, but not equally applicable to the whole range of potential surveys.

Schober and Conrad's study exemplifies a recurrent pattern, in which research shows that commonly taught practices do not necessarily have the intended effects. That the limited benefits of "rapport" for data quality have repeatedly been (re)discovered suggests that, for whatever reasons, practice has not always followed research-based conclusions, and that the folklore of the field has been powerful. Recommendations on the relations between interviewer and respondent have changed considerably, whether the aim is "rapport" or just access. One of the earliest statements on this topic was made by Walter Bingham and Bruce Moore (1931): "The interviewee is frank when he feels that his own point of view is appreciated and respected, that the interviewer has some right to the information, and that the questions are relevant and not impertinent" (p. 11). This is rationalistic, corresponding to the assumption that the respondent is of relatively high status and is being approached for factual information; it is not typical of later discussion with other assumptions.

When the interview is seen as deep and richly qualitative, or as a large-scale survey interview with members of the general public, other approaches follow. The early survey literature typically suggested that interviewers need to establish rapport to get access and cooperation, but that they should also, when questioning, appear unshockable, have no detectable personal opinions, and, behind the front of friendliness, be objective and scientific.[6] Not every writer offered as businesslike a conception of rapport as William J. Goode and Paul K. Hatt (1952): "A state of rapport exists between interviewer and respondent when the latter has accepted the research goals of the interviewer, and actively seeks to help him in obtaining the necessary information" (p. 190). But the ideal was clearly an instrumental relationship.

Before the modern survey was fully developed, it was often not seen as so important to keep the interviewer as a person out of the picture. Lundberg (1942) suggests several ways of getting an informant "started": "refer to important friends of the informant as if one were quite well acquainted with them; . . . tell of one's own experiences or problems and ask the informant's advice or reactions to them" (pp. 365-66). These are just the kinds of techniques that survey organizations trained their interviewers to avoid. I have quoted above Kinsey et al.'s (1948) advocacy of a less impersonal and unbiased style. Elements of such an approach have now come around again in recent qualitative work, where there has often been a sociopolitical commitment to treating the respondent as an equal. This is taken to imply the researcher's not playing a detached role while expecting the other partner to reveal him- or herself:

> We can no longer remain objective, faceless interviewers, but become human beings and must disclose ourselves, learning about ourselves as we try to learn about the other.
> . . . As long as . . . researchers continue to treat respondents as unimportant, faceless individuals whose only contribution is to fill one more boxed response, the answers we . . . will get will be commensurable with the questions we ask and with the way we ask them. (Fontana and Frey 1994:374)

This line can, however, be presented in a more manipulative way. In Douglas's (1985) unique style:

> Creative interviewing . . . involves the use of many strategies and tactics of interaction, largely based on an understanding of friendly feelings and intimacy, to optimize cooperative, mutual disclosure and a creative search for mutual understanding. . . . Most Goddesses feel the need for a significant amount of self-disclosure before they will . . . reveal their innermost selves in their most self-discrediting aspects. When they seem to be proceeding to the inner depths with reluctance, I normally try to lead the way with a significant bit of self-discrediting self-disclosure. (P. 122)

Research on the partners' perceptions of each other has shown that respondents do not necessarily detect interviewers' biases or manipulative strategies; to that extent the impulse is moral or political rather than scientific. The barrier between the role and the self is broken down—or is it? Is this just another mode of instrumental presentation of self, as fellow human rather than as detached professional?

A method of data collection that cannot make plausible claims to validity is of no

use, so it is surprising that widely discrepant levels of concern for validity, and conceptions of it, have been shown in relation to interviews. It has commonly been agreed that less rigidly structured methods may score higher on validity, although this has to be traded off against the greater reliability of more structured methods. But concern with the problem has come more from those who employ other people to do their interviews; those who carry out their own have usually seemed to regard their validity as self-evident, not requiring checks. This sometimes reflects a hostility toward "science" or "positivism" prevalent among qualitative researchers. However, in some of the literature on the standard survey there has also been surprisingly little concern about validity as such. The question of the substantive meaningfulness of the data, except on purely factual questions, somehow gets elided in the concern over interviewer error and questionnaire improvement.

Of course it is difficult in the survey, as in other contexts, to demonstrate validity, although some authors have suggested ways of doing so. Eleanor E. Maccoby and Nathan Maccoby (1954) proposed a traditional measure: "It remains to be seen whether unstandardized interviews have sufficiently greater validity so that ratings based upon them will predict criterion variables better than will ratings based on standardized interviews" (p. 454). Where there is a clear criterion to use as the standard of prediction, as in voting results, that standard has been used. But for many topics there are no criteria. There has been some discussion in terms of whether the respondent is telling the truth. Kinsey et al. (1948) take an inimitably robust stand on this:

> It has been asked how it is possible for an interviewer to know whether people are telling the truth. . . . As well ask a horse trader how he knows when to close a bargain! The experienced interviewer knows when he has established a sufficient rapport to obtain an honest record. (P. 43)

Even if one accepts the horse-trading approach as adequate, it could be applied only in relatively deep and unstructured types of interviews, where the interviewer has time to establish a relationship. For the in-depth or psychoanalytic interview, of course, the issue of validity has not arisen in the same sense, because the focus has been on the interpretations made by the analyst rather than on correct factuality. Warner and Lunt (1941) take a different approach:

> The information gathered about social relations is always social fact if the informant believes it, and it is always fact of another kind if he tells it and does not believe it. If the informant does not believe it, the lie he tells is frequently more valuable as a lead to understanding his behavior or that of others than the truth. (P. 52)

Warner and Lunt assume that the researcher will have ways of *knowing* that the respondent is lying. In intensive, long-term studies of a community, like Warner and Lunt's, that is a relatively plausible assumption; Arthur Vidich and Joseph Bensman (1954) have also reported detecting much intentional misrepresentation. Plainly, however, this assumption would not be met in many other cases.

Galtung (1967) is one of the earliest representatives of what might be seen as a truly sociological position, even if not one that exactly solves the problem:

> The spoken word is a social act, the inner thought is not, and the sociologist has good reasons to be most interested and concerned with the former, the psychologist perhaps with the latter. But this only transforms the problem from the problem of correspondence between words and thoughts to the problem of how representative the interview situation is as social intercourse. (P. 124)

Holstein and Gubrium (1995), writing much more recently, take this one step further and, informed by ethnomethodological perspectives, stop worrying about such representativeness:

> One cannot expect answers on one occasion to replicate those on another because they emerge from different circumstances of production. Similarly, the validity of answers derives not from their correspondence to meanings held within the respondent but from their ability to convey situated experiential realities in terms that are locally comprehensible. (P. 9)

This assumes that there is no stable underlying reality to identify, thus, in a sense, abolishing the problem.

Elliot Mishler's (1986) emphasis on the interview response as a narrative in which the respondent makes sense of, and gives meaning to, experience has a similar stance. The issue has thus moved from the interview as an adequate measure of a reality external to it to the content of the interview as of interest in its own right. This is a long way from the concerns of some survey researchers to get correct reports concerning such matters as bathroom equipment and medical treatment received. All of the extreme perspectives on "the interview" have different paradigms in mind, as well as different research topics; each has shown little interest in the problems relevant to the needs and concerns of the others.

◆ The Historical Pattern

Not all of the work on interviewing fits into a clear historical pattern, and empirical studies may be idiosyncratic in relation to the methodological literature. Nonetheless, below I sketch a broad trajectory that summarizes major lines of thinking. The dates suggested are not meant to be precise, given that different workers move at different speeds.

Up to the late 1930s, *interview* was distinguished from *questionnaire*, which generally connoted a self-completed instrument; if an interview was administered by an interviewer, that person's contribution was not seen as requiring serious attention. The interview was unstructured, if with an agenda, and wide ranging; the interviewer was likely to be the researcher. Subjects were often used as informants with special knowledge to pass on, rather than as units to be quantified. This kind of interview was not strongly distinguished from interviews for job selection or journalism or, when "interviewing down," for social casework. (Indeed, data from social work interviews in particular were widely used by social scientists at a time when the idea that professors might themselves go into the field was a new one.) Little concern was shown for reliability or validity; a few rules of thumb were suggested for success. It was assumed that subjects might not accept overt interviewing of the modern kind, so some concealment was necessary. In parallel to this, however, much of what we might today call interviewing was done under such rubrics as "life history," "fieldwork," and "case study." For these, there was serious discussion of technical matters, such as how to keep the respondent talking without affecting the direction of the conversation too much (see, for example, Palmer 1928: 171-75).

Meanwhile, political polling and market research were developing. Here interviews were carried out by crews of interviewers instructed and supervised from the center. The private research agency came into existence, alongside developments within government. The modern survey began to emerge, and hence concern with the technique of interviewing within a relatively elaborate fixed schedule. Often, the work done was to be published in newspapers or was of direct commercial interest to the cli-

ent, which meant that predictions might be testable, and numerical accuracy became important. There were also repeated studies of similar kinds carried out by the same agencies. Reliability began to be taken seriously as the data to evaluate it became available, and this led to concern with "interviewer effects" and the control of the interviewing force.

The development of ideas about sampling was also important because it was only when, in the late 1930s, it began to be seen as desirable to have nationally representative samples that the issue of how to control a large, scattered, and not very highly trained body of interviewers came to the fore. Whatever the intellectual preferences of the surveyors, the realities of dealing with such a labor force had weight. Less was left to the interviewer's initiative, and training became more detailed and serious. Much of the work was done by psychologists, so an experimental and stimulus-response model was influential, and attitudes rather than factual information became a focus of interest.

The hothouse atmosphere of wartime research brought different strands of work together, and the modern survey emerged fully. There were controversies concerning structured versus unstructured approaches and open-ended versus closed questions, and different teams developed different styles, but there was much cooperation and consensus on many practical and technical issues. Nonexperimental aspects of psychology were prominent as inspiration; on the level of technique, Rogers's "nondirective" approach and psychoanalytic approaches were popular sources in the more qualitative styles. For those in the lead on survey research, however, question construction, sampling, and scaling became of more interest than interviewing as such. Researchers not in the survey world developed their own detailed qualitative techniques, often designed to deal with their own particular subject matters; some were heavily criticized by methodologists from

the perspectives that they had now developed.

After the war, new practices were incorporated into textbooks and training procedures. Systematic research on interviewing started, and it showed that some of the folk wisdom was unfounded. Social scientists turned to the survey as a major method, and it became a standard practice. Those out of favor defended alternatives, often under the banner of "participant observation" (Becker and Geer 1957). They differentiated their perspective from the survey by stressing direct observation over questioning, although certainly much "conversation with a purpose" (a frequently cited definition of *interview*) was part of their observation. Discussions of participant observation technique have, however, given attention to the social relations involved in such conversation rather than to the fine detail of what takes place in the encounter; obviously, repeated contacts with the same subjects raise different issues (see Atkinson and Coffey, Chapter 38, this volume).

Soon surveys were widespread enough for nonmethodologists to take an interest in them—although this interest was often skeptical. From the late 1960s, the upheaval in political and theoretical interests of the time was related to interviewing, and work was done on the implicit assumptions of interviewing in such matters as epistemology. Much more interest was shown in the social relations of interviewing; this was the heyday of reflexivity and autobiographical accounts of research. Specialist work on interviewing with particular groups (children, elites) also started to be written as the general application of survey method brought to light the special problems involved (see the contributions to Part II of this *Handbook*).

By the 1970s, interviewing was being taken for granted as an established practice in the survey world; specialists continued with increasingly sophisticated methodological research and refined details of method yet further, often in relation to new

technologies using telephones and/or computers. The qualitative world became ideologically more separate from quantitative research, and qualitative researchers developed their own discussions, which showed little concern with the technical issues they might have in common with the survey world. Social scientists active in the growing feminist movement often saw qualitative methods as particularly appropriate to women as subjects and developed ideas about the special requirements of a feminist approach. The barrier between interviewer and respondent was attacked, and efforts were made to define ways of co-opting respondents rather than using them; whether this has been successful, and how it feels from the respondent's point of view, has hardly been investigated.

There is a sense in which interviewing has come full circle. Although in its early beginnings the typical stance of the researcher toward mass respondents was that of the social worker rather than of the social equal, for some researchers the interviewer again has a high degree of freedom and initiative and may make direct use of personal experience in conversation with subjects.

In much of the survey world, however, the pattern has been different. From a starting point where the interviewer's behavior was not much programmed, it has gone through a phase of high programming with relatively unsophisticated techniques to one where the areas formerly left unspoken, such as probing, are themselves intended to be programmed. What really happened in the field might not live up to those hopes, but less was done "in the field" now. The coming of the telephone interviewing system opened up fresh possibilities of near-total surveillance and control of interviewer behavior. Thus the flexibility needed for adaptation to the respondent's needs became no longer an arena of initiative. But meanwhile, another strand of development, the "cognitive" approach, has reopened some of the earlier possibilities of

unprogrammed conversational initiative by the survey interviewer and shows an interesting convergence between otherwise very separate areas of work.

Nevertheless, the interview remains an area of richly diverse practice about which few convincing generalizations can be made. Some of the changes that have taken place over time have arisen internally, from methodological concerns—although just which methodological concerns have been salient has depended on the problems studied and on the organizational and technological frameworks within which particular studies have taken place. Other changes have responded to broader intellectual movements and to agendas defined in sociopolitical rather than methodological terms.

■ Notes

1. For readers who would like to look at some of the discussions within another national tradition, a few references to French work: Bizcul (1998), Blanchet and Gotman (1992), Demaziere and Dubar (1997), Mayer (1995), Michelat (1975). I am grateful to Jean Peneff and Pierre Fournier for drawing these references to my attention.

2. I am indebted to John Smith for details of Elton Mayo's background and methodological development.

3. Later, however, Riesman contributed, in his chapter in Lazarsfeld and Thielens's collection titled *The Academic Mind* (1958), what is in effect—although he does not present it as such—an extended research-based discussion of validity based on respondent reports on the experience of being interviewed.

4. James Bennett (1981) has suggested the circumstances under which some types of these appear appropriate.

5. Some kinds of error, such as mistakes in following the schedule's instructions regarding which question to ask next, have been eliminated by the computer-assisted methods now commonly used by survey organizations. Lars Lyberg and Daniel Kasprzyk (1991:257) point out, how-

ever, that errors specific to computer-assisted telephone interviewing (CATI) may also arise.

6. This is another area where CATI must have changed the issues, although it has been little written about from that point of view; perhaps the physical separation from the respondent has placed the focus on control of the interviewer rather than on understanding the respondent's reactions to the situation.

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